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IZA DP No. 17340

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ABSTRACT

Layoff Costs and Learning about Employer Financial Distress*

While many employees risk losing their job and having their career disrupted due to employers' financial distress, it is widely recognized that many leave their employer in anticipation of layoff. In this paper, we assess how employee costs of financial distress depend on employees learning and acting on future layoff risk. To this end, we use random assignment of bankruptcy judges as an instrument for employer shutdown and administrative data on petition and quit dates to examine how earnings costs are shaped by employee reallocation. We show that shutdown causes a 24% fall in earnings over a five-year period despite one-quarter of employees having already left their firm. We document substantial heterogeneity in reallocation and earnings losses, typically displaying an inverse relationship, with higher reallocation in strong labor markets and from high-wage firms. The reallocation attenuates earnings losses by about 50%, approximately equal to the insurance from taxes and transfers. To assess the value of information, we estimate a model where risk averse workers learn about distress, search for jobs and access public insurance. Using the model, we calculate that employees' willingness to pay for their current job increases by 14% when the firm is liquidated without any advance information. Our findings suggest that making firms' financial risk information more accessible to employees can yield important benefits.

JEL Classification: J62, J63, J65, J68, G33, G51, G52

Keywords: job loss, search, occupational choice, bankruptcy, unemployment insurance

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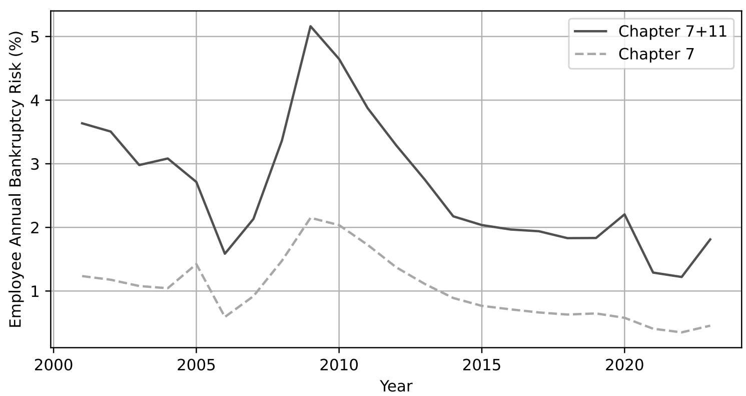
1 Introduction

Each year, approximately two percent of the US workforce risk losing their job due to employer financial distress. Figure 1 illustrates the fraction of jobs at risk in bankruptcy, peaking at almost five percent during the Great Recession, and falling to one and a half percent in the aftermath of the pandemic. While bankruptcy in Chapter 7 and 11 usually means that a large fraction of employees are laid off, often leading to long-lasting earnings losses (see, e.g., Lachowska *et al.*, 2020; Bertheau *et al.*, 2022; Schmieder *et al.*, 2023), it is also widely recognized that many workers leave in anticipation of future layoffs (e.g., Stephens, 2004, and Baghai *et al.*, 2021). Understanding the extent to which labor markets insure and absorb risk matters for optimal policy and debates over disruption costs from future technological advancements (Agrawal *et al.*, 2023).

Figure 1: Employee Bankruptcy Risk in the US



Notes: Own calculations using data from US Courts, CPS and Bernstein *et al.* (2019b). See Appendix B.3 for details.

However, assessing the employee costs of financial distress has proven difficult due to an adverse selection of employers and employees. First, employers in declining industries or firms that underperform relative to peers are more likely to go bankrupt. A consequence of this attrition is that workers in the least productive firms are more likely to experience the loss of an employer. Second, employers are likely to target the least profitable employees to lay off (e.g., Gibbons & Katz, 1991). Even when all workers remaining in the firm are laid off, as in Chapter 7 bankruptcy, learning that the job is at risk can induce quits in anticipation of costly layoffs. When workers with better outside options are more likely to leave, employees experiencing layoff may be those who are particularly disadvantaged.

In this paper, we address this selection using features of the Norwegian context that allow us to control for the quality of employers and employees who leave the firm before layoff begins. Our research design combines the random assignment of bankruptcy judges with administrative records of employment relationships, job openings and balance sheet information for employees and employers. There are three key ingredients to our empirical strategy. First, judges differ in their likelihood to liquidate otherwise similar firms, which allows us to address the selection of worker and employer quality. Second, information on creditor petition and employee quit dates allows us to account for workers who leave the firm before the petition is received. Third, we link this information with rich administrative data to perform a comprehensive analysis of the causal effects of shutting down a firm.

Our empirical analysis delivers three main conclusions. First, the loss of an employer causes a substantial loss to employees in firms that shut down. On average, liquidating a firm reduces earnings by one-quarter of pre-petition earnings over a five-year period. Second, job mobility rates in marginal, but non-liquidated firms, rise sharply the month the petition is filed – showing that costs are substantial even when many workers learn about the financial distress and switch employers before layoffs. Third, there is substantial heterogeneity in reallocation rates and size of earnings losses. While there are no differences in reallocation rates across skills or age groups, employees in high-wage firms and in stronger local labor markets leave their distressed employers at higher rates. Moreover, the size of earnings losses is inversely related to pre-bankruptcy employee reallocation rates: worker turnover rates are three times higher and earnings losses are halved when comparing outcomes after financial distress in a strong vs. weak labor market. In comparison, public insurance attenuates the net income loss by a roughly equal amount.

Understanding the welfare costs of financial distress requires a framework to account for how workers learn about employers' financial health and their labor market opportunities. Employees can only respond to layoff risks if they are aware of the financial circumstance of their employer. Moreover, their response depends on the value of a worker's current employer relative to alternative jobs: While workers with more to lose have a stronger motive to spend resources and effort to find new jobs to avoid future layoff, workers with less to lose from layoff may be more likely to find an equally well-paid job in the outside market (e.g., Caldwell & Danieli, 2023).

We address these conceptual challenges by developing and estimating a job ladder model. Central to our framework is that employees can learn about financial distress and face different local labor market conditions, in addition to making savings and job search decisions. Gross wages are determined by the heterogeneous productivity of an employer (i.e., a job ladder) and human capital—which accumulates over time—and income available for consumption is net after progressive taxes and unemployment insurance. We estimate the model using a combination of key population statistics and the causal evidence in two steps. First, some parameters are estimated directly from data. Second, we estimate the remaining parameters using the method of simulated moments to match our quasi-experimental findings. The model successfully replicates our key findings and matches several untargeted moments and estimates from empirical studies of unemployment benefits.

Next, we use the model to quantify the worker's willingness-to-pay (WTP) to keep the employer. Our WTP measure is the level of assets workers in liquidated firms would be willing to forgo to have their firm's bankruptcy case dismissed at the *time the judge makes the decision*. This captures not only the net present value of income lost from employer shutdown but also accounts for risk aversion and the cost of future search and expected human capital depreciation as workers foresee having to re-climb the job ladder. On average, the WTP is approximately equal to the net present value of the income loss but varies by the strength of the labor market. We find that the average WTP decreases by 0.21 percent for every one percent increase in the ratio of job openings to unemployed job seekers.

To explore the value of information and how it affects turnover dynamics, we conduct a counterfactual policy experiment that removes information about future layoffs. We label this experiment as "ideal" because it isolates the full value of an employer while also capturing the value of workers who might leave the firm before shutdown, irrespective of whether the employment relationships are of high- or low-value to the employee. This experiment allows us to calculate the insurance from information and labor market search by comparing the aggregate WTP with and without learning. Overall, we find that information reduces the welfare costs of layoffs by 12 percent. Next, we compare the wage losses of workers who stay with their employer until liquidation to the overall wage losses observed in the ideal experiment and the overall wage losses using our two-stage least-squares approach. This comparison allows us to calculate an implied bias from layoff regressions, arising from employees with relatively good outside options self-selecting out of distressed firms. On average, we calculate that this selection of high-value employment relationships increases the "measured earnings loss" by 10 percent.

Our paper advances research on sources of earnings losses following employer distress (see, e.g., Davis & von Wachter, 2011, and Carrington & Fallick, 2017 for reviews). In recent work, Lachowska *et al.* (2020) show that the loss of match-specific wage components are important in the US, while Schmieder *et al.* (2023) and Bertheau *et al.* (2022) find that employers play a more important role explaining earnings losses in Europe.[1] In a closely related paper, Rose & Shem-Tov (2023) first document earnings losses from mass layoffs in low-wage jobs, and then calibrate a dynamic job ladder model to quantify the willingness to pay to keep the job in the US. They find that workers are willing to pay more than half an annual salary to keep a job, indicating important frictions even in low-wage jobs.[2] Stepner (2019) shows that taxes play an important role mitigating the economic costs of layoff in the US, and Andersen *et al.* (2023) show that households cut down on private savings to maintain consumption levels after a mass layoff in Denmark.

Relative to existing research, we make several distinct contributions. First, we are the first to study how self-insurance from job mobility shape the earnings costs, and how it interacts with the

[1]Our paper is thematically related to research on the consequences of corporate bankruptcy, aiming to understand how employee costs in bankruptcy shape a firm's financing decisions (see Matsa, 2018 for a survey). In two closely related papers, Araujo *et al.* (2020) and Bonfim & Nogueira (2021) show that reorganization leads to higher employee post-bankruptcy wages than when firms are liquidated in the Brazilian and Portuguese bankruptcy systems. We contribute to this research by showing that accounting for public and private insurance is crucial to quantify the employee costs of financial distress, consistent with Agrawal & Matsa (2013), who show expanding unemployment insurance coverage can allow for more leveraged firms. Caggese *et al.* (2019) and Baghai *et al.* (2020) study worker reallocation in firms facing financial distress in Sweden.

[2]Huckfeldt (2022) shows that excess earnings loss during recessions is primarily driven by workers who switch to occupations that pay lower wages—consistent with a model of occupation-specific and endogenous hiring standards. Jarosch (2022) shows that much of the earnings loss comes from repeated separations from new jobs in non-desirable firms. Our human capital process is similar to Ljungqvist & Sargent (1998), Herkenhoff *et al.* (2023), and Huckfeldt (2022), but differs by letting human capital be specific to a career (i.e., it is lost when interrupted by involuntary layoff), which weakens the incentives to invest in the labor market as people near retirement age. Our quantitative assessment relates to Burdett *et al.* (2020) and Audoly *et al.* (2022), who incorporate general and specific skills in equilibrium models with on-the-job search to rationalize the earnings losses upon layoffs. We extend their frameworks by including savings, as in Lise (2013), and by allowing for advance information about layoff risks.

tax and transfer system. Second, we offer a clear source of randomness to layoff experiences and an event date that allows us to account for pre-layoff attentuation.[3] This combination allows us to address a lingering concern that employee turnover affects the composition of workers experiencing a layoff.[4] Our paper is also related to a recent literature on expectations and informational policies in labor markets (see Mueller & Spinnewijn, 2023 for a review). Cederlöf *et al.* (2021) show that longer periods of mandatory notice increase the time spent searching for a new job. Other papers have shown that many workers learn about risks informally. Brown & Matsa (2016) show that financially distressed firms receive fewer job applications, and Baghai *et al.* (2020) show that high-skill workers are more likely to leave in anticipation of bankruptcy. Hendren (2017) shows that knowledge of future unemployment risk affects the pool of workers entering unemployment, which understates the value of UI. In contrast, we show that advance information affects the out-migration from firms in distress—overstating the value of public insurance.[5]

We contribute to a large body of research studying the degree to which individuals are insured against unemployment and income shocks (see Meghir & Pistaferri, 2011 for a review). One branch of this research focuses on the impact of income shocks or unemployment on consumption expenditure, with seminal contributions by Cutler & Katz (1992) and Blundell *et al.* (2008). We expand on existing research by showing that anticipatory turnover and private savings also play an important role in insuring against layoff costs.[6] Another branch uses structural frameworks to examine the implications of unemployment insurance (UI), exemplified by the work of Saporta-Eksten (2014) and Braxton *et al.* (2020). While Low *et al.* (2010), Friedrich *et al.* (2019), and Liu (2019) extend the partial insurance framework with on-the-job search, the most closely related papers to our work are Lise (2013), who incorporates precautionary savings within a job ladder model, and Simmons (2021), who extends the job ladder model by allowing for partial information about layoff risk. We extend these papers by integrating a model of learning with both private and public insurance, and by providing evidence consistent with substantial insurance from job search.

[3] Our paper is related methodologically to several papers that use random assignment of judges and case examiners to examine a range of economic questions (see, e.g., Kling, 2006, Doyle, 2007, Aizer & Joseph J. Doyle (2013), Dahl *et al.*, 2014, Dobbie & Song, 2015, Galasso & Schankerman, 2015, Bhuller *et al.*, 2020, and Humphries *et al.*, 2019).

[4] Our results are consistent with the findings in Caldwell & Danieli (2023), who show that few outside options can explain a substantial fraction of earnings losses, and Cederlöf (2020), who shows that large layoffs lead to more pronounced earnings losses. Others have examined the implications of layoff misreporting for the empirical evidence (e.g., Flaaen *et al.*, 2019 and Birinci *et al.*, 2023).

[5] Ruhm (1992) studies the role of mandatory notification to employees targeted for layoff, and finds only small effects on non-employment spells. Ruhm (1994) finds that a combination of job counseling, training, and early warning leads to higher wages, while Cederlöf *et al.* (2021) provide evidence on a net productivity effect of mandatory notice in Sweden. Our evidence and framework is complementary to these studies. Ifergane *et al.* (2022) calibrates an equilibrium model of the labor market to quantify the insurance value of mandatory notice and understand how informational policies interact with traditional unemployment insurance.

[6] An extensive and growing body of research explores consumption changes around unemployment (see, e.g., Gruber, 1997, Chetty & Szeidl, 2007, and Ganong & Noel, 2019). Halla *et al.* (2020) and Andersen *et al.* (2023) find no role of the added worker effects following job displacement. Our paper also contributes to understanding the role of credit markets in mitigating the consequences of job loss. Keys (2018) shows that laid-off workers are more likely to go personally bankrupt, weakening the subsequent access to credit, while Braxton *et al.* (2020) show that existing credit lines provide valuable insurance to laid-off workers. Our evidence suggests that the credit market access mechanism remains available even after job loss but that people prefer to smooth consumption using assets rather than by increasing debt.

The remainder of the paper is organized as follows. Section 2 presents the data and empirical setting. Section 3 presents our instrumental variable strategy using judge fixed-effects to generate quasi-random variation in *employer shutdown.* Section 4 presents our main empirical findings. Section 5 develops a job search and consumption model and Section 6 brings it to the data. Section 7 uses the model to quantify the welfare implications of our quasi-experimental findings and to assess how worker turnover affects costs of financial distress. Section 8 concludes.

2 Institutional Setting, Data, and Descriptive Evidence

This section presents the Norwegian labor market institutions and the bankruptcy system, and describes the administrative data we will use.

2.1 Employment Protection and Unemployment Insurance

Employment protection in Norway is relatively strong, ranking about the median among OECD countries.[7] Firms can hire employees on either fixed-term or permanent contracts. Resigning employees must notify the employer three months prior to leaving. Similarly, firms planning to downsize are required to notify workers, where dismissal time varies from one to six months depending on age and tenure. Wrongful discharge can result in a lawsuit where firms must compensate dismissed employees for lost income.

The unemployment insurance system is funded by payroll taxes and there is no experience rating on firms. The cash benefits compensate 62 percent of lost wage income but only up to six times the social security base amounts (Folketrygdens grunnbeløp, about NOK 100,000 in 2021, NOK/USD≈10). This feature creates a kink in the benefit schedule, similar to the US (see, e.g., Landais, 2015), where cash benefits are constant, and replacement rates decline non-linearly after this point.[8] Appendix Figure A.1 plots unemployment rates in Norway from 2001 to 2016.

Layoffs due to bankruptcy differ from mass layoffs in a few important ways. First, there is no formal requirement of a notice period or dismissal time. Second, in contrast to mergers and acquisitions, workers lose tenure-based employment protection in bankruptcy. Third, a government wage guarantee scheme covers one month of unpaid wage bills and workers are eligible for unemployment insurance (UI) benefits immediately after bankruptcy is opened by the judge.[9]

[7] European labor laws regulate employment protection and mass layoffs. Union membership in Norway is relatively high compared to other countries in the OECD and the US. Still, it has fallen from 58 to 53 percent from 1992 to 2013 (OECD Statistics Trade Union Statistics, Accessed August 31st, 2020).

[8] The potential benefit duration is two years for everyone with previous income above two times the base amount. After UI benefits expire, workers may qualify for other transfers, such as vocational training or health-related benefits, typically replacing approximately two-thirds of lost income. The social assistance program is typically the last resort and is means-tested, with replacement rates varying across municipalities (see, e.g., Autor *et al.*, 2019).

[9] The wage insurance covers up to two base amounts takes (about NOK200,000) of non-paid wages the last six months and covers one month salary after the bankruptcy process starts. Retirement pensions are not held by the firm, but are managed by third-party financial institutions.

2.2 The Bankruptcy System

When a firm is temporarily or permanently unable to pay what it owes, it negotiates with its bank and other creditors outside the court system. The threat of liquidation is usually the creditors' outside option in the negotiation process. The debtor or its creditors (e.g., banks, suppliers, tax authorities, or employees) can file for bankruptcy, which is usually initiated after the cessation of payments and three months of unsuccessful debt enforcement. The tax authority and private creditors account for the vast majority of petitions.

Evaluating whether the firm is solvent requires discretion when assessing whether the financial distress is temporary and whether a firm's assets can be sold without business disruption. While these conditions apply uniformly to all firms, this discretion underlies significant variation in liquidation rates across judges, which we will use as part of our identification strategy in the next section. When the judge decides to open bankruptcy, a trustee is assigned to take control over the firm's assets and propose a plan to the judge. When the plan is approved, the trustee auctions the assets piece by piece or sells the firm to new owners, similar to Chapter 7 in the US system.[10]

Timing. Creditors must present documentation of whether the debtor firm is illiquid and insolvent. This information must be presented to the court in writing. When the petition is received in the district court, the case is assigned to a bankruptcy judge, who spends, on average, slightly more than one month examining the criteria for liquidating the firm. The case can be dismissed, meaning the firm can continue its operations, or the judge can start the liquidation process. Ninety-five percent of dismissed cases are processed within three months.

2.3 Data

Our empirical analysis is made possible by linking several data sources using anonymized identifiers of firms and their employees. The primary data source is the administrative employer-employee data, which offers detailed information about earnings and work contracts and covers transitions between firms from 1999 to 2018. We link this information with three additional data sources.

First—and central to our contribution—are the bankruptcy court files. These files cover every bankruptcy case filed from 2005 to 2018 and provide time stamps when cases enter the system. The data includes firm identifiers, anonymized identifiers for the assigned judge, and indicates whether or not the judge initiated the liquidation process. Second, we obtain information on individual wealth, and income sources—including bank deposits, capital income, and government

[10]Chapter 7 means liquidation through auction, with the goal of maximizing the recovery rate for creditors. The Norwegian bankruptcy system shares other similarities with the US system, for example, that cases are randomly assigned to judges (e.g., Bernstein *et al.*, 2019a). The priority of claims is the same, where administrative costs are paid before other claimants to the firm. Unpaid taxes, debts to government agencies, and wage claims have priority over secured creditors, and unsecured creditors get priority over shareholders. The major difference between the US and Norwegian bankruptcy systems is the use of reorganization. While this option is rarely used in Norway, Chapter 11 bankruptcy is the most common option among larger firms in the US. In 2019, 14,524 US firms filed for Chapter 7, while 5,814 filed for Chapter 11. Liquidation under US Chapter 7 is typically used by smaller firms (see, e.g., Table I in Bris *et al.*, 2006).

transfers—from tax records. We deflate income and wealth measures available from 1999 to 2017 to 2015-values using the consumer price index (CPI). To reduce noise from outliers, we winsorize market income and bank account statements at the 99th percentile and total debt and disposable income net of taxes at the 95th percentile. Third, we collect demographic information, such as gender, age, family composition, education of every resident since 1999, and participation in the social insurance program, such as unemployment insurance. The linked employer-employee data and income components from tax registers are third-party reported (e.g., by employers and financial intermediaries) and have been rated as exceptional by international data quality assessments (see, e.g. Atkinson *et al.*, 1995). Online Appendix B provides further details on the data sources. Lastly, we use data on vacancies from the Norwegian Welfare Administration that include information about the occupation and workplace location of the job opening.

2.4 Sample Construction and Summary Statistics

We impose four restrictions on the population of firms and employees. First, we use bankruptcy petitions from 2006 to 2013 to ensure a balanced sample over four years and a consistent measure of assets and income for the full sample.[11] Second, we restrict to full-time employees aged 25 to 62 to mitigate concerns about entry and exit from the labor force.[12] Third, we require that firms have at least five employees during the calendar year before the petition to focus on viable employers. Our last restriction is that workers must be employed at the firm 12 months before the petition. This choice provides us with the population at risk of layoff, allowing us to account for workers who leave the firm, voluntarily or involuntarily, before the petition date, and assess the role of anticipation effects and worker mobility.[13]

We impose two restrictions on the sample of district court judges. First, we require that a judge has handled at least 20 cases over the entire sample period. Second, we exclude court-year observations with fewer than five judges. These restrictions increase precision in the judge stringency measure and ensure a sufficient number of potential judges handling the case. Appendix B.2 describes the sample construction in detail. All control variables are measured the year before the petition.

Table 1 reports summary statistics for our estimation sample. We also compare our sample to the overall labor force and workers in the same age range starting an unemployment spell in 2011. Panel (a) reports averages and standard deviations for the workforce, and Panel (b) reports the

[11] Due to changes in the measurement of real estate from a hedonic pricing model introduced from 2010 onward, we only have a consistent measure of gross wealth for eight years.

[12] The full-time employee restriction is common in the literature on job loss (see, e.g., Davis & von Wachter, 2011 and Lachowska *et al.*, 2020). Our data defines full-time employment as working more than 30 hours per week. We measure this at the end of the year before the petition. Existing research on layoff costs using administrative data typically requires the sample of employees to be long-tenured and the non-laid-off to be continuously employed by the distressed firm. We do not impose these restrictions.

[13] Two additional restrictions help reduce noise in our data. First, we exclude non-residents, which account for less than half a percent of the sample. This choice allows us to measure demographic variables. Second, we exclude manager-owners who are registered as employees. Managing-owners typically have different incentives and compensation schemes than regular employees.

same statistics for the unemployed. Panel (c) illustrates that our estimation sample is younger and has slightly lower income and liquid assets in bank accounts than the labor force but is older and has higher wage and disposable income than the unemployed. We see that unemployment and distress risk is higher for people with only a high school diploma or less. In contrast, Appendix Table C.1 reports summary statistics for long-tenured employees experiencing mass layoffs. The displacement sample is older and significantly higher paid than our sample and the overall workforce. The average firm in our sample has 22 employees. Because the firm size distribution has a long right tail, the average employee works for a firm with 153 employees. We illustrate the firm size distribution in Appendix Figure A.2.

Table 1: **Summary Statistics**

Sample:	**(a) Labor Force Aged 25–62**		**(b) Unemployed Aged 25–62**		**(c) Employees in Distressed Firms**	
	Mean	St. Dev	Mean	St. Dev	Mean	St. Dev
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Age	42.4	10.9	37.7	11.2	40.3	10.0
Female	0.409		0.347		0.233	
High School Graduate	0.356		0.376		0.433	
Bachelor Degree	0.283		0.157		0.152	
Higher Degree	0.110		0.055		0.042	
Wage Income (NOK1,000)	477.502	300.689	357.829	153.951	461.557	277.399
Disposable Income (NOK1,000)	366.016	303.528	285.013	128.000	361.436	211.676
Liquid Assets (NOK1,000)	146.978	286.620	86.668	198.372	101.637	202.894
Industry Shares						
Mining, utilities, construction	0.137		0.179		0.199	
Manufacturing	0.120		0.212		0.222	
Trade and transportation	0.188		0.190		0.255	
Information, finance, prof. services	0.167		0.126		0.218	
Educational and health services	0.243		0.129		0.018	
Arts, recreation, hospitality services	0.030		0.060		0.055	
Number of observations	1,484,242		26,821		14,830	

Notes: This table reports summary statistics of our estimation samples. Panel (a) reports averages and standard deviations of key characteristics, income, and wealth of the labor force in 2011. Panel (b) reports the same characteristics for unemployed individuals whose spell started in 2011. Panel (c) reports statistics for full-time employees aged 25–62 in firms petitioned for bankruptcy in 2006–2013. Nominal values are deflated to 2015-values using the consumer price index. NOK/USD $\approx$ 10.

3 Measuring the Effects of Employer Distress

This section describes how we use judge assignment to address the challenges of identifying the labor market effects of employer shutdown.

3.1 Identification Challenge

Consider the treatment indicator, L_i, equal to 1 when employee i's employer is liquidated and otherwise equal to 0. The outcome of interest y_i can be denoted as the potential outcome y_{0i}, observed if the employee is untreated, and by y_{1i}, observed when the individual loses her job. The target parameter is the individual treatment effect—the costs of employer shutdown—defined by $\beta_i = y_{1i} - y_{0i}$.

The identification challenge arises due to observing only one of these outcomes for each individual. Suppose, for example, that employees with low productivity (i.e., both y_{1i} and y_{0i} are low) are more likely to be employed by firms that go bankrupt. A similar selection of workers arise if workers with good outside options tend to have high values of both potential outcomes. This type of selection would lead to large differences between treated and non-treated workers, confounding selection with treatment effects. We address this challenge by using an instrumental variable that replaces the endogenous employment choices with plausibly exogenous variation in layoffs, and using the dates to include workers who leave in anticipation of bankruptcy.

3.2 Random Assignment of Bankruptcy Cases

Next, we describe the institutions that generate the plausibly exogenous variation in employer liquidation. Within each district court, the assignment of a case to judges is done by the chief judge.[14] To ensure equal treatment by the law, the court rules state that case assignment shall be done "by drawing lots." In practice, the assignment is made without knowledge of case content on a rotating basis by the date the petition is received: Each time a new case arrives, it is mechanically assigned to the next judge on the list (see, e.g., Bohn, 2000 and Bhuller *et al.*, 2020).

Although we do not observe a judge's stringency directly, we can consistently estimate it using the bankruptcy petitions. Our measure of judge stringency is calculated for each case as the average rate at which the liquidation process is initiated in the other cases a judge has handled (i.e., a leave-one-out mean). Importantly, we use all cases—not only the estimation sample from 2006 to 2013—including firms without employees. Because the randomization occurs among judges within a district court, we use the residual variation in stringency after removing average liquidation rates within a court and given year.

To assess whether courts comply with the assignment protocol, we examine how judge stringency relates to the attributes of the firm and its employees. Columns 2 and 3 of Table 2 document that some worker and firm characteristics predict whether the firm is liquidated, and the joint test rejects the null hypothesis that they are not related to the outcome of the case. We also see that older or foreign-born workers are more likely to experience that the judge opens bankruptcy. Perhaps not surprisingly, we find that a one-year lag in total debt is a strong predictor of bankruptcy, but also note that debt is only available for firms with relatively high levels of revenue and at least 50 employees.

[14] The Norwegian court system is organized into three levels: the district, the appeals, and the supreme court. The vast majority of bankruptcy cases are settled in the district courts.

Table 2: **Summary Statistics and Test of Random Assignment Mechanism**

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:		**Liquidation of Firm**		**Judge Stringency (%)**	
	Average	coef.	s.e.	coef.	s.e.
Panel (a):	**Pre-determined Worker Characteristics**				
Age (at the time of decision)	40.289	0.0040***	(0.0011)	0.0125	(0.0147)
Female	0.233	-0.0133	(0.0378)	-0.2459	(0.4127)
Married	0.469	0.0449**	(0.0195)	0.0373	(0.2649)
Foreign born	0.225	-0.0309	(0.0392)	0.4051	(0.5304)
Less than high school	0.373	-0.0054	(0.0263)	0.4514	(0.3317)
High school	0.433	0.0149	(0.0227)	-0.2011	(0.3773)
Bachelor degree	0.152	-0.0280	(0.0368)	-0.3938	(0.3701)
Master degree	0.042	0.0300	(0.0531)	-0.1379	(0.6379)
Tenure (months)	57.342	0.0004	(0.0003)	0.0029	(0.0029)
Wage income (NOK1,000)	451,770	-0.0000	(0.0000)	-0.0007	(0.0011)
Disposable income (NOK1,000)	345,892	-0.0000	(0.0000)	-0.0015	(0.0022)
Liquid assets (NOK1,000)	101.638	0.0000	(0.0000)	-0.0007	(0.0005)
Debt (NOK1,000)	950.327	-0.0000	(0.0000)	-0.0001	(0.0001)
Panel (b):	**Pre-determined Firm Outcomes**				
Employment	153.296	-0.0001	(0.0003)	-0.0018	(0.0017)
Revenue (NOK mill.)	363.114	0.0000	(0.0000)	-0.0000	(0.0002)
Wage Bill (NOK mill.)	66.022	0.0000	(0.0005)	-0.0004	(0.0014)
Total Debt (NOK mill.)	227.440	0.0001***	(0.0000)	-0.0001	(0.0002)
Observations	14,826	14,826		14,826	
F-stat and p-value		33.9554	<0.0001	1.1579	0.3012

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Notes: This table reports summary statistics of the baseline estimation sample and tests of the random allocation procedure. There are 312 judges. All characteristics are measured one year prior to when the case was received in the court. Column 1 reports averages of pre-determined individual characteristics (Panel (a)) and pre-determined firm-variables (Panel (b)). OLS regression coefficients of these characteristics on a dummy variable for whether the firm was liquidated are reported in column 2 and on the measure of judge stringency in column 4. F-statistics are obtained from regressions on combined set of characteristics. The baseline sample of bankruptcy cases consists of petitions filed during 2006–2013 that are handled by judges with at least 20 cases. The sample includes firms with at least five employees before the petition is received, and full-time employees aged 25–62. Each regression controls for fully interacted year and court indicators, and a linear and quadratic term of the (leave-one-out) judge processing time. Revenue, total debt, and wage bill is in millions. Nominal values are deflated to 2015-values using the consumer price index. NOK/USD $\approx$ 10.

Lastly, Columns 4 and 5 show no statistically significant relationship between any of the worker or firm characteristics and the stringency of the judge. Consistent with the random assignment mechanism stipulated by the law, we cannot reject that all characteristics jointly have no relationship with the stringency of the judge, with a p-value of 0.3.

3.3 Instrumental Variables Approach

We now describe how we use the judge fixed effect as an instrument for layoff. The key to our instrumental variable approach is that judges are both randomly assigned and that some judges are more likely to liquidate a firm than others. The research design uses this naturally occurring variation in liquidation propensities as a separation shock to otherwise stable employment relationships. We use panel data on wealth, income, and employment to study household consequences and implement the following instrumental variables (IV) model

$$L_i = \phi Z_{j(i)} + X_i'\psi + e_i \tag{1}$$

$$y_{it} = \beta_t L_i + X_i'\theta_t + u_{it}. \tag{2}$$

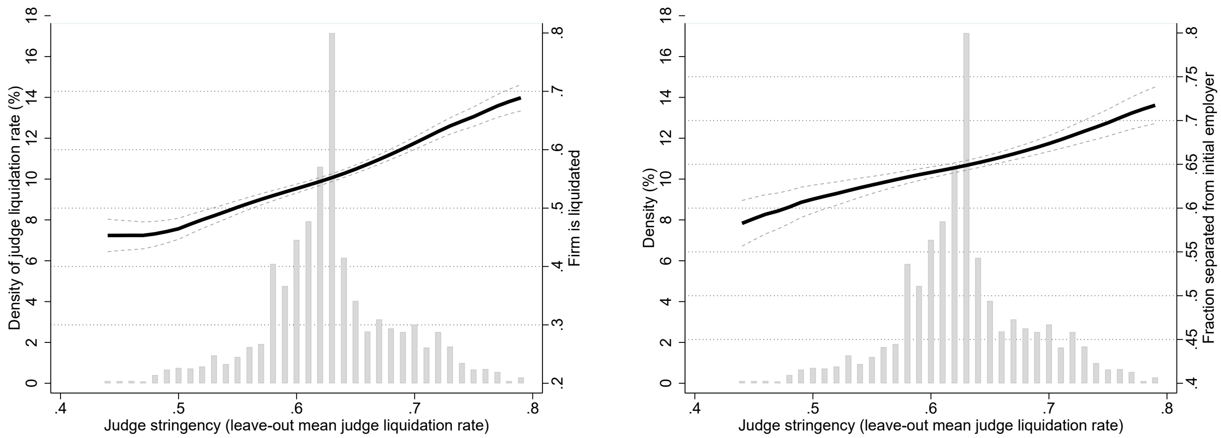
The instrument, $Z_{j(i)}$, is the stringency measure for judge j to which person $i's$ employer is assigned. The vector X_i contains pre-determined observable characteristics for the person and always includes a constant and a full set of year-by-district-court dummies.[15] The first stage coefficient ϕ gives the fraction of workers who lose their jobs when moving from a hypothetical judge with zero liquidation rate to a hypothetical judge that liquidates all cases. In the second stage equation, the outcome variable y_{it} is measured t years after the liquidation process starts (e.g., wage income four years after the decision). Due to the random assignment procedure, judge stringency is uncorrelated with equation 2's residual.

Our goal is to estimate the average of β_t among those separating from their employer because of the judge. We estimate the causal effect of layoff using the two-stage least squares (2SLS) estimator with first and second stage equations given by (1) and (2). The endogenous variable in this two-equation system indicates whether a worker is laid off because the firm is liquidated at time $t = 0$, rather than continuing with the employer or possibly separating at a later date for other reasons. Hence, the 2SLS estimate of β_t captures the effect of the *initial* layoff, which can operate through several channels, including loss of human capital, loss of tenure and the job protection rights associated with it, or shorter time to search out a good match in a new firm.[16] Throughout our analysis, we will also decompose our treatment effect estimate into potential outcomes of treated and untreated workers in marginal firms (i.e., employees in non-liquidated firms). Imbens & Rubin (1997) develop this decomposition for discrete instruments with heterogeneous treatment effects and Dahl *et al.* (2014) extends the decomposition framework to settings with continuous instruments.

[15]To reduce residual noise in our outcome variables, we also include dummies for age groups, marital status, foreign born, education levels, and income quintiles.

[16]The effect could also be attenuated if employees of dismissed firms lose their job during subsequent restructuring or new bankruptcy petitions. We use the first case ever observed for firms in the court system and do not use any subsequent case(s). These cases are part of the outcome of continuing firms. We have explored the possibility of using this second experiment, but the sample is too small to yield precise inference. We do not observe informal negotiations with creditors or restructuring in the data.

Figure 2: Firm is Liquidated and Worker Separates



(a) Firm is Liquidated

(b) Separation from Employer

Notes: Panel 3a displays the relationship between judge stringency and firm liquidation (i.e., the first stage). Panel 3b displays the relationship between judge stringency and whether an employee separates from the distressed firm by the end of the petition year. The solid line plots a local linear regression of employment on judge stringency with a bandwidth of 0.2. Local regressions are conditional on fully interacted year and district court dummies. The histogram of judge stringency is shown in the background of both figures (top and bottom 1 percent excluded from the graph). Dashed lines are 95 percent confidence intervals.

Assessing the Instrument. We begin by assessing the relevance of the instrument. Figure 3a provides a graphical representation of the first stage of the IV model, where the background is a histogram of the empirical judge stringency distribution. The mean and median of the raw stringency variable is 0.63, with a standard deviation of 0.13. While controlling for fully interacted year and district-court dummies shrinks the standard deviation to 0.085, the range of judge stringency is still wide—with a 1st to 99th percentile range from 0.44 to 0.79.

We present the relationship between average liquidation probabilities and the firm's assigned judge by the solid black line and 95 percent confidence intervals with dashed lines. The lines are fitted from local linear regressions with a bandwidth of 0.2, showing that the relationship is monotonically increasing. Judges above the 90th percentile (i.e., 71 percent liquidation rate) liquidate about two-thirds of bankruptcy cases in our sample. In contrast, judges below the 10th percentile (i.e., 54 percent rate) liquidate about 45 percent of the distressed firms. Figure 3b plots the reduced form relationship between judge stringency and the likelihood an employee separates from their firm. While the relationship is somewhat flatter than liquidation, it shows that the judge stringency rate also raises separation rates—indicating a causal relationship between liquidation and job displacement.

Next, we turn to the regression counterpart of this figure. We estimate the first stage coefficient by regressing liquidation on judge stringency, controlling for fully interacted court and petition year fixed effects. We also partial out the covariance with processing time to be consistent with our empirical implementation in the next section. In Panel (a) of Table 3, we report the estimated first stage coefficient at the firm level. While each judge has handled an average of 92 cases, there are about six firms per judge in our empirical sample. This highlights that our judge fixed effect is estimated on a much larger sample than our estimation sample. We cluster standard errors at the judge and petition year level and find the first stage coefficient is positive and statistically significant. A 10 percentage point increase in judge stringency leads to a 75 percentage point increase in the likelihood that a firm is liquidated. The second column restricts the sample to courts with at least five judges handling cases in a year. The point estimate is quantitatively similar but is more precisely estimated. In Panel (b), we repeat the exercise at the employee level. Moving to the third column, we show that the judge processing time does not significantly alter the first stage coefficient. The last column includes dummies for each age, marital status, education level, and immigrant status, and the third column includes a measure of judge experience. The two specifications show that the point estimate remains similar and statistically significant if we condition on a broad range of characteristics, demonstrating that the stringency measure captures important differences in how judges view the solvency question.

Table 3: First Stage Estimates

	First stage: Firm is Liquidated					
	(a) Firm Weights		(b) Worker Weights			
Sample / specification:	I. All Courts	II. Baseline	I. All Courts	II. Baseline	III. Processing Time	IV. Individual controls
Judge stringency	0.753***	0.765***	0.818***	0.831***	0.781***	0.794***
	(0.131)	(0.122)	(0.182)	(0.171)	(0.204)	(0.202)
Mean liquidation	0.560	0.551	0.553	0.553	0.553	0.553
Minimum Judges in Court	2	5	2	5	5	5
Number of Judges:	357	312	357	312	312	312
Avg. Cases per Judge:	87.8	92.2	87.8	92.2	92.2	92.2
Number of Firms / Workers:	2,084	1,869	16,998	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.
Note: This table reports the first stage coefficients of equation 1. Baseline sample consists of petitions filed during 2006–2013 and handled by judges with at least 20 cases. The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges in the baseline sample. Regressions always controls for fully interacted year and court dummies. Individual controls include age, education, marital status, immigration indicators, and dummies for quintiles of pre-petition wage income levels. Judge average processing time is measured in all bankruptcy cases in the raw data.

To help interpret the instrument, we calculate the implied share of employees in marginal firms. This fraction can be obtained non-parametrically from Figure 2 as the difference in liquidation rates between the 99th and 1st percentile of judge stringency. We find the population share to be about 25 percent. In comparison, a relatively large share of firms (45 percent) always goes bankrupt irrespective of the judges they are assigned—seen in the figure as the liquidation rate at the lowest value of the stringency measure. The remaining fraction (i.e., 30 percent) are never-bankrupt firms whose outcome is independent of the judge. These firms are observed in court due to, for instance, overly aggressive creditors or automatic bankruptcy filings from tax authorities for missing annual reports.[17] Under the linear specification, the fraction of marginal employees equals the first stage coefficient multiplied by the range of the stringency measure from Figure 3a. This specification

[17] We bootstrap the standard errors on the population share of compliers, always- and never-takers, and find the estimates very precise. The results are available upon request.

gives a share of marginal jobs of about 0.3. Appendix Table A.1 shows that the compliers are equally distributed across worker characteristics but are three times more likely to be employed by a firm above the median size (30 employees in the year prior to bankruptcy).

Additional Assumptions Interpreting the 2SLS estimates of equation (2) as the causal effects of *liquidation* requires two additional assumptions. The first assumption is that judge stringency affects outcomes of interest only through its impact on closing down the firm. This exclusion restriction appears likely in our setting. Since the bankruptcy outcome is limited to liquidating the firm or not, and there is no personal contact between the judges and the worker, the scope for the judge to affect employee outcomes directly is limited.[18]

However, judges may vary in how long they spend handling a case. In our setting, the correlation between the average liquidation rate and the processing time of judges is 0.38, raising a concern that judges who are more likely to liquidate would reinforce the layoff effects if processing time hurts workers' outcomes. Alternatively, time could be beneficial for workers as they have more opportunity to search for a better job. These potentially opposing forces means that ordering the two treatments is difficult, which may confound the causal interpretation of 2SLS estimates (see Mogstad *et al.*, 2021). A key feature in our data, however, is that processing time depends on the case's outcome: Only when the judge decides to open bankruptcy (i.e., liquidation) does the process significantly exceed one or two months. This feature allows us to examine how processing time affects outcomes by conditioning on liquidation.[19] Our regression evidence indicates that the leave-out-average processing time does not have a significant impact on wages. However, we include average processing time and its square as controls in our regression analysis to alleviate any concerns of confounding effects.

The second assumption required for a causal interpretation of the IV estimates is that (a) treatment effects only vary by observable characteristics (i.e., constant treatment effects) or that (b) the relationship between layoff and the instrument is monotone. This monotonicity assumption requires that, for each firm, the probability of bankruptcy is at least as high if assigned to a strict judge (high value of $Z_{j(i)}$) as if assigned to a lenient judge (low value of $Z_{j(i)}$).[20] The monotonic

[18] The trustee would inform the court if there are reasons to impose a bankruptcy quarantine on board members or the CEO. Such a quarantine may be imposed if there is a suspicion of a criminal act in connection with the bankruptcy or the person is deemed unfit to establish a new company due to improper business conduct. Suppose the court decides to place, for example, the CEO in bankruptcy quarantine. In that case, s/he cannot serve as a board member or CEO for two years. As we have removed manager-owners from the sample, this judge outcome does not concern regular employees' outcomes.

[19] While 95 percent of dismissed cases are closed within 82 days, the mean processing time for firms being liquidated is 656 days, and the median is 472 days. We regress wages one year after on a set of dummies for court and petition year, age, income quintiles, gender, marital and immigration status, and a polynomial of judge processing time. Experimenting with linear, quadratic, and cubic specifications, we find the quadratic specification has a lower Akaike information criterion (AIC). This specification minimizes AIC when conditioning on liquidation and in the overall sample. With this specification, we find that 100 days of processing time reduces wages one year after the petition by 34,557, or about 7.5 percent of the average wage from Table 1. The point estimate of processing time is 346, with a clustered standard error of 278.2. The coefficient estimate on the processing time squared is -0.441, with a standard error of 0.355.

[20] When the monotonicity assumption does not hold, individual weights (on β_i) may be negative, and the weights would not sum to 1. This would bias the results and, for example, yield a positive local average treatment effect (LATE)

relationship between the judge fixed effect and the outcomes in Figure 2 provides some support to the assumption. Another implication of monotonicity is that, in the absence of sampling error, first stage estimates should be non-negative for all subsamples. Again, since there is never any direct communication between the judge and employees, there is little reason to expect judge stringency to reverse for some employees and not others. We nevertheless show that all sub-sample estimates of the relationship between judge stringency and liquidation rates are positive and significant in Column (I) of Appendix Table A.1. To further gauge the monotonicity assumption, we break the instrument into the same subsamples, but redefine the instrument for each subsample to be the liquidation rate for all cases outside of the subsample. For example, for the low-wage workers, we use the judge's liquidation rate constructed from all cases except low-wage workers. Column (II) of Appendix Table A.1 reports the first stage estimates using this "reverse-sample instrument." While it may be a weak test of monotonicity, it shows that all estimates remain positive and statistically different from zero, suggesting that stricter judges for one type of case are also stricter for other case types.

4 Main Empirical Findings

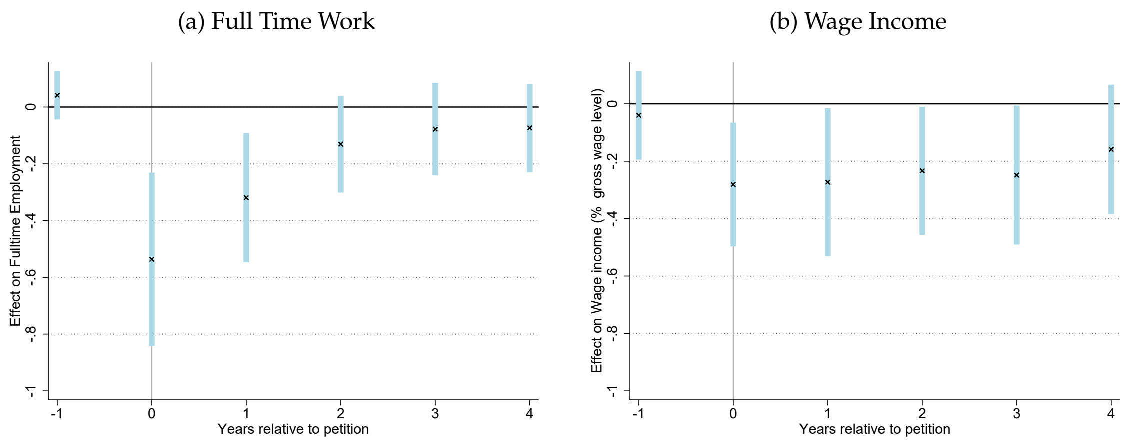
4.1 Average Employment and Earnings Costs

We begin by examining the impacts of liquidating the firm on full-time employment, measured as being employed at the end of the calendar year. Panel (a) of Table 4 presents our 2SLS estimates and Figure 3a plots the point estimates together with 95 percent confidence intervals, illustrating that liquidation causes a statistically significant decline of 55 percent in the first year. By contrast, the average employment rate among employees in marginal firms (i.e., untreated mean) is slightly above 90 percent. The following year, the effect declines slightly to a 32 percentage point decline. After the first two years, the estimates are less precise but stable at around 7–8 percentage points.[21] In Panel (b), we report estimates of the effect on any type of employment, including working at least 4 hours per week. We see that the estimates are similar in size.

estimate when negative individual effects are assigned negative weights. De Chaisemartin (2017) shows that researchers can identify a slightly differently weighted LATE when the assumption is violated as long as two equal proportions of defiers and compliers with identical potential outcomes exist.

[21] Appendix Table A.4 reports 2SLS estimates on unemployment insurance receipt in the last year of our observation window. We cannot reject that the effect is null. While we do not detect a statistically significant impact on disability insurance receipt, our evidence indicates that liquidation may increase withdrawal from the labor force and the likelihood of not having any labor earnings. We note that the estimates are imprecisely estimated, and we cannot reject a zero impact on labor force exit with conventional confidence levels. The impacts on labor force participation are not explained by early retirement eligibility at age 62. Excluding workers above 58 does not change the point estimates appreciably. These estimates are available upon request.

Figure 3: Labor Market Impacts of Employer Shutdown



Notes: This figure plots IV estimates. Panel 3a shows the evolution of the 2SLS estimates of impacts on employment and Panel 3b shows the corresponding estimates on wage income. The shaded blue spikes are 95 percent confidence intervals. See Table 4 for a description of the baseline sample and specification. Average exchange rate in 2024 is NOK/$=10.

Table 4: 2SLS Estimate: Employment and Earnings

Years after decision (t):	**0**	**1**	**2**	**3**	**4**
Panel (a):			**Full Time Work**		
Liquidation (β)	-0.537***	-0.319***	-0.131	-0.078	-0.074
	(0.156)	(0.116)	(0.087)	(0.083)	(0.079)
Overall mean of dependent variable	0.675	0.662	0.670	0.673	0.672
Untreated mean	0.925	0.815	0.716	0.789	0.805
Panel (b):			**Part or Full Time Employment**		
Liquidation (β)	-0.574***	-0.281**	-0.166*	-0.132*	-0.099
	(0.156)	(0.109)	(0.089)	(0.077)	(0.078)
Overall mean of dependent variable	0.704	0.708	0.723	0.733	0.731
Untreated mean	0.981	0.838	0.779	0.826	0.865
Panel (c):			**Total Wage Income (NOK1,000)**		
Liquidation (β)	-125.971**	-122.341**	-104.519**	-111.192**	-71.045
	(49.269)	(58.839)	(51.004)	(55.318)	(51.543)
Overall mean of dependent variable	429.722	403.118	402.357	406.839	407.947
Untreated mean	485.238	441.915	438.853	454.817	450.598
F-statistic	15.450	15.450	15.450	15.450	15.450
Number of observations:	14,826	14,826	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. The sample includes firms with at least five employees before the petition is received and full-time employees aged 25–62. There are 312 judges. Each regression controls for fully interacted year and court indicators, a linear and quadratic of leave-out judge processing time, and individual controls for age, education, marital status, immigration indicators, and dummies for quintiles of pre-petition wage income levels. Employment is defined as having any wage income in a year. Appendix Table A.2 reports the first stage and reduced form effects of judge stringency. Average exchange rate in 2024 NOK/USD ≈ 10.

Panel (c) of Table 4 reports our estimates of the effects of closing the firm on annual total gross wage. As we find important effects on any employment, reported in Panel (c), we include zeros for those without a job and plot the evolution of the 2SLS estimates in Figure 3b. The estimates show that annual wage income falls abruptly in the petition year, corresponding to a one-quarter decline in earnings relative to untreated employees. The earnings loss remains statistically significant and large for the following two years. By year four, however, the estimate is almost halved and is no longer statistically significant. Comparing the wage and employment effects, the relative impact on wages is larger than the relative fall in employment. This suggests the wage effect is driven by a combination of hours worked, hourly wages, and the extensive margin. This evidence is similar to, but less precisely estimated than Cederlöf (2020), who uses a quasi-experimental approach to study earnings losses from layoff in Sweden.

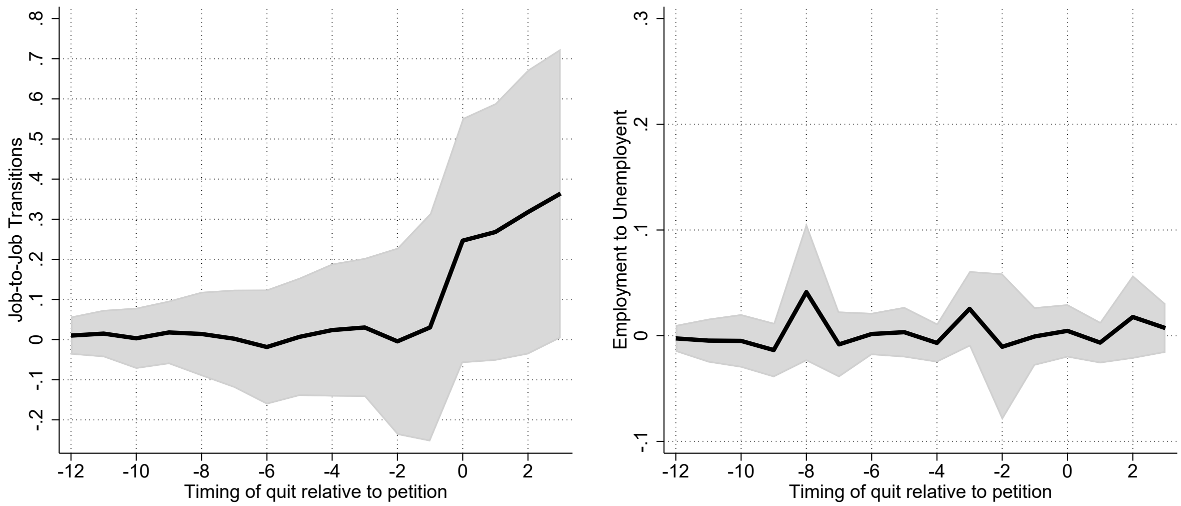
4.2 Employee Turnover

How many workers avoid the costs of layoff by moving to another firm? To assess the role of self-insurance from worker mobility, we need a measure of what workers know, how quickly they learn about distress, and how acting on information affects earnings in the next job. An important drawback with our data is that it does not provide exogenous variation in distress exposure. However, the information about petition dates allows us to examine the extent to which and how long information is available to the public using time-variation in changes to firm sales. We find that sales decline by 10 percent in the year before a petition is received, followed by an additional 30 percent drop in the same year the petition is received. Appendix Figure A.3a shows average sales relative to the petition date. Moreover, we find that a 10 percent decline in sales bears little signal of bankruptcy risk the following year, as illustrated by Appendix Figure A.3b. The increase in petitions at negative ten percent sales growth is barely noticeable.

To explore worker turnover around the petition date, we use the monthly employer-employee data to test the idea that abrupt changes in turnover before or at the petition date suggest that employees are learning about bankruptcy risk. We define a voluntary job-to-job (EE) move as a transition between employers from one month to another without an intervening spell of unemployment or non-employment for each individual starting 12 months before to up to three months after the petition date.

Next, we estimate the potential outcomes of *untreated* compliers and plot the estimated potential EE and unemployment spells around the petition date. As seen by the abrupt change in potential turnover rates, about 24 percent of all workers in marginal firms leave the firm before the judge has made a decision. While the confidence interval is wide, Figure 4 shows that turnover clearly increases in the month of the petition. In contrast, the employment-to-unemployment transition rate among the untreated is low and quite stable over the period. The increase in job-to-job moves prior to the liquidation is likely to attenuate the estimated earnings losses compared to a scenario where workers cannot anticipate future job loss.

Figure 4: Transition Rates in Distressed Firms



Notes: The left figure shows the share of untreated workers in distressed firms with a job-to-job transition between 12 months prior to and 3 months after the bankruptcy petition date. The right figure shows the incidence of unemployment spells in the months around the bankruptcy petition. For job-to-job transition rates, the resignation time is imputed based on the average three month mutual dismissal time. Untreated means are potential outcomes of employees in marginal firms (see Dahl *et al.*, 2014).

4.3 Outside Options

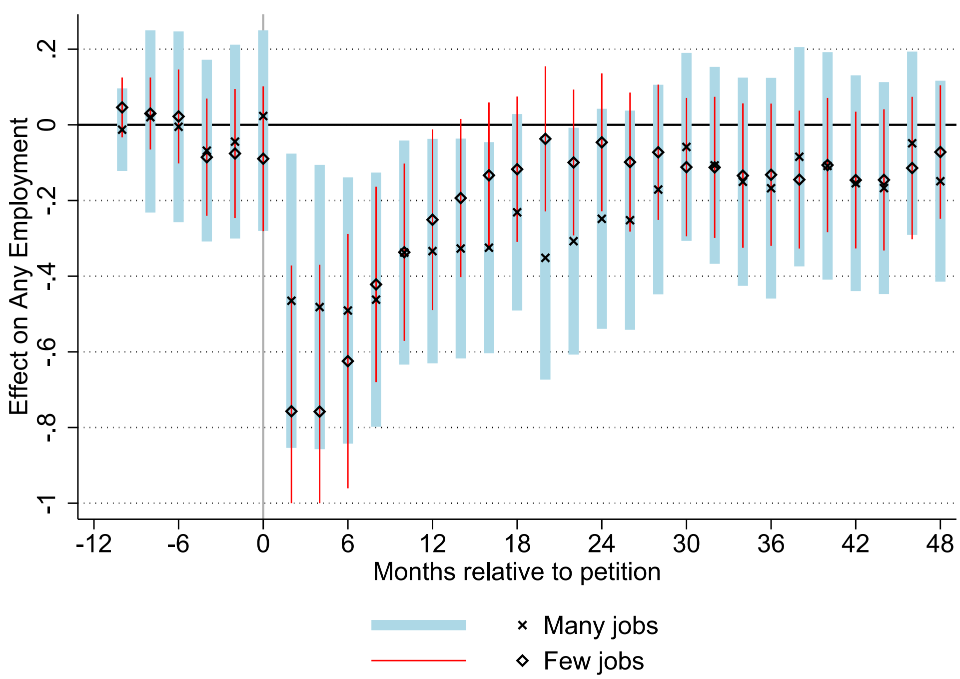
To explore how changes in pre-layoff transitions, wage losses and outside options interact, we make use of information about local labor market conditions. To do so, we need to address the challenge of how to measure the local labor market. Berger *et al.* (2023) show that 4-digit occupations capture about 40 percent of transitions from one employer to another in the Norwegian labor market, and that this self-flow rate increases to 45 percent using 3-digit occupations. Moreover, the geographic mobility rate across commuting zones is quite low.[22] These findings indicate that the combination of 3-digit level occupations in the commuting zone can serve as a reasonable proxy for the local labor market. We use the number of job openings divided by the number of registered job seekers within each local labor market as the tightness.

While we do not have experimental data on offer arrival and interview rates, we can alleviate some concerns about confounders across labor market conditions by residualize the tightness measure. In our main specification, we remove the covariance of tightness and permanent differences between occupations and commuting zones, as well as the female share and polynomials for av-

[22]We examine whether liquidation affects the likelihood of moving to new local labor markets. While we find no statistically significant impact, we lack the statistical precision to draw firm conclusions. We use the geographic unit defined by commuting statistics (see, Berger *et al.*, 2023), and report our estimate in Panel (c) of Appendix Table A.5. Similarly, an influential line of research has established that switching careers is associated with larger earnings losses (see, e.g., Neal, 1995, Huckfeldt, 2022, and Braxton & Taska, 2023). While a career outside the firm may be difficult to define, we use the occupational code, a natural definition of the career ladder for several skilled occupations. For instance, the group "pharmacists" includes over-the-counter shop assistants, licensed occupations for handling prescriptions, and senior positions in private pharmacies and clinical and hospital pharmacists. We examine whether liquidation increases the sectoral mobility, defined as 4-digit occupation or industry, but find no statistically significant impact by the end of year 4. These results are reported in Appendix Table A.5.

erage age and tenure in the occupation and commuting zone. Moreover, combining the tightness measure with the linked employer-employee data allows us to distinguish between employees in the same firm who simultaneously experience strong and weak labor market conditions arising from belonging to different occupational groups (e.g., engineers vs. economists). After this first step, we divide the sample by the median vacancies per registered job seeker, and calculate the average vacancy-unemployment ratios in weak and strong markets, which are 0.245 and 0.798 respectively. In our sample, 43 percent of firms have workers in both strong and weak markets.

Figure 5: Heterogeneous Employment Impacts



Notes: This figure plots heterogeneity in impacts of shutting down a firm by the strength of the labor market the year of the petition. Strong and weak labor markets are defined by residualized vacancy-unemployment ratios in the prior 3-digit occupation and commuting zone. See variable, sample descriptions, and regression details in Table 4. The spikes are 95 percent confidence intervals.

We begin by exploring whether differences in outside options translate into differences in employment rates. If it does, differences in wage effects reflect differences in impacts on the extensive margin. To assess the extensive margin effects, we re-estimate equation 2 separately for the two samples using bi-monthly data. The outcome of interest is equal to one if the worker has any employment by the end of the month, and Figure 5 shows no clear evidence of differential impacts, suggesting that the availability of jobs does not affect the likelihood of being employed.

Next, we investigate whether differences in job opportunities lead workers to accept jobs with lower wages or shorter hours. Our focus is on the average annual earnings and the pre-bankruptcy job mobility rates, and we report our headline results in the second columns of Panels (a) and (b) of Table 5. The 2SLS estimates show that earnings losses are 21 percent in weak markets and 9 percent in strong markets but we lack the statistical precision to firmly conclude that the estimates are different from each other. Turning to the employee reallocation rates, we find that turnover is three times higher in strong labor markets than in weak ones.

Table 5: 2SLS Estimates: By Labor Market Conditions

Years after decision (t): average 0–4, NOK1,000 = USD100						
	(a) Weak Labor Market			**(b) Strong Labor Market**		
	Firm Closes	Wage Income	Pre-layoff turnover	Firm Closes	Wage Income	Pre-layoff turnover
	(1)	(2)	(3)	(1)	(2)	(3)
Judge Stringency	0.911***	-91.402**		0.661***	-22.902	
	(0.234)	(45.018)		(0.209)	(52.146)	
Liquidation (β)		-100.303**			-34.665	
		(47.507)			(78.006)	
Overall mean	0.502	390.572		0.610	431.996	
Untreated mean		475.263	0.137		381.057	0.346
Observations:		7,861			6,945	

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the first stage, reduced form, and second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. See variable, sample descriptions, and regression details in Table 4. Strong and weak labor markets are defined by residualized vacancy-unemployment ratios in the prior 3-digit occupation and commuting zone. When residualizing year-of-petition vacancy-rates, we remove the covariance between gender, commuting zone, and occupation-fixed effects and a quadratic function of age and tenure.

To assess the strength of our conclusions about outside options, we bootstrap the sample with 200 repetitions and estimate a bootstrapped standard error of the differences in potential turnover rates. In our baseline specification, we find that the difference equals 0.111, allowing us to weakly reject the null hypothesis of equal means. We estimate two other specifications, where we omit (a) worker controls, and (b) the dummies for commuting zones in another specification and report estimates in Appendix Table A.6. The results remain quantitatively similar, where the differences in estimated turnover rates are significant at conventional levels.[23] As an additional robustness check, we split the three tightness measures into quintiles and re-estimate the wage loss and turnover rates for each of these 15 subsamples. In Figure A.4, we plot the association between turnover and wage losses, finding it is positive, indicating that turnover induced by more job opportunities may reduce the size of estimated earnings loss from 25 to 5 percent.

4.4 Heterogeneity

Next, we explore differences in estimated impacts across additional splits of our data. Our outcomes of interest are the average earnings from when the petition is filed to four years after, and the average anticipatory turnover among untreated workers in marginal firms.

We begin by splitting the sample by firms that pay above median estimated premiums, and by workers that are high- vs. low wage according to the Abowd *et al.* (1999) model of log wages

[23] We bootstrap the sample with replacement 200 times, and estimate the standard error of the differences in potential turnover rates. In both specifications, the probability of falsely rejecting the null is less than 1 percent. The estimated difference in impact on average earnings is not statistically significant at conventional levels, but yield p-values of 20 to 30 percent.

and two-way fixed effects. Table 6 reports estimates of the first stage, the reduced form effects of judge stringency, and the 2SLS estimate of each sub-sample. Panels A and B show that while our estimated five-year earnings losses are similar, that the out-migration rates are much larger for workers in the high-wage firms. This finding is consistent with workers with more to lose from layoff searching more intensely, offsetting some of the earnings loss. In Panels C and D, we see that high-wage workers suffer larger earnings losses than low-wage workers. These estimates are statistically significant different from each other with a p-value of 0.028.

We further split these subsamples by interacting high- and low wage-firms and workers, and plot the estimates in Figure 6. The figure illustrates that higher employee reallocation rates seem to reduce the five-year employee costs. In contrast, Appendix Figure A.4 shows a flat relationship between earnings losses and pre-bankruptcy unemployment risk. The evidence points toward labor market insurance from job search, rather than differential scarring from unemployment.

In addition, we explore further sample splits, including age and skill level, and report estimates in Appendix Table A.7. We see that five-year earnings losses are concentrated among workers above 40, potentially indicating that older workers have weaker incentives to invest in a new career because they have fewer years remaining until retirement. In contrast, we find that earnings losses are quite similar for high- and low skilled, measured as having completed high-school or not. Somewhat surprisingly, there are no clear differences in reallocation rates between high vs. low skill or young vs. older workers. Rather, differences in turnover seem to be driven by differences in labor market conditions and whether a firm is high or low-wage.

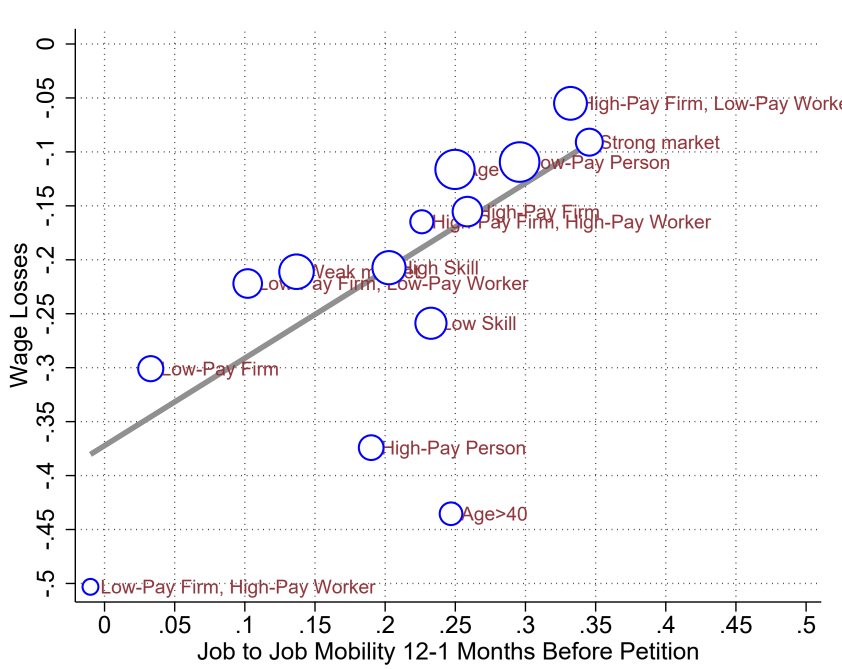
Table 6: 2SLS Estimates: High-Pay Firms and Workers

Years after decision (t): average 0–4, NOK1,000 = USD100								
	A. High-Wage Firm		B. Low-Wage Firm		C. High-Wage Worker		D. Low-Wage Worker	
	Firm Closes	Wage Income	Firm Closes	Wage Income	Firm Closes	Wage Income	Firm Closes	Wage Income
	(1)	(3)	(1)	(3)	(1)	(3)	(1)	(3)
Judge Stringency	0.985***	-74.834	0.578**	-60.472	0.687***	-137.989**	0.911***	-39.465
	(0.337)	(68.315)	(0.254)	(44.590)	(0.234)	(58.821)	(0.199)	(33.245)
Liquidation (β)		-75.970		-104.651		-200.978**		-43.315
		(64.692)		(89.757)		(92.792)		(36.233)
Overall mean	0.560	464.397	0.553	356.426	0.571	451.824	0.533	366.277
Untreated mean		489.045		347.771		537.195		395.922
Pre-layoff turnover		0.259		0.033		0.190		0.296
Observations:	7,801		5,967		7,241		7,801	

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the first stage, reduced form, and second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. Untreated means are potential outcome of employees in marginal firms (see Dahl *et al.*, 2014). The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Each regression controls for fully interacted year and court indicators and includes the individual demographic control variables from Table 2 and dummies for each quintile of pre-petition wages. The mean wage income is winsorized at the 99th percentile and disposable income is winsorized at the 95th percentile. Outcomes are averaged over the five-year period and are deflated to 2015-values using consumer price index. Average exchange rate in 2024 is NOK/$=10.

Figure 6: Heterogeneous Wage Impacts by Pre-Petition Worker Turnover Rates



Notes: This figure plots heterogeneity in impacts of shutting down a firm on wage income against the fraction of untreated workers who leave their employer before or during the month of the petition. See variable, sample descriptions, and regression details in Table 4. The size of scatter plots reflect the inverse of the standard errors of the estimated impact on wage income.

4.5 Private and Public Insurance

To contrast the variation in earnings losses across labor market conditions, we investigate the role of the tax and transfer system and self-insurance from private assets. Starting with taxes and unemployment insurance, Table 7 reports the five-year impact estimates, illustrating that the impact on UI benefits is about 11 thousand, corresponding to a 130 percent increase relative to workers in non-liquidated firms. The third column reports the estimated impact on five-year average disposable income, which accounts for capital and business income, government transfers such as social assistance (i.e., welfare), housing, unemployment, and disability insurance benefits, all net of taxes. The average impact over the period is equal to 53 thousand—about half of the earnings loss, but still a sizable loss in income. Appendix Figure A.6 illustrates that the tax system automatically buffers the impact on disposable income over the entire period, whereas the UI system plays an important role, but only in the first two years after the petition has been sent.[24]

These estimates may exaggerate the costs of layoffs if other family members can increase labor supply (e.g., Cullen & Gruber, 2000). We examine insurance from within the household from added workers (i.e., increasing work from a spouse). The third and fourth columns of Table 7

[24]In Panel (b) of Appendix Table A.9, we examine the role of other market income sources, including business and capital income. We do not reject the null hypothesis that liquidation has no effect on other sources of market income. We cannot detect any statistically significant effect on any of the other sources of transfer payments.

show how liquidating the firm affects individual and household disposable income. While adding spousal income reinforces the decline, we cannot offer a definitive conclusion due to a lack of statistical precision.[25] Households can also smooth consumption by pooling assets. We estimate the impacts of liquidating the firm on net assets at the individual and household levels; columns (5) and (6) show statistically significant effects corresponding to a relative decline in net wealth of about 60 percent of the cumulated income loss. Again, we do not see any statistically significant differences between the impacts on individual and family-level assets, possibly reflecting the low marriage rates and added noise from changes in the composition of marriage rates over time. Appendix Table A.9 shows that the impact on net wealth is quantitatively similar if we only focus on the last year and if we adjust for passive savings from housing market appreciation.

Table 7: 2SLS Estimates of Impacts on Disposable Income and Assets

Years after decision (t): average 0–4, NOK1,000 = USD100						
	Wage Income	**UI Benefits**	**Disposable Income**	**Add Spouse Income**	**Δ Net Assets**	**Add Spousal Assets**
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation (β)	-107.014**	10.529*	-52.557**	-69.839**	-247.460**	-237.064*
	(48.702)	(5.582)	(22.859)	(34.250)	(116.132)	(133.039)
Overall mean	409.996	15.785	340.647	434.663	242.420	347.502
Untreated mean	454.284	8.956	354.026	457.273	451.661	569.728
Number of observations:	14,826	14,826	14,826	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. Untreated means are potential outcome of employees in marginal firms (see Dahl *et al.*, 2014). See variable, sample descriptions, and regression details in Table 4. Outcomes are averaged over the five-year period and are deflated to 2015-values using consumer price index. Average exchange rate in 2024 is NOK/USD $\approx$ 10.

Previous research has documented that laid off workers experience higher personal bankruptcy rates (Keys, 2018), and that credit conditions deteriorate among laid off low-wage workers (Aaronson *et al.*, 2019). Braxton *et al.* (2020) show that access to (pre-established) lines of credit is an important insurance mechanism for job loss in the US. We examine these channels in the Norwegian setting and report our 2SLS estimates in Appendix Table A.10. Our findings suggest that the decline in net wealth reflects the use of personal assets and not debt increases. While somewhat imprecise, our 95 percent confidence interval rejects debt increases by more than half of an annual salary over five years after the firm's liquidation. We do not find clear evidence that liquidation worsens access to credit markets, proxied by the debt relief from personal bankruptcy. Furthermore, we estimate the impacts of liquidation on an indicator equal to one if the person's wage is garnished at some point after the petition date and the average interest rate paid on debt. We cannot reject the null hypothesis of zero impact on either of these outcomes.

[25] A natural question is whether losing a valuable employment relationship affects marriage rates. While Appendix Table A.3 shows that we do not detect any statistically significant effect on marriage rates, marriage rates are low at the beginning of the period and steadily increase over time. This is why, instead of conditioning the sample on marital status, we include the spousal income when we observe a registered spouse in the data.

4.6 Household Consumption and Public Expenditure

Our last exercise explores the impacts of liquidation on consumption expenditure and net government transfers. To impute consumption expenditure, we apply the accounting identity, which states that the effect on consumption, ΔC must equal the effect on the change in net wealth ΔW plus disposable income ΔI (see, e.g., Browning & Leth-Petersen, 2003 and Eika *et al.*, 2020). Because the market valuation of housing is only available from 2010, our target parameter is the effect of employer shutdown on the five-year average consumption expenditure.[26] Column (1) of Table 8 copies the impact on disposable income, and column (2) includes the impact on active dissaving. Column (3) reports the average impact on annualized imputed consumption expenditure. Eika *et al.* (2020) estimate the value of owner-occupied housing relative to the rental cost and find the rent-to-value ratio for 1994–2014 is 2.88 percent. Column (4) shows that adding the consumption value of owner-occupied housing reduces the impact on annualized consumption by about 16 thousand.

To explore the fiscal cost of liquidating a firm, we report our estimated impact on the sum of all transfers minus taxes in last column of Table 8. This point estimate shows that a liquidation causes a net increase in public expenditure of 63 thousand NOK per year— equal to NOK315,000 over the first five years, or, about sixty percent of the pre-bankruptcy annual gross earnings.[27]

Table 8: 2SLS Estimates of Impacts on Imputed Consumption and Fiscal Costs

Years after decision (t): average 0–4, NOK1,000 = USD100					
	Disposable Income	Active Dissaving	Imputed Consumption		Fiscal Cost
			Baseline	Add Housing	
	(1)	(2)	(3)	(4)	(5)
Liquidation (β)	-52.557**	-207.374*	-8.710	-24.988	63.429***
	(22.859)	(115.727)	(28.433)	(28.784)	(22.974)
Overall mean	340.647	332.833	285.083	318.877	-72.420
Untreated mean	354.026	324.445	297.553	339.089	-117.570
Number of observations:	14,826	14,826	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. Untreated means are potential outcome of employees in marginal firms (see Dahl *et al.*, 2014). See variable, sample descriptions, and regression details in Table 4. Fiscal costs include all transfers from the government, less taxes paid, where transfers and taxes are windsorized at the 99th percentile. Disposable income, assets, and imputed consumption are winsorized at the 95th percentile. Disposable income is averaged over the five-year period, the other outcomes are cumulative outcomes, measured four years after petition. All outcomes are deflated to 2015-values using consumer price index. Average exchange rate in 2024 is NOK/USD $\approx$ 10.

[26] The four-year average uses the last observation for housing, which is available for the entire sample. An advantage of using the cumulative change in income and wealth is that noise from within-year changes in wealth due to portfolio rebalancing (i.e., from selling stock or housing market transactions) is averaged out. We examine the effects of employer shutdown on other types of capital income, including interest rates, sales of listed and unlisted stocks, and realized gains from selling real estate in Appendix Table A.9. Reassuringly, we find no evidence of capital income effects nor do we detect statistically significant effects on business income.

[27] These costs are relatively small compared to the average debt per employee among larger financially distressed employers in our sample, which amounts to NOK1.5 million (approx. \$150,000).

4.7 Internal and External Validity

Before we explore the welfare implications of our findings, we discuss the internal and external validity of our findings. We report the estimates of wage income and disposable income effects of liquidation in Appendix Table A.11, and summarize the output from the exercises here.

To explore the internal validity, we start by excluding judges who handle fewer than 30 cases. This allows us to assess the extent that our findings are sensitive to noise in our judge stringency measure. Second, we address a concern that the relationship between judge stringency and individual outcomes is non-linear, where weights for subgroups of compliers with heterogeneous treatment effects would be misspecified. To assess such potential misspecifications, we implement the IV model using a quadratic and cubic specification of judge stringency. Third, we allow for a more flexible specification of judge processing time. Reassuringly, our conclusions remain unchanged across these three specification checks. Lastly, we show that average impacts are similar but much less precisely estimated when we include numeric values above the 99th percentile.

In terms of external validity, we follow the empirical specifications in Lachowska *et al.* (2020) and show that mass layoffs lead to similar earnings losses in Norway as in the US. These results are presented in Appendix C.

5 A Job Ladder Model With Learning and Self-Insurance

To quantify the value of a specific employer and how information affects reallocation, we develop a model that can match our key empirical facts.

5.1 Environment

The model runs in discrete time, starting from the month a worker enters the labor force at age 25 until the month the worker turns 67 and retires. Workers search for jobs while employed and unemployed, and decide how much to save for retirement or to offset cuts in consumption upon job loss. We let $c_t \geq 0$ denote consumption in month t. Individuals maximize

$$\mathbb{E}_0 \left[\sum_{t=1}^{T} \beta^{t-1} \left(u(c_t) - \psi(e_t) \right) + \beta^T \nu(a_{T+1}) \right] \tag{3}$$

subject to the constraints we specify below. Workers receive new job offers by exerting search effort, denoted e_t, where $\psi(e_t)$ is the corresponding disutility of effort. The net value of all assets held by the worker at the end of the T-period working life is denoted by $a_{T+1} \geq \underline{a}$ and we let $\nu\left(a_{T+1}\right)$ capture the utility of bringing assets into retirement. Following Lise (2013), we assume that within-period consumption preferences are given by

$$u(c_t) = (c_t^{1-\gamma} - 1)/(1-\gamma), \tag{4}$$

where γ is the degree of relative risk aversion. Moreover, search effort incurs a disutility following the power form,

$$\psi(e_t) = \mu \frac{e_t^{\eta}}{\eta} \tag{5}$$

where $\eta > 1$ is the elasticity of search costs with respect to effort and μ is a scaling parameter. We further assume that residual utility gained from net assets held at the end of the final working period T is given by

$$\nu(a_{T+1}) = \varphi \frac{(a_{T+1} - \underline{a})^{1-\gamma} - 1}{1-\gamma} \tag{6}$$

where φ is a scaling parameter and $\underline{a}$ is the borrowing limit at the time of retirement. This residual utility term captures both the benefits the worker enjoys from carrying assets to be consumed during retirement as well as any warm-glow utility the worker obtains by leaving bequests at the end of the life-cycle. We present the workers' value function in full detail in Appendix D.1.

Search and Income Process A frictional matching process determines the per-period income of employed workers. The worker receives a constant pre-tax wage w while employed, but can search for an employer offering a higher wage in each period. Workers determine the likelihood of receiving an employment offer, $\lambda_{i,d,M}e_t$, by choosing effort $e_t \in [0, 1]$. The maximum offer rate $\lambda_{i,d,M}$ varies with the labor market condition, M, and whether or not the worker's current firm is undergoing bankruptcy proceedings, d. This captures differences in the labor market tightness that workers face. Motivated by the findings in Faberman *et al.* (2022), we allow the offer rate to vary with employment status, i, reflecting differences in search efficiency on-the-job and during unemployment. Employment offers are drawn from a constant wage distribution $G(w|k, M)$, that may vary by market condition M and worker's skill type k (a "low" or "high" type, corresponding to the skill categories defined in Section 4.4).

Employed workers' earnings are a function of the pre-tax wage w, supplemented by stochastically increasing human capital, h. In particular, there are a finite number of observable human capital types $h \in \{0, h_1, ..., h_n\}$ and workers are paid $w \cdot exp(h)$ before taxes each period. Human capital decreases stochastically during spells of unemployment, representing the degradation of skills and allowing the model to match the size of earnings loss when shutting down the firm that we observe empirically. Each period, employed workers gain one level of human capital with probability Δ_h^+ and unemployed workers lose one level of human capital with probability Δ_h^-. Workers observe their new level of human capital at the beginning of each period, prior to making any decisions.

Workers are exogenously separated from their employers with probability $\delta_{k,M}$ each period, which may vary by market conditions and the worker's skill level. Experiencing this separation shock means a worker enters unemployment the next period. Unemployed workers receive a per period benefit which depends on their labor force participation and earnings in the most recent period of employment, $(\tilde{w}, \tilde{h})$. Workers who are actively searching for employment are eligible for an unemployment insurance (UI) benefit which is proportional to past earnings $\tilde{w} \cdot exp(\tilde{h})$, up to some maximum level $\bar{b}$. The proportional UI benefit system generates voluntary unemployment in the sense that workers may reject some employment offers. Workers who do not actively search for employment are not eligible to collect UI benefits, but are instead eligible for a basic social assistance program which provides non-participants in the labor force with a minimum level of income, denoted by $\underline{b}$.

Workers face a variety of taxes which are not explicitly modeled. Instead, we follow Heathcote *et al.* (2014) in approximating the net of tax income by $\omega_1 \left[i \cdot (w \cdot exp(h)) + (1-i)b(\tilde{w}, \tilde{h}, e)\right]^{\omega_2}$, where ω_1 and ω_2 capture the level and progressivity of taxes in our setting, respectively. Post-tax income can be used for consumption, and individuals can save and borrow through a one-period bond with a return normalized to one. We allow the bond price to differ when saving versus borrowing to reflect potential differences in interest rates and we let q_b and q_s express the per-unit bond price when borrowing and saving, respectively. Workers face a natural borrowing limit as the result of the constraint that consumption in each period is non-negative and that assets held at the time of retirement must exceed the borrowing limit $\underline{a}$.

Learning about Distress. On top of the standard separation risk, workers can experience a distress shock—representing the realization or anticipation of a bankruptcy petition—with a potential liquidation following. The distress shock occurs with probability $\pi_{k,M}$, varying by the worker's skill type and the labor market conditions. Importantly, distress means an individual updates her expectation of her employment being terminated, lowering the value of the current position. We assume that firms which enter distress do not exit distress without proceeding through the bankruptcy courts. In some period after the onset of distress, a formal petition is filed and one period later a bankruptcy judge decides to liquidate the firm or dismiss the case. Some workers learn that their firm is in distress before their employer is formally petitioned and workers who anticipate a potential liquidation revise their beliefs about its likelihood over time. Other workers may not fully internalize the increased risk of their position being terminated even after a formal petition has been filed against their employer.

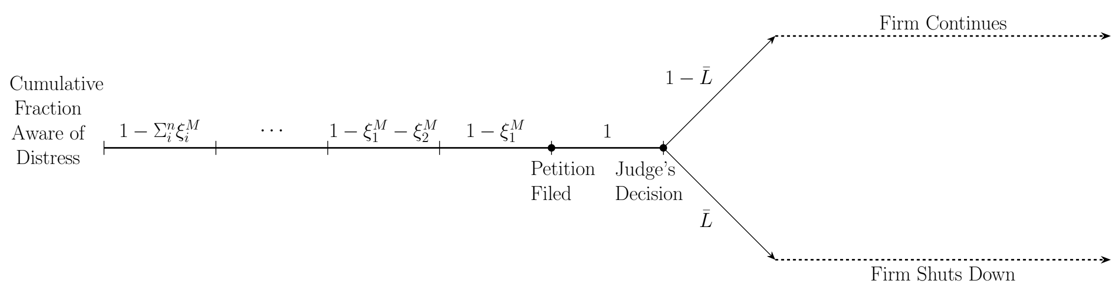
We model workers' information and beliefs to capture both of these properties. The share of workers who first realize their job is at risk due to bankruptcy n periods before the judge's decision is made is denoted ξ_n^M. Assuming a maximum of three periods of advance information, a worker who becomes aware that her firm is in distress does not know when the judge's decision will be made, but knows the distribution of workers becoming aware $\{\xi_n^M\}_{n=1}^3$. With this knowledge, the worker can form Bayesian beliefs about the probability that the judge makes a decision in each period. For example, if the distribution of advance information were defined by $\{0.5, 0.3, 0.2\}$, then in the first period the worker expects the judge to make a decision at the end of the current period with probability 0.5. If the judge's decision does not occur at the end of the first period, she updates her belief because she knows she is not part of the 0.5 share of workers who receive one period of information. In the second period, she believes she will face a potential liquidation at the end of the period with probability $\frac{0.3}{1-0.5} = 0.6$. Appendix D.2 provides a full example of how worker beliefs update over time as a firm progresses through bankruptcy proceedings.

5.2 Timing

In the period where firms enter distress, all workers are aware that a bankruptcy case is assigned to one of two types of judges. With probability $\bar{L}$, the case is assigned to a strict judge who always liquidates the firm; with probability $1 - \bar{L}$, the case is assigned to a lenient judge who always dismisses the case. The judge makes a decision at the end of the period. When the case is dismissed, the distress shock is resolved, with no long-term effects for the worker.[28] When the case is not dismissed, the worker's employment is immediately terminated and the worker enters the next period unemployed.

Figure 7 illustrates the timing of the firm-bankruptcy process together with workers' advance information. The judge's decision and the subsequent liquidation are timed such that workers make their search, consumption, and savings decisions after learning if their firm is distressed but before learning how the distress shock will be resolved. Thus, workers in distressed firms face heightened earnings risk. As a result, workers in distressed firms will have lower reservation wages when searching on the job and may exert greater search effort or increase their precautionary savings as a form of self insurance.

Figure 7: Timeline of Firm Distress Proceedings



Notes: This figure shows the timing of the bankruptcy process and workers' advance information of the process in our model. We condition the sample on workers being in the distressed firm twelve months prior to the bankruptcy petition being filed, and we allow distressed firms to face a formal bankruptcy petition in any month of year 0. We measure outcomes from the beginning of year 0 through the end of year 4. When we refer to the sub-sample of workers in "strong" and "weak" market conditions, we refer to the market conditions M that the worker is experiencing at the beginning of year 0.

6 Bringing the Model to the Data

This section discusses the estimation of parameters and the fit of our estimated model.

[28] Workers in distressed firms still face the baseline probability $\delta_{k,M}$ of separating from their firms even if liquidation does not occur, which we treat as the worker choosing to leave for other non-wage reasons.

6.1 Samples and Market Conditions

In the model, as in the empirical setting, workers are either treated (i.e., their employer is liquidated) or untreated (i.e., their employer experienced distress but not liquidation), even if they leave the distressed firm before the judge's decision is made.

The overall distress rate and labor market flows can vary by labor market conditions M and we allow for heterogeneity in the effect by skill type (changes in $\delta_{k,M}$ and $\pi_{k,M}$). In addition, both skill types face lower probabilities of receiving an employment offer conditional on effort (a decrease in $\lambda_{0,d,M}$ and $\lambda_{1,d,M}$) and we allow for a different distribution of advance information $\{\xi_n^{\tau M}\}$ across strong and weak labor markets. Consistent with our empirical design, when we refer to the subsample of workers in strong and weak labor markets, we refer to the market conditions at the start of year 0 (i.e., the year in which the judge's decision occurs).

6.2 Estimation

We borrow some parameters from the literature, such as the coefficient of relative risk aversion. We use $\gamma = 1.7$, which is consistent with the median value reported by Chiappori & Paiella (2011). The subjective discount factor is set to to $\beta = 0.9975$, implying an annual subjective discounting rate of approximately three percent, which is in line with common estimates from the literature.

External estimation. Other parameters are derived from the institutional setting. The replacement rate, $r = 0.624$, and maximum unemployment insurance benefit, $\bar{b} = 28.080$ NOK1,000, are set in accordance with the Norwegian UI system, and the minimum income provided by social assistance programs for workers who are unemployed and not actively searching is set to $\underline{b} = 8.333$ NOK1,000 monthly. We obtain estimates $\omega_1 = 5.336$ and $\omega_2 = 0.844$ from the 2015 tax system.[29] The price of the one-period bond when saving is set to $q_s = 1$ consistent with zero real interest rate on savings and we set $q_b = 0.9925$ to be consistent with an annual real interest rate of 9.4% on borrowing which is in line with typically observed rates in our setting. We set the borrowing limit at retirement to $\underline{a} = -593.617$ NOK1,000 which is the (negative of) net present value of the lowest retirement pension income available in our setting over a horizon based on the typical length of retirement.

We estimate some parameters from auxiliary data sources. The transition between strong and weak market conditions follows a Markov process, with probability τ_M of remaining in state M. We set $\tau_0 = 0.9788$ so that the expected duration between spells of weak market conditions is 47 months, and we set $\tau_1 = 0.9612$ so that the expected duration of weak market conditions is 26 months, consistent with the average length of local labor market conditions in our sample. The distribution of wage offers is estimated from data on the joint distribution of wages, vacancies, and

[29]This procedure is implemented in two steps. First, we simulate the actual tax system in 2015 for values of gross wage income ranging from 100 to 1,500 thousand NOK. Second, we regress the log of simulated disposable income on the log of gross wage income to obtain the parameters ω_1 and ω_2.

job seekers, where we first obtain residualized and recentered wage and vacancy-unemployment ratios. These control for dummies for each labor market region and occupation and a flexible function of age. We then compute the average wage in each occupation-region cell and use the labor market tightness as weight, and we perform this calculation separately for each skill type and labor market condition. We find that for all skill type and market condition combinations, the distribution of accepted offers is approximately log-normal and we calculate the CDF of residualized tightness and average wages. The mean and standard deviations of the resulting CDFs are the parameters for the log-normal offer distribution G for skill type k in market condition M.

The aggregate employment transition rates are calculated using overall population statistics and sample weights. The separation probabilities $\delta_{k,M}$ for strong and weak labor market conditions are taken from the data and combined with the share of each skill type in our overall and unemployed samples, as reported in Table 1, to estimate conditional probabilities by skill type. Similarly, we calculate distress risk, $\pi_{k,M}$, by combining the approximate share of firms entering distress each year in each market condition with sample shares of high and low-skilled to determine how the risks differ by skill type. Conditional on the firm being distressed, we set the probability of being assigned to a strict judge (i.e., *always liquidate*) to $\bar{L} = 0.553$, equal to the mean liquidation rate from Table 3. We impose constraints on the offer arrival rate based on the observed market tightness, but allow the exact rates $\lambda_{i,d,M}$ to be calibrated internally. In particular, for each employment status i, we fix the ratio of $\lambda_{i,d,M}$ values from the residualized ratio of vacancies to unemployed workers in each local labor market described in Section 4.3. Next, we use the recentered average ratios and combine them with the sample of workers in distress to measure the tightness by distress status.

Internal estimation. The remaining 13 parameters are estimated internally: the strength of the retirement savings motive (φ), the elasticity and scale of search costs (η, μ), the maximum offer arrival rates (λ_i), the distribution of advance information about distress (ξ_1^M,ξ_2^M,ξ_3^M), and the human capital process (Δ_h^+, Δ_h^-). We directly estimate 11 of these parameters with 12 moments to target.[30] These parameters are estimated using the Method of Simulated Moments (MSM), which finds parameter values that minimize the distance between simulated moments and some of our key reduced form evidence. For each candidate set of parameter values, we solve the worker's problem by backward induction and simulate a cohort of workers from age 25 to age 67. Workers experience a local business cycle, changing the aggregate state of unemployment risk, liquidation risk, and employment offer arrival rates, lasting 26 months and happening with a 47 month spacing over the simulated life-cycle.[31]

[30] While there are 13 total parameters remaining in the model, we only need to calibrate 11 of these parameters, as the value of ξ_1^M in each market condition M is implied by the values chosen for ξ_2^M and ξ_3^M along with our assumption that workers receive only up to three periods of advance notice about potential liquidations. In this sense, the model is over-identified despite having 12 targeted moments and 13 parameters that are not set externally.

[31] To ensure that the share of observations from each market condition in our simulated data match those in our empirical data when calculating moments, we take weighted averages for population-level calculations and randomly select a properly-balanced sub-sample for simulated regressions.

Workers who learn that their firm is in distress become part of our simulated "quasi-experimental sample." To mimic the sample restrictions in the data, we condition our simulated bankruptcy sample of workers on having twelve months of potential tenure at the firm at the time of the judge's decision and being aged 62 or below so we can observe simulated outcomes at least four years after the petition.[32] Workers whose firm is liquidated—including those who leave the firm before the judge makes the decision—comprise the treated workers in our simulated sample, with the remainder of workers who experience distress being untreated.

Moments replicating 2SLS estimates from Section 4 are weighted according to their t-statistics, while moments that refer to the overall population receive a weight of one. Because worker reallocation away from distressed firms is central to understanding the costs of layoff, we assign a weight of ten to the reallocation rates in each local market state in order to ensure that the model replicates the rates found in our empirical evidence. The full list of targeted moments and their respective weights can be found in Appendix Table D.2.

Identification. Although we have no proof of identification, we provide a discussion of how different moments provide information that aid in estimating the parameters of interest.

There are four key parameters determining search behavior in the model. We start by considering how the elasticity of search costs with respect to effort, η, and the scale of search costs, μ, affect the worker's choice of effort. A higher search effort in response to layoff risk, implies a lower risk of experiencing a layoff and a faster recovery of their income, suggesting that the effect of liquidation on wage income in year 1 helps identify the elasticity η. Similarly, the higher the scale of search costs, μ, the less search effort workers will exert, and thus the longer unemployment spells will last. Thus, the effect of liquidation on UI benefit income in year 4 pins down the scale of search costs μ. Holding a worker's search behavior fixed, the maximum offer arrival rates affects employee transitions from unemployment to employment, λ_u, and from one employer to another, λ_e.

The distribution of advance information, captured by the parameters $\{\xi_n^M\}_{n=1}^3$, determines workers' ability to respond to a pending bankruptcy petition. Greater access to advance information means more time to search for a new employer and, thus, higher likelihood of making an EE transition before the judge's decision. Moreover, the beliefs induced by the distribution of advance information play an important role in determining workers' reservation wages. We argue that the combination of the share of workers leaving their firm prior to the bankruptcy judge's decision in each market state and the average loss of wage income from year 0 to year 4 in each state help pin down the distribution of beliefs.

The rate at which workers gain human capital while employed, Δ_h^+, determines how quickly workers can recover their prior level of earnings following a layoff. Thus, the rate of human capital gain, Δ_h^+, is determined by the effect of liquidation on wage income in year 4. Similarly, the

[32] Because distress is, by construction, randomly assigned to workers in the model, we do not need to rely on the 2SLS approach and instead use the analogous OLS regressions.

rate at which workers lose human capital while unemployed, Δ_h^-, determines the size of persistent earnings loss following a layoff, with more rapid loss of human capital making prolonged unemployment more costly. Thus, the rate of human capital loss, Δ_h^-, is identified by the effect of liquidation on employment in year 4.

Finally, the residual utility scaling parameter, φ, determines workers' savings behavior near retirement, with larger values of φ encouraging greater savings. Targeting the ratio of average wealth of workers at age 67 to average wealth of workers at age 64 allows the model to approximate the savings behavior we observe empirically. In particular, the scaling of the residual utility term $\nu(a_{T+1})$ prevents workers from excessively dissaving as they approach the end of the model's finite horizon.

6.3 Results and Fit

We report the full set of calibrated parameters andsimulated moment values in Appendix Table D.2. The model successfully replicates our 2SLS estimates. The model generates simulated estimates that fall within the 95% confidence interval around our empirical estimates of the impacts on employment in year 4, wage income and UI benefit income in both year 1 and year 4, and the average wage income loss from year 0 to year 4 in each local market condition. In terms of population moments, the fitted model generates slightly lower UE transition rates and slightly higher EE transition rates than what we observe in the data, as well as slightly higher savings near the age of retirement.

In addition to our targeted moments, we verify that the model generates reasonable estimates of several non-targeted moments. We calculate the average effect of liquidation on disposable income in year 0 through year 4 and report the results in Appendix Table A.12. We find that the average effect of liquidation on disposable income in the model is approximately 42 percent of the effect of liquidation on wage income, which is roughly consistent with our finding in Table 7, and suggests that the UI benefit system in the model offsets a similar portion of the loss in wage income as in the data. Moreover, we find that asset decumulation offsets an additional 43 percent of the loss in wage income, leaving a consumption expenditure pass-through of around 15 percent in the model and an average effect of liquidation on consumption of approximately -NOK27,700, both of which are within the 95% confidence interval around our estimates from Table 8. The overall asset decumulation due to liquidation is approximately 57 percent of the untreated mean, which matches our finding from Table 7, and suggests that the model does a reasonable job of matching the use of savings as an insurance mechanism when facing potential loss of employment. Finally, we use simulated data to estimate the average unemployment duration at pre-unemployment wages near the kink in the UI benefit schedule. We estimate an elasticity of unemployment duration with respect to benefits close to 1, which is in line with the literature using a regression kink design (RKD) to estimate the elasticity of unemployment duration with respect to benefits (see, e.g., Schmieder & Von Wachter (2016)). We also evaluate the model by performing a wage decomposition in Appendix Figure D.2, showing that human capital and search play an important role in shaping the earnings recovery.

7 Assessment

This section uses the estimated model to understand how worker reallocation shapes the welfare costs of financial distress.

7.1 The Value of an Employer

We begin by quantifying the value of an employer. To do so, we calculate the employees' willingness to pay to keep their current job (i.e., avoiding liquidation). This quantity is defined as the amount of assets the worker would be willing to forgo at the moment the judge makes her decision to have the case dismissed. Workers who leave the distressed firm prior to the bankruptcy judge's decision have a WTP of zero by definition. For everyone who remains in the distressed firm until the decision occurs, we calculate an asset differential Δ_a satisfying the indifference condition

$$V^e_{\hat{t}+1}\left(a-\Delta_a, h, w, d_0, k, M\right) = V^u_{\hat{t}+1}\left(a, h, b(w,h), d_0, k, M\right) \tag{7}$$

where V^i_t denotes the value of each employment status $i \in u, e$; d_0 indicates being in the non-distressed state; and $\hat{t}$ denotes the worker's age when the judge's decision is made. The left-hand side of the indifference condition denotes the worker's value if the case is dismissed and the worker's net wealth is reduced by Δ_a. The right-hand side of the condition denotes the worker's value at the moment the job is terminated—capturing the potential loss of human capital during the subsequent unemployment spell plus the expected utility cost of finding a new job and spending savings now rather than later, net of the unemployment benefits—assuming people are aware of the outside options in their local labor markets.

Table 9: The Value of an Employer and Selection on Future Layoff Costs

Scenario:	***I. Baseline***	***II. Outside Options***		***III. Ideal Experiment***
		Strong Market	**Weak Market**	**No Advance Information**
Willingness to Pay	439.312	407.740	468.149	502.151
NPV of Income Loss	-445.967	-430.651	-463.852	-475.861
Share Remaining at Firm	0.735	0.624	0.833	1.000
WTP Conditional on Remaining	524.622	653.430	562.003	502.151

Notes: This table reports the value of keeping the current employer to workers in distressed firms. We report the average amount of liquid assets that the worker would be willing to forgo to have their firm's bankruptcy case dismissed by the judge as well as the average net present value of the simulated impact on disposable income over the remaining life-cycle. The first column reports values from our baseline fitted model. The second and third columns report the estimated values among the sub-samples of workers who face strong and weak local labor market conditions, respectively. The fourth column reports the estimates in a counterfactual model where workers receive no advance information about the pending liquidation and instead immediately face layoff.

On average, we calculate that the WTP is about 439,000 NOK. We benchmark the average WTP to the net present value (NPV) of the impact of liquidation on disposable income over the remaining life-cycle. To compute these present values, we calculate the average effect of liquidation on the simulated workers' disposable income $\tilde{t}$ periods after the judge's decision. We then compute the average age of workers at the time of the judge's decision, and compute the NPV as the discounted sum of the effects of liquidation each period of the average worker's horizon. We find that the average WTP equals approximately the NPV of the income loss at the time a judge makes her decision.[33] These calculations are reported in Panel I of Table 9, together with the share of worker's who remain at the distressed firm at the time of the judge's decision and the average WTP among those who stay. As illustrated in Appendix Figure A.10a, the WTP increases sharply with the employer quality.

7.2 The Value of Outside Options

Next, we examine how the value of the employer depends on whether the firm is liquidated in a strong or weak local market. In the data, there are 0.578 jobs per job seeker in strong labor markets, and 0.178 in weak labor markets, measured at the time of distress. In the second and third columns of Table 9, we report how the average WTP and corresponding NPV of disposable income losses differ for employees facing strong and weak local market conditions. The average WTP in weak market conditions is approximately 15 percent higher than the WTP in strong market conditions, corresponding to an elasticity of -0.21 with respect to changes in the number of job openings per job seeker. This differential is partly driven by changes in the share of workers who remain at the distressed firm at the time of the judge's decision: About 17 percent of employees avoid layoffs through employer-to-employer transitions in weak markets, whereas 37 percent avoid layoff in strong labor markets, lowering the average WTP. As expected, Appendix Figure A.10b shows that the share of workers leaving the employer prior to the judge's decision declines with job quality, except at the lowest rung, where workers have almost no value to lose.

The model also allows us to decompose the average WTP into leavers (with a WTP of zero) and workers who stay in troubled firms (i.e., the stayers). The last row of Table 9 reports the WTP among stayers, showing that selection on future layoff costs is particularly prevalent in strong labor markets.

[33] This amount reflects risk aversion and added income uncertainty following employer shutdown as well as the disutility incurred when exerting search effort as workers seek to re-climb the ladder. We quantify the role of each in columns (1) and (2) of Panel (b) of Appendix Table A.13 by computing counterfactual WTPs, removing these forces one at a time. Column (1) of Appendix Table A.13 reports the WTP from an identical sample of workers but under the assumption that workers are risk neutral. Turning off the desire for consumption smoothing makes it easier for workers to recover from the effects of liquidation in utility terms, as they can more effectively offset current-period losses by increasing future consumption. However, the costs of recovering lost employment positions ensure that workers still have a substantial WTP, even without risk aversion. Column (2) reports the counterfactual WTP we obtain when calculating the WTP among an otherwise identical-to-the-baseline sample, assuming that search effort incurs zero cost. Search costs play a dual role in calculating the WTP, as they reduce the worker's utility both directly and by prolonging the time it takes for workers to recover their pre-liquidation income level and thus have a slightly larger contribution to the baseline WTP figure compared to risk aversion. Combined, these two factors are sufficient to explain approximately one-third of the workers' baseline willingness to pay to avoid liquidation.

7.3 The Value of Self-Insurance

Next, we calculate how learning about pending layoffs affects welfare by constructing a counterfactual experiment without advance information. In this experiment, we set the share of workers who learn about the pending layoff to zero. This configuration effectively removes workers' ability to respond to the petition before the judge's decision. Because it holds the composition of workers fixed, we refer to it as the *ideal experiment*, as it represents a natural benchmark to evaluating the benefit of worker reallocation. We report the findings of this experiment in Panel III of Table 9, and simulated effects on employment, wage income, disposable income, and liquid assets are reported in Appendix Table A.12.

Our headline result is that the average WTP to avoid liquidation increases by 14 percent under the ideal experiment. Comparing the last two rows of Table 9 highlights that approximately one-quarter of workers leave the distressed firm before the decision in the baseline model—and thus have a WTP of zero—while all workers in the no-information scenario remain at the firm when the employer shuts down. Importantly, stayers in the baseline model have a higher WTP than stayers in the experiment, suggesting that welfare costs would be almost five percent higher when focusing on stayers compared to the average WTP for the full population of employees.

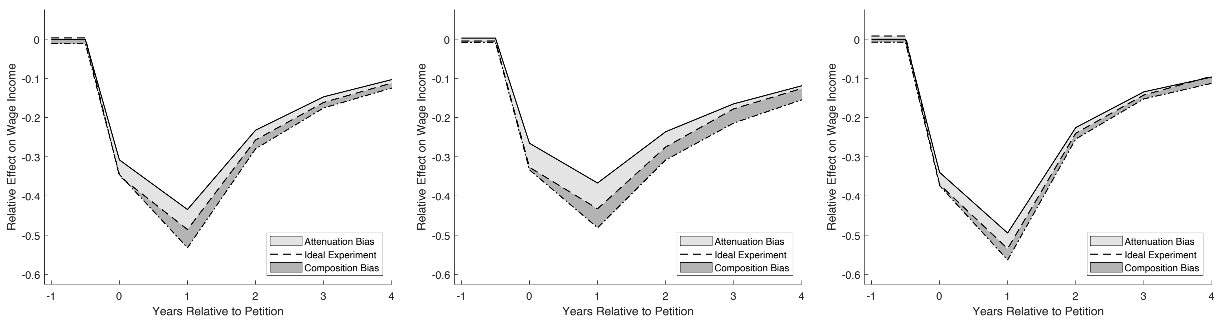
7.4 Implications for Measured Layoff Costs

Our final exercise quantifies how knowledge about financial risk shapes the composition of workers who experience layoff. In Panel (a) of Figure 8, we illustrate how earnings costs vary across three scenarios. The dashed line represents the ideal experiment, where all workers, irrespective of outside options experience layoff. The top line includes workers who leave in anticipation of layoff, where worker transitions result in smaller reductions in wage income and in some cases may even lead to wage gains as workers find higher quality jobs due to the increase in search effort. The bottom line includes only the earnings change for stayers, who have relatively poor outside options and are the workers with the most to lose. The shaded area from the top represent the bias from attenuation, while the shaded area from the bottom reflect composition bias. On average, the measured layoff cost is 20 percent higher than the fitted model, and about 10 percent higher than the " ideal experiment". The averages over the five-year period are reported in Panels (a) through (c) in Appendix Table A.12.

Two important restrictions are worth noting when evaluating these calculations. First, we have imposed no restrictions on tenure in the firm. This restriction is important, as human capital accumulates over the career and deteriorates during unemployment, thus longer-tenured workers face larger earnings losses and have comparatively weaker outside options than shorter-tenured workers on average. Hence, imposing longer tenure requirements, as is typically done in layoff research, would both reduce the anticipation effect—bringing our quasi-experimental estimate closer to the ideal experiment—and increase the composition bias—further increasing the difference between the layoff estimate and ideal experiment.[34] Second, we allow no more than three months of advance information. Allowing for additional periods of advance information could further attenuate, if not eliminate, the estimated effect of liquidation on wage income. This attenuation would be driven by the increased likelihood of finding a suitable offer given additional search opportunities and by the fact that those searching to avoid layoff could afford to have higher reservation wages due to the increased horizon for search.

Figure 8: Effects of Liquidation on Wage Income by Experiment Type

(a) Aggregate (b) Strong Markets (c) Weak Markets



Notes: This figure illustrates how our estimate of the effect of liquidation on wage income in years 0 to 4 differs across our counterfactuals. Figure 8a reports average estimates while Figures 8b and 8c report estimates in the sub-samples of workers facing strong and weak local labor market conditions, respectively. The solid black line in each figure represents the estimate from our baseline fitted model (i.e., the judge experiment), while the dashed line represents the results under our ideal experiment where workers receive no advance information about the pending liquidation and the dash-dotted line represents the results of a layoff regression, constructed by conditioning on workers who remain at the firm at the time the bankruptcy judge's decision is made. The light-grey shaded area between our baseline and ideal experiment estimates represents the attenuation of the estimated effect of liquidation due to workers' anticipatory search behavior. The dark-grey shaded area between our ideal experiment and standard layoff estimates represents the amplification of the estimated effect due to adverse selection on layoff costs (i.e., the workers who remain at the firm are those with the poorest outside options).

Finally, we illustrate how the composition effect depends on workers' outside options. Figure 8b shows that learning about risk plays a much larger role in attenuating the estimated effect of liquidation in strong local markets, when workers have a greater ability to respond to information. As a consequence of this heightened responsiveness, the compositional changes are much more prevalent in strong markets. Figure 8c demonstrates that both attenuation and composition biases are much smaller in weak markets, because the relative lack of job opportunities reduces workers' ability to respond to learning about a future layoff. This brings both judge-design and layoff regression evidence closer to the *ideal* layoff regression without attenuation or composition effects.

[34] Appendix Figure A.9 performs a selection correction in which we measure both the effects of employer shutdown and the WTP in sub-samples defined by the worker's human capital level and then re-weight our results to match the distribution of human capital in the sample of unemployed workers who do not experience distress rather than the distribution among those satisfying the potential tenure condition. We find that correcting for this selection on human capital causes our baseline WTP estimate to fall from NOK439,000 to NOK428,000 and generates slightly smaller estimates of the impact on wage and disposable income.

8 Conclusion

In this paper, we assessed the costs of employer distress to employees. We built a novel data set measuring income, wealth, job opportunities, and quasi-experimental variation in bankruptcy risk in Norway to address concerns of selection and omitted variables. The key to our empirical approach was that bankruptcy judges are randomly assigned and vary in how often they liquidate a firm. We found that employer shutdown causes a five-year average earnings decline of 24 percent. We also found that about a quarter of employees avoided layoff by transitioning to a new employer before the judge decided to liquidate the firm. Moreover, strong local labor markets and high-wage employers are associated with higher pre-bankruptcy employee reallocation and lower layoff costs, indicating that worker mobility is an important insurance mechanism. By comparison, the insurance from taxes and public transfers reduced the income loss by an equal amount.

To understand the welfare benefits of employee turnover, we estimated a structural model with savings, learning about bankruptcy risk, and on-the-job search. Using the model, we found that workers were willing to pay about the same as the net present value of the income loss to keep their current employment. Heterogeneity in the value of an employer and outside options were key drivers of (i) worker turnover in the data and (ii) willingness to pay to save an employer in the model. Our main finding is that shutting off learning about financial risk increased the welfare cost of liquidation by 14 percent, indicating that the search process provides workers with important insurance against risk. Taken together, our calculations indicate that employers disclosing financial information to employees can yield important benefits to employees.

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For Online Publication:

Online Appendix:

Layoff Costs and Learning About Employers' Financial Distress

BY ANDREAS R. KOSTØL, MATTHEW MERKLE, AND MORTEN GRINDAKER,

A Appendix: Additional Tables and Figures

Table A.1: Cross-Sample First Stage Estimates

	(I) Baseline instrument		(II) Reverse-sample instrument	
Dependent variable	$Pr(Allow)$		$Pr(Allow)$	
Young workers (below 35)	.948	Dep. mean: .538	.890	Dep. mean: .539
	(.026)	N: 24796	(.032)	N: 22761
Older workers	.926	Dep. mean: .582	.847	Dep. mean: .584
	(.023)	N: 29221	(.029)	N: 26043
Females	.935	Dep. mean: .572	.871	Dep. mean: .573
	(.040)	N: 13053	(.046)	N: 12215
Males	.906	Dep. mean: .559	.685	Dep. mean: .557
	(.028)	N: 40964	(.043)	N: 29991
Married	.932	Dep. mean: .581	.877	Dep. mean: .584
	(.023)	N: 22836	(.027)	N: 21092
Unmarried	.934	Dep. mean: .548	.864	Dep. mean: .552
	(.026)	N: 31181	(.032)	N: 27666
Foreign-born	.896	Dep. mean: .537	.774	Dep. mean: .542
	(.035)	N: 9678	(.043)	N: 9137
Native-born	.926	Dep. mean: .567	.706	Dep. mean: .557
	(.035)	N: 44339	(.054)	N: 29884
Less than high school	.917	Dep. mean: .552	.872	Dep. mean: .555
	(.027)	N: 23167	(.031)	N: 21287
At least high school	.948	Dep. mean: .570	.897	Dep. mean: .574
	(.024)	N: 30850	(.030)	N: 27914
High wage	.959	Dep. mean: .549	.872	Dep. mean: .553
	(.024)	N: 27007	(.037)	N: 25021
Low wage	.883	Dep. mean: .575	.767	Dep. mean: .573
	(.031)	N: 27010	(.040)	N: 23384
Many Employees	1.27	Dep. mean: .587	.468	Dep. mean: .576
	(.053)	N: 26438	(.160)	N: 23370
Few Employees	.434	Dep. mean: .537	.101	Dep. mean: .537
	(.060)	N: 27579	(.044)	N: 14481

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Notes: This table reports heterogeneity in first stage estimates using the baseline instrument (I) and the reverse-sample instrument (II). The first stage specification in (I) corresponds to subsample estimates of the first stage using our baseline judge stringency measure. The reverse-sample instrument (II) is constructed by calculating judge stringency using all cases *except* for those in the specified subsample (e.g., judge stringency for the subsample of older applicants is constructed using judges' decisions for younger applicants). The baseline and the reverse sample instrument in this exercise are based on all firm cases with employees (including cases outside of our estimation sample but not including firms without employees).

Table A.2: **Reduced Form Effects on Employment**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**
Panel (a):	Full Time Work				
Judge stringency	-0.426***	-0.254***	-0.104	-0.062	-0.058
	(0.112)	(0.082)	(0.069)	(0.067)	(0.061)
Overall mean of dependent variable	0.675	0.662	0.670	0.673	0.672
Panel (b):	Any Employment				
Judge stringency	-0.456***	-0.223***	-0.132**	-0.104*	-0.078
	(0.105)	(0.076)	(0.066)	(0.059)	(0.060)
Overall mean of dependent variable	0.704	0.708	0.723	0.733	0.731
Panel (c):	Total Wage Income (NOK1,000)				
Judge stringency	-100.082**	-97.198**	-83.039**	-88.340**	-56.444
	(39.578)	(48.547)	(41.461)	(42.036)	(40.507)
Overall mean of dependent variable	429.722	403.118	402.357	406.839	407.947
Number of observations:	14,826	14,826	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the reduced form estimates of judge stringency on employment and total wage income. The sample includes firms with at least five employees before the petition is received and full-time employees aged 25–62. There are 312 judges. Each regression controls for fully interacted year and court indicators, a linear and quadratic of leave-out judge processing time, and individual control variables (see Table 4 for details). Employment is defined as having any wage income in a year. Unemployment is receiving at least NOK1,000 in unemployment benefits over the calendar year.

Table A.3: 2SLS Estimates: Worker Mobility Rates

Years after decision (t): 4			
	(1)	(2)	(3)
Dependent variable:	Occupational Mobility	Industry Mobility	Geographical Mobility
Liquidation (β)	-0.030	-0.024	0.003
	(0.132)	(0.045)	(0.048)
Overall mean of dependent variable	0.565	0.370	0.081
Untreated mean	0.520	0.449	0.042
Number of observations	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Each regression controls for fully interacted year and court indicators, a linear and quadratic of leave-out judge processing time, and individual control variables (see Table 4 for details).

Table A.4: 2SLS Estimate: Labor Force Participation

Years after decision (t): 4				
	(1)	(2)	(3)	(4)
Dependent variable:	**UI Receipt**	**DI Receipt**	**Any Labor Earnings**	**Labor Force Participation**
Liquidation (β)	0.025	-0.025	-0.103*	-0.107*
	(0.056)	(0.024)	(0.058)	(0.058)
Overall mean of dependent variable	0.117	0.027	0.868	0.876
Untreated mean	0.107	0.013	0.937	0.951
Number of obs.	14,826	14,826	14,826	14,826
15,073				

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the second stage coefficients of equation 2, overall, and untreated means of the dependent variable four years after the bankruptcy petition. The sample includes firms with at least five employees before the petition is received and full-time employees aged 25–62. There are 312 judges. Each regression controls for fully interacted year and court indicators, a linear and quadratic of leave-out judge processing time, and individual control variables (see Table 4 for details). Full-time employment is equal to one if the year-end work contract states at least 30 hours per week and is otherwise zero. Disability insurance receipt is an indicator equal to 1 if a person receives any DI benefits during the year. The indicator for any labor earnings equals 1 if the person has at least NOK1 in labor earnings in a given year. Labor force participation equals having any labor earnings or receiving any unemployment benefits in a given year.

Table A.5: Sectoral Mobility

Dependent variable:	**A. Occupation Move**	**B. Industry Move**	**C. Geography Move**
Liquidation (β)	-0.030	-0.024	0.003
	(0.132)	(0.045)	(0.048)
Overall mean of dependent variable	0.565	0.370	0.081
Untreated mean	0.520	0.449	0.042
Number of observations	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the 2SLS estimates of equation 2 and the average of the dependent variable for the overall and untreated samples. The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Each regression controls for fully interacted year and court indicators, a linear and quadratic of leave-out judge processing time and individual control variables (see Table 4 for details). Mobility is measured as the cumulative chance of being observed in a different occupation, industry, or commuting zone over the four years after the petition.

Table A.6: Alternative Methods to Residualize Vacancy-Unemployment Ratios

Years after decision (t): average 0–4, NOK1,000 = USD100								
	A. Occupation FE and Individual Controls				**B. Occupation FE**			
	I. Weak Market		**II. Strong Market**		**I. Weak Market**		**II. Strong Market**	
	Firm Closes	**Wage Income**	**Firm Closes**	**Wage Income**	**Firm Closes**	**Wage Income**	**Firm Closes**	**Wage Income**
	(1)	(3)	(1)	(3)	(1)	(3)	(1)	(3)
Judge Stringency	0.930***	-122.783**	0.743***	-46.593	0.923***	-118.096**	0.721***	-48.375
	(0.250)	(54.939)	(0.227)	(44.011)	(0.250)	(53.769)	(0.224)	(45.046)
Liquidation (β)		-131.981**		-62.692		-127.951**		-67.096
		(57.738)		(59.458)		(56.592)		(63.424)
Overall mean	0.546	385.255	0.559	431.102	0.549	385.058	0.556	431.218
Untreated mean		461.875		448.321		461.679		454.686

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the first stage, reduced form, and second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. Untreated means are potential outcome of employees in marginal firms (see Dahl *et al.*, 2014). The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Each regression controls for fully interacted year and court indicators and includes the individual demographic control variables from Table 2 and dummies for each quintile of pre-petition wages. Average exchange rate in 2024 is NOK/$=10.

Table A.7: 2SLS Estimates: Heterogeneity

Years after decision (t): average 0–4, NOK1,000 = USD100								
	A. Above Age 40		**B. Below Age 40**		**C. Low Skilled**		**D. High Skilled**	
	Firm Closes	**Wage Income**	**Firm Closes**	**Wage Income**	**Firm Closes**	**Wage Income**	**Firm Closes**	**Wage Income**
	(1)	(3)	(1)	(3)	(1)	(3)	(1)	(3)
Judge Stringency	0.591***	-136.210**	0.966***	-47.718	0.737***	-69.926*	0.811***	-83.002*
	(0.224)	(55.525)	(0.210)	(36.606)	(0.219)	(42.106)	(0.215)	(43.781)
Liquidation (β)		-230.536**		-49.375		-94.928		-102.363**
		(110.542)		(36.852)		(58.357)		(51.439)
Overall mean	0.593	420.667	0.519	400.889	0.549	322.655	0.555	462.021
Untreated mean		529.514		424.839		366.773		493.737
Pre-layoff turnover	0.247		0.250		0.233		0.203	
Observations:	6,817		7,992		5,526		9,281	

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the first stage, reduced form, and second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. Untreated means are potential outcome of employees in marginal firms (see Dahl *et al.*, 2014). The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Each regression controls for fully interacted year and court indicators and includes the individual demographic control variables from Table 2 and dummies for each quintile of pre-petition wages. The mean wage income is winsorized at the 99th percentile and disposable income is winsorized at the 95th percentile. Outcomes are averaged over the five-year period and are deflated to 2015-values using consumer price index. Median age is 40 years. Low skilled is having less than high-school and high skill is high school or more. Average exchange rate in 2024 is NOK/$=10.

Table A.8: 2SLS Estimates: Currently Married

Years after decision (t):	**0**	**1**	**2**	**3**	**4**
	Currently Married				
Liquidation (β)	0.046	0.061	0.067	0.022	-0.028
	(0.056)	(0.059)	(0.061)	(0.065)	(0.075)
Overall mean of dependent variable	0.378	0.389	0.400	0.408	0.414
Untreated mean	0.339	0.346	0.359	0.394	0.438
Number of observations:	14,826	14,826	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.
Note: This table reports the second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Each regression controls for fully interacted year and court indicators, a linear and quadratic of leave-out judge processing time, and individual control variables (see Table 4 for details). Marital status is measured by the end of the year and does not account for co-habitation.

Table A.9: Impacts on Components of Imputed Consumption

Years after decision (t):		**4**		**0–4**	**0–4**	**0–4**
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Δ **Net Assets**	Δ **Active**	Δ **Debt**	**Business Income**	**Capital Income**	**Stock Income**
Liquidation (β)	-296.550**	-207.374*	-46.448	-0.191	-0.886	-4.660
	(147.991)	(115.727)	(138.873)	(0.954)	(0.980)	(14.183)
Overall mean of dependent variable	498.733	332.833	316.679	1.406	3.495	32.777
Untreated mean	557.069	324.391	448.758	2.018	3.266	17.991
Number of obs.	14,826	14,826	14,826	14,826	14,826	14,826
15,073						

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.
Note: This table reports the second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Each regression controls for fully interacted year and court indicators, a linear and quadratic of leave-out judge processing time, and individual control variables (see Table 4 for details). Outcome variables are deflated to 2015-values using consumer price index and winsorized at 95th percentile. Average exchange rate in 2024 is NOK/$=10.

Table A.10: 2SLS Estimates: Credit Standing

Years after decision (t): 4	Δ Debt	Net Debtor	Personal Bankruptcy	Wage Garnishment	Average Interest Rates
	(1)	(2)	(3)	(4)	(5)
Liquidation (β)	-46.448	0.080	0.007	0.046	-0.007
	(138.873)	(0.077)	(0.011)	(0.059)	(0.008)
Overall mean of dependent variable	316.679	0.339	0.004	0.118	0.046
Untreated mean	448.758	0.313	-0.006	0.148	0.039
Number of observations:	14,826	14,826	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: This table reports the second stage coefficients of equation 2 and the average of the dependent variable for the overall and untreated samples. The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Each regression controls for fully interacted year and court indicators, a linear and quadratic of leave-out judge processing time, and individual control variables (see Table 4 for details). Personal bankruptcy is equal to one if the person is granted debt relief, and otherwise zero. Net debtor is equal to one if debt exceeds total wealth, and otherwise zero. Wage garnishment is equal to one if the person is observed with any garnishment during the year, and otherwise zero. Car collateral is an indicator variable for whether someone uses their car as collateral for a loan. All variables are deflated to 2015-values using consumer price index. Interest rates are calculated from top 1% coded interest payments over total year-end debt. Average exchange rate in 2024 is NOK/$=10.

Table A.11: Robustness of 2SLS Estimates

Panel (a):	Wage Income (Average 0–4 NOK1,000)					
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation (β)	-107.014**	-113.079**	-109.235**	-102.762***	-106.490*	-121.633*
	(48.702)	(50.746)	(49.229)	(32.393)	(55.444)	(66.674)
Panel (b):	Disposable Income (Average 0–4 NOK1,000)					
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation (β)	-52.557**	-55.315**	-58.752**	-39.677***	-55.614**	-87.167
	(22.859)	(26.058)	(23.300)	(14.501)	(25.736)	(54.101)
F-value	15.45	9.84	7.92	10.73	6.436	15.45
Court*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Minimum Cases per Judge	20	30	20	20	20	20
First Stage Specification	Linear	Linear	Quadratic	Cubic	Linear	Linear
Judge Processing time	Quadratic	Quadratic	Quadratic	Quadratic	Cubic	Quadratic
Winsorizing	Yes	Yes	Yes	Yes	Yes	No
Observations	14,826	12,002	14,826	14,826	14,826	14,826

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge-year level.

Note: The sample includes firms with at least five employees the year before the petition. The estimation sample includes full-time employees aged 25–62 employed 12 months prior to petition. There are 312 judges. Regressions always control for year and court indicators and dummies for each age, educational group, marital, immigration status, pre-petition wage quintile, and a quadratic function of leave-out judge processing time, unless otherwise specified. Wage income is winsorized at 99th and disposable income is winsorized 95 percentile levels and deflated to 2015-values using consumer price index. Average exchange rate in 2024 is NOK/$=10.

Table A.12: Average Effects of Liquidation by Market and Experiment Type

Years after decision (t): average 0–4								
Outcome:	**Employment**		**Wage Income**		**Disposable Income**		**Liquid Assets**	
	Level	*Share*	*Level*	*Share*	*Level*	*Share*	*Level*	*Share*
(a) Fitted Model	-0.113	-0.122	-179.627	-0.247	-75.384	-0.154	-76.552	-0.568
(b) Ideal Experiment	-0.130	-0.141	-201.880	-0.276	-84.308	-0.171	-83.838	-0.651
(c) Mass Layoff	-0.146	-0.156	-220.450	-0.295	-90.983	-0.182	-95.813	-0.652

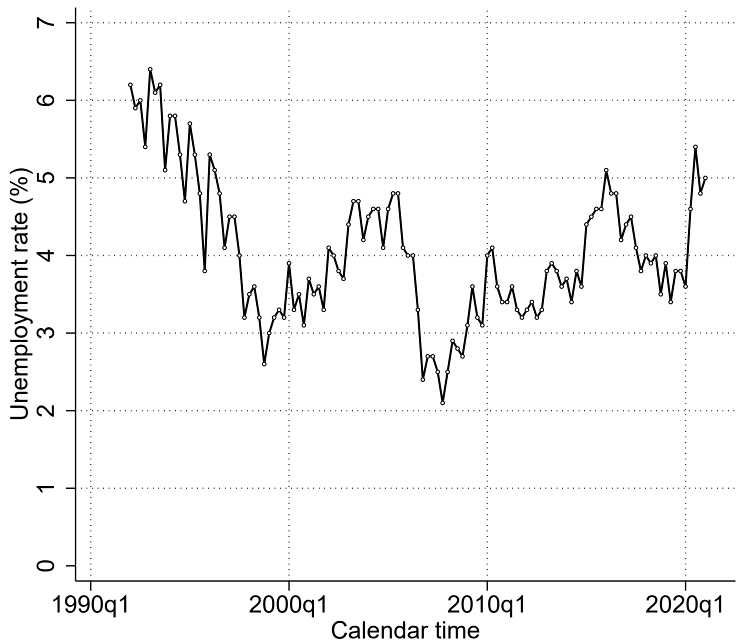
Notes: This table shows the estimated effect of treatment on outcomes in year 0 through year 4 as a level and as a percentage of the untreated average value. We report results from our baseline fitted model, from the counterfactual model where workers do not receive any advance notice that their firm is in distress (i.e., the ideal experiment), and in our baseline fitted model conditioning on workers remaining at the firm at the of the judge's decision (i.e., a mass-layoff). Average exchange rate in 2024 is NOK/$=10.

Table A.13: Decomposing the Value of Keeping an Employer

	Panel (a):	**Panel (b):**		
	Baseline	**No Risk Aversion**	**No Search Costs**	**Residual**
		(1)	(2)	(3)
Willingness to Pay	439.740	394.756	343.862	
Difference		44.985	95.878	298.877
As Share of Baseline		10.230%	21.803%	67.967%

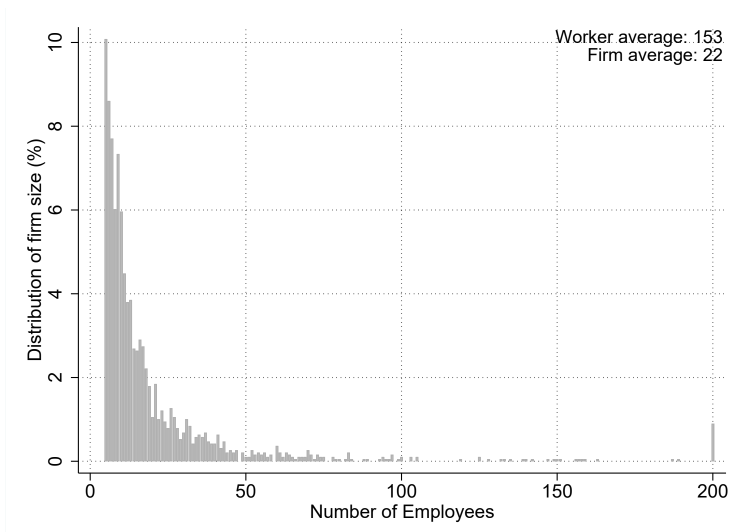
Notes: This table reports the value of keeping the current employment position to workers in distressed firms. Panel (a) reports the average amount of liquid assets that the worker would be willing to forgo to have their firm's bankruptcy case dismissed by the judge rather than undergoing liquidation. Panel (b) decomposes the sources of workers' WTP using counterfactual experiments. Columns (1) and (2) report the estimated WTP under counterfactuals where workers are assumed to be risk-neutral and to face zero search costs, respectively, and report the shares of our baseline WTP figure which can be attributed to these two components. Column (3) reports the residual, which includes any potential interaction effects between risk aversion and search costs (which may be either positive or negative) as well as other factors such as the persistent effects of liquidation on wage income.

Figure A.1: Unemployment Rates



Notes: The figures plots unemployment rate for the adult population from the Labor Force Survey in Norway from 1992 to 2021. The series are from Statistics Norway.

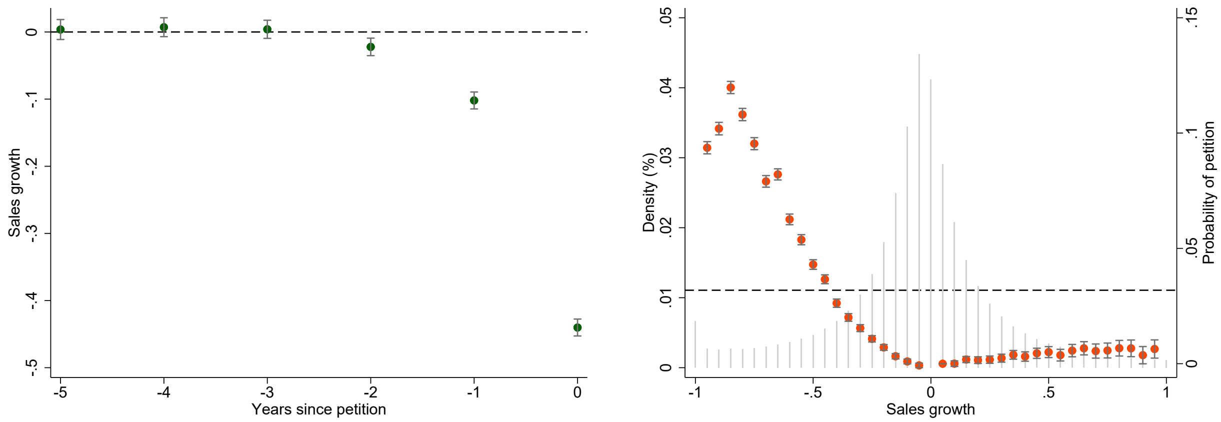
Figure A.2: Firm Size Distribution



Notes: This figure plots the number of full-time employees aged 25–62 in firms petitioned for bankruptcy in 2006–2013.

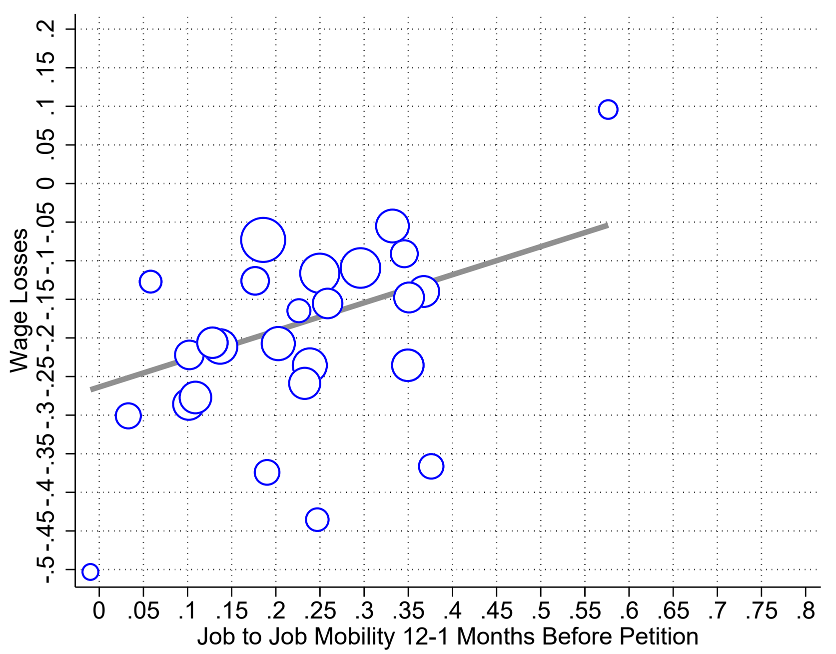
Figure A.3: Sales and Bankruptcy Petitions

(a) Annual Sales Growth (b) Sales Growth and Petition Rates



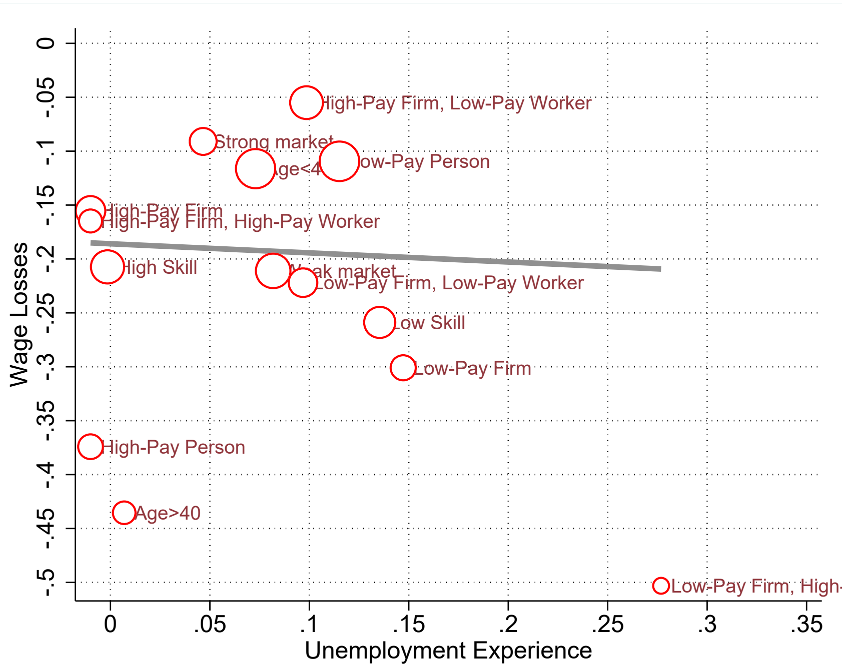
Notes: Panel A.3a shows the average change in sales relative to the petition date for our baseline sample of firms and Panel A.3b shows the distribution of annual sales growth and the relationship between petition rates and sales.

Figure A.4: Robustness to Alternative Methods for Residualizing Labor Market Tightness



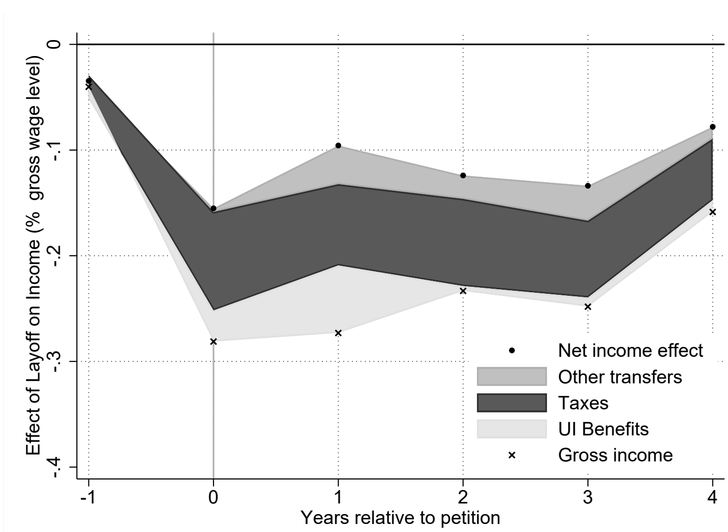
Notes: Baseline specification removes controls for fixed effects for gender, commuting zone and occupations, and a polynomial of age and tenure when obtaining the (residualized) vacancy-unemployment ratio. Alternative specification 1 controls for gender and occupation fixed effects, and a polynomial of age and tenure. Alternative specification 2 only controls for occupation fixed effects.

Figure A.5: Heterogeneous Wage Impacts by Pre-Petition Unemployment Risk



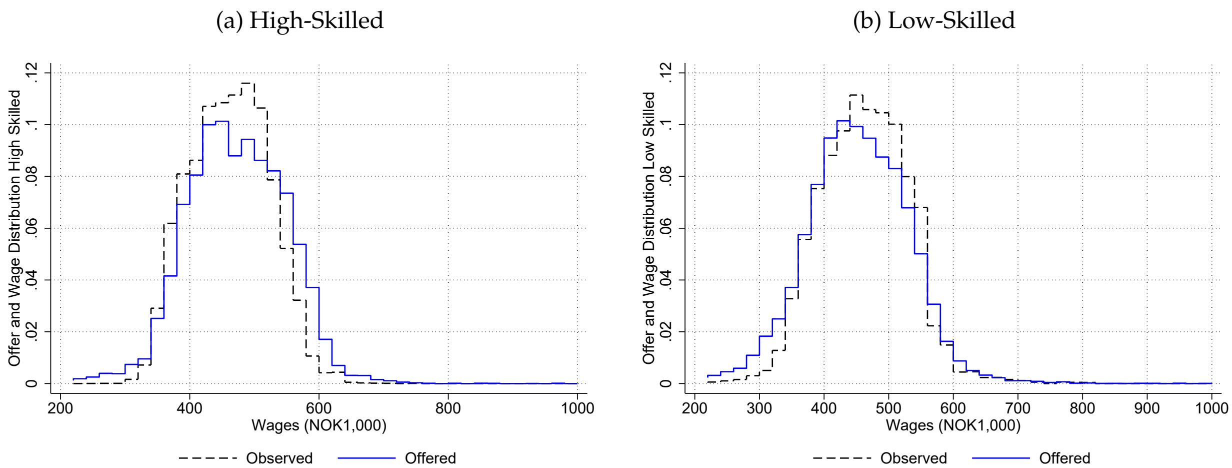
Notes: This figure plots heterogeneity in impacts of shutting down a firm on wage income against the fraction of workers who experience unemployment at some point from 12 months before up to month of the petition. The size of scatter plots reflect the inverse of the standard errors of the estimated impact on disposable income.

Figure A.6: Impacts of Layoff on Income



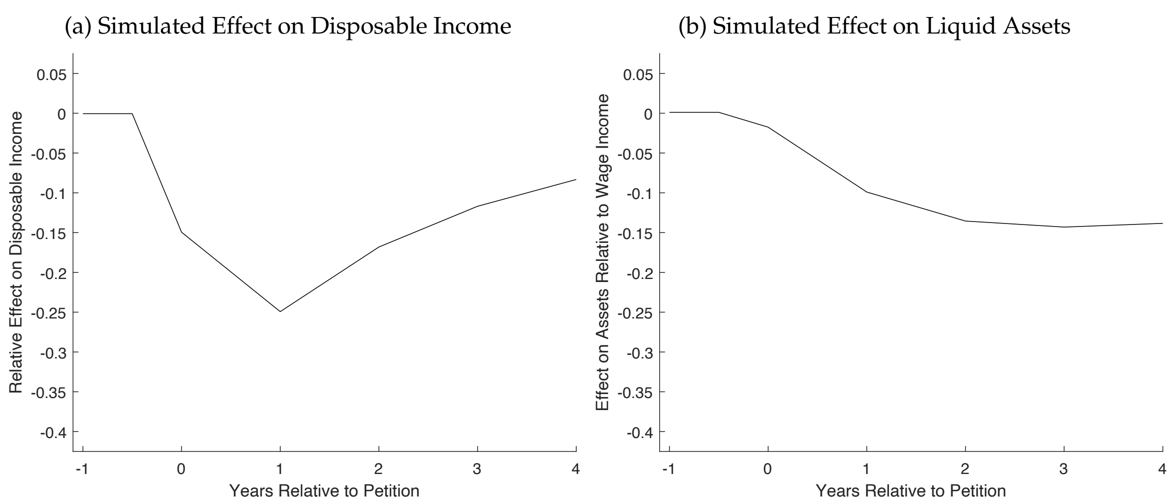
Notes: This figure plots IV estimates on sources of income, all normalized to percent of pre-layoff level of gross earnings. Average exchange rate in 2024 is NOK/USD $\approx$ 10.

Figure A.7: Distribution of Observed and Offered Wages



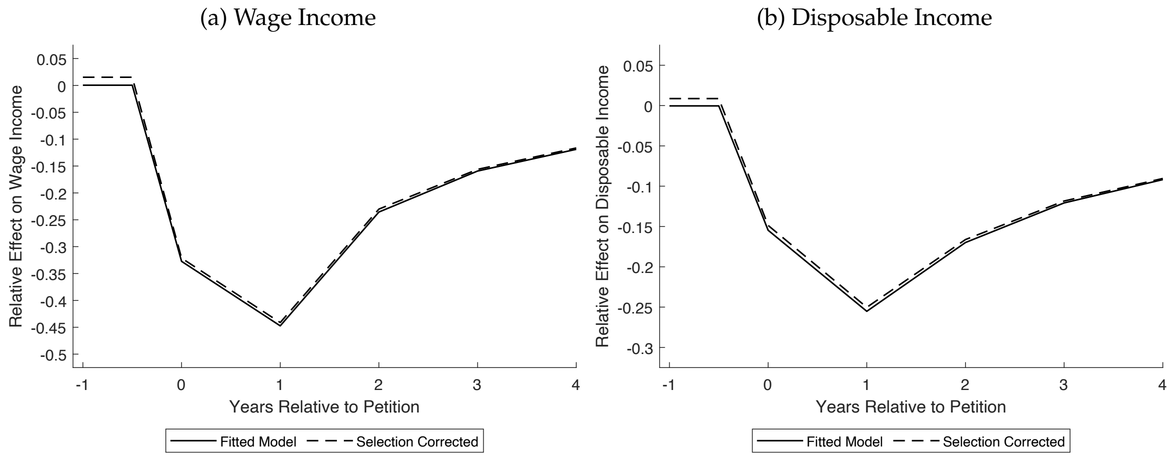
Notes: These figures show the observed and offered wage distributions for each skill type. Observed wage uses residualized wages and the empirical distribution of employees at each wage level. Offered wage uses residualized vacancy-unemployment rates within 3-digit occupation and commuting zone cells as weights. Residualized wages and vacancy-unemployment ratios are normalized to the average and controls for gender, commuting zone- and occupation-fixed effects, and a quadratic function of age and tenure. Average exchange rate in 2024 is NOK/$=10.

Figure A.8: Untargeted Moments



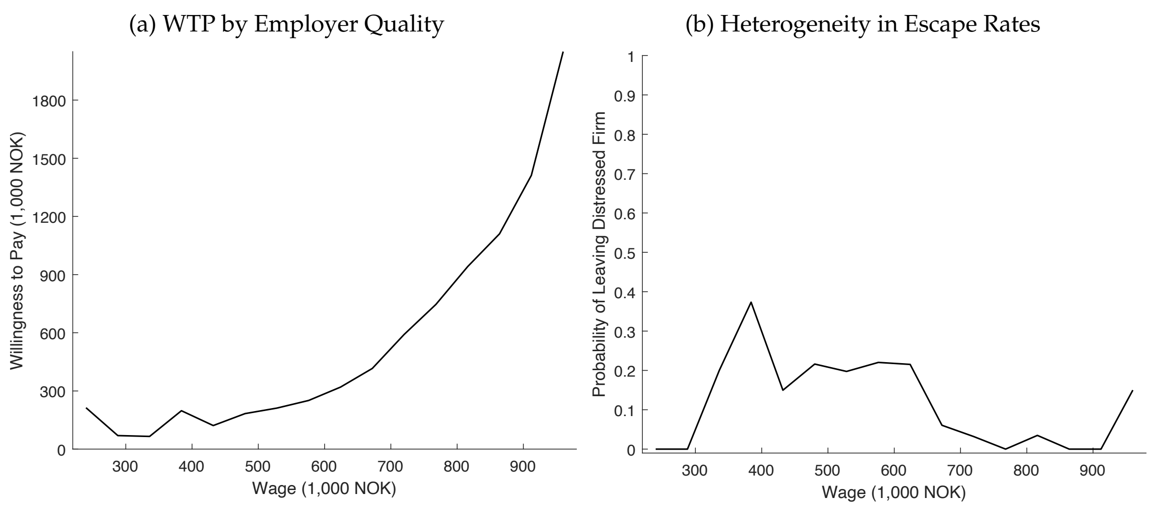
Notes: These two figures show the simulated effect of layoff on disposable income and liquid assets as a share of pre-layoff average annual wage income.

Figure A.9: Selection-Corrected Effects of Employer Shutdown



Notes: These figures plot the estimated effects of employer shutdown on both wage income and disposable income in our baseline fitted model and when correcting for selection on human capital due to the restrictions on our simulated bankruptcy sample. The baseline fitted model conditions the bankruptcy sample on workers having at least 12 months of potential tenure at the firm at the time the judge's decision is made. To construct the selection corrected effects, we estimate the effects on sub-samples corresponding to each level of human capital in the model and then take a weighted-average of these sub-sample estimates based on the distribution of human capital among workers experiencing the standard exogenous separation shock. Effects are reported as a fraction of the untreated complier mean.

Figure A.10: Heterogeneity



Notes: These figures plot how the WTP and share of workers leaving the distressed firm prior to the judge's decision varies with the quality of the employer (i.e., the rung of the wage ladder).

B Data Sources

B.1 Data Sources

Judicial System/DA. This data set provides us with information on cases related to bankruptcy. It provides us with the legal paragraphs, an anonymized identifier for the assigned judge, dates in which the case was received, and decided. It includes some information on creditors, and it includes some information on the proceeds of the court. The leave-out mean judge instrument is based on the full set of cases which a judge has handled.

Administrative Population Registers. Information on workers' geographic location, education, annual earnings, and household income. Social security data is provided by longitudinal administrative registers provided by Statistics Norway. This data covers every Norwegian resident from 1967 to 2019, and contain individual demographic information (including sex, age, zip codes, and years of education) and, since 1993, all sources of annual income, including earnings, self employment income, capital income, taxes, and cash transfers. Most of the income is third-party reported to tax authorities.

Administrative Employer-Employee Register. Workers' earnings and employment histories, and transitions between jobs come from the Norwegian Matched Employer-Employee Register maintained by Statistics Norway. The data is reported by the employer to tax authorities at the end of the year. The reporting was automated at the firm level and based on monthly payments after 2015 and covers a slightly broader set of low-paying jobs. Before 2015 contracts with fewer than 4 hours per week or below an annual amount of NOK10,000 were not reported. The data includes information on the dates of alterations to the contract, and the corresponding wage, industry and occupational codes, geographic location, and tenure at the establishment.

B.2 Sample Construction

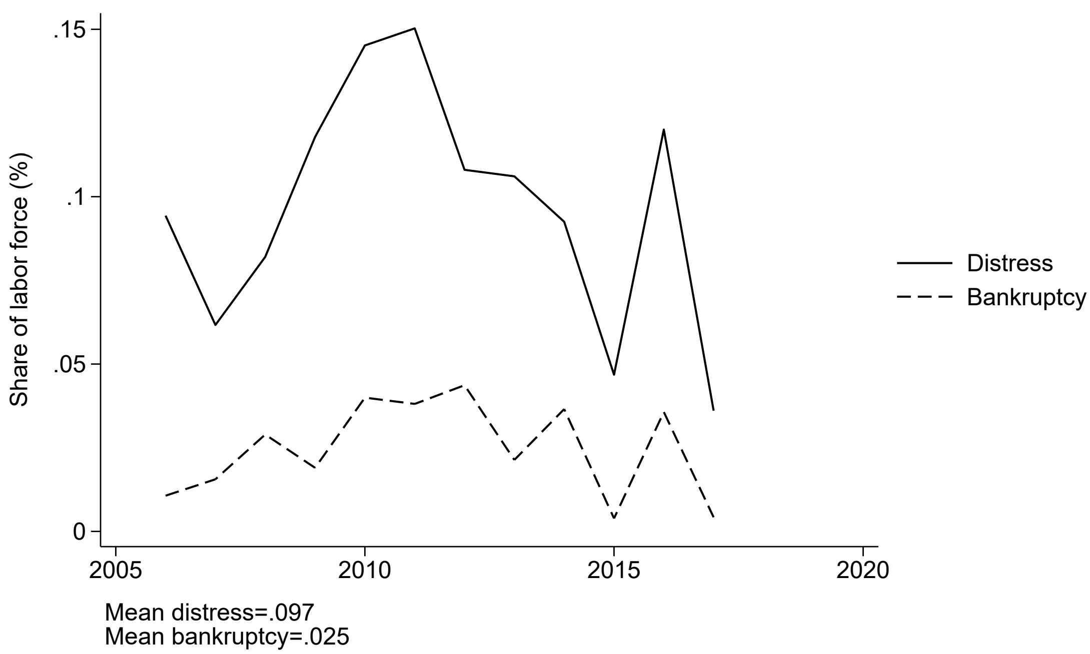
There are 102 961 unique bankruptcy cases and 1 652 different judges. We exclude cases where the firm is involved in several cases in a given year and keep the bankruptcy cases where we observe either a manager or a managing owner. We also exclude cases where a manager appears in several cases each year and only keep the first observation for each firm or manager. After these restrictions, there are 60,822 bankruptcy petitions from which the judge stringency measure is generated.

B.3 Estimating Jobs at Risk of Bankruptcy

To estimate the US series, we collect the Quarterly Bankruptcy Filings from US courts for each year from 2001 to 2023. We collect the total US Chapter 7 and Chapter 11 filings where the nature of debt is business if the debtor is a corporation or partnership, or if debt related to the operation of a business predominates. To get the jobs at risk, we first divide the total number of filings by the yearly average of the monthly level of employment from the CPS. Our estimates are obtained by further multiplying the annual Chapter 7 rate per employee by 72, the average number of employees in Chapter 7 firms, and 309 for Chapter 11 firms, as reported by Bernstein *et al.* (2019b).

To compute the numbers from Norway, we measure the total employment directly before the petition is received. Figure B.1 illustrates a similar pattern for Norway as in the US. The fraction of jobs at risk in bankruptcy but dismissed by a judge peaked at almost 15 percent percent during the Great Recession. In contrast, the jobs at risk for liquidation show a slower increase, peaking a few years later.

Figure B.1: Employee Bankruptcy Risk in the US



Notes: The figure plots the total number of employees exposed to either a liquidation or petition that is subsequently dismissed by a judge divided by the total workforce from 2005 to 2018.

C Mass Layoff Design

This section examines the earnings losses associated with a mass layoff event. We describe the data and sample construction and present the main findings.

C.1 Data and Sample Construction

To construct the mass layoff event, we proceed in two steps. In the first step, we compute a monthly dataset with firm employment using the matched employer-employee dataset from 2004 to 2014. We restrict the sample to full-time employees and only keep workers with one employment relationship in a given month. A mass layoff event follows from the definition in Lachowska *et al.* (2020). An employer is classified as having a mass layoff in a month if (i) employment dropped by 30 percent or more compared to the previous year's highest employment level, (ii) maximum employment in the previous year did not exceed 130 percent of the employment the year before, and (iii) employment at the month before the layoff exceeded 50. By this definition, we define 1234 firms that experienced a mass layoff during the sample period. Finally, we define the first month as the ML-event time.

In the second step, we identify all workers in these firms from 2000–2014. This dataset is then combined with information on their wage earnings and disposable income from the tax registry data and their age and education from Statistic Norway. Wage earnings and disposable income are deflated by CPI, with 2015 as the base year. To define the mass layoff sample of workers, we again follow Lachowska *et al.* (2020). A worker is defined as displaced if the worker is separated from his employer within 12 months of the lay-off event. The control group consists of workers who remained in the firm for at least 48 months after the event. As in Lachowska *et al.* (2020), the main sample consist of workers with at least six years of tenure in the firm.

Table C.1 reports the summary statistics for the employees of firms that experience a mass layoff event. Panel (a) reports averages and standard deviations for characteristics of the displaced workers and Panel (b) for the non-displaced workers. Compared to the workers in financially distressed firms in Table 1, workers in the mass layoff sample are older, somewhat lower educated, and higher paid. The sample is mainly concentrated in manufacturing industries and information, financial, and professional services.[1] Within the mass layoff sample, displaced workers have somewhat lower tenure and wage earnings than non-displaced workers.

[1] This industry category consist of workers in information services (ITC), finance and insurance, professional and scientific services, and real estate.

Table C.1: **Summary Statistics**

Sample:	**(a) Displaced**		**(b) Non-Displaced**	
	Mean	St. Dev	Mean	St. Dev
	(1)	**(2)**	**(3)**	**(4)**
Age	48.04	(10.404)	47.16	(8.618)
Female	0.35	(0.475)	0.32	(0.467)
High School Graduate	0.39	(0.487)	0.40	(0.489)
Bachelor Degree	0.19	(0.395)	0.18	(0.386)
Higher Degree	0.08	(0.264)	0.09	(0.284)
Tenure (years)	11.84	(6.557)	12.70	(6.585)
Wage Income (NOK1,000)	541.49	(397.381)	584.34	(340.951)
Industry (proportions)				
Mining, utilities, construction	0.074	(0.262)	0.093	(0.290)
Manufacturing	0.327	(0.469)	0.357	(0.479)
Trade and transportation	0.148	(0.355)	0.163	(0.369)
Information, finance, prof. services	0.216	(0.412)	0.151	(0.358)
Educational and health services	0.092	(0.288)	0.063	(0.243)
Arts, recreation, hospitality services	0.056	(0.230)	0.067	(0.251)
Number of workers	24,473		13,117	

Notes: This table reports summary statistics of our sample of workers in firms experiencing a mass layoff event and having at least six years of tenure. Tenure and wage earnings are measured the year prior to the mass layoff event. Panel (a) reports averages and standard deviations of key characteristics for displaced workers in these firms (separated from the firm within 12 months of the event). Panel (b) reports statistics for non-displaced workers (who remained in the firm for at least 48 months after the event). Workers that leave the firm between 12 and 48 months after the event are not included in the groups of displaced or non-displaced workers. Information, finance, and professional services include finance, insurance, real estate, professional, scientific services, and management of companies. Nominal values are deflated to 2015-values using the consumer price index. NOK/\$≈9.

C.2 Event Study Estimates from Mass Layoff Sample

We begin by presenting the evolution of employment and wage earnings among the displaced and non-displaced workers in the sample. Figure C.1a plots the average employment for displaced and non-displaced workers five years prior to the mass layoff event and four years after. Four years after, only 85 percent of the treatment group (displaced workers) are employed. In Figure C.1b we make a similar plot of displaced and non-displaced workers' average wage earnings. There is a drop in wage earnings of approximately 50,000 NOK for displaced workers after the event, while the wage earnings of the non-displaced workers continue on an upward trajectory.

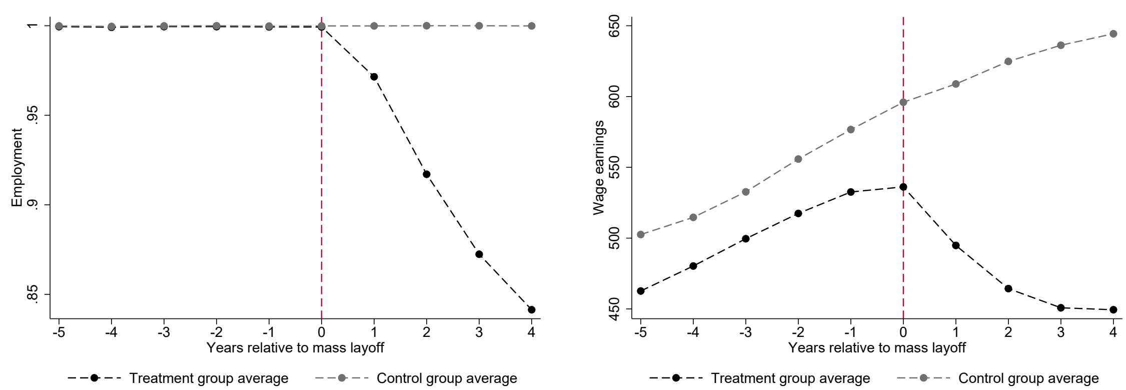
To estimate the earnings losses associated with the mass layoff event, we use the multi-period difference-in-difference estimator of Jacobson *et al.* (1993). This estimator compares the outcomes of displaced workers before, during, and after displacement with the control group of non-displaced workers. This estimator allows us to compare our estimates against the estimated earnings losses from the mass layoff literature. Following Jacobson *et al.* (1993) and Lachowska *et al.* (2020) we include worker and year fixed effects in our regression and estimate the following model:

$$Y_{it} = c_i + \gamma_t + \sum_{k=-5}^{4} (\delta_k * D_{itk}) + e_{it} \tag{8}$$

where Y_{it} is an employment outcome (employment or earnings) of worker i in year t, $c_i$is a worker-specific fixed effect, and γ_t is a vector of calendar year indicators. Each D_{itk} is an indicator equal to one when the worker is observed in year t relative to displacement. The coefficient of interest δ_k measures the difference in outcomes between displaced and non-displaced workers for each time k relative to the event. Figure C.2 displays the estimated employment and earnings difference (δ_k) for each year k relative to the mass layoff event. The event study estimates show an employment decline of approximately 15 percentage points four years after the event (C.3a) and a decline in wage earnings of approximately 150,000 NOK (C.3b). This constitutes an earnings loss of about 30 percent relative to the displaced workers' earnings the year prior to the event. This percentage decline is similar to the long term earnings losses reported in other studies (e.g, see Table 3 in Lachowska *et al.* (2020)). [2]

Figure C.1: Average Employment and Wage Earnings of Displaced and Non-Displaced Workers

(a) Employment (b) Wage Income



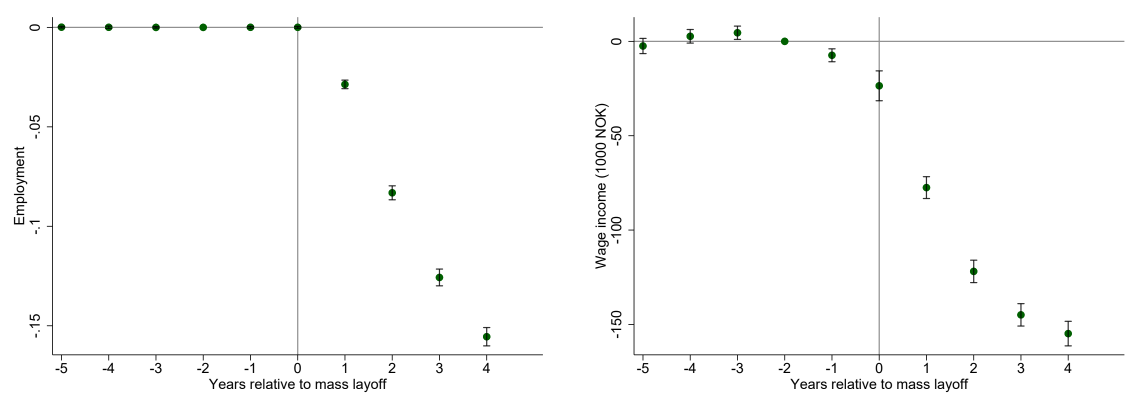
Notes: This figure displays the average employment and wage earnings for displaced (treatment group) and non-displaced (control group) workers for the years $t = -5$ to $t = 4$ relative to the mass layoff event. Panel C.1a shows the evolution in employment and Panel C.1b shows the evolution in wage income.

In the standard mass layoff design, the control group is restricted to workers who remain in the firm throughout the estimation period. By construction, this restriction does not take into account that workers in the control group can lose their job or quit the firm after the event. To illustrate how this restriction affects the employment and wage estimates, Figure C.3 displays the estimated employment and earning effect in an alternative specification where we only restrict

[2] We also estimate the average wage and employment effect using the same specification as above, but using a dummy for the full post treatment period. The average estimated wage loss is 104.047 (2.278) NOK and the average estimated employment effect is 0.078 (0.001) percentage points.

non-displaced workers to remain in the firm for at least two years after the event. While the estimated employment effect remains fairly similar, the estimated decline in wage earnings is considerably lower with the alternative specification. Figure C.4b shows an estimated earnings decline of approximately 110,000 NOK. This is 25 percent lower than the earnings effect obtained by the standard specification. By ignoring that non-displaced workers might leave or lose their job after the mass-layoff, the estimated earnings effect in the standard specification appears to overstate the earnings losses of displaced workers.

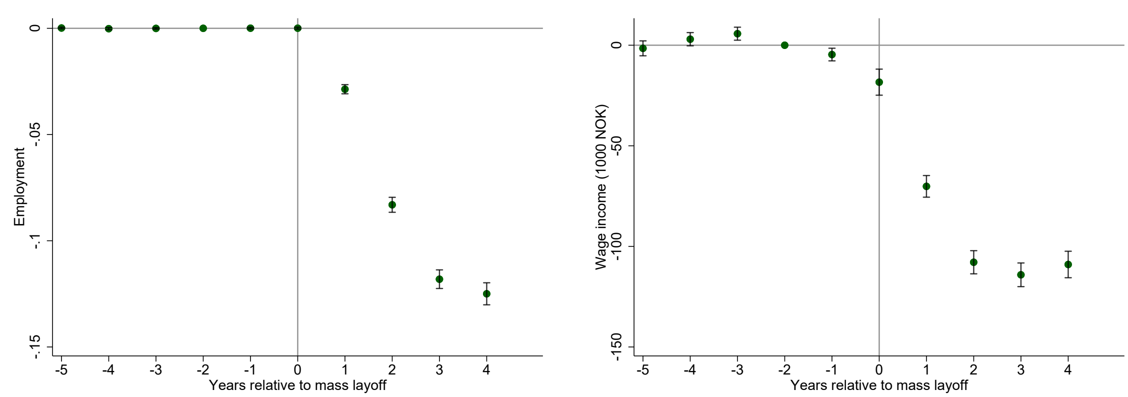
Figure C.2: *Control group: Workers who remain at least four years after event*



(a) Employment (b) Wage Income

Notes: This figure displays the estimated employment and earnings effect (δ_k) using equation 8. Panel C.3a displays the estimated effect on employment and Panel C.3b displays the estimated effect on wage earnings.

Figure C.3: *Control group: Workers who remain at least two years after event*



(a) Employment (b) Wage Income

Notes: This figure displays the estimated employment and earnings effect (δ_k) using equation 8. Panel C.4a displays the estimated effect on employment and Panel C.4b displays the estimated effect on wage earnings.

D Model Details

This section outlines additional details of our structural model. We present the full worker's problem including the worker's value function in each state and relevant constraints. We then provide the detailed results of our estimation of the model, including an outline of the 13 moments that are used in our Method of Simulated Moments estimation.

D.1 Worker's Value Function

We let t denote the age of the worker in months, which ranges from 1 to 504, and let V_t^i denote the worker's value at age t with employment status i. At each age, the worker observers her state and chooses an optimal level of net assets a^*, consumption c^*, and search effort e^* in order to maximize the expected discounted value of her utility, subject to a standard budget constraint and to the exogenous stochastic processes that determine the evolution of human capital, search outcomes, and the evolution of labor market conditions.

We let a denote the worker's level of net liquid-assets, which take the form of a one period bond with return normalized to one and price q_b when $a < 0$ and q_s when $a \geq 0$. We impose a borrowing limit $\underline{a}$ at the time of retirement, which ensures that workers do not borrow more than they can repay based on pension income.[1] In every preceding period, workers face a natural borrowing limit implied by the retirement borrowing constraint and non-negativity constraint on consumption. In addition to income from previously held bonds, workers collect income from the labor market in the form of a wage, an unemployment insurance benefit, or a basic social assistance benefit. The income of currently employed workers is determined by state variables w and h denoting the pre-tax base wage and human capital level of the worker, respectively. The income of currently unemployed workers is determined by state variables w and h denoting the analogous values in the most-recent employment period—which determine the level of UI benefit that the worker is eligible for—and the worker's choice of search effort e, which determines if the worker receives a UI benefit $b(w, h)$ or a basic social assistance benefit $\underline{b}$ in that period.

Workers have a distress status d which describes the number of periods in which they have been aware that their firm is in distress. We let d take on value zero when the worker's firm is not involved in bankruptcy proceedings. Unemployed workers always have distress status $d = 0$ by construction, while employed workers have distress status $d = 0$ when the firm is not at risk of bankruptcy or when the worker has not become aware of the risk. Once the worker becomes aware of distress, d denotes the number of periods that the worker has been aware of distress, which determines the worker's belief about when the judge's decision will occur. Workers with any non-zero value of d return to the non-distressed $d = 0$ status any time they leave the current employer through search or through the exogenous separation shock $\delta_{k,M}$. Workers transition from the non-distressed state to distress $d = 1$ with probability $\pi_{k,M}$ and transition from $d > 0$ to $d = 0$

[1] Specifically, we set the borrowing limit $\underline{a}$ equal to the net present value of the lowest retirement pension income available in our setting over a horizon based on the average length of retirement.

as a result of the judge's decision being made with probability $\frac{\xi_d^M}{\Sigma_{i=d}^3 \xi_i^M}$.

Beyond differences in employment and assets, workers are distinguished by their skill-type k which determines the wage offer distribution and probabilities of distress, exogenous separation, and offer arrival (conditional on search effort) that the worker faces. Workers also face differences in local labor market conditions M which affect the search process and distress processes.

The worker's state is defined by the combination of (a, h, w, d, k, M, t, i) when employed and $(a, h, \tilde{w}, \tilde{h}, k, M, t, i)$ when unemployed,where a denotes liquid assets, h denotes current human capital, w denotes wage, d denotes distress status for employed workers, k denotes skill type, M denotes labor market conditions, and t and i denote the age and employment status of the worker. For unemployed workers, $\tilde{w}$ and $\tilde{h}$ denote the wage and human capital during the most recent period of employment, which determine the worker's UI benefit income. After observing her state, the worker chooses an optimal level of assets to carry into the next period, a^*, a level of consumption c^*, and a level of search effort e^*—all of which potentially depend on the full tuple of state variables—in order to maximize expected utility. To ease notation, we suppress the dependence of the policy functions a^*, c^*, and e^* throughout the remainder of this section.

At age $t = T = 504$—the last period before retirement—the worker's optimal search effort is trivially $e^* = 0$, because search is costly and the worker cannot accept offers to work in retirement in our model. The retiring worker's continuation value is given by the residual utility function $\nu(a_{T+1})$ which we use as a reduced-form way of capturing various motivations to save for retirement. The problem of a retiring employed worker is given by

$$\begin{aligned} V_T^1(a, h, w, d, k, M) = \max_{a^*, c^*} & \; u(c^*) + \beta \nu(a^*) \\ s.t. \;\; & c^* + \tilde{q} a^* \leq \omega_1 (w \cdot exp(h))^{\omega_2} + a \\ & c^* \geq 0 \\ & a^* \geq \underline{a} \end{aligned}$$

and the problem of a retiring unemployed worker is given by

$$\begin{aligned} V_T^0\left(a, h, \tilde{w}, \tilde{h}, k, M\right) = \max_{a^*, c^*} & \; u(c^*) + \beta \nu(a^*) \\ s.t. \;\; & c^* + \tilde{q} a^* \leq \omega_1 \left(b\left(\tilde{w}, \tilde{h}\right)\right)^{\omega_2} + a \\ & c^* \geq 0 \\ & a^* \geq \underline{a} \end{aligned}$$

where we let $\tilde{q} = q_s$ when $a^* \geq 0$ and $\tilde{q} = q_b$ when $a^* < 0$.

At ages $t < T$, unemployed workers have the ability to search for a job and accept any wage offer that yields higher value than their current state of unemployment. We assume that newly hired workers do not face any immediate risk of their firm becoming distressed. Thus the problem of an unemployed non-retiring worker who searches for new employment is given by

$$
\begin{aligned}
V_t^0\left(a,h,\tilde{w},\tilde{h},k,M\right) = \max_{a^*,c^*,e^*} \; & u\left(c^*\right) - \psi\left(e^*\right) + \\
& + \beta\lambda_{0,0,M}e^*\left(\mathbb{E}_{h',w',M'}\left[\max\left\{V_{t+1}^0\left(a^*,h',\tilde{w},\tilde{h},k,M'\right),V_{t+1}^1\left(a^*,h',w',0,k,M'\right)\right\}\right]\right) + \\
& + \beta\left(1-\lambda_{0,0,M}e^*\right)\mathbb{E}_{h',M'}\left[V_{t+1}^0\left(a^*,h',\tilde{w},\tilde{h},k,M'\right)\right] \\
s.t. \;\; & c^* + \tilde{q}a^* \leq \omega_1\left(b\left(\tilde{w},\tilde{h}\right)\right)^{\omega_2} + a \\
& c^* \geq 0 \\
& e^* \in [0,1]
\end{aligned}
$$

Unemployed workers who choose not to search for new employment face a nearly-identical problem, with the only difference arising in the worker's budget constraint. Receiving pre-tax UI benefit $b\left(w,h\right)$ requires workers to be actively searching for a job, consistent with the policy environment that we model. In contrast, unemployed workers who do not actively search for new employment receive a basic income from a separate social assistance program—equal to $\underline{b}$ post-tax—which does not depend on their prior earnings.

The problem of a non-retiring employed worker can be written similarly, though employed workers must also account for the probability $\pi_{k,M}$ that their firm becomes distressed and the probability $\delta_{k,M}$ of being exogenously separated from their employer. Thus the problem of an employed worker in a non-distressed ($d=0$) firm can be written as

$$
\begin{aligned}
V_t^1\left(a,h,w,0,k,M\right) = \max_{a^*,c^*,e^*} \; & u\left(c^*\right) - \psi\left(e^*\right) + \\
& + \beta\lambda_{1,0,M}e^*\left(\mathbb{E}_{h',w',d',M'}\left[\max\left\{V_{t+1}^1\left(a^*,h',w,d',k,M'\right),V_{t+1}^1\left(a^*,h',w',0,k,M'\right)\right\}\right]\right) + \\
& + \beta\left(1-\lambda_{1,0,M}e^*\right)\left(1-\delta_{k,M}\right)\pi_{k,M}\mathbb{E}_{h',M'}\left[V_{t+1}^1\left(a^*,h',w,1,k,M'\right)\right] + \\
& + \beta\left(1-\lambda_{1,0,M}e^*\right)\left(1-\delta_{k,M}\right)\left(1-\pi_{k,M}\right)\mathbb{E}_{h',M'}\left[V_{t+1}^1\left(a^*,h',w,0,k,M'\right)\right] + \\
& + \beta\left(1-\lambda_{1,0,M}e^*\right)\delta_{k,M}\mathbb{E}_{h',M'}\left[V_{t+1}^0\left(a^*,h',w,h,k,M'\right)\right] \\
s.t. \;\; & c^* + \tilde{q}a^* \leq \omega_1\left(w \cdot exp(h)\right)^{\omega_2} + a \\
& c^* \geq 0 \\
& e^* \in [0,1]
\end{aligned}
$$

Workers in distressed firms form conditional beliefs about the likelihood that the decision will be made at the end of the current period. Workers in distressed firms may leave their current employer through on-the-job search or through the standard exogenous separation shock $\delta_{k,M}$. If the worker remains at the distressed firm at the end of the period and the judge's decision is made, then with probability $\bar{L}$ the judge liquidates the firm and the worker enters the next period unemployed. With the complementary probability $1-\bar{L}$, the case is dismissed and the worker remains at the current employer in the subsequent period. If the decision is not made at the end of the current period, then the worker remains in the distressed state, but revises her beliefs based on an additional period of being aware of distress. Therefore the problem of a worker in a distressed firm ($d > 0$) can be written as

$$
\begin{aligned}
V_t^1(a,h,w,d,k,M) = \max_{a^*,c^*,e^*} \; & u(c^*) - \psi(e^*) + \\
& + \beta\lambda_{1,d,M} e^* \left(\mathbb{E}_{h',w',M'} \left[\max\left\{ \tilde{V}(a^*,h',w,k,M'), V_{t+1}^1(a^*,h',w',0,k,M') \right\} \right] \right) + \\
& + \beta(1-\lambda_{1,d,M}e^*)(1-\delta_{k,M})\left(1 - \xi_d^M / \textstyle\sum_{n=d}^3 \xi_n^M\right) \mathbb{E}_{h',M'}\left[V_{t+1}^1(a^*,h',w,d+1,k,M') \right] + \\
& + \beta(1-\lambda_{1,d,M}e^*)(1-\delta_{k,M})\left(\xi_d^M / \textstyle\sum_{n=d}^3 \xi_n^M\right)(1-\bar{L}) \mathbb{E}_{h',M'}\left[V_{t+1}^1(a^*,h',w,0,k,M') \right] + \\
& + \beta(1-\lambda_{1,d,M}e^*)(1-\delta_{k,M})\left(\xi_d^M / \textstyle\sum_{n=d}^3 \xi_n^M\right)(\bar{L}) \mathbb{E}_{M'}\left[V_{t+1}^0(a^*,h,w,h,k,M') \right] + \\
& + \beta(1-\lambda_{1,d,M}e^*)\delta_{k,M} \mathbb{E}_{M'}\left[V_{t+1}^0(a^*,h,w,h,k,M') \right] \\
s.t. \quad & c^* + \tilde{q}a^* \leq \omega_1 (w \cdot exp(h))^{\omega_2} + a \\
& c^* \geq 0 \\
& e^* \in [0,1]
\end{aligned}
$$

where $\tilde{V}(a^*,h',w,k,M')$ denotes the worker's value accounting for the possibilities of no decision being made (leading to distress status $d' = d+1$), the firm being liquidated, the bankruptcy case being dismissed, and the worker exogenously separating from the firm for non-distress reasons.

In each of the preceding equations, expectations with respect to the offered wage w' are computed according to the offer distribution $G(w'|k,M)$, expectations with respect to M' are computed according to the Markov process defined by the state-persistence probabilities τ_0 and τ_1, and expectations with respect to h' are computed according to the transition matrix implied by Δ_h^+ and Δ_h^- given the worker's employment status. Where not explicitly written out, expectations with respect to d' imply that $d' = 1$ with probability $\pi_{k,M}$ and $d' = 0$ with the complementary probability $1 - \pi_{k,M}$.

D.2 Belief Updating Example

Workers form Bayesian beliefs about the probability of the bankruptcy judge's decision being made at the end of the current period. We assume that workers know the distribution $\left(\xi_1^M, \xi_2^M, \xi_3^M\right)$ where ξ_n^M denotes the share of workers who receive exactly n periods of advance notice of the judge's decision in market condition M. When workers first become aware that their firm is distressed, their prior belief about the probability that the judge's decision will occur in the current period is given by ξ_1^M. Workers update their beliefs each period that they remain distressed because the lack of decision in the preceding period is informative about what part of the distribution the worker falls in.[2]

As an example, suppose that the distribution of advance information about the firm's distress is given by $\left(\xi_1^M, \xi_2^M, \xi_3^M\right) = (0.5, 0.3, 0.2)$. Suppose that a bankruptcy petition is filed against the firm in period $\tilde{t}$ and thus the bankruptcy judge decides the case at the end of period $\tilde{t}$, potentially

[2]This style of belief updating is similar to the belief updating behavior of agents solving an unobservable queueing problem under a first-come-first-serve service rule (see, e.g., Che & Tercieux (2021)).

leading to workers being laid off. Because we only allow workers to have up to three periods of advance notice of the pending judge's decision, no workers directly consider the threat of potential layoff due to liquidation prior to period $\tilde{t}-2$.[3]

In period $\tilde{t}-2$, three periods before the judge's decision, a fraction $\xi_3^M = 0.2$ of workers become aware that the firm is in distress. These workers know that the only way that the judge's decision will be made at the end of the current period is if they only received one period of advance information. Thus, these workers believe that the judge's decision will occur in the current period with probability $\xi_1^M = 0.5$.

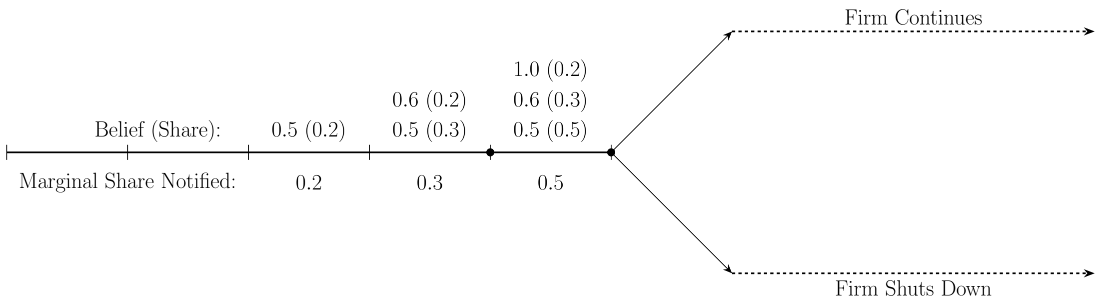
In period $\tilde{t}-1$, two periods before the judge's decision, an additional share $\xi_2^M = 0.3$ of workers become aware that the firm is in distress. By the same rationale as in the previous period, these workers believe that the judge's decision will occur in the current period with probability $\xi_1^M = 0.5$. At the same time, workers who were aware of the firm's distress in period $\tilde{t}-2$ but have not yet experienced a decision will update their beliefs. Because a decision did not occur in the first period that they were aware of the distress, these workers know that they cannot be part of the fraction $\xi_1^M = 0.5$ of workers who received one period of advance information. Moreover, they know that the judge's decision will only occur in the current period if they are part of the fraction $\xi_2^M = 0.3$ of workers who received two periods of advance information. Thus, they believe the judge's decision will occur at the end of the current period with the conditional probability $\frac{\xi_2^M}{1-\xi_1^M} = 0.6$.

In period $\tilde{t}$, one period before the judge's decision, the remaining share $\xi_1^M = 0.5$ of workers become aware that the firm is in distress, believing that the decision will occur in the current period with probability $\xi_1^M = 0.5$. At the same time, workers who first became aware of the firm's distress in period $\tilde{t}-1$ update their beliefs as previously described, believing that the decision will occur in the current period with probability $\frac{\xi_2^M}{1-\xi_1^M} = 0.6$. Finally, workers who first became aware of the firm's distress in period $\tilde{t}-2$ and who have not yet experienced the decision again update their beliefs, now knowing that they are not part of the fraction ξ_1^M of workers who receive one period of advance notice nor the fraction ξ_2^M of workers who receive two periods of advance notice. Thus, because we only allow up to three periods of advance information, these workers know with certainty that the judge's decision will be made in the current period, because $\frac{\xi_3^M}{1-\xi_1^M-\xi_2^M} = 1$.

Figure D.1 illustrates how the distribution of beliefs among workers in a firm changes as the firm approaches the period of the bankruptcy judge's decision. In particular, only fraction ξ_3^M of workers fully internalize the risk of the judge's decision at the time it occurs. The remaining workers have non-degenerate Bayesian beliefs based on their knowledge of the distribution $(\xi_1^M, \xi_2^M, \xi_3^M)$ and updated based on the number of periods that they have experienced distress without the judge's decision being made.

[3] Because we assume that workers have perfect foresight, workers prior to period $\tilde{t}-2$ will still incorporate the possibility of becoming aware that their firm is in distress—leading to an eventual layoff possibility—into their expectations.

Figure D.1: Belief Distribution in Distress Cases



Notes: This figure illustrates an example of how the distribution of workers' beliefs about the probability of the judge's decision being made at the end of the current period changes as the firm approaches the actual time of the judge's decision. Workers who are notified more than one period prior to the judge's decision update their beliefs over time. By the time the judge's decision is actually made, all workers believe that the decision will occur with positive probability; however, only workers with the most advance information fully internalize the risk and believe that the decision will occur with certainty. Prior to being notified of the firm's distress, workers believe there is zero probability of facing a layoff due to the employer being shutdown in the current period.

D.3 Model Moments and Fit

In this section we describe the full set of moments that we target when calibrating structural parameters internally and present the calibrated values of the structural parameters along with the simulated moment values.

D.3.1 Targeted Moments

We target a set of 12 moments to internally calibrate 11 structural parameters of the model.[4] Section 6.2 describes the main identification argument for each parameter. We report the target and simulated values of each moment in Appendix Table D.2.

Retirement Savings Because we do not model an explicit retirement period, we rely on a residual utility function $\nu(a_{T+1})$ to capture the value of entering retirement with a given level of assets. We include this term in the model primarily to ensure that workers do not exhaust their savings as they approach the end of the model's finite horizon, as this would distort the model's ability to capture the role of private assets as an insurance mechanism. We target the ratio of workers' net wealth at age 67 to workers' net wealth at age 64, which summarizes the relative level of savings that occurs near the end of the life-cycle and provides identifying variation for the scaling coefficient φ in the residual utility term.

We compute the target value using our overall population of workers by computing the average net wealth of all workers at age 67 and the average net wealth of all workers at age 64 and taking the corresponding ratio. Because the model has only liquid assets, we exclude the effects of passive savings through the ownership of housing (i.e., the change in net wealth attributable to the change in housing prices) in order to avoid forcing the model to fit excessive levels of saving in liquid assets.

[4]We explicitly calibrate 11 of the 13 unassigned structural parameters. The values of the remaining 2 parameters are implied by the constraints that ξ_1^M, ξ_2^M, and ξ_3^M must sum to 1 for both values of M.

Aggregate Employment Transition Rates In addition to differences in the choice of search effort, workers in the model face different offer arrival rates conditional on their employment status. In order to ensure that these offer rates fit with what we observe empirically, we include the aggregate transition rates from unemployment to employment (UE) and from employment to employment (EE) as targeted moments in the model. While we primarily use these moments to identify the maximum offer arrival rates λ_i, these moments also have a significant effect on workers' choice of effort and thus exert some influence over our choice of the scale of search costs.

We measure transition rates among our overall sample of workers, excluding those who are classified as not participating in the labor force (which affects our estimate of the UE rate). We calculate analogous values in the model, where we conclude that workers are not participating in the labor force if they are unemployed and choose effort $e^* = 0$. We calculate the aggregate UE and EE rates in the model by taking a weighted average of the UE and EE rates in strong and weak market conditions separately, because the number of periods we simulate in each market condition inside the model may not be exactly proportional to the data.

Precautionary Search Because we are interested in using our structural model to asses the role of on-the-job search as an insurance mechanism, we want to ensure that the precautionary search behavior of workers in the model matches what we observe empirically. Workers' ability to engage in precautionary search is significantly influenced by the availability of advance information about the pending layoff, which lowers the value of the current employment position and makes search relatively more attractive. To determine the potential duration of advance information, we analyze how EE rates vary in the months leading up to and immediately following the bankruptcy petition and subsequent judge's decision and interpret excess EE separations as evidence that workers are aware of the firm's distress.

We target the share of workers who leave the distressed firm through an EE transition in each aggregate state, with the understanding that the majority of such "precautionary" transitions out of the distressed firm occur at the time the bankruptcy petition is filed or just before due to advance information.These moments provide identifying variation that helps determine the distribution of advance information defined by ξ_i^M.

Effects of Liquidation on Labor Market Outcomes We target several of our 2SLS estimates from Section 4.We include the average effect of liquidation on employment in year 4 to capture the fact that the effect does not fully diminish within the four year horizon. To provide further structure to how liquidation affects workers' employment outcomes, we measure the effects of liquidation on both wage income and UI benefit income. We target the relative change in wages in year 1 and year 4, which we measure as the ratio of the estimated effect of liquidation on wage income to the mean wage income of untreated compliers. Similarly, we target the relative change in UI benefit receipts in year 1 and year 4, which reinforces the effect of employer shutdown on employment and which depends in part on which workers are subject to unemployment spells and which workers instead make employment-to-employment transitions before the judge's decision is made. We also target the average effect of liquidation on wage income from year 0 to year 4 separately by local labor market conditions, which provides further evidence which workers are able to engage in precautionary employment transitions and which workers are subject to layoff as a result of the firm being shut down.

D.3.2 Calibration Results

Table D.1 describes the model parameters which we set externally, either based on estimates from the literature or by estimating values directly from our data. The source of each value is discussed in Section 6.2.

Table D.2 presents the result of our joint calibration of the remaining model parameters as well as the values of the 12 moments that we target. For each moment, we report the simulated and target values, as well as the weight we give to the moment when minimizing the sum of squared residuals in our Method of Simulated Moments estimator. We weight moments replicating our 2SLS estimations of the effect of layoff according to their t-statistics. Population moments describing the overall sample of workers are given weight of one. The rate of precautionary transitions out of distressed firms in each state is given a weight of ten in order to ensure that the model replicates the use of search as a form of self insurance.

In sum, we find that the model does a good job of fitting the insurance behavior of workers compared to the targets. In particular, we generate relatively accurate values of the share of workers who leave distressed firms through EE transitions prior to the judge's decision in each state. In addition, the effects of liquidation on workers' labor market outcomes fall within the 95% confidence intervals of our empirical estimates for all targeted moments.

Table D.1: External Model Parameters

Panel (a): Calibrated Parameters	**Parameter**	**Value**
Coefficient of Relative Risk Aversion	γ	1.700
Subjective Discount Factor	β	0.9975
Bond Price (Savings)	q_s	1.000
Bond Price (Borrowing)	q_b	0.993
Borrowing Limit at Retirement	$\underline{a}$	-593.617
Distress Risk (Low Skill, Strong Market)	$\pi_{0,0}$	0.00080
Distress Risk (High Skill, Strong Market)	$\pi_{1,0}$	0.00035
Distress Risk (Low Skill, Weak Market)	$\pi_{0,1}$	0.00103
Distress Risk (High Skill, Weak Market)	$\pi_{1,1}$	0.00018
Separation Risk (Low Skill, Strong Labor Market)	$\delta_{0,0}$	0.0208
Separation Risk (High Skill, Strong Labor Market)	$\delta_{1,0}$	0.0089
Separation Risk (Low Skill, Weak Labor Market)	$\delta_{0,1}$	0.0193
Separation Risk (High Skill, Weak Labor Market)	$\delta_{1,1}$	0.0088
Probability of Liquidation	$\bar{L}$	0.553
Scale of Taxation	ω_1	5.366
Progressivity of Taxation	ω_2	0.844
UI Replacement Rate	r	0.624
Maximum UI Benefit (monthly NOK1,000)	$\bar{b}$	28,080
Minimum Social Assistance Income (monthly NOK1,000)	$\underline{b}$	8,333
Scale of Wage Distribution (Low Skill, Strong Market)	$\mu_G^{0,0}$	13.054
Scale of Wage Distribution (High Skill, Strong Market)	$\mu_G^{1,0}$	13.007
Scale of Wage Distribution (Low Skill, Weak Market)	$\mu_G^{0,1}$	13.038
Scale of Wage Distribution (High Skill, Weak Market)	$\mu_G^{1,1}$	12.961
Shape of Wage Distribution (Low Skill, Strong Market)	$\sigma_G^{0,-}$	0.176
Shape of Wage Distribution (High Skill, Strong Market)	$\sigma_G^{1,0}$	0.206
Shape of Wage Distribution (Low Skill, Weak Market)	$\sigma_G^{0,1}$	0.183
Shape of Wage Distribution (High Skill, Weak Market)	$\sigma_G^{1,1}$	0.250
Labor Market Tightness (Non-Distress, Strong Market)	$\theta_{0,0}$	1.000
Labor Market Tightness (Distress, Strong Market)	$\theta_{1,0}$	0.578
Labor Market Tightness (Non-Distress, Weak Market)	$\theta_{0,1}$	0.234
Labor Market Tightness (Distress, Weak Market)	$\theta_{1,1}$	0.178
Probability of Remaining a Strong Market	τ_0	0.999
Probability of Remaining a Weak Market	τ_1	0.961

Notes: This table shows the values of structural model parameters which we take externally. Preference parameters such as the coefficient of relative risk aversion and subjective discount factor are taken from common estimates in the literature. Institutional details such as the UI replacement rate are based on the empirical institutions. The remaining parameter values are estimated directly from the data, either using the overall sample of workers or the sub-sample of workers who are employed by firms undergoing bankruptcy proceedings. Labor market tightness values are combined with the internally calibrated maximum offer rates λ_i to produce the full set of offer arrival rates $\lambda_{i,d,M}$.

Table D.2: Model Fit

Panel (a): Calibrated Parameters	Parameter	Value	
Scale of Retirement Savings Motive	φ	49.660	
Scale of Search Cost (Unemployed)	μ	2.930	
Elasticity of Search Cost	η	7.630	
Distribution of Advance Info (Strong Market)	$(\xi_1^0, \xi_2^0, \xi_3^0)$	(0.219; 0.045; 0.736)	
Distribution of Advance Info (Weak Market)	$(\xi_1^1, \xi_2^1, \xi_3^1)$	(0.523; 0.220; 0.257)	
Offer Arrival Rate (Unemployed)	λ_0	0.313	
Offer Arrival Rate (Employed)	λ_1	0.900	
Pro. of Gaining Human Capital (Employed)	Δ_+^h	0.067	
Prob. of Losing Human Capital (Unemployed)	Δ_-^h	0.815	
Panel (b): Targeted Moments	**Simulation**	**Data**	**Weight**
Aggregate UE Rate	0.091	0.173	1.000
Aggregate EE Rate	0.035	0.012	1.000
Age 67 to Age 64 Savings Ratio	1.656	1.137	1.000
Effect of Layoff on Any Employment (Yr 4)	0.004	-0.099	1.269
Relative Effect on Wage (Yr 1)	-0.382	-0.277	2.079
Relative Effect on Wage (Yr 4)	-0.082	-0.158	1.378
Relative Effect on UI Income (Yr 1)	2.257	2.229	3.206
Relative Effect on UI Income (Yr 4)	-0.029	1.023	0.706
Relative Effect on Wage (Year 0-4; Strong Market)	-0.174	-0.091	0.444
Relative Effect on Wage (Year 0-4; Weak Market)	-0.243	-0.211	2.111
Precautionary Transition Rate in Strong Market	0.376	0.346	10.000
Precautionary Transition Rate in WeakMarket	0.167	0.137	10.000

Notes: This table shows the results of our joint calibration of structural model parameters. Panel (a) reports the calibrated value for each structural parameter that is part of the joint estimation. Panel (b) reports the simulated value of each of our targeted moments, the associated target value, and the weight we assign to the moment in our evaluation of the estimator. We choose the vector of structural parameters in Panel (a) to minimize the weighted sum of squared residuals based on the simulated moment values reported in Panel (b).

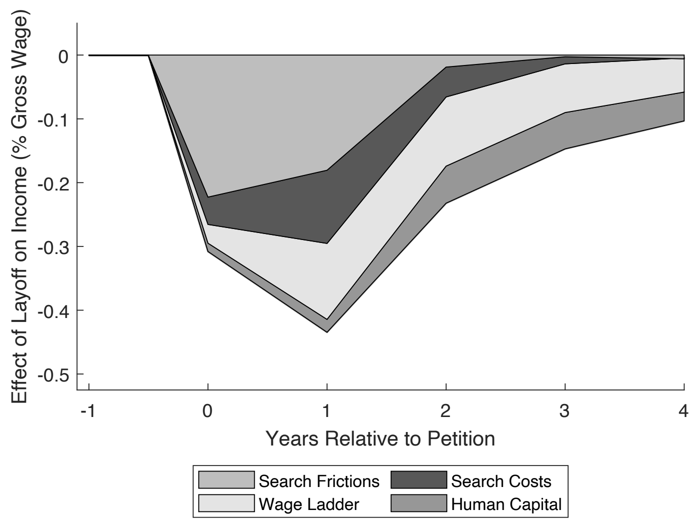
D.4 Decomposing Wage Losses

To understand the mechanisms, we assess the contribution of human capital, the job ladder, search frictions, and the disutility cost of search to wage losses. The most closely related frameworks are Jarosch (2022), who incorporates heterogeneous layoff risk in a model with on-the-job search, and Lise (2013), who incorporates risk aversion into a job ladder model. Like existing frameworks, workers recoup some lost earnings as they re-climb the ladder and rebuild their human capital.

The key innovation of our framework is that upon learning about heightened layoff risk, workers start accepting outside offers with a lower wage, as the increased layoff risk decreases the reservation wage before the separation occurs, generating the endogenous selection of workers in distressed firms.

Figure D.2 provides a graphical decomposition of the simulated effect of liquidation on wage income. We begin by constructing a counterfactual where workers retain their previous level of human capital when they find new employment. This effect is illustrated by the dark grey area, leading to persistent wage income loss that does not fully diminish by the end of the four-year horizon. Our next counterfactual additionally assigns the worker the same position on the wage ladder upon re-employment. We see that position on the wage ladder has a slightly larger relative effect at the end of the period, as illustrated by the light shade.

Figure D.2: Decomposed Effect on Wage Income

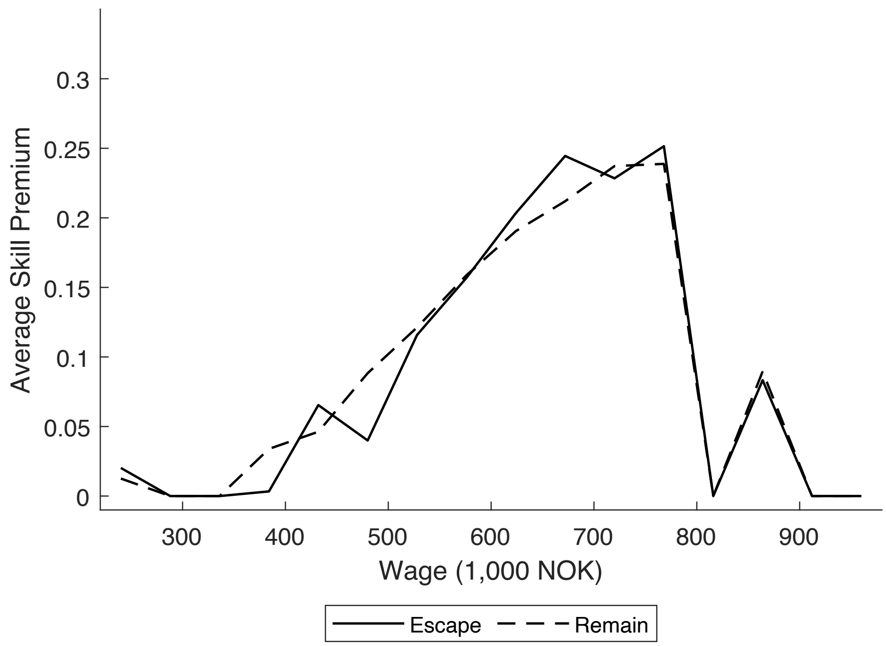


Notes: This figure shows how search frictions, the costs of search effort, the wage ladder, and human capital contribute to the effect of liquidation on wage income in year 0 through year 4 in our fitted baseline model. We construct a series of counterfactual estimates of the effect of liquidation on wage income where we progressively remove these mechanisms from the model by allowing workers to recover their prior level of human-capital and pre-tax wage upon becoming re-employed after liquidation and by exogenously setting the probability of receiving an offer to the maximum probability $\lambda_{i,d,M}$ or by guaranteeing an offer. The reduction in the estimated wage effect as each component is progressively removed determines the effect of that mechanism on our baseline estimate.

Figure D.3 demonstrates that average human capital levels are not monotonically increasing in employer quality, suggesting that workers do not traverse the whole wage ladder "rung by rung"; rather, workers employed by the highest quality firms are hired directly from relatively low-quality firms or unemployment, while workers who move up the ladder "rung by rung" find it optimal to stop searching before reaching the highest quality employers. This dynamic makes workers in the highest quality firms less likely to naturally recover their pre-liquidation positions, exacerbating the wage effect.

Next, we decompose the wage effect by separating the two components contributing to the employment-driven effect on wages. We estimate an additional counterfactual effect of liquidation on wage, assuming that the probability of receiving an offer is exogenously given by the maximum offer rate $\lambda_{i,d,M}$ that we calibrated in the model and assuming that any offer a worker receives yields the same pre-tax wage and human capital as the worker's terminated employer. Finally, we estimate the wage effect when the worker is immediately re-offered their pre-liquidation position (with the same pre-tax wage and human capital) after the liquidation occurs. When both search frictions and costly effort are present, the employment-driven wage effect diminishes by year 3. When only search frictions are present, the effect diminishes slightly more rapidly.

Figure D.3: Skill Premia by Employer Quality



Notes: This figure plots the average skill premium from human capital by employer quality (base wage) for workers who leave the distressed firm prior to the judge's decision and for workers who remain at the distressed firm at the time of the judge's decision.