Corporate Reorganization as Labor Insurance in Bankruptcy

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Abstract

This paper investigates the consequences of corporate reorganization and liquidation on the reallocation of labor in bankruptcy using a random judge assignment design and reorganization filings from Portugal. Reorganization provides labor insurance to workers in bankruptcy, having a positive and persistent effect on wages, even as most workers leave reorganized firms. Reduced human capital losses and improved worker-firm matches with new employers are two mechanisms that explain the effect of reorganization on wages. The positive effect of reorganization on worker outcomes is concentrated in thin and low-growth labor markets.

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1 Introduction

How does corporate reorganization affect labor outcomes in bankruptcy? In frictionless markets, the choice between reorganization and liquidation does not affect the reallocation of resources. Workers are always employed in firms where they are the most productive.

This null hypothesis may not hold in the presence of frictions. The literature argues that reorganization may affect bankruptcy outcomes because it retains resources in bankrupt firms. Conflicts of interest between managers, debtors, and creditors may lead to the inefficient reallocation of workers and other production inputs controlled by bankrupt firms (Jensen and Meckling (1976), Myers (1977), Gertner and Scharfstein (1991), Caballero and Hammour (1996), Hart and Moore (1998)). In some cases, bankrupt firms remain alive for too long and retain workers inefficiently. In other cases reorganization reduces the probability of inefficient liquidation. The empirical evidence suggests that these conflicts of interest lead to excessive resource retention in some bankruptcy systems and insufficient retention in others (e.g., Strömberg (2000), Franks et al. (2017), Antill (2019), Bernstein et al. (2019b)).

However, resource retention within efficient or inefficient firms is not the only determinant of labor reallocation outcomes in bankruptcy. In this paper we provide evidence that reorganization has a positive and persistent effect on labor outcomes in bankruptcy because workers use firms as providers of labor insurance against negative production shocks. As first stated by Titman (1984)), there is a principal-agent problem in bankruptcy between claimholders of the capital structure (creditors, equity holders) and other participants in the firm (other stakeholders). While claimholders decide bankruptcy outcomes, other stakeholders are affected by this choice because they use firms as insurance providers. Workers are among these stakeholders. They cannot insure themselves against negative production shocks. Therefore, they use firms as insurance providers by establishing worker-firm job contracts (Baily (1974), Guiso et al. (2005), Berk et al. (2010)). In the absence of reorganization, workers lose these job contracts and are exposed to the persistent costs of job loss (Jacobson et al. (1993)). Workers who lose their job contracts may not find a new job easily in

markets with few job opportunities (Rogerson et al. (2005), Barlevy (2002)). Also, workers may get lower-paying jobs because of forgone human capital accumulation or transitions to new jobs at lower levels of the wage ladder (Barlevy (2002), Jarosch (2021)). Reorganization has a positive and persistent effect on labor outcomes because workers are less likely to lose the insurance provided by job contracts when the costs of job loss are high.

In this paper we analyze empirically the effect of corporate reorganization on the reallocation of labor in bankruptcy, using filings from Portugal between 2012 and 2016. Using this setting brings unique advantages. The Portuguese data contain detailed information on worker and firm characteristics. We track workers from firms that file for reorganization even as they move to other jobs. Besides wages, we observe job characteristics such as occupations. We complement these data with financial statements for the near-universe of firms operating in Portugal, which also includes private firms. These data are important because they allow us to study the mechanisms that contribute to the effect of reorganization on wages. We measure the effect of reorganization on the probability that workers face job downgrading to less skill-intensive occupations, and study how reorganization affects the probability that workers find better-paying jobs in new employers. Using firm financial statements we test for the existence of trade-offs between higher wages after bankruptcy and depressed reallocation to productive firms.

The Portuguese setting is valuable also because there is considerable heterogeneity in labor market conditions. This setting allows us to measure the effect of reorganization on worker outcomes and on the reallocation of labor to productive firms in markets affected by economic distress and by high search frictions. Finally, as documented previously in the bankruptcy literature, the decision to reorganize or liquidate firms in bankruptcy is not random (Chang and Schoar (2013), Bernstein et al. (2019a), Bernstein et al. (2019b), Antill (2019)). As reorganization cases in Portugal are allocated randomly to judges in the same court, we follow this literature and use the percentage of reorganization plans accepted by judges in other reorganization cases as an instrumental variable for reorganization.

We measure the effect of reorganization on wages up to the fifth year after the reorganization

filing. We decompose wages into the extensive margin (employment) and the intensive margin (jobs with higher wages). Reorganization has a positive and persistent effect on wages. In the year of the filing reorganization causes an 18.5 pp increase in wages, mostly explained by higher employment. In the longer term reorganization causes a 19.4 increase in wages, mostly driven by the intensive margin.

Human capital losses and transitions into jobs at lower levels of the wage ladder are two mechanisms that explain the persistent costs of job loss (Burdett et al. (2020), Jarosch (2021)). In the first mechanism, workers who face job loss suffer long-term human capital losses, which in turn lead to persistently lower wages. We provide evidence that reorganization reduces the effect of human capital loss on wages. We characterize occupations in terms of cognitive, manual, and interpersonal skill intensity (Lise and Postel-Vinay (2020), Deming (2017)). Reorganization reduces the probability that workers move to less skill-intensive occupations. Using a reduced form wage determination model, we show that, on average, reorganization has a positive 2.0 pp effect on the wage premium associated with occupations.

The second mechanism – transitions to new jobs at lower levels of the wage ladder – is another important channel of wage setting. The opportunity cost of unemployment is lower than the opportunity cost of leaving a job, hence workers are more likely to switch to high-paying jobs when they are employed than when they are unemployed. We examine the effect of reorganization on the amount of time that it takes for workers to leave the job they had before the filing. Reorganization adds one year to the average time it takes to leave a firm that files for reorganization. We establish a causal relationship between reorganization and the probability that workers are matched with high-paying jobs with new employers. While reorganization reduces the probability that workers move to low paying jobs, it increases the probability that they get jobs with new employers in the highest quintile of the wage distribution by 7.4 pp.

Finally, we analyze how the effect of reorganization on worker outcomes varies with labor market conditions. We study the relationship between effect of reorganization on wages and the opportunity cost of labor. When there are many outside options, one would expect the effect of reorganization on

wages to be small. As vacancies are abundant, workers from liquidated firms find similar jobs with new employers easily. In markets with low labor opportunity cost, there are few alternative jobs, and the effect of reorganization on labor outcomes is ambiguous. On the one hand, reorganization might preclude the reallocation of workers to new occupations that are more productive (Caballero and Hammour (1996)). On the other hand, jobs created during periods of economic distress might have low quality (Barlevy (2002)), and reorganization avoids reallocating workers to these low quality jobs. In our analysis, we measure the opportunity cost of labor as the job growth rate for the occupation and county where workers are employed in the year of the reorganization filing. This variable is analogous to the one used by Bernstein et al. (2019b) to measure the opportunity costs of using establishments from bankrupt firms. We show that the effect of reorganization on wages varies inversely with the opportunity cost of labor. When the opportunity cost is low, reorganization increases employment and wages, and workers are less likely to switch to low-quality occupations. In these markets, reorganization increases the utilization of labor, rather than reducing the reallocation to more productive or profitable firms. In markets with high labor opportunity costs the effect of reorganization on wages is negligible.

We also analyze how the effect of reorganization on worker outcomes varies with the intensity of search frictions. The effect of reorganization on wages should be greater in labor markets with stronger search frictions. In these markets, workers from liquidated firms have more difficulty in finding a job with similar characteristics, which would force them to accept lower wages. This effect is akin to the role of trading frictions on the prices of real assets (Gavazza (2011)). We compute the intensity of labor search frictions as the percentage of workers employed in each occupation over the total number of workers in the county. We show that the effect of reorganization on wages varies negatively with search frictions.

By analyzing the effect of reorganization on wages in labor markets with different characteristics, we conclude that the labor cost of bankruptcy is a countercyclical cost of excessive liquidation. Reorganization leads to higher employment and wages in bankruptcy. This effect is concentrated in markets with low opportunity cost and high search frictions. Excessive reorganizations could reduce

the cleansing effect of firm destruction by hampering the reallocation of labor to more productive firms (Caballero and Hammour (1996)). We do not find evidence of this effect. Instead, reorganization affects labor reallocation by increasing the utilization rate of labor in markets with high search frictions.

The remainder of the paper is structured as follows. Section 2 reviews the literature. Section 3 establishes testable hypotheses. Section 4 describes the institutional features of the Portuguese bankruptcy system. Section 5 lists the datasets used in the analysis and provides descriptive statistics. Section 6 develops the empirical strategy. Section 7 reports the results, with Section 8 forming the conclusion.

2 Literature review

This paper contributes primarily to the literature that studies the effect of bankruptcy and distress on labor outcomes. Brown and Matsa (2016) show that workers are less likely to apply to distressed firms. Barbosa et al. (2017) and Berton et al. (2018) analyze the relationship between the credit constraints faced by firms and employment. Babina (2020) documents the relationship between financial distress and labor transitions into entrepreneurial jobs. Baghai et al. (2020) study the effect of bankruptcy risk on workers' decisions to leave firms before bankruptcy. Grindaker et al. (2021) analyze the effect of bankruptcy on CEO careers. In this literature, Graham et al. (2019) and Araújo et al. (2020) are the two papers that are the closest to ours. Graham et al. (2019) measure the effect of bankruptcy on wages. Workers who have a job contract at a firm that files for bankruptcy have persistently lower wages after the bankruptcy event. Araújo et al. (2020) show that workers at bankrupt firms assigned to pro-labor courts face larger declines in earnings after bankruptcy than workers at bankrupt firms assigned to other courts. This effect is driven by workers' limited access to information about other jobs and is concentrated in markets with many job opportunities. Our paper differs from the papers by Graham et al. (2019) and Araújo et al. (2020) because we measure the causal effect of reorganization on worker outcomes, while they study the bankruptcy system broadly

by analyzing reorganized and liquidated firms together. We contribute to the bankruptcy literature by providing evidence of a novel channel for the effect of reorganization on labor outcomes – the provision of labor insurance in bankruptcy. If we interpret reorganization as being a pro-labor bias policy, the mechanisms documented in this paper and by Araújo et al. (2020) affect labor markets differently. The effect documented by Araújo et al. (2020) is relatively more important in high-growth labor markets in which workers have limited access to information about job opportunities. The labor insurance effect documented in our paper is more important in labor markets in which jobs are scarce, such as thin labor markets and labor markets with low job growth.

The paper also contributes to the literature on optimal bankruptcy design. There is a rich discussion about the merits of reorganization and liquidation in bankruptcy (e.g., Gertner and Scharfstein (1991), Aghion et al. (1992), Hart and Moore (1998), Strömberg (2000), Corbae and D'Erasmo (2017)). In this literature bankruptcy design affects outcomes because distressed firms may retain or shed resources inefficiently. The empirical literature estimates that some bankruptcy systems cause excessive resource retention and other systems cause excessive liquidations (e.g., Strömberg (2000), Franks et al. (2017), Antill (2019), Bernstein et al. (2019a), Bernstein et al. (2019b)). In this paper we show that the labor insurance provided by firms is an additional factor to be considered in the design of bankruptcy systems. We use the existing research on labor economics and bankruptcy to guide our analysis. Labor represents a large share of value added in most economic activities, and workers are increasingly central to the purpose of firms (Harrison et al. (2019)). However, there is an agency problem between creditor committees and workers in bankruptcy (Titman (1984)). While workers might be strongly affected by bankruptcy outcomes, their intervention in creditor committees is limited by their role as creditors. Our paper contributes to the discussion on the trade-off between excess liquidations and excess reorganizations in bankruptcy, as we show that the loss of labor insurance is a countercyclical cost of excessive liquidations in bankruptcy.

Our paper also relates to the literature on the costs of job loss. Rogerson et al. (2005) review the literature on search-theoretical models of the labor market. Berk et al. (2010) argue that workers require a wage premium to work in levered firms because they cannot insure their human capital

against negative production shocks when firms file for bankruptcy. Guiso et al. (2005) provide empirical evidence that firms shield workers' wages against idiosyncratic production shocks. Other papers analyze the factors that contribute to the long-term persistence of wage losses after job loss. Barlevy (2002) argues that persistent wage losses might be driven by the high search frictions faced by workers who want to switch jobs. Burdett et al. (2020) and Jarosch (2021) show that long-term wage losses after job loss can be explained by forgone human capital acquired on the job and by transitions to jobs at lower levels of the wage ladder. In this paper we establish that corporate reorganization provides labor insurance against the costs of job loss. We show that workers in reorganized firms have higher wages and remain in more skill-intensive occupations. We also show that reorganization improves worker outcomes for workers who leave to new employers.

The literature shows that displacement through mass layoffs causes long-term wage losses (Jacobson et al. (1993)), especially when workers have industry, firm, and occupation-specific capital that cannot easily be transferred to other firms (Neal (1995), Robinson (2018), Raposo et al. (2019)). However, corporate reorganization and job displacement are different concepts (Graham et al. (2019)). We focus on reorganization as a financial shock on workers, while this literature studies pure job displacements. Because establishments are occupied by other firms, workers are not necessarily displaced when firms are liquidated. Many workers are still required to leave reorganized firms. Using job separators would not be adequate to measure the effect of reorganization on workers. As we show, workers who leave reorganized firms are important drivers of the effect of reorganization on labor reallocation. Additionally, employees who remain at distressed firms differ from regular workers because reorganized firms go through a restructuring process that affects wages and skill requirements.

3 Predictions

In this section we discuss the mechanisms that drive the effect of corporate reorganization on labor outcomes. Our exposition follows the structure that is adopted by Agrawal et al. (2022). The

decision to reorganize or liquidate firms creates an agency relationship between creditors and workers in bankruptcy (Titman (1984)). When creditors choose a bankruptcy outcome (reorganization or liquidation), they do not take into account the effect of their choice on workers. Workers cannot fully insure their human capital, so they establish job contracts with firms that provide this insurance (Berk et al. (2010)). Liquidation might be more costly than reorganization to workers, as workers who lose job contracts face persistent wage losses (Jacobson et al. (1993)).

Wage losses after liquidation arise from unemployment, but might persist even as workers from liquidated firms find jobs with new employers. The literature documents the sources for the wage cost of job loss (Burdett et al. (2020), Jarosch (2021)). First, the human capital of workers who lose their job depreciates over time. When workers get a new job, they cannot perform their previously held tasks because of forgone human capital accumulation. This lost human capital leads to lower wages. Second, workers from liquidated firms search for a new job while unemployed, which reduces wages at new jobs. If searching for a new job is costly, workers who search for a new job while unemployed move permanently to jobs with lower wages than workers who search while they are still in their previous job.

The labor cost of corporate liquidation varies with labor market conditions. First, we discuss the effect of labor market thickness on labor outcomes. Thin markets increase search frictions (Gavazza (2011)). In these markets workers are less likely to receive job offers that are similar to their previous job. Therefore, they are more likely to stay unemployed for longer or to accept lower-paying jobs with new employers. Second, we discuss the effect of low job growth on labor outcomes. Firms post fewer and lower-quality vacancies in low growth labor markets (Barlevy (2002)). With fewer vacancies, workers from liquidated firms either stay unemployed or accept lower-paying jobs in new employers, which leads to greater wage losses in these labor markets.

In light of this context, we examine the following empirical predictions for the effect of reorganization on labor outcomes:

Prediction 1: reorganization increases wages. Reorganization reduces the probability that workers lose their job immediately. In the short term, workers have higher wages because they do not lose their jobs. In the longer term, reorganization increases wages because workers suffer smaller human capital losses and find better-paying jobs with new employers.

Prediction 2: reorganization reduces the loss of human capital in bankruptcy. Reorganization reduces the probability of job loss and forgone human capital accumulation. Reorganization decreases the probability that workers move to less skill-intensive occupations that have lower wage premiums.

Prediction 3: reorganization matches workers with higher-paying jobs with new employers. Reorganization increases the probability that workers search for a new job while they are in their previous job and reduces the probability that they search for a new job while they are unemployed. As workers accept better-paying job matches while searching on the job, reorganization increases the probability that workers find high-paying jobs with new employers.

Prediction 4: the effect of reorganization is concentrated in markets with low job growth and in thin markets. In markets with high job growth and in thick labor markets, workers can find a new job easily, so the additional search time provided by reorganization is not valuable. Reorganization is more valuable in markets with low job growth and in thin markets, as finding a job with characteristics that are similar to the pre-bankruptcy job is more time consuming.

There are alternative theories suggesting that reorganization leads to lower wages after bankruptcy. First, reorganization might retain resources in reorganized firms and prevent their reallocation to more productive uses (Caballero and Hammour (1996)). Resource retention could lead to lower wages at reorganized firms than at the establishments of liquidated firms that were transferred to new and more productive owners. Additionally, workers might stay in lower-paying jobs at reorganized firms because they have insufficient information about other job opportunities (Araújo et al. (2020)). The results in Section 7 show that the labor insurance effect of reorganization dominates

over these alternative theories in our setting, as reorganization has a positive and persistent effect on wages, reduces human capital losses, and improves job transitions.

4 The Portuguese bankruptcy system

Across most legal systems bankruptcy is regulated by two procedures: reorganization and liquidation (Djankov et al. (2008)).[1] These two legal procedures differ in their objectives. In liquidation, firms' assets are auctioned under the supervision of the court system and proceedings are distributed to claimants according to a legally established priority schedule. In reorganization, firms negotiate the reallocation of resources with creditors. While firms may shed a large portion of their labor and capital throughout the restructuring process, the common intent of reorganization is to allow businesses to remain open.

Until 2012 the Portuguese bankruptcy law did not have a separate codified reorganization system. In 2012 the bankruptcy code was amended to include a new chapter on reorganization.

We provide a concise description of the Portuguese reorganization system in this section. We also provide a more detailed description of the Portuguese bankruptcy code in Appendix A. Figure 1 illustrates the Portuguese corporate reorganization system in the bankruptcy code. Only firms (debtors) may file for reorganization in Portugal. Under the new bankruptcy system, firms can file petitions where they are headquartered or engage in most of their business. Firms have three months to agree upon a reorganization plan with a majority of creditors. Cases are randomly assigned to judges who work in the same court. Judges intervene in the reorganization process to recognize creditor claims and to accept or reject reorganization plans. Bankruptcy managers oversee negotiations between firms and creditors without the intervention of the judge. Judges intervene only once to approve or reject reorganization plans. Firms cannot submit new plans after a rejection for a period of two years. Bankruptcy regulations give judges considerable leeway to reject reorganization plans, such as for violations of court procedure or because at least one creditor is considerably worse

[1] Some countries have separate laws that regulate the transmission of establishments or firms as a going concern to new owners. In Portugal, the liquidation process subsumes these procedures.

off on account of reorganization. After reorganization is rejected, the case might be dismissed or converted into a liquidation case.

[Figure 1]

5 Data

5.1 Bankruptcy Filings

We gather data on bankruptcy filings from *Citius*, a repository of court documents maintained by the Portuguese government. We collect information for each reorganization case, including the filing date, the case and firm identification numbers, the court in which the case was filed, the judge assigned to the case, and an indicator of whether the reorganization plan was accepted or rejected. In Appendix A we provide more detail about the process we use to collect and treat the data, data coverage, and variable definitions.

The dataset covers filings between 2012 and 2016 and court documents between 2012 and 2018. The dataset covers a total of 6,731 cases but we use only 2,463 cases of firms with employees. The dataset contains the cases of other institutions such as firms without employees, trusts, independent workers, households, associations, and condominiums. While these cases are not within the scope of the paper, we use them to estimate the tendency of judges to accept or reject reorganization plans.

5.2 Firm Financial Statements

We use firm-level data from *Base de Dados de Contas Anuais* (BDCA), a universal and compulsory firm census performed annually by Banco de Portugal, the Ministry of Finance, the Ministry of Justice, and Statistics Portugal. This dataset contains complete financial statements for the near-universe of firms operating in Portugal. We use annual data between 2009 and 2018, and merge the firm dataset with the employer-employee matched dataset using the firm tax ID number.

5.3 Worker-Level Data

We use *Quadros de Pessoal*, an employer-employee matched dataset maintained by the Portuguese Ministry of Social Security, between 2009 and 2018. This dataset covers all employers regulated by the Portuguese labor code with at least one wage earner in October (the reference date of the survey). *Quadros de Pessoal* also provides important demographic information on workers such as age, gender, education, wage, and occupation. Data collected from these administrative records are less sensitive to measurement error compared to survey data because employers who misreport data to the government are subject to audits and legal penalties.

We merge the employer-employee matched dataset with the bankruptcy dataset using the firms' tax identification number. Our sample consists of 47,807 workers employed at firms that filed for reorganization. We follow common practice in the literature (Couch and Placzek (2010)) and exclude part-time workers and workers who earn less than the minimum wage, foreign nationals, and employees under 23 and over 50 years of age, who are less attached to firms or more likely to continue their education or retire early. We detect reporting gaps in the employer-employee matched dataset. First, some firms do not submit data to *Quadros de Pessoal* for the last period before the filing (9% of all observations) apparently because they are closed in the following year, when they should submit employee records. Additionally, some firms fail to report worker data after the filing but report having more than 100 workers in their financial statements (1% of all observations).

We perform two actions to address these reporting gaps. First, we include in the sample workers who are affected by under-reporting in the year before the filing and who do not establish other job contracts. We replace worker data in the year before the filing with data from the previous year for all workers, whenever such data are available. We impute worker data from previous years in fields with missing data. Second, we address the reporting gaps after the filing by imputing missing data with data from previous years. In Table A1 we repeat the analysis by measuring the effect of reorganization on worker outcomes for workers who are employed at filers two years before the filing. In this alternative analysis we use the second year before the filing as the reference year and do not perform any data imputation either before or after the filing. The alternative approach is

not affected by under-reporting and does not use imputed worker data.

5.4 O*NET

We use the *Occupational Informational Network* (O*NET), a survey of occupation characteristics administered by the North Carolina Department of Commerce and sponsored by the US Department of Labor. The survey has two parts. In the first section, randomly sampled workers from each occupation in the *Standard Occupational Classification* (SOC) system answer questions about their own jobs. The second part of the survey is completed by a panel of occupational analysts, who analyze all occupations.

O*NET has over 200 questions that score occupations in terms of job requirements. We follow Deming (2017) and convert average scores per occupation on a 0-1 scale that reflects their weighted percentile rank, using the number of workers per occupation in 2011 as sample weights. Following Lise and Postel-Vinay (2020), we create three indicators of the skill content of occupations: cognitive skills, manual skills, and interpersonal skills. We construct the cognitive skill index as the average of the indicators for mathematical reasoning, fluency of ideas, written comprehension, and oral comprehension. Manual skills are an average of the indices for finger dexterity, repairing and maintaining mechanical equipment, arm-hand steadiness, and manual dexterity. Interpersonal skill requirements are an average of the indicators for selling and influencing others, negotiation, persuasion, and speaking.

Occupations in O*NET are classified according to the SOC system, while our employer-employee matched dataset uses the *International Standard Classification of Occupations* (ISCO-08). We cross occupation codes using the crosswalk maintained by the US Bureau of Labor Statistics and take score averages when ISCO codes have more than one SOC code correspondence.

5.5 Descriptive Statistics

Table 1 shows descriptive statistics for the final sample, which includes 2,486 firms and 47,807 workers. We winsorize ratios and estimated quantities (e.g., labor gap) at the 5% level. For 59%

of the firms and 64% of the workers, reorganization ends with an accepted plan.

[Table 1]

Panel A of Table 1 depicts reorganization case outcomes. The case outcome is reorganization when the reorganization plan is accepted. When the plan is rejected firms are either liquidated or the case is dismissed. We classify the case outcome as a liquidation if the firm files for liquidation or ceases to exist up to the first year after the reorganization filing.[2] In 36% of the cases firms are liquidated. Dismissal is the final outcome for 5% of all cases.

Panel B of Table 1 reports firm descriptive statistics. On average, firms with accepted reorganization plans are larger, have more workers, have better operational performance, and are better capitalized. These differences are expected, as creditors are more likely to accept reorganization plans for better-performing firms. We also measure the marginal revenue product-cost gap of labor for firms in the sample, which is the difference between revenues and costs generated by hiring an additional worker. We use the following expression to estimate it empirically:

$$\tau_{it} = MRP^{L}_{it} - w_{it} \tag{1}$$

τ_{it} is the marginal revenue product-cost gap of labor, MRP_{it} is the marginal revenue generated by an additional worker, and w_{it} is the total wage bill divided by the number of workers. The estimation procedure follows Lenzu and Manaresi (2019) and Gandhi et al. (2020) and is explained at length in Appendix B.

In the absence of frictions, firms should hire workers up to the point where revenue equals cost. However, firms in the sample seem to be constrained, as the mean labor gap is positive. Firms with accepted reorganization plans have a slightly greater labor gap than firms with rejected reorganization plans. However, this difference is not statistically significant.

The second part of the table shows descriptive statistics for workers. Workers employed at firms

[2]The transmission of part or all of the firm to other entities as a going concern is done through the liquidation process described in Appendix A. In contrast to what happens in other countries, there is no separate legislation regulating these procedures.

with successful reorganization plans have higher wages and are less likely to be female. There are other statistically significant differences, but they are economically small. Additionally, in Table A2 we report the distribution of workers and firms that file for reorganization by industry.

6 Empirical strategy

Our baseline specification of interest is:

$$Y_{e,i,\tau} = \alpha + \beta Reorganization_i + \gamma X_{e,i,t-1} + \epsilon_{e,i,\tau} \tag{2}$$

where e is the worker identifier, i is the firm identifier, t is the year of the reorganization filing, and τ is the year of the outcome. $Reorganization_i$ is equal to 1 if firm i reorganizes, and $X_{e,i,t-1}$ is a vector of firm and worker-level controls. We take the year before the filing as the reference year instead of the year of the filing, as in our setting worker outcomes are already affected by reorganization in the year of the filing. Following Bernstein et al. (2019b), year 1 is the first year after the reference year, which in our setting is the year of the filing. We want to estimate β, the effect of reorganization on worker outcomes.

In Table A3 we estimate Equation 2 using OLS. However, selection might affect the estimation of β because reorganization is a choice made by firms, creditors, and judges. Bias might run in both directions. Some firms might not be reorganized because they face poorer prospects. At the same time, reorganization is more prevalent among capital-intensive companies (Kermani and Ma (2020)), for which employees' wages are more likely to be tied to capital-labor complementarity (Fonseca and Van Doornik (2019)). Also, practitioners cite filing early as an important driver of a successful reorganization plan (Pinheiro (2013)). Wage losses happen partially before bankruptcy (Graham et al. (2019)) and should affect more intensely firms that file for reorganization at a later date. These concurring effects are visible from descriptive statistics shown in Table 1. Reorganized firms are larger, more profitable and productive, and their workers have higher wages.

We mitigate selection concerns by exploring judge heterogeneity in the propensity to approve

reorganization plans. Our approach is similar to the rich literature that employs research designs based on the random assignment to one or more "deciders" (e.g., Kling (2006), Dobbie and Song (2015)). The bankruptcy code gives judges substantial leeway to reject reorganization plans (see Appendix A). As judges' interpretations of the law vary considerably, we use the tendency to accept reorganization plans to instrument for reorganization.

We implement an instrumental-variables approach using the following first-stage equation:

$$Reorganization_i = \rho + \pi Z_{i,j,c,t} + \delta X_{e,i,t} + \delta_{c,t} + \xi_{e,i,t} \tag{3}$$

$Reorganization_i$ is a dummy equal to 1 for firms with accepted reorganization plans, $Z_{i,j,c,t}$ is judge j's tendency to accept reorganization plans in the filing year, $X_{e,i,t}$ is a vector of firm and worker controls, and $\delta_{c,t}$ are court-year fixed effects.

We compute judge j's tendency to accept reorganization plans with the following leave-one-out measure of judge leniency, as has been done previously in the literature (e.g., Dobbie and Song (2015), Gupta et al. (2016)):

$$Z_{i,j,c,t} = \frac{1}{n_{c,j,t}}\Big(\sum_{b=1}^{n_{cjt}}(Reorganization_b) - Reorganization_i\Big) - \frac{1}{n_{ct}-1}\Big(\sum_{b=1}^{n_{ct}}(Reorganization_b) - Reorganization_i\Big) \tag{4}$$

Our final sample consists of cases filed in courts for which we can compute $Z_{i,j,c,t}$ for more than one judge identifier j, with $n_{c,j,t}$ being the number of case for court and judge identifiers c and j in the year of the filing t. On average, in each year cases are distributed across 23 court and 129 judge identifiers.[3] Firms must file for reorganization in the jurisdiction where they are headquartered or conduct most of their business. Judge assignment is random within each court. The system that allocates cases to judges is regulated by law and uniform across all courts.

[3]Some courts were renamed or merged in 2014 after a court reform. We use the last denomination of each court.In Table A1 we repeat the analysis using original court names. Some cases are allocated to more than one judge. We create separate judge identifiers for such cases. Additionally, we repeat the analysis using the identity of the last assigned judge instead. Appendix A provides more detail about variable definitions.

Figure 2 depicts the distribution of $Z_{i,j,c,t}$. The first term of $Z_{i,j,c,t}$ is the average of $Reorganization_i$ for all firms faced by judge j except for firm i. The second term is the average for all firms at court c, again excluding firm i. Intuitively, $Z_{i,j,c,t}$ is the difference between the average leniency of judge j and of court c, excluding firm i. Equation 3 removes the mechanical correlation that would exist between the outcome of firm i and the instrument. We assume that leniency is at the court level whenever there are insufficient data to compute it. Alternatively, in Table A1 we drop cases for which there are not enough data. Equation 4 uses filings that happen both before and after filing i. In Table A1 we compute the instrumental variable $Z_{i,j,c,t}$ by using only filings that happen before filing i.

[Figure 2]

The second stage equation is given by the following expression:

$$Y_{e,i,\tau} = \alpha + \beta \widehat{Reorganization}_i + \gamma X_{e,i,t-1} + \delta_{c,t} + \epsilon_{e,i,\tau} \tag{5}$$

where $\widehat{Reorganization}_i$ gives the predicted values from Equation 3. In all regressions we cluster errors at the court-year level.

If the conditions for a valid instrument hold, β captures the causal effect of reorganization on worker outcomes. Some firms would reorganize or liquidate regardless of the judge. β measures only the local average treatment effect, that is, the effect on firms that are sensitive to more lenient or strict judges.

We also study the relationship between the effect of reorganization on labor outcomes and labor market conditions. We estimate the second stage equation:

$$\begin{aligned} Y_{e,i,\tau} = \alpha + \beta_1 . \widehat{Reorganization}_i + \beta_2 \widehat{Reorganization_i \times Condition}_{m,c,t} + \\ \beta_3 Condition_{m,c,t} + \gamma X_{e,i,t-1} + \delta_{c,t} + \epsilon_{e,i,\tau} \end{aligned} \tag{6}$$

$\widehat{Reorganization_i}$ and $\widehat{Reorganization_i} \times Condition_{m,c,t}$ are predicted using $Z_{i,j,c,t}$ (judge leniency) and $Z_{i,j,c,t} \times Condition_{m,c,t}$ as instrumental variables. $Condition_{m,c,t}$ measures labor market conditions (e.g., thickness, labor market growth) for occupation m in county c in the year of the filing.

We also estimate the reduced form relationship between the instrumental variable $Z_{i,j,c,t}$ and the outcome of interest $Y_{e,i,\tau}$ by estimating the following model:

$$Y_{e,i,\tau} = \alpha + \beta Z_{i,j,c,t} + \gamma X_{e,i,t-1} + \delta_{c,t} + \epsilon_{e,i,\tau} \tag{7}$$

where $Z_{i,j,c,t}$ is the instrumental variable obtained using the expression from Equation 4.

6.1 First Stage

Table 2 presents results from estimating Equation 3. In Column (1) we include court-year fixed effects, and in Column (2) we add firm and worker-level controls, and industry fixed effects. Judge leniency is a strong predictor of reorganization. On average, a 1 pp more lenient judge is associated with a 0.365 pp higher reorganization rate.

[Table 2]

Following the literature (e.g., Bernstein et al. (2019a), Bernstein et al. (2019b)), we weigh all regressions by the inverse of the number of workers in each firm, which ensures that results are not driven by some very large firms in the sample. Alternatively, we estimate the model with unit sample weights in Table A3.

In Table A3 we verify that our results are robust to other empirical models used in the literature. We obtain the instrumental variable using only cases that were filed before the case for which we are computing the instrumental variable. We compute bootstrap standard errors that correct for the estimation error in judge leniency. We resample the data at the judge level with replacement, and generate the instrumental variable using the resampled data. We repeat the procedure 500

times to obtain bootstrap standard errors. We also estimate an alternative empirical model to avoid mechanical correlations that could arise from the interaction between court dummy variables and the instrumental variable. For each court-year pair we order cases by the filing date. We place odd-numbered cases in one subsample and even-numbered cases in the other subsample. For each court-year-subsample pair we obtain the instrument with the other subsample and use court-year-subsample fixed effects. Additionally, we average worker-level variables for each firm and estimate the model at the firm level, and estimate the model using absolute judge leniency by excluding the second term of Equation 4.

Hüther and Kleiner (2022) argue that unsecured hedge fund creditors predict the assignment of cases to judges and sway the timing of bankruptcy filings to get more reorganization-friendly judges. Judges that are busy with large cases are less likely to get new cases. Therefore, unsecured hedge funds incentivize filings in the days after less reorganization-friendly judges get new cases. This behavior makes the allocation of bankruptcy cases to judges in the US partially nonrandom. Hüther and Kleiner (2022) propose a recentered instrumental variable for causal analysis that purges any omitted variable bias caused by predictability in judge assignment. In Table A3 we provide a version of the estimation procedure proposed by Hüther and Kleiner (2022) adapted to our setting. First, we estimate the following equation:

$$Z_{i,j,c,t} = \alpha + \beta S_{i,j,c,t} + \delta X_{e,i,t-1} + \delta_{c,t} + \epsilon_{e,i,t} \tag{8}$$

$Z_{i,j,c,t}$ is the instrumental variable obtained with Equation 4. $S_{c,t}$ is the average case acceptance rate at the court where case i was filed, measured in the seven days before the filing date. If firms seek a more reorganization-friendly judge assignment, they will be more likely to file for reorganization in the days after less reorganization-friendly judges get new cases. Therefore, firms seek a high $Z_{i,j,c,t}$ file when $S_{i,j,c,t}$ is low, which leads to negative estimates for β. We estimate an alternative model based on then findings of Hüther and Kleiner (2022) in Table A3. We report the estimate for β in the first row of the table (7-day acceptance rate). We do not find a negative relationship

between $S_{c,t}$ and $Z_{i,j,c,t}$. Our results are consistent with the findings of Hüther and Kleiner (2022), as they find evidence of strategic filing only for larger US firms, and firms in Portugal are smaller than firms in the US. We estimate Equation 5 using $S_{i,j,c,t}$ from Equation 8 as an additional control variable. Adding this control variable excludes cases when there are no filings in the same court in the previous seven days.

We analyze the model's identification assumptions. The instrument F-statistic at the end of Column (2) in Table 2 is 36.58 , well above the oft-cited threshold of 10 for weak instruments (Staiger and Stock (1997)). We discuss the validity of the exclusion restriction. Interpreting two-stage least square estimates as the causal impact of reorganization on worker outcomes requires that judges affect workers only through reorganization and not through alternative channels. As discussed in Section 4, judges are randomly assigned to bankruptcy cases and do not participate in the negotiation of reorganization plans between creditors.

We provide evidence to support the exclusion restriction. In Table 2 we show that the inclusion of control variables has very little impact on the first stage coefficient. The reported effect of judge leniency on reorganization is not attributable to the control variables introduced in the first stage.

We provide evidence that cases are randomly assigned to judges. We estimate the equation:

$$Z_{i,j,c,t} = \alpha + \theta . X_{e,i,t-1} + \varepsilon_{e,i,t} \tag{9}$$

where θ measures the sensitivity of the instrument $Z_{i,j,c,t}$ to a set of worker and firm characteristics $X_{e,i,t-1}$.

Table 3 shows estimates for Equation 9. In Column (1) we include a set of firm and worker controls that we also use in Equation 3. In Columns (2)-(11) we do pairwise regressions of the instrument on each of the controls. Column (1) differs from Columns (2)-(11) because in Column (1) we regress the instrumental variable on all other variables together, while in Columns (2)-(11) we regress the instrumental variable on each variable separately. At the end of Column (1) we also report a joint significance test for the industry fixed effects. There is no evidence that the instrumental

variable is correlated with worker and firm characteristics. None of the variables are statistically significant, and coefficients are small. In Table A4 we repeat the analysis for time-changing variables in years -2 and -3, and do not find evidence of pre-trends.

[Table 3]

The instrument might be associated with improved labor outcomes because of factors that are not related to reorganization. First, the allocation of reorganization cases to more lenient judges may improve the quality of reorganization plans. Second, subsequent liquidation cases might be allocated to judges who supervised reorganization cases, and judicial decisions during the liquidation phase may affect worker outcomes. In Table A5 we estimate the relationship between filer and worker outcomes – firm survival, employment at firms that file for reorganization, and wage growth – and judicial leniency, separately for firms with accepted and rejected reorganizaiton plans. In Panels A, B, and C we report coefficients for all firms, firms with accepted reorganization plans, and firms with rejected reorganization plans, respectively. In Panel A we see a positive and strong relationship between judge leniency and the number of years the firm stays alive, the number of years workers stay in firms that file for reorganization, and wage growth. As expected, there is a strong and positive correlation between these outcomes and the instrument for the sample that has all firms. However, we do not observe these strong relationships in the subsamples of firms with accepted and rejected reorganization plans. These results suggest that the random allocation of judges does not have a large impact on the quality of reorganization plans. These finds also suggest that the results are not driven by decisions taken in subsequent liquidation cases.

We must assume monotonicity to interpret our two-stage least square estimates as the local average treatment effect (LATE) of reorganization on worker outcomes. The monotonicity assumption requires that plans accepted by a strict judge would also be accepted by a more lenient judge. Likewise, plans rejected by lenient judges would also be rejected by a stricter judge.

We provide evidence to support the monotonicity assumption by running the first stage regression on subsamples of the data. Table 4 shows estimates of first stage coefficients for these subsamples.

We split the data according to observable characteristics. For each continuous variable of Table 1 we divide the sample into two groups, at the median. Consistent with the monotonicity assumption holding, coefficients are positive and significant for each of the subsamples.

[Table 4]

As the instrumental variable is continuous, we estimate the first stage in different subsamples to characterize compliers, i.e. workers whose judicial assignment might affect the case outcome. The first stage coefficient is larger in subgroups that contain a larger percentage of compliers (e.g., Dahl et al. (2014)). We compare the first stage coefficient in the subsamples of Table 4. In Column (3) we calculate the difference between these estimates. Coefficients are relatively similar across all subgroups, but compliers tend to be concentrated in more poorly capitalized firms and have higher wages.

7 Results

Table 5 shows estimates of the impact of reorganization on bankruptcy outcomes using the model from Equation 5. A 1 pp increase in the probability of reorganization at firms where workers are employed before the filing leads to an approximate 1 pp decrease in the probability of liquidation at these firms. The local average treatment effect of reorganization on dismissal is close to 0. Therefore, we use the term "reorganization" to describe the bankruptcy outcome for firms with an accepted reorganization plan and "liquidation" for the remaining firms. In Table A1 we estimate the empirical model excluding dismissed cases.

[Table 5]

Table 5 also measures the effect of reorganization on the survival of the firms where workers are employed before the filing, and on the probability that workers stay in these firms. In Columns (3) and (4) we report estimates for the short term (first year post-bankruptcy). In Columns (5) and (6) we report estimates for the long term. Long-term results are measured in year 5, or the last observed

period for which the sample does not cover five years of data. From 2SLS estimates we see that reorganization increases firm survival by 30 pp and retention in the same firm by 26 pp . These results are not surprising, as reorganization plans often mandate managers to keep firms open. This effect lasts in the long term, as firm survival increases by 44 pp and worker retention by 29 pp. These results confirm that reorganization has a material, long-term effect on the way workers are allocated to firms. It also shows that not all reorganized firms survive the reorganization process or retain their workers, as coefficients are far from being close to 100 pp.

In Figure 3a we compare survival rates for firms that file for reorganization against a benchmark that contains a 1% random sample of all workers employed in Portugal in 2011 who were between 23 and 50 years old, were Portuguese, worked full time, earned at least the minimum wage and were employed at firms with financial statement data in *BDCA* and had more than one worker in *Quadros de Pessoal*. In year 5 about 50% of all firms that reorganize survive. This value is lower than the unconditional 5-year probability of survival (about 80%), but much greater than the continuation rate for firms that do not reorganize (approximately 10%).

[Figure 3]

Figure 3b depicts the percentage of workers who remain in the same firm up to five years after the bankruptcy filing. Reorganized firms retain about 23% of their workers, while worker retention for the benchmark sample is 55% after five years. A visual inspection of Figures 3a and 3b suggests that reorganized companies go through significant restructuring and shed a large percentage of their employees. Nevertheless, they retain considerably more workers than firms that do not reorganize (5%).

In Table A6 we report the effect of reorganization on the characteristics of firms where workers are employed. This analysis includes both workers that stay in firms that file for reorganization and workers who move to other firms. Consistent with reorganized firms going through extensive restructuring, we do not find evidence that reorganization affects the reallocation of workers to less productive firms. We also do not find evidence that reorganization prevents the reallocation of

workers from unprofitable to profitable firms.

7.1 Effect of reorganization on wages

Table 6 measures the impact of reorganization on wages. We split the effect of reorganization on an extensive margin component (effect on employment) and on an intensive margin component (wages of employed workers). Reorganization has a positive effect on wages in both the short term and the long term. In the short term, employees from reorganized firms have 18.5 pp higher wages. Employment contributes 13.5 pp to this effect, which suggests that employment is the driver of wage growth in the short term.

[Table 6]

In the long term, reorganization increases wages by 19.4 pp. Most of the wage growth in the long term arises from the intensive margin (13.6 pp). The effect of reorganization on employment in the long term is relatively small (5.8 pp) and statistically not significant. While in the long term workers from firms that do not reorganize move to other jobs, they still have considerably lower wages. In Table A7 we measure the effect of reorganization on cumulative wage growth between the year of the filing and the last year of the sample. The cumulative increase in wages caused by reorganization represents 87.4% of the pre-filing wage. The cumulative increase in wages arises evenly from the intensive margin (43.4 pp) and the extensive margin (44.0 pp).

We test the sensitivity of our results to different specifications, following previous research (e.g., Walker (2013), Graham et al. (2019)) and bound wage estimates by reclassifying missing wages using different assumptions. In the main analysis missing wages are equal to 0. Column (2) of Table A8 presents results in which we have replaced wages for workers with no job with the last wage recorded before the reorganization filing. While coefficients are smaller, they remain large and statistically significant. In Column (3) we measure the effect of reorganization on the wages of employed workers. As pointed out by Heckman (1979), a potential concern of measuring the effect of reorganization on employed workers is that the decision to work depends on the wage offer. In Column (4) we

perform a selection correction of the wage process by estimating a probit regression of wage growth on observable worker characteristics and including the inverse Mills ratio in the wage equation. We describe the correction procedure in Appendix C.

Portugal has a relatively high level of employment protection compared to other countries such as the US (Blanchard and Portugal (2001)). This characteristic of the Portuguese labor market might hinder the external validity of the results, as workers from reorganized firms in countries with lower employment protection might not benefit from the insurance provided by reorganization in bankruptcy. We test the effect of reorganization on worker outcomes in labor markets with fewer employment protections by exploring the fact that firms can break existing labor relationships more easily with workers who do not have permanent job contracts. In Table A1 we measure the effect of reorganization on workers who do not have permanent job contracts. We find that reorganization provides labor insurance in bankruptcy to these workers.

In Figure 4 we repeat the analysis of Table 6 separately for each of the years that surround the reorganization event. We repeat Equation 5 for each year between year -2 and year 5. As expected, before reorganization there is no economically meaningful relationship between the instrumental variable and wage growth. After reorganization, wages increase over the years. This effect remains even as workers find other jobs.

[Figure 4]

7.2 Mechanisms

We discuss the contribution of human capital accumulation to the effect of reorganization on labor outcomes. We classify occupations by skill intensity using data from O*NET, and we rank occupations in terms of cognitive, manual, and interpersonal skill intensity using the procedure described in Section 5. Workers move to a more skill-intensive occupation when their current job is at a higher skill level than their previous job.

Table 7 shows the effect of reorganization on the probability that workers move to occupations

with different levels of skill intensity.[4] Reorganization reduces the probability of transition to less cognitive skill-intensive occupations by 20.2 pp and to less interpersonal skill-intensive occupations by 19.1 pp . This result could arise from workers moving to new occupations (both with higher and lower skill intensity), as their original job ceases to exist. In this case, reorganization should also reduce transitions to more skill-intensive jobs. Reorganization should symmetrically decrease transitions to more skill-intensive occupations and to less skill-intensive occupations. Empirically, we do not observe that. The 2SLS coefficients associated with the transition of workers to more skill-intensive occupations are small and statistically not significant.

[Table 7]

Can the reallocation of workers to more skill-intensive occupations explain the effect of reorganization on wages? We show that the variation in wages caused by reorganization can be partially accounted for by changes in occupation fixed effects.

We estimate a reduced form model of wage formation similar to the one originally proposed by Abowd et al. (1999), using data from *Quadros de Pessoal*. We explain the estimation procedure at length in Appendix D. In the wage equation we include occupation-year fixed effects and control for other factors that influence wages by including firm fixed effects, worker fixed effects, and quadratic controls for wage and tenure. Omitted variable bias might affect the estimation of the model because of endogenous mobility to new firms and occupations. In Appendix D we follow Card et al. (2013) and argue that endogenous mobility is unlikely to affect our estimates. We estimate the effect of reorganization on the occupation premium (given by the difference between estimates for occupation fixed effects before and after reorganization) in Column (4) of Table 7. We find that reorganization has a positive effect on the occupation premium of about 2.0 pp.

As we discuss in Section 3, the literature predicts that reorganization increases transitions to better-paying jobs in new employers because workers have more time to search for new jobs while they are still on their previous job. We provide evidence that additional job search time improves

[4] In Table A7 we report the effect of reorganization on the probability that workers switch to new industries or occupations.

reallocation outcomes for workers who get jobs in new employers.

First, we provide evidence that reorganization increases the amount of time it takes for workers to get jobs with new employers. In Figure 5 we compare the annual attrition rate for workers from reorganized firms against the attrition rate for workers from firms that were not reorganized, and to the attrition rate for the average worker in Portugal. After the filing, workers both from reorganized firms and firms that are not reorganized have higher attrition rates than the average worker in Portugal. However, workers from firms that were not reorganized have much higher attrition rates than workers from reorganized firms, especially in year 1. These results suggest that workers from reorganized firms have more time to search for a new job.

[Figure 5]

In Table 8 we estimate the effect of reorganization on labor reallocation to new firms using Equation 5. In Column (1) the dependent variable is the number of years workers take to leave their job at the firm that files for reorganization. In Columns (2)-(5) the dependent variable is an indicator equal to 1 if workers leave their job, separately for each year after the filing. Reorganization increases the amount of time workers stay in their initial job by 1.124 years. Workers are less likely to leave firms in year 1 by 23.9 pp and are more likely to leave firms in year 4 or later by 8.4 pp .

[Table 8]

We show that reorganization is associated with higher wages both for workers who stay in the same job and for workers who quit. We cannot use Equation 5 to estimate the effect of reorganization on labor outcomes for workers who stay in reorganized firms and for workers who move to new employers because the identification assumptions of the model do not hold. Instead, we estimate Equation 7 to measure whether reorganization is associated with higher labor income for workers who stay in reorganized firms and for workers who leave for new employers in the last year of the sample.

Table 9 shows estimates for Equation 7. In Columns (1) and (2) we show results for all employed workers. In Columns (3) and (4) we compare employed workers from firms with rejected

reorganization plans against workers from firms with accepted reorganization plans who find a job with new employers. In Columns (5) and (6) we compare employed workers from firms with rejected reorganization plans against workers from firms with accepted reorganization plans who stay in the same firm. In Columns (1), (3), and (5) we estimate the correlation between wage growth and the instrument before the reorganization filing. Before reorganization, the instrument does not correlate with wage growth in the three groups. In Columns (2), (4), and (6) we estimate the correlation between wage growth and the instrument in the last year of the sample. After reorganization, judge leniency and wage growth have a positive correlation in all three groups.

[Table 9]

In Figure 6 we estimate Equation 7 year by year, both for workers who stay in reorganized firms and for workers who have jobs with new employers in the last year of the sample. Before filing there is no significant correlation between reorganization and judge leniency. After reorganization there is a positive and persistent correlation between reorganization and judge leniency both for workers who stay in reorganized firms and for workers who leave for new employers.

[Figure 6]

In Figure 7 we depict coefficients from estimating the effect of reorganization on job transitions by wage quantile using the model from Equation 5. We compare the wages of each worker who has a job after the reorganization filing against the wage distribution for workers in Portugal who earn at least the minimum wage.[5] We create indicator variables $\{\mathbb{1}_i^{job,Q}, \mathbb{1}_i^{job\ new\ employer,Q}\}$ for each $Q \in \{1, ..., 5\}$ to estimate the effect of reorganization on the probability that workers transition to jobs at different levels of the wage distribution. $\mathbb{1}_i^{job,Q}$ is equal to 1 when worker i has a job in quintile Q of the wage distribution. $\mathbb{1}_i^{job\ new\ employer,Q}$ is equal to 1 when worker i has a job in quintile Q of the wage distribution and this job is not in the firm that files for reorganization.

In general, reorganization should have a negative effect on the probability that workers transition to new employers because some workers may choose to stay in reorganized firms. However, that

[5] Note that we use wages measured *after* the reorganization event, not before. Table A9 provides point estimates.

effect should be smaller for transitions into high paying jobs, as the labor insurance provided by the reorganization process allows workers to find higher-paying jobs with other employers. The empirical evidence is consistent with both effects affecting the transition of workers to jobs in new employers. We find that reorganization reduces the probability that workers find jobs with new employers at low wage percentiles but not at high wage percentiles. In fact, reorganization has a positive effect on the probability that workers find high paying jobs with new employers.

[Figure 7]

7.3 Labor market conditions

We analyze the relationship between the effect of reorganization on labor outcomes and labor market conditions. As discussed in Section 3, we expect that reorganization has a greater effect on worker outcomes in thin labor markets and in markets with low job growth.

We define labor market thickness as:

$$Thickness_{m,c,t} = \frac{N_{m,c,t}}{\sum_d N_{d,m,t}} - mdn_j(\frac{N_{j,c,t}}{\sum_d N_{d,m,t}}) \tag{10}$$

$N_{m,c,t}$ is the number of workers in occupation m in the year of the filing. $mdn_j(\frac{N_{j,c,t}}{\sum_d N_{d,m,t}})$ is the median labor market thickness in county c in the year of the filing.

Labor market growth is given by:

$$Growth_{m,c,t} = \frac{N_{m,c,t} - N_{m,c,t-1}}{N_{m,c,t-1}} - mdn_j(\frac{N_{j,c,t} - N_{j,c,t-1}}{N_{m,c,t-1}}) \tag{11}$$

where $mdn_j(\frac{N_{j,c,t}-N_{j,c,t-1}}{N_{m,c,t-1}})$ is the median labor market growth in county c in the year of the filing.

In Table 10 we split the sample by labor market thickness and by job growth.[6] The effect of reorganization on wages is large in thin markets and small and statistically not significant in thick

[6] Alternatively, we interact thickness and growth (as defined in Equations 10 and 11) with the instrumental variable in the first stage in Table A10.

labor markets. Coefficients are larger in thin markets than in thick markets for wage growth in both the intensive margin and extensive margin. Occupation premiums and the probability of transition into jobs in the highest quintile of the wage distribution are also larger in thin markets and in markets with low job growth.

[Table 10]

We test whether improved worker outcomes in thin markets and in markets with low job growth come at the cost of reduced worker reallocation into profitable and productive firms. We do not find evidence to support this tradeoff. The effect of reorganization on the probability that workers transition into profitable or productive firms is positive but not statistically significant in low-growth labor markets. In thin markets, the effect of reorganization on the probability of transition into profitable firms is positive while the effect on the probability of transition into productive firms is negative. In both cases estimates are statistically not significant.

We provide evidence that labor insurance is a source of excessive liquidation costs in bankruptcy. Liquidations are costly to workers because reorganization has a positive effect on labor outcomes in thin and low-growth markets. These costs are not compensated by higher employment or wages after liquidation in thick or high-growth labor markets. Moreover, we do not find evidence that reorganization reduces the cleansing effect of firm destruction documented by Caballero and Hammour (1996), as there is no evidence that reorganization precludes the reallocation of workers to profitable or productive firms.

8 Conclusion

How does reorganization affect the reallocation of labor in bankruptcy? In this paper we show that reorganization is an important source of labor insurance in bankruptcy.

We use the Portuguese labor market as a laboratory to analyze this question. The Portuguese setting offers some unique advantages. First, we track workers even as they leave firms that file for reorganization. Second, we use the near-universe of financial statements for Portuguese firms and data on workers' occupations to study the mechanisms that contribute to the effect of reorganization on wages. Finally, we use the random assignment of reorganization cases to judges to create exogenous variation in the probability of reorganization.

We show that reorganization provides labor insurance to workers in bankruptcy. Reorganization increases wages by 19.4 pp up to five years after the reorganization filing. In the year of the filing higher employment explains most of the wage growth. In the last year of the sample most of the wage growth arises from the intensive margin, meaning that workers from reorganized firms have better-paying jobs.

We show that two mechanisms contribute to the effect of reorganization on labor outcomes in bankruptcy. First, we show that reorganization reduces the loss of human capital in bankruptcy. Reorganization reduces the probability that workers move to less skill-intensive occupations that have lower wage premiums. Second, we show that reorganization shifts the wage distribution for workers who move to new employers to the right. With reorganization, workers take more time to get a job with a new employer. While reorganization reduces the probability that workers transition to new employers, it increases the probability that workers shift to jobs with new employers in the highest quintile of the wage distribution.

We relate the effect of reorganization on wages to local labor market conditions. The effect of reorganization on wages is concentrated in thin labor markets and in labor markets with low job growth. Reorganization could potentially reduce the cleansing effect of firm liquidation in bankruptcy (Caballero and Hammour (1996)) by precluding the reallocation of labor to more productive firms.

Instead, we find that reorganization affects labor reallocation in markets with high search frictions by increasing the utilization of labor instead of reducing the reallocation of workers to productive or profitable firms.

Tables

Table 1: Descriptive statistics

	All firms (1)	Accepted reorganization (2)	Rejected reorganization (3)	Difference (4)
Panel A: outcomes				
Reorganization	59.051	100.000	0.000	
Liquidation	36.082	0.000	88.114	
Dismissal	4.867	0.000	11.886	
Panel B: firm characteristics				
Assets (€ Million)	6.477	7.417	5.122	2.295**
	(28.297)	(29.921)	(25.730)	
Workers †	30.308	32.993	26.435	6.558**
	(72.002)	(80.053)	(58.295)	
EBITDA/Assets (%)	-5.343	-4.303	-6.842	2.539***
	(15.066)	(14.175)	(16.154)	
Equity ratio (%)	-7.514	-5.082	-11.020	5.939***
	(50.709)	(49.267)	(52.545)	
Labor gap (€ thousand)	12.912	13.475	12.100	1.375
	(20.885)	(20.931)	(20.802)	
Panel C: worker characteristics				
Age (years)	37.462	37.413	37.549	-0.1*
	(7.510)	(7.492)	(7.542)	
Female (%)	40.818	39.449	43.289	-3.8***
	(49.150)	(48.875)	(49.549)	
Schooling (years)	8.530	8.493	8.598	-0.1**
	(4.589)	(4.608)	(4.554)	
Tenure (years)	7.446	7.354	7.611	-0.3***
	(7.593)	(7.433)	(7.869)	
Wage (€)	1098.302	1112.595	1072.508	40.1***
	(879.694)	(913.871)	(813.789)	
Number of firms	2,486	1,468	1,018	
Number of workers	47,807	30,761	17,046	

Notes. The table shows descriptive statistics for firms and workers in the sample. Column (1) reports statistics for the whole sample. Column (2) reports statistics for firms that have an accepted reorganization plan. Column (3) reports statistics for firms that have a rejected reorganization plan. Column (4) reports the difference between Columns (2) and (3). Standard deviations are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels. † This value is greater than the number of workers in the worker sample because we follow the literature and exclude workers who are under 23 or over 50 years old, part-time workers, and foreign nationals from the worker sample (Couch and Placzek (2010)).

Table 2: First stage

	Reorganization	
Variable	(1)	(2)
Instrument	0.362***	0.365***
	(0.061)	(0.060)
Log assets		-0.003
		(0.017)
Log workers		0.027**
		(0.011)
Labor gap		0.009
		(0.027)
Equity ratio		0.001
		-0.001
EBITDA/Assets		0.224***
		(0.075)
Age		-0.001
		(0.001)
Tenure		-0.0004
		(0.001)
Female		-0.023
		(0.014)
Years schooling		-0.001
		(0.001)
Log wage		0.024
		(0.017)
Instrument F-stat	35.74	36.58
Observations	47,807	47,807
R-squared	0.077	0.096

Notes. The table reports first stage results. The dependent variable is a dummy equal to 1 if the reorganization filing is accepted. The instrument is the percentage of reorganization plans approved by the judge in the year of the filing minus the percentage of reorganization plans approved in the court where the judge is employed, excluding the case itself. Other variables are defined in the text. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Random judge assignment

	Instrumental variable										
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Log assets	0.007	0.006									
	(0.007)	(0.005)									
Log workers	-0.002		-0.0003								
	(0.004)		(0.003)								
Labor gap	0.004			-0.0004							
	(0.011)			(0.009)							
Equity ratio	0.0001				0.0002						
	(0.000)				(0.000)						
EBITDA/Assets	-0.047					-0.031					
	(0.032)					(0.028)					
Age	0.0003						0.0002				
	(0.000)						(0.000)				
Tenure	0.0001							0.0003			
	(0.001)							(0.000)			
Female	0.005								0.007		
	(0.005)								(0.006)		
Years schooling	0.001									0.001	
	(0.001)									(0.001)	
Log wage	-0.002										0.004
	(0.007)										(0.007)
R-squared	0.01139	0.00072	0	0	0.00035	0.00047	0.00005	0.00008	0.00028	0.0003	0.00007
F-statistic	1.1										
Observations	47,807	47,807	47,807	47,807	47,807	47,807	47,807	47,807	47,807	47,807	47,807

Notes. This table reports randomization tests to illustrate the random assignment of reorganization to judges within a court. The dependent variable is the instrumental variable, as defined in Equation 3. Column (1) reports coefficients from regressing the instrumental variable on all variables. Columns (2)-(11) show pairwise regressions for each variable. The F-statistic corresponds to a joint significance test for industry dummies, using the list of industries from Table A2. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: First stage by group

Variable	Below median (1)	Above median (2)	Above median - below median (3)
Assets	0.383***	0.467**	0.084
	(0.062)	(0.207)	(0.216)
Workers	0.365***	0.279***	-0.086
	(0.062)	(0.077)	(0.099)
Equity ratio	0.458***	0.293***	-0.165
	(0.077)	(0.093)	(0.121)
Labor gap	0.421***	0.339***	-0.082
	(0.063)	(0.078)	(0.100)
EBITDA/Assets	0.411***	0.408***	-0.003
	(0.077)	(0.060)	(0.098)
Age	0.335***	0.403***	0.068
	(0.081)	(0.067)	(0.105)
Tenure	0.309***	0.299***	-0.01
	(0.067)	(0.067)	(0.095)
Education	0.419***	0.431***	0.012
	(0.071)	(0.059)	(0.092)
Log wage	0.281***	0.432***	0.151
	(0.078)	(0.067)	(0.103)

Notes. This table estimates the first stage equation for subsamples of the data. We split workers by groups according to observable characteristics from Table 1. In Column (1) we report the first stage coefficient from Equation 3 for workers who are below the median with respect to each of the listed observable characteristics. In Column (2) we report the coefficient for workers who are above or at the median. Column (3) shows the difference between Columns (2) and (1). Standard errors, clustered at the court-year level, are shown in parentheses. We assume that coefficients are uncorrelated to obtain standard errors in Column (3). *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Reallocation outcomes

	Bankruptcy outcome		Short term (year 1)		Long term (up to year 5)	
Variable	Liquidation (1)	Dismissal (2)	Firm survival (3)	Job same firm (4)	Firm survival (5)	Job same firm (6)
Reorganization	-1.028***	0.028	0.304***	0.261***	0.440***	0.293***
	(0.066)	(0.066)	(0.108)	(0.091)	(0.127)	(0.071)
Observations	47,807	47,807	47,807	47,807	47,807	47,807
R2	0.806	0.036	0.178	0.130	0.202	0.109

Notes. This table shows the effect of reorganization on the reallocation of workers in the short term and the long term. Short-term outcomes are measured in the year of the filing (year 1). Long-term outcomes are measured in year 5 or in the last year of the sample when there are less than five years of data after the filing. *Liquidation* is equal to 1 if the firm where worker e from Equation 5 is employed before the filing starts a liquidation process or stops reporting firm data up to the year after the reorganization filing. *Dismissal* is equal to 1 if the firm where worker e from Equation 5 is employed before the filing has a rejected reorganization plan but is not liquidated. *Firm survival* is an indicator equal to 1 if the firm where worker e from Equation 5 is employed before the filing that files for reorganization remains open. *Job same firm* is an indicator equal to 1 if the worker is employed at the firm that files for reorganization. We display 2SLS estimates from Equation 5 for both dependent variables. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Wage growth

	Short term (year 1)			Long term (up to year 5)		
Variable	Wage growth (1)	Employment (extensive margin) (2)	Intensive margin growth (3)	Wage growth (4)	Employment (extensive margin) (5)	Intensive margin growth (6)
Reorganization	0.185**	0.135	0.05	0.194**	0.058	0.136**
	(0.089)	(0.082)	(0.052)	(0.085)	(0.073)	(0.056)
Observations	47,807	33,137	47,807	47,807	33,332	47,807
R-squared	0.083	0.092	0.060	0.064	0.116	0.077

Notes. This table shows the effect of reorganization on wages. Short-term outcomes are measured in the year of the filing (year 1). Long-term outcomes are measured in year 5 or in the last year of the sample when there are less than five years of data after the filing. *Wage growth* is the ratio between the wage after the filing and the wage before the filing. *Employment (extensive margin)* is an indicator equal to 1 if the worker has a job in the employer-employee matched dataset. *Intensive margin growth* is the difference between wage growth and the employment dummy. We display 2SLS estimates from Equation 5 for all dependent variables. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Transition to new occupations

(a) Transition to less skill-intensive occupations and occupation wage premium

	Lower skill intensity = 1			Occupation
	Cognitive	Manual	Interpersonal	premium
	(1)	(2)	(3)	(4)
Reorganization	-0.202***	-0.143***	-0.191***	0.020*
	(0.065)	(0.049)	(0.066)	(0.011)
Observations	47,807	47,807	47,807	47,807
R-squared	0.006	0.019	0.009	0.072

(b) Transition to more skill-intensive occupations

	Higher skill intensity = 1		
	Cognitive	Manual	Interpersonal
	(1)	(2)	(3)
Reorganization	-0.046	-0.105*	-0.058
	(0.056)	(0.055)	(0.056)
Observations	47,807	47,807	47,807
R-squared	0.038	0.035	0.034

Notes. This table shows the effect of reorganization on the occupations held by workers in the long term. In Columns (1)-(3) of Panels A and B we show the effect of reorganization on the probability that workers move to less or more skill-intensive occupations. We consider three skill categories: cognitive, manual, and interpersonal. Skill intensity is obtained using the procedure described in Section 5. In Column (4) of Panel A we show the effect of reorganization on the premium associated to the occupation in which the worker was last employed. We compute the occupation premium using the procedure from Appendix D. We display 2SLS estimates from Equation 5. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Reallocation to new firms

	Time to leave	P(leave firm), by year			
	(years)	Year 1	Year 2	Year 3	Year 4+
	(1)	(2)	(3)	(4)	(5)
Reorganization	1.124***	-0.239***	-0.161**	0.023	0.084**
	(0.257)	(0.089)	(0.074)	(0.052)	(0.040)
Observations	47,807	47,807	47,807	47,807	47,807
R-squared	0.174	0.138	0.014	0.032	0.065

Notes. This table shows the effect of reorganization on the transition of workers to jobs in new firms. We display 2SLS estimates from Equation 5. *Time to leave (years))* is the number of years between the year before the filing and the year when the worker leaves the job. When workers do not leave the job the variable is equal to the number of years after the filing of the last observation. *Leave firm* is an indicator variable equal to 1 in the year of the filing (year 1) or in one of the subsequent years. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Relationship between judge leniency and wages by worker group

	Employed workers		New job		Job same firm	
Dependent variable	Before filing (1)	After filing (2)	Before filing (3)	After filing (4)	Before filing (5)	After filing (6)
Reorganization	0.01	0.080***	0.018	0.108***	0.029	0.070**
	(0.020)	(0.027)	(0.023)	(0.031)	(0.025)	(0.035)
Observations	33,332	33,332	25,279	25,279	19,414	19,414
R-squared	0.459	0.156	0.502	0.162	0.421	0.162

Notes. This table shows the relationship between judge leniency and wage growth for workers who find jobs with new employers and for workers who stay in the same firm using Equation 7. Columns (1), (3), and (5) show coefficients for the year before the filing. Columns (2), (4), and (6) show coefficients for the last year of the sample. Columns (1) and (2) show values for all employed workers. In Columns (3) and (4) we exclude workers from firms with accepted reorganization plans who stay in reorganized firms. In Columns (5) and (6) we exclude workers from firms with accepted reorganization plans who move to other employers. *, **, *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

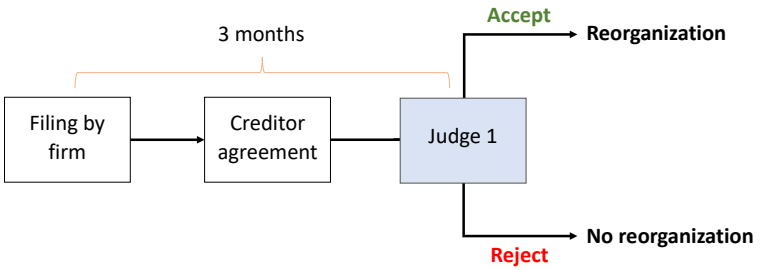
Table 10: Reorganization outcomes, labor market thickness, and job growth

	Thickness		Labor market growth	
	<p(50)	≥ p(50)	<p(50)	≥ p(50)
	(3)	(4)	(1)	(2)
Instrument	0.314***	0.415***	0.266***	0.456***
	(0.075)	(0.064)	(0.085)	(0.060)
Wage growth	0.337**	0.033	0.514**	0.067
	(0.144)	(0.095)	(0.225)	(0.088)
Job (extensive margin growth)	0.210*	-0.065	0.332**	-0.058
	(0.109)	(0.088)	(0.167)	(0.076)
Wage growth (intensive margin)	0.127*	0.098	0.182	0.125**
	(0.069)	(0.073)	(0.111)	(0.059)
P(profitable firm)	0.068	-0.024	0.235	-0.066
	(0.119)	(0.098)	(0.152)	(0.086)
Lower cognitive skill	-0.123	-0.254***	-0.245**	-0.179***
	(0.093)	(0.079)	(0.114)	(0.067)
Lower interpersonal skill	-0.153	-0.201***	-0.217*	-0.171***
	(0.096)	(0.071)	(0.128)	(0.066)
Lower manual skill	-0.171*	-0.152**	-0.266**	-0.113**
	(0.087)	(0.069)	(0.122)	(0.055)
Occupation premium	0.029	0.007	0.041*	0.009
	(0.018)	(0.013)	(0.023)	(0.011)
P(job quintile 5)	0.092*	0.057**	0.077	0.062**
	(0.049)	(0.026)	(0.052)	(0.032)

Notes. In this table we split the sample by labor market thickness (defined in Equation 10) and job growth (defined in Equation 11). Columns (1) and (3) show estimates for Equation 5 in labor markets with thickness and job growth below the median. Columns (2) and (4) show estimates for Equation 5 for labor markets with thickness and job growth at the median or above the median. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

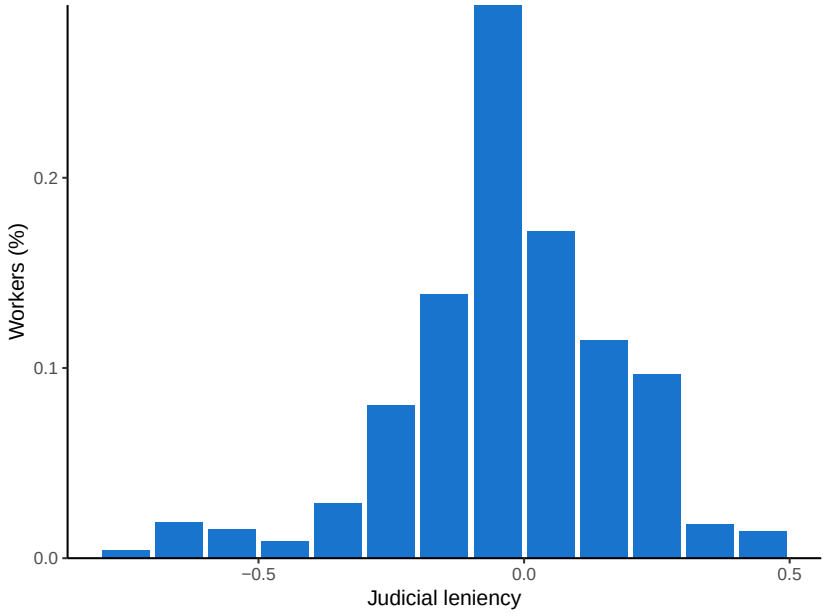
Figures

Figure 1: Reorganization in the Portuguese Bankruptcy Code



Notes. This figure depicts the corporate reorganization system of the Portuguese bankruptcy code. See Appendix A for a detailed description of the Portuguese bankruptcy system.

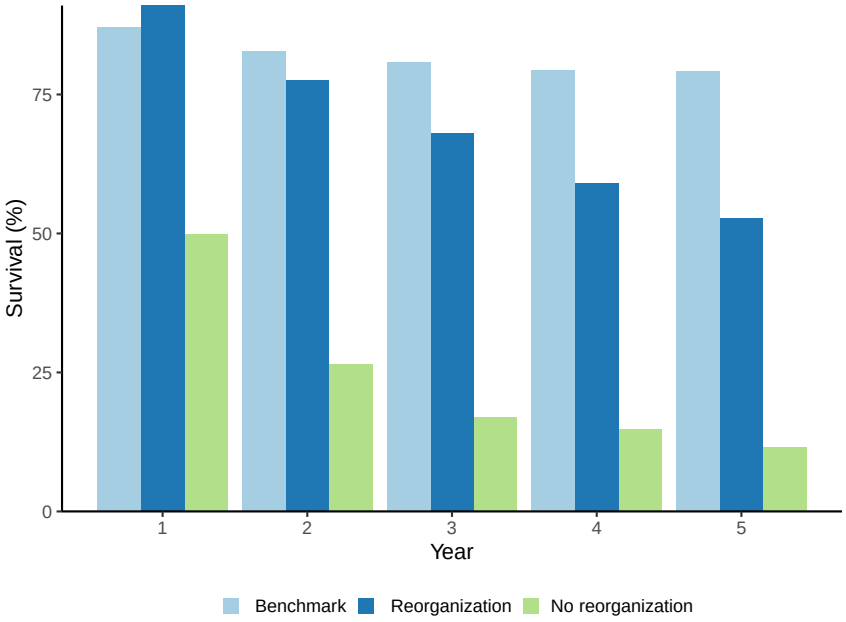
Figure 2: Judicial leniency distribution



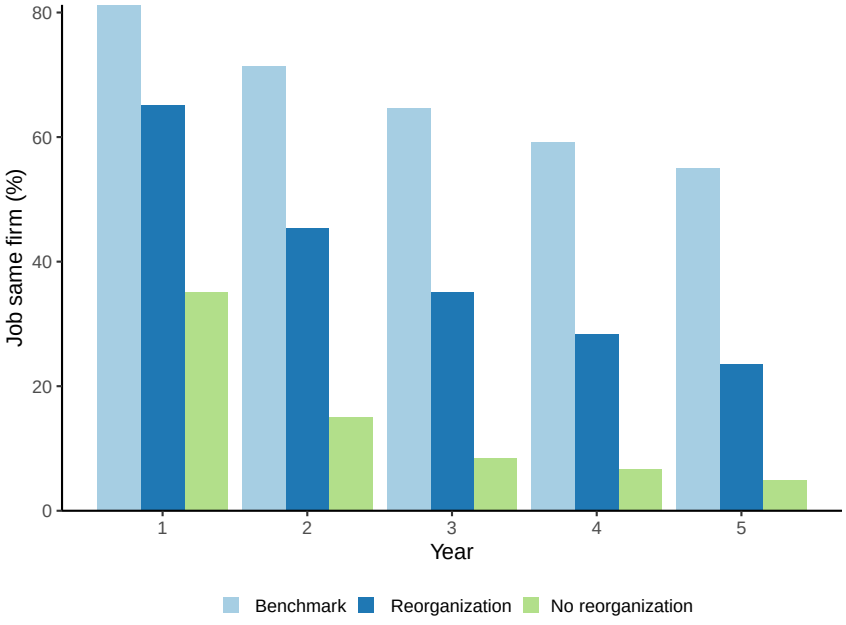
Notes. This figure depicts the distribution of the instrumental variable obtained with Equation 3.

Figure 3: Worker reallocation over time

(a) Firm survival

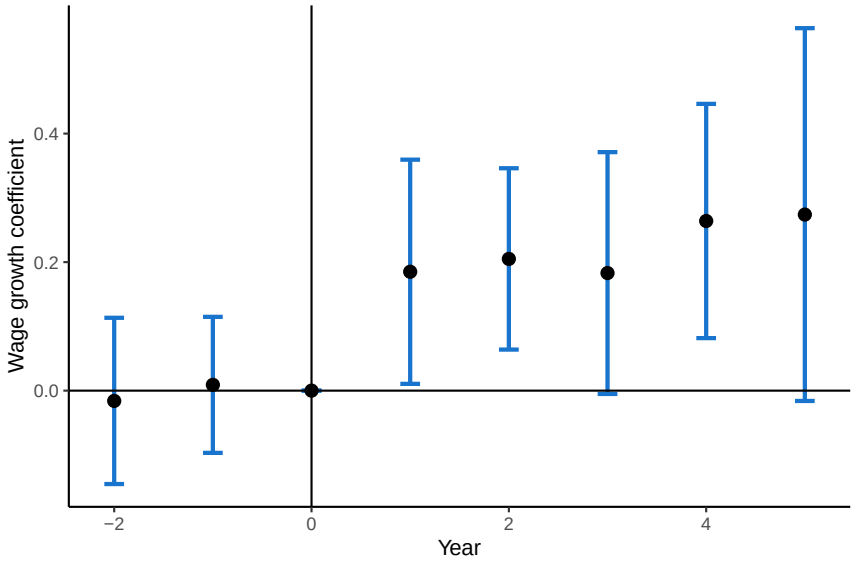


(b) Worker retention



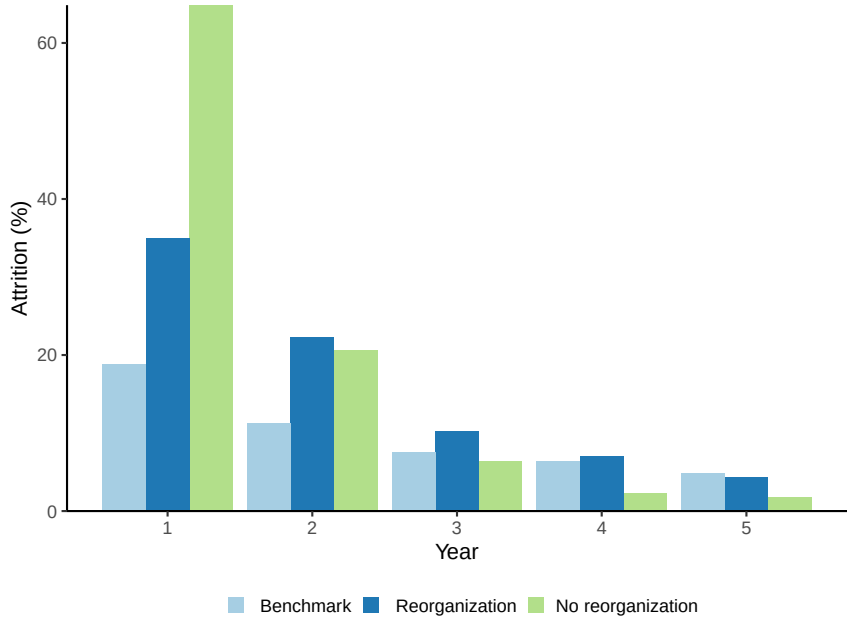
Notes. Panel A depicts the percentage of workers from firms that survive up to five years after the reorganization filing. Panel B depicts the percentage of workers who stay in reorganized firms. We restrict the sample to firms that have five years of data after the filing. We compare these workers to a benchmark of workers employed in Portugal in 2011 described in Section 7.

Figure 4: Year-by-year second stage results



Notes. This figure depicts 2SLS estimates for the effect of reorganization on wages up to five years after reorganization. We estimate Equation 5 each year between year -2 and year 5. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects. Standard errors are clustered at the court-year level. Error bars denote 95% confidence intervals.

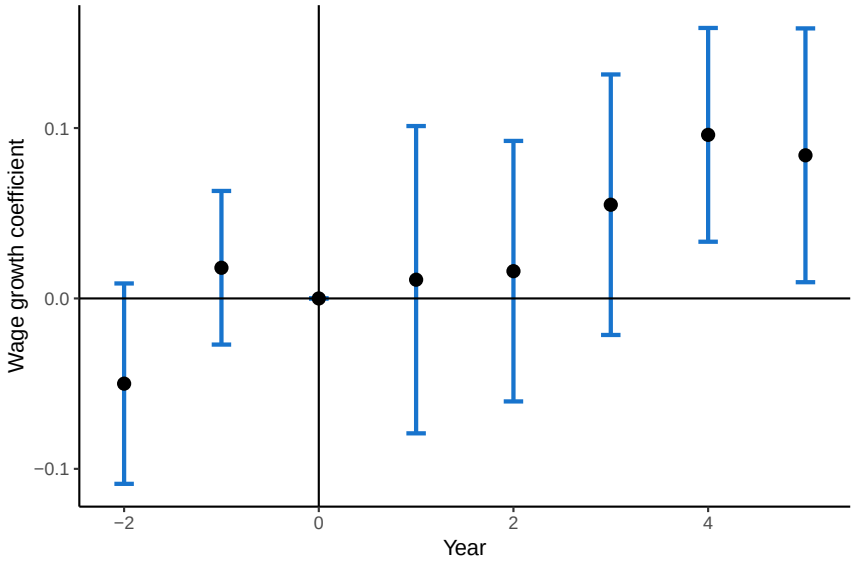
Figure 5: Worker attrition



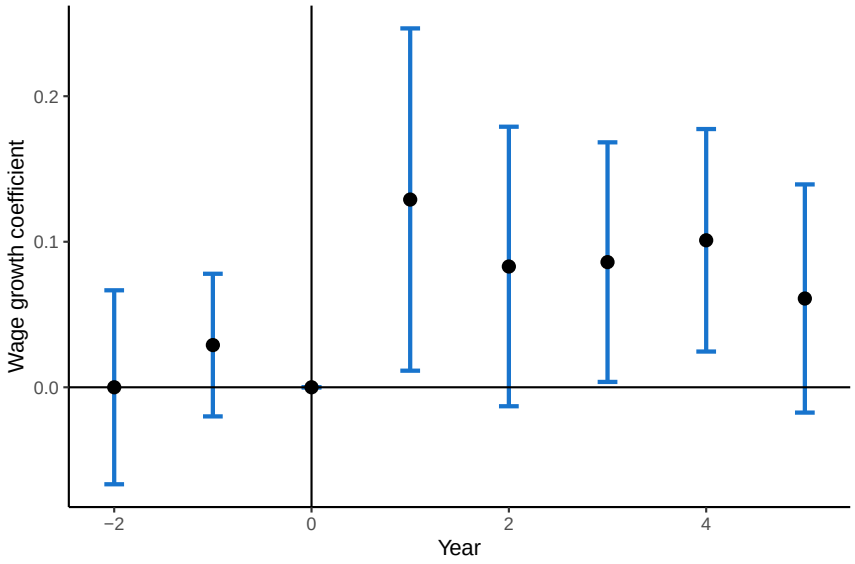
Notes. This figure shows the percentage of workers who leave firms. Dark blue bars (green bars) depict annual attrition rates for workers employed at reorganized (not reorganized) firms. We compare these workers to a benchmark of workers employed in Portugal in 2011.

Figure 6: Wage growth and judge leniency: year-by-year results

(a) Workers who leave to new employers

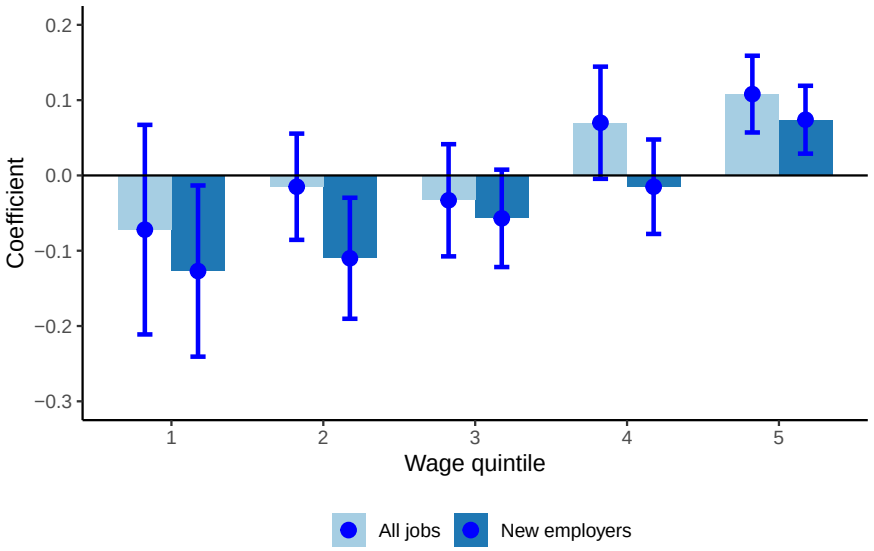


(b) Workers who stay in the same firm



Notes. In this figure we estimate the relationship between wage growth and judge leniency. We estimate Equation 7 each year between year -2 and year 5. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects. Standard errors are clustered at the court-year level. Error bars denote 95% confidence intervals.

Figure 7: Job transitions and wage quintiles



Notes. The table depicts estimates for the effect of reorganization on employment transitions by wage quintile. We compute wage quintiles using a sample of employed workers earning at least the minimum wage. $\mathbb{1}_{i,k}^{job,Q} = 1$ when worker i has a job in quintile Q of the wage distribution. $\mathbb{1}_{i,k}^{job\ new\ employer,Q} = 1$ when worker i has a job in quintile Q of the wage distribution and this job is not in the firm that files for reorganization. Light bars depict estimates for Equation 5 using $\mathbb{1}_{i,k}^{job,Q} = 1$ as the dependent variable. Dark bars depict estimates for Equation 5 using $\mathbb{1}_{i,k}^{job\ new\ employer,Q} = 1$ as the dependent variable. The horizontal axis indicates quintiles of the wage distribution. Error bars denote 95% confidence intervals.

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Appendix Tables

Table A1: Additional results

	t=-2 (1)	Drop cases (2)	Drop dismissals (3)	Old court FE (4)	Judicial decision (5)	Non-permanent workers (6)
Instrument	0.361***	0.345***	0.394***	0.326***	0.222***	0.468***
	(0.064)	(0.061)	(0.064)	(0.066)	(0.055)	(0.076)
Job same firm	0.240***	0.300***	0.298***	0.293***	0.304**	0.254***
	(0.071)	(0.076)	(0.069)	(0.080)	(0.125)	(0.077)
Wage growth	0.229**	0.192**	0.217***	0.297***	0.324*	0.271**
	(0.093)	(0.091)	(0.082)	(0.112)	(0.169)	(0.125)
Job (extensive margin growth)	0.140**	0.051	0.085	0.092	0.158	0.08
	(0.071)	(0.077)	(0.068)	(0.087)	(0.131)	(0.099)
Wage growth (intensive margin)	0.089	0.142**	0.132***	0.205***	0.166*	0.190**
	(0.054)	(0.061)	(0.051)	(0.073)	(0.099)	(0.080)
P(profitable firm)	0.124	-0.00001	0.022	0.045	0.229	0.074
	(0.075)	(0.088)	(0.074)	(0.092)	(0.168)	(0.105)
Lower cognitive skill	-0.109*	-0.206***	-0.200***	-0.217***	-0.214**	-0.296***
	(0.061)	(0.070)	(0.063)	(0.084)	(0.099)	(0.111)
Lower interpersonal skill	-0.1	-0.198***	-0.186***	-0.230***	-0.273**	-0.218**
	(0.061)	(0.070)	(0.064)	(0.084)	(0.108)	(0.109)
Lower manual skill	-0.105**	-0.149***	-0.133***	-0.155***	-0.009	-0.293***
	(0.048)	(0.052)	(0.049)	(0.060)	(0.088)	(0.089)
Occupation premium	0.015	0.020*	0.023**	0.026*	0.053**	0.035**
	(0.011)	(0.012)	(0.011)	(0.014)	(0.022)	(0.016)
Wage other firm	0.062*	0.106***	0.113***	0.122***	0.086**	0.168***
	(0.033)	(0.031)	(0.032)	(0.033)	(0.035)	(0.059)
Years to get new job	0.782***	1.140***	1.075***	1.067***	0.785*	0.782***
	(0.245)	(0.276)	(0.259)	(0.284)	(0.401)	(0.290)
P(job quintile 5)	0.059*	0.077***	0.073***	0.099***	0.052	0.113***
	(0.031)	(0.024)	(0.022)	(0.031)	(0.046)	(0.041)
Wage growth (thin)	0.344**	0.348**	0.346**	0.435**	0.549*	0.541**
	(0.145)	(0.158)	(0.144)	(0.189)	(0.291)	(0.238)
Wage growth (thick)	0.116	0.027	0.06	0.061	0.097	0.132
	(0.147)	(0.100)	(0.092)	(0.108)	(0.152)	(0.135)
Wage growth (low growth)	0.379*	0.568**	0.481**	0.674**	0.717	0.853**
	(0.217)	(0.281)	(0.203)	(0.335)	(0.532)	(0.380)
Wage growth (high growth)	0.168*	0.078	0.097	0.17	0.176	0.02
	(0.090)	(0.090)	(0.086)	(0.106)	(0.157)	(0.127)
Observations	50,505	45,035	45,730	47,807	47,807	12,393

Notes. The table reports robustness checks for alternative empirical models. In Column (1) we measure the effect of reorganization on workers employed at filers two years before the filing and do not perform any worker data imputations. In Column (2) we drop cases for which there is not enough data to compute $Z_{i,j,c,t}$ from Equation 4. In Column (3) we drop reorganization cases that are dismissed. In Column (4) we use the alternative court identities described in Section 6. In Column (5) we obtain the instrumental variable using the identity of the judge that had the last interaction with the case. In Column (6) we restrict the sample to workers that do not have a permanent contract. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A2: Industries

Industry (1)	Workers (2)	Firms (3)
Manufacure of food products	1,346	61
Manufacture of textiles	1,997	56
Manufacture of wearing apparel	2,818	76
Manufacture of leather and related products	1,504	54
Manufacture of wood and related products	1,043	53
Manufacture of fabricated metal products, except machinery and equipment	1,960	104
Manufacture of furniture	963	57
Construction of buildings	4,234	225
Civil engineering	1,819	61
Specialized construction activities	2,480	135
Wholesale and retail trade and repair of motor vehicles and motorcycles	1,296	97
Wholesale trade, except of motor vehicles and motorcycles	3,516	300
Retail trade, except of motor vehicles and motorcycles	5,816	273
Land transport and transport via pipelines	921	69
Food and beverage service activities	1,047	97
Other industries	15,047	768
Total	47,807	2,486

Notes. The table reports the distribution of workers and firms that file for reorganization by industry. Column (1) reports the industry name. Columns (2) and (3) show the number of workers and firms from each industry in the sample. "Other industries" contains industries with fewer than 50 firms in the sample.

Table A3: Alternative empirical models

Variable	OLS (1)	Past cases (2)	Worker weights (3)	Bootstrap (4)	Sample split (5)	Firm aggregation (6)	Absolute leniency (7)	Hüther and Kleiner (2022) (8)
7-day acceptance rate								0.021
								(0.014)
Instrument		0.290***	0.329***	0.338***	0.284***	0.367***	0.273***	0.435***
		(0.047)	(0.112)	(0.061)	(0.056)	(0.063)	(0.055)	(0.067)
Job same firm	0.174***	0.368***	0.289**	0.29***	0.408***	0.290***	0.345***	0.192***
	(0.011)	(0.091)	(0.147)	(0.087)	(0.105)	(0.072)	(0.105)	(0.066)
Wage growth	0.055***	0.166*	0.135	0.209*	0.257**	0.191**	0.253**	0.178**
	(0.017)	(0.098)	(0.113)	(0.122)	(0.120)	(0.088)	(0.124)	(0.077)
Job (extensive margin growth)	0.045***	0.087	0.017	0.048	0.123	0.056	0.064	0.099
	(0.013)	(0.087)	(0.074)	(0.092)	(0.092)	(0.075)	(0.101)	(0.071)
Wage growth (intensive margin)	0.01	0.078	0.117	0.161**	0.133**	0.135**	0.189**	0.079*
	(0.009)	(0.053)	(0.083)	(0.081)	(0.067)	(0.058)	(0.086)	(0.043)
P(profitable firm)	0.018	0.048	-0.02	0.009	0.144	0.008	0.015	0.093
	(0.013)	(0.098)	(0.153)	(0.104)	(0.093)	(0.085)	(0.114)	(0.071)
Lower cognitive skill	-0.058***	-0.162**	-0.157**	-0.226***	-0.222***	-0.202***	-0.257***	-0.105
	(0.009)	(0.069)	(0.071)	(0.087)	(0.084)	(0.066)	(0.098)	(0.072)
Lower interpersonal skill	-0.054***	-0.168**	-0.150**	-0.158**	-0.215**	-0.191***	-0.246**	-0.106
	(0.009)	(0.069)	(0.068)	(0.071)	(0.085)	(0.068)	(0.099)	(0.071)
Lower manual skill	-0.036***	-0.121**	-0.148**	-0.214**	-0.211***	-0.140***	-0.183**	-0.092*
	(0.010)	(0.059)	(0.065)	(0.087)	(0.072)	(0.050)	(0.071)	(0.054)
Occupation premium	0.006***	0.021	0.008	0.02	0.030**	0.020*	0.025	0.012
	(0.002)	(0.014)	(0.012)	(0.015)	(0.014)	(0.012)	(0.016)	(0.011)
Wage other firm	0.024*	0.053*	0.073**	0.108***	0.066**	0.056**	-0.17	0.072**
	(0.014)	(0.028)	(0.032)	(0.037)	(0.029)	(0.028)	(0.377)	(0.030)
Years to get new job	0.786***	1.180***	1.242***	1.106***	1.280***	1.120***	1.280***	0.960***
	(0.045)	(0.309)	(0.471)	(0.319)	(0.341)	(0.265)	(0.368)	(0.269)
P(job quintile 5)	-0.003	0.079***	0.084	0.085**	0.054	0.073***	0.102***	0.063**
	(0.005)	(0.028)	(0.071)	(0.040)	(0.040)	(0.023)	(0.036)	(0.026)
Wage growth (thin)	0.078***	0.231	0.235*	0.414*	0.352*	0.258*	0.469**	0.310***
	(0.022)	(0.159)	(0.131)	(0.241)	(0.181)	(0.142)	(0.239)	(0.115)
Wage growth (thick)	0.03	0.049	0.105	0.024	0.127	0.096	0.031	-0.043
	(0.022)	(0.117)	(0.129)	(0.125)	(0.142)	(0.105)	(0.123)	(0.106)
Wage growth (low growth)	0.075***	0.500*	0.392	0.782	0.528**	0.236*	0.874	0.481***
	(0.023)	(0.263)	(0.339)	(1.014)	(0.255)	(0.142)	(0.587)	(0.164)
Wage growth (high growth)	0.032*	0.017	0.038	0.055	0.07	0.069	0.08	-0.031
	(0.019)	(0.102)	(0.093)	(0.111)	(0.109)	(0.093)	(0.108)	(0.105)
Observations	47,807	47,807	47,807	47,807	47,807	2,486	47,807	33,985

Notes. The table reports robustness checks for alternative empirical models. The dependent variable is listed in each row. Column (1) shows OLS estimates. In Column (2) we compute judge leniency using past cases instead of past and future cases. In Column (3) we repeat the first stage by giving unit weights to all observations, instead of weighting each observation with the inverse of the number of workers in the firm. In Column (4) we bootstrap our specification following Dobbie et al. (2018). We resample the data at the judge level, with replacement, and generate the instrumental variable using the resampled data. We repeat the procedure 500 times to obtain bootstrap standard errors. In Column (5) we split the sample in two equal parts. In each case we estimate judge leniency using data only from the other sub-sample. In Column (6) we estimate the aggregate model at the firm level. In Column (7) we estimate judge leniency excluding the second term from Equation 3 and include the court case acceptance rate as an additional explanatory variable to correct for exclusion bias (Fafchamps and Caeyers (2020)). In Column (8) we use the procedure proposed by Hüther and Kleiner (2022) to purge omitted variable bias possibly caused by predictability in the judge assignment. We include the set of control variables from Column (2) of Table 2. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A4: Instrument robustness: pre-trends

	Instrumental variable	
	2 years before filing (1)	3 years before filing (2)
Log assets	-0.00002	-0.0002
	(0.001)	(0.001)
Log workers	-0.0002	-0.0001
	(0.003)	(0.002)
Labor gap	0.0001	-0.0001
	(0.000)	(0.000)
Equity ratio	0.0001	-0.0001
	(0.000)	(0.000)
EBITDA/Assets	0.0002	-0.0003
	(0.000)	(0.001)
Log wage	0.001	0.0003
	(0.002)	(0.001)
Observations	47,807	47,807

Notes. This table reports randomization tests to illustrate the random assignment of reorganization to judges within a court, two and three years before filing. The dependent variable is the instrumental variable, as defined in Equation 4. We assume that the values are equal to zero when values are missing. Columns (1) and (2) show pairwise regressions for each variable. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A5: Worker outcomes within filers

(a) All firms

Variable	Years firm survival (1)	Cumulative employment (2)	Wage growth (3)
Instrument	0.756***	0.514***	0.071**
	(0.205)	(0.124)	(0.031)
Observations	47,807	47,807	47,807
R-squared	0.147	0.117	0.073

(b) Firms with accepted reorganization plans

Variable	Years firm survival (1)	Years employed at firm (2)	Wage growth (3)
Instrument	0.220	0.234*	0.052
	(0.197)	(0.138)	(0.045)
Observations	30,761	30,761	30,761
R-squared	0.231	0.154	0.082

(c) Firms with rejected reorganization plans

Variable	Years firm survival (1)	Years employed at firm (2)	Wage growth (3)
Instrument	-0.033	0.002	0.058
	(0.280)	(0.157)	(0.047)
Observations	17,046	17,046	17,046
R-squared	0.182	0.148	0.11

Notes. The table reports estimates for the effect of reorganization on worker outcomes at firms that file for reorganization using the model from Equation 7. *Years firm survival* is the cumulative number of years the firm remains open. *Years employed at firm* is the number of years the worker stays in the firm. *Wage growth* is wage growth measured at the last year of the sample. In Panel A we include all firms. In Panel B we include only firms with accepted reorganization plans. In Panel C we include only firms with rejected reorganization plans.

Table A6: Labor marginal revenue-cost gaps and profitability

	Labor gap (1)	Labor gap (2)	P(labor gap> pre-filing) (3)	P(EBITDA coverage>1) (4)
Reorganization	-4.564	-1.054	-0.027	0.077
	(4.024)	(6.552)	(0.085)	(0.073)
Observations	31,575	31,575	47,807	47,807

Notes. This table shows the effect of reorganization on the characteristics of firms where workers are employed. In Columns (1)-(2) the dependent variable is the marginal labor revenue-cost gap in the last year of the sample, as defined in Appendix B. In Column (3) the dependent variable is an indicator that is equal to 1 if the labor gap at the current employer is larger than the labor gap before the filing. In Column (4) the dependent variable is an indicator equal to 1 for workers who are employed in firms with EBITDA coverage ratio above 1 (EBITDA greater than interest expense). In Column (2) we correct for selection into employment using the procedure from Appendix C. We display 2SLS estimates from Equation 5. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects. In Columns (1) and (3) and (4) we display clustered standard errors at the court-year level in parentheses. In Column (2) we obtain standard errors using a cluster bootstrap procedure. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A7: Additional labor outcomes

	Cumulative wage growth				Same	Same
Variable	Total (1)	Extensive margin (2)	Intensive margin (3)	Same occupation (4)	industry (2 digits) (5)	industry (5 digits) (6)
Reorganization	0.874***	0.440*	0.434***	0.248***	0.073	0.138*
	(0.262)	(0.230)	(0.161)	(0.069)	(0.076)	(0.078)
Observations	47,807	47,807	47,807	47,807	47,807	47,807

Notes. This table shows the effect of reorganization on additional labor outcomes. We display 2SLS estimates for Equation 5. In Columns (1) to (3) we estimate the effect of reorganization on cumulative wages after the filing. *Total* includes the whole wage effect. *Extensive margin* measures the number of years with recorded employment. *Intensive margin* measures total wages obtained by workers when employed. In Column (4) the dependent variable is an indicator equal to 1 if the last recorded occupation is equal to the occupation before the filing. In Columns (5) and (6) the dependent variable is an indicator variable equal to 1 if the worker remains employed in the same industry after the filing. Column (5) shows estimates for 2-digit industries and Column (6) for 5-digit industries. We use industry codes from *Classificação Portuguesa das Atividades Económicas* (CAE), which are harmonized with Europe-level NACE codes. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A8: Missing wages

	Wage growth (%)			
Variable	(1)	(2)	(3)	(4)
Reorganization	0.194**	0.136**	0.219***	0.193*
	(0.085)	(0.056)	(0.081)	(0.116)
Observations	47,807	47,807	33,332	33,332

Notes. This table uses different assumptions to replace missing values. In Column (1) we replace missing wages by 0. Following Walker (2013) and Graham et al. (2019), in Column (2) we bound estimates by replacing wages for workers with no jobs by wages recorded before the reorganization filing. This procedure is equivalent to the procedure used in Table 6 to obtain wage growth at the intensive margin. In Column (3) we drop workers with no job. In Column (4) we perform a selection correction of the wage process by following the procedure from Appendix C. In Columns (1)-(3) we cluster errors at the court-year level. In Column (4) we compute cluster bootstrap standard errors. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels.

Table A9: Employment transitions by wage quintile

(a) All employed workers

	Job = 1				
	Quintile 1 (1)	Quintile 2 (2)	Quintile 3 (3)	Quintile 4 (4)	Quintile 5 (5)
Reorganized	-0.072	-0.015	-0.033	0.070*	0.108***
	(0.071)	(0.036)	(0.038)	(0.038)	(0.026)
Observations	47,807	47,807	47,807	47,807	47,807
R-squared	0.052	0.045	0.029	0.04	0.126

(b) Workers who stay in the same firm

	Job same firm = 1				
	Quintile 1 (1)	Quintile 2 (2)	Quintile 3 (3)	Quintile 4 (4)	Quintile 5 (5)
Reorganized	0.056	0.094***	0.024	0.084***	0.034
	(0.040)	(0.030)	(0.030)	(0.022)	(0.021)
Observations	47,807	47,807	47,807	47,807	47,807
R-squared	0.064	0.024	0.057	0.029	0.069

(c) Workers who move to new employers

	Job new employer = 1				
	Quintile 1 (1)	Quintile 2 (2)	Quintile 3 (3)	Quintile 4 (4)	Quintile 5 (5)
Reorganized	-0.127**	-0.110***	-0.057*	-0.015	0.074***
	(0.058)	(0.041)	(0.033)	(0.032)	(0.023)
Observations	47,807	47,807	47,807	47,807	47,807
R-squared	0.03	0.02	0.022	0.041	0.079

Notes. The table reports estimates for the effect of reorganization on employment transitions by wage quintile using the model from Equation 5. We compute wage quintiles using a sample of employed workers earning at least the minimum wage. In Panel A the dependent variable is an indicator equal to 1 if the worker has a job with wage in quintile Q. In Panel B the dependent variable is an indicator equal to 1 if the worker stays in the firm that files for reorganization and has a job with wage in quintile Q. In Panel C the dependent variable is an indicator equal to 1 if the worker moves to a new employer and has a job with wage in quintile Q.

Table A10: Reorganization outcomes, labor market thickness and growth

(a) Labor market thickness

Variable	Wage growth (1)	Job (e.m.) (2)	Wage growth (i.m.) (3)	P(profitable firm) (4)	Lower skill cognitive (6)	Lower skill interp. (8)	Lower skill manual (7)	Occup-ation premium (9)	P(job quintile 5) (10)
Reorganization	0.544	0.321	0.224	0.131	-0.205**	-0.188	-0.152	0.044	0.109
	(1.023)	(0.749)	(0.287)	(0.410)	(0.103)	(0.121)	(0.099)	(0.065)	(0.121)
Reorganization*	-0.382	-0.284	-0.098	-0.129	-0.008	-0.013	-0.002	-0.024	-0.041
Thickness	(1.181)	(0.870)	(0.317)	(0.475)	(0.062)	(0.104)	(0.086)	(0.073)	(0.131)
Observations	47,807	47,807	47,807	47,807	47,807	47,807	47,807	47,807	47,807
R-squared	-0.434	-0.423	-0.018	-0.046	0.007	0.009	0.021	-0.062	0.044

(b) Labor market growth

Variable	Wage growth (1)	Job (e.m.) (2)	Wage growth (i.m.) (3)	P(profitable firm) (4)	Lower skill cognitive (6)	Lower skill interp. (8)	Lower skill manual (7)	Occup-ation premium (9)	P(job quintile 5) (10)
Reorganization	0.211**	0.069	0.142**	0.017	-0.193***	-0.181***	-0.143***	0.021*	0.079***
	(0.091)	(0.071)	(0.060)	(0.083)	(0.066)	(0.065)	(0.050)	(0.011)	(0.026)
Reorganization*	-0.595**	-0.411**	-0.184	-0.289	-0.248	-0.279*	0.007	-0.04	-0.138
Growth	(0.293)	(0.209)	(0.134)	(0.195)	(0.171)	(0.163)	(0.111)	(0.030)	(0.099)
Observations	47,807	47,807	47,807	47,807	47,807	47,807	47,807	47,807	47,807
R-squared	0.027	0.025	0.064	0.032	0.006	0.009	0.018	0.058	0.062

Notes. This table shows the relationship between the effect of reorganization on labor outcomes, labor market thickness, and labor market growth. *Reorganization* is an indicator variable equal to 1 for firms with accepted reorganization plans. *Thickness* and *Growth* are defined in Equations 10 and 11. We display 2SLS estimates from Equation 6. All specifications contain the controls used in Column (2) of Table 2, including court-year and industry fixed effects. Standard errors clustered at the court-year level are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

A The Portuguese bankruptcy system and data

A.1 The Portuguese bankruptcy system

Recent history of the Portuguese bankruptcy code We provide a brief history of the Portuguese bankruptcy system between 2004 and 2020 using Kalil (2017), Vasconcelos (2017), and Simões (2019) as references, which also discuss earlier versions of the Portuguese bankruptcy system, beginning with its origins in Roman Law.

Portugal is a civil law country and most of the legal texts that regulate bankruptcy are codified in the Portuguese bankruptcy code, *Código da Insolvência e da Recuperação de Empresas* (CIRE), which covers both firms and households. The first version of the current bankruptcy code was introduced by Decree-Law *Dec. Lei n.º 53/2004*. This law was based on the German insolvency system (Insolvenzordnung). The focus of the law was asset liquidation and creditor reimbursement. This system differed from the US bankruptcy system, which gave priority to debtor recovery in the case of both firms (mainly through Chapter 11) and househods (mainly through Chapter 13). The Portuguese system discouraged firm recovery in bankruptcy, especially when promoted by debtors. Debtor in possession (i.e., bankrupt firms being controlled by the debtor) had to be approved by the judge and by the entity that filed the bankruptcy petition (art. 224º of CIRE). Debtors filing for bankruptcy faced the risk of having no opportunity to reorganize. Automatic stay provisions (freezing of creditor claims) were very limited. Trustees could start closing establishments (art. 157º of CIRE) and liquidating some assets (arts. 158º and 254º of CIRE) immediately after the first hearing. Otherwise, trustees could start liquidating assets after the first meeting with creditors, unless there was a motion promoted by a majority of creditors opposing liquidation (art. 158º of CIRE). Debtors could propose recovery plans only once. Additional proposals would have to be pre-approved by the trustee (art. 207º of CIRE). Reorganization in bankruptcy in this system was rare. Fewer than 1% of the firms that filed for bankruptcy were reorganized and survived (Ministério da Economia e do Emprego (2012)).

With the implementation of the Law *Lei n.º 16/2012* in May 2012, Portugal added a separate

chapter on reorganization to the bankruptcy code. This new reorganization system was based on Chapter 11 from the US bankruptcy code and shared many characteristics of US reorganization law. In this system debtors had the right to file for reorganization. They had a 3-month period to negotiate a bankruptcy plan with creditors. During this period, they retained possession of the business and were protected from creditor claims by automatic stay provisions. Reorganization plans had to be approved by a majority of creditors and by a judge. The bankruptcy code underwent some additional changes between 2012 and 2017. In 2015 the Decree-Law *Dec. Lei n.º 26/2015* introduced voting rules that made it easier to approve reorganization plans. In 2017 Decree-Law *Dec. Lei n.º 79/2017* created a separate reorganization system for individuals, which allowed the establishment of separate jurisprudence for individuals and firms. This decree-law also required the certification of reorganization petitions by an authorized accountant. This requirement had the purpose of reducing petitions from economically non-viable firms.

The Portuguese reorganization system In this section we expand the description of the Portuguese reorganization system provided in Section 4. This description reflects versions of the Portuguese bankruptcy code and related jurisprudence that affect firms filing for reorganization between 2012 and 2016. While individuals may file for reorganization in bankruptcy, we focus on the rules that apply to firms.

Figure 1 depicts the Portuguese reorganization system. The filing is initiated by the debtor with the support of at least one creditor (art. 17.º-C of CIRE). Firms may file when they face a "difficult economic situation" or "imminent insolvency" (art. 17.º-A of CIRE). Firms are "insolvent" when they cannot repay overdue debt or their assets are considerably greater than liabilities (art. 3.º of CIRE).

Firms should file for reorganization where they are headquartered or have their main center of interests (art. 7.º of CIRE), i.e. the place from where the business is administered. The random allocation of cases to judges in trial courts (*tribunais de primeira instância*) is stipulated by the Portuguese code of civil procedure, *Código do Processo Civil* (CPC), (art.º 204 of CPC), and regulated

by Ordinance *Portaria n.º 280/2013*[7]. Cases are distributed automatically twice per day.

In the first hearing after the filing the judge of the case starts the reorganization process and makes it public. Firms may choose a trustee when they file for reorganization (art.º 32 of CIRE). According to the 2016 statistics provided by the Portuguese association of trustees (*Comissão para o Acompanhamento dos Auxiliares de Justiça (2016)*), approximately 74% of the firms exert this option. Judges pick a trustee when firms do not choose one. From March 2013 on the choice of the judge should be random (art.º 13 of Law *Lei n.º 22/2013*).

After the first hearing creditors have a 20-day period to claim debts. Thereafter firms have two months to negotiate a reorganization plan with creditors. Firms may request a one-month extension of the deadline, which is given automatically (art.º 17-D of CIRE).

At the end of the negotiation period firms and creditors reach an agreement when at least one third of the votes are cast, two thirds of the votes cast are for the approval of the plan, and one half of the votes cast are from non-subordinated creditors. Votes are counted in dollar terms (art.º 212 of CIRE). Since 2015, art.º 17-F of CIRE (changed by Decree-law *Decreto-Lei n.º 53/2004*) plans are also approved when one half of all votes (cast and non-cast) are for approval and at least one half of the votes cast come from non-subordinated creditors.

When creditors approve a reorganization plan the judge may accept or reject it. If the judge accepts the plan, firms are reorganized (art.º 17-F of CIRE). The judge may reject a reorganization when procedural rules, deadlines, or norms related to the content of the plan are not respected (art.º 215 of CIRE). The judge may also reject a plan at the request of a creditor. The plan is rejected if the creditor is predictably worse off with the plan than without the plan, or if the plan pays some creditor more than the nominal debt value (art.º 216 of CIRE). These rules may not apply in specific situations described in art.º 216 of CIRE.

The reorganization process is closed when firms are not reorganized (art.º 17-G of CIRE). After the process is closed the bankruptcy case might be dismissed or attached to a liquidation filing

[7]Ordinance *Portaria n.º 280/2013* was implemented on 1 September 2013 and replaced art.º 16 of Ordinance *Portaria n.º 114/2008*, implemented in 2008. Neverhteless, the process distribution system is similar in the two ordinances.

(i.e., a filing under the original bankruptcy system set up by Decree-law *Dec. Lei n.º 53/2004*). The bankruptcy manager submits a liquidation filing if the firm is "insolvent" at the end of the reorganization process. Aside from bankruptcy managers, the debtor and creditors may also submit a subsequent liquidation filing.

A.2 Reorganization data collection and treatment

Data collection We collect data from Citius, a public repository of bankruptcy documents maintained by the Portuguese ministry of justice. The repository can be accessed through https://www.citius.mj.pt/portal/consultas/consultascire.aspx.

We collect information for cases that were filed between May 2012 (inception of the reorganization system) and December 2016. For each reorganization case we collect all records (*Atos*) dated between the filing date and December 2018. Figure A1 is an example of one of these records. Records usually contain the following elements: 1) court name (*Tribunal*); 2) record type (*Ato*); 3) process name (*Processo*)[8]; type of case (*Espécie*), e.g. reorganization; 4) record date (*Data*); 5) original case filing date (*Data de propositura da acção*); 6) debtor designation and unique tax ID (*Requerente* or *Devedor* or *Insolvente*); 7) Trustee ID (*Administrador Insolvência*); 8) Creditor names (*Credor*) and tax IDs (*NIF/NIPC*).

Some records have an associated PDF file with additional information (under *Ver mais* from Figure A1). Figure A2 shows the PDF file associated with the record from Figure A1. We retrieve the judge identification (*Juiz de Direito*) from PDF files.

Data treatment. We create a dataset of reorganization cases using the records collected from Citius. This dataset has one entry for each case and contains the following variables:

- Case ID: case identification number obtained from field *Processo* in Figure A1.
- Tax ID: tax ID of the debtor obtained from field *NIF/NIPC*. Some cases do not have an associated tax ID. In these situations we use the reported company name to search for the

[8] From September 2014 onwards some courts have sub-units that deal with specific types of case. The field *Processo* also contains the name of these sub-units.

tax ID. Some documents have incorrect tax IDs, often because debtors show up as creditors and vice-versa. We verify tax IDs using *Informação Empresarial Simplificada* (a dataset with financial statements for non-financial corporations available at Banco de Portugal) and *SPAI* (a firm register kept by Banco de Portugal containing data on firms' sector of activity). We create a list of cases containing the filings of the tax IDs that are the largest in terms of assets and filings from firms that operate in the financial sector. We check whether these firms actually filed for reorganization or are just creditors of the filer. Whenever we find an incorrect tax ID we search for the name of the debtor to obtain the correct one. We use the tax ID to merge the bankruptcy dataset with the employer-employee dataset and firm financial statements described in Section 5.

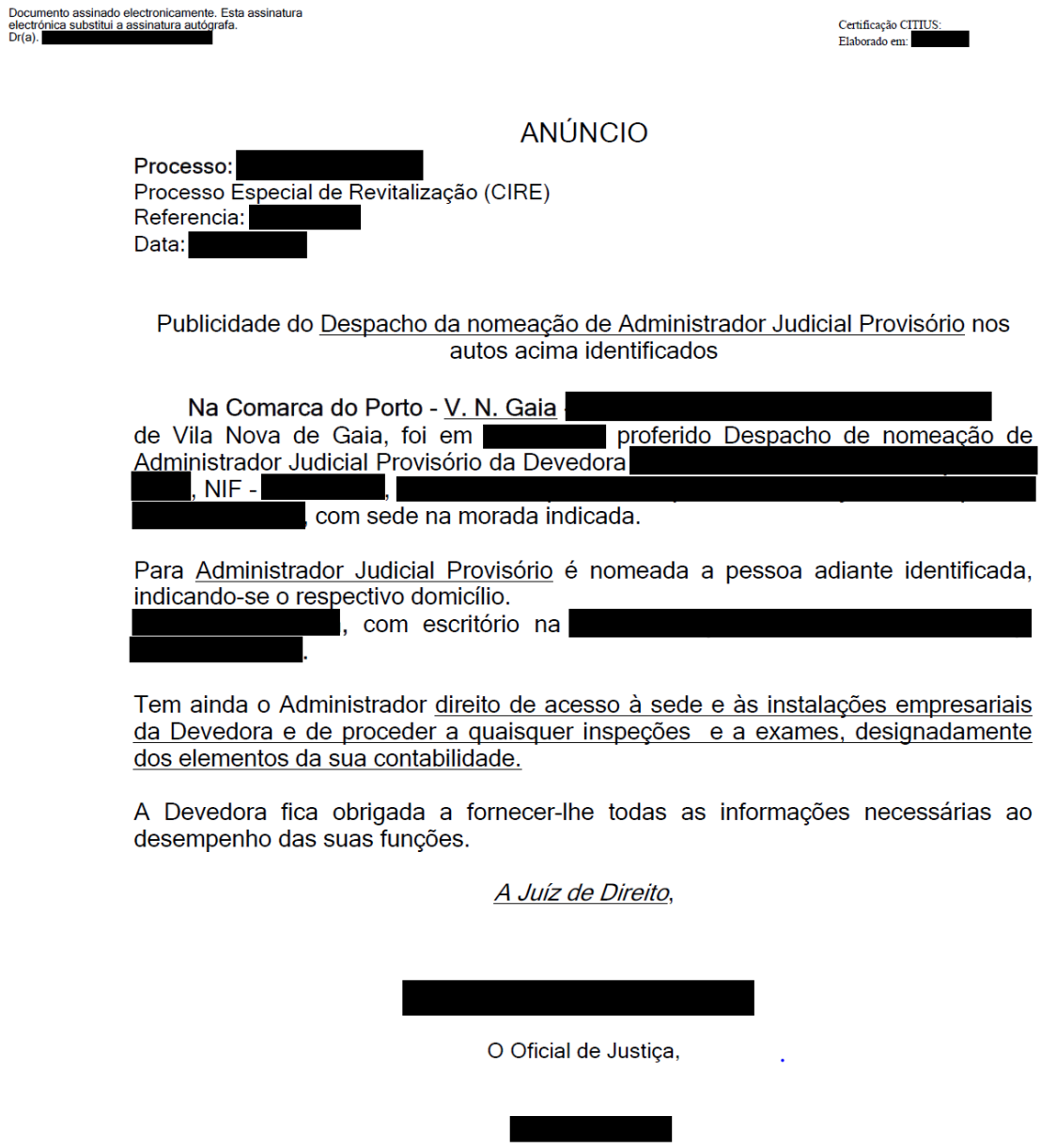
- Training sample: indicator variable that is equal to 1 for entities outside the scope of the paper. The case is outside the scope of the paper if it does not satisfy at least one of these conditions: 1) there is financial statement data for the debtor in the BDCA dataset described in Section 5; 2) the firm has more than one employee in *Quadros de Pessoal* (i.e., not a firm without employees or independent workers). We do not include cases from the training sample in the analysis but use them to obtain the instrumental variable.
- Court ID: court identification number generated from court names reported in field *Tribunal* from Figure A1. Portuguese courts are organized in districts (*comarcas*). In 2014 Decree-law *Dec. lei n.º 49/2014* reformed the Portuguese court map. This law changed court names, extinguished some courts, reallocated other courts to new districts, and created court sub-units to handle special types of case. The court ID variable reflects the last name of each court. We obtain this name by establishing a correspondence between court names before and after the reform. We create a list of all cases that are transferred between old and new court names. For each old court we associate the new court name that has the most transfers. In Table A1 we create alternative court fixed effects using the original court names from the field *Tribunal* in Figure A1, adding the sub-unit from the field *Processo* to the court name whenever the case is assigned to one of these sub-units.

- Filing date: date when the case was filed by the debtor, reported as *Data de propositura da ação* in Figure A1.
- Year: filing year, generated from the filing date.
- Judge ID: judge identification number generated from judge names reported in PDF files (Figure A2). Judges are allocated to courts annually by the institution that ensures the self-management of the judiciary (*Conselho Superior de Magistratura*, CSM). The allocation process is regulated by Decree-law *Dec. lei n.º 49/2014* since September 2014. Previously the process was regulated by *Law Lei n.º 3/99*. Some documents do not have a judge name assigned to it. In such cases we order documents by date and impute judge names from the previous document. Some cases are allocated to more than one judge. In the main analysis we create a separate ID for these situations within each court-year pair. Alternatively, in Table A1 we use the ID available in the most recent document.
- Case outcome: dummy variable that is equal to 1 for cases that end with an accepted reorganization plan. We create this variable using *Ato* from Figure A1.

Figure A1: Example of court record from Citius

Notes. This figure depicts a court record associated with a reorganization case. The record was extracted from Citius, a public repository of Portuguese bankruptcy documents.

Figure A2: Example of PDF file from Citius



Documento assinado electronicamente. Esta assinatura electrónica substitui a assinatura autógrafa.
Dr(a). [redacted]

Certificação CITIUS:
Elaborado em: [redacted]

ANÚNCIO

Processo: [redacted]
Processo Especial de Revitalização (CIRE)
Referencia: [redacted]
Data: [redacted]

Publicidade do Despacho da nomeação de Administrador Judicial Provisório nos autos acima identificados

Na Comarca do Porto - V. N. Gaia [redacted] de Vila Nova de Gaia, foi em [redacted] proferido Despacho de nomeação de Administrador Judicial Provisório da Devedora [redacted], NIF - [redacted], [redacted], com sede na morada indicada.

Para Administrador Judicial Provisório é nomeada a pessoa adiante identificada, indicando-se o respectivo domicílio.
[redacted], com escritório na [redacted].

Tem ainda o Administrador direito de acesso à sede e às instalações empresariais da Devedora e de proceder a quaisquer inspeções e a exames, designadamente dos elementos da sua contabilidade.

A Devedora fica obrigada a fornecer-lhe todas as informações necessárias ao desempenho das suas funções.

A Juíz de Direito,

[redacted]

O Oficial de Justiça,

[redacted]

Notes. This figure depicts a PDF file of a court record associated with a reorganization case. The PDF file was extracted from Citius, a public repository of Portuguese bankruptcy documents.

B Estimating production functions

In order to compute the marginal revenue product of labor used in Equation 1, we need to estimate firms' output elasticity of labor θ_L. We estimate the following second-order translog revenue production function at the firm level:

$$q_{i,t} = (\omega_{i,t} + \epsilon_{i,t}) + f(k_{i,t}, l_{i,t}, \gamma) \tag{12}$$

with:

$$f(k_{i,t}, l_{i,t}, \gamma) = \gamma_K k_{it} + \gamma_L l_{i,t} + \gamma_M m_{i,t} + \gamma_{KK} k_{i,t}^2 + \gamma_{KL} k_{i,t} l_{i,t} + \gamma_{KM} k_{i,t} m_{i,t} + \gamma_{LL} l_{i,t}^2 + \gamma_{LM} l_{i,t} m_{i,t} + \gamma_{MM} m_{i,t}^2 \tag{13}$$

where q_{it} is revenue, w_{it} is the component of productivity observed by the firm when it makes the choice of inputs, ϵ_{it} is the idiosyncratic component of productivity, l_{it} is log labor, k_{it} is log capital, and m_{it} is log intermediate inputs. We estimate production functions separately for each 2-digit industry.

Our baseline estimates follow the estimation procedure from Lenzu and Manaresi (2019) and Gandhi et al. (2020). We deflate nominal variables using the procedure from Blattner et al. (2019). We retrieve price indices for Portugal from Eurostat. We obtain real output and intermediate inputs by deflating variables with 2-digit or 3-digit industry price indices. In industries without price indices we use the agricultural price index, service price index, or consumer price index, depending on the industry. We deflate capital using the capital goods price index.

We estimate capital using the deflated book value of capital. In unreported results we compute capital with the perpetual inventory method, starting with the stock of fixed assets from 2008. For subsequent years we update capital using the equation:

$$K_{it} = (\delta_{i,t} K_{i,t-1} + \frac{I_{i,t}}{def_t}) \tag{14}$$

where $\delta_{i,t}$ is the depreciation rate, $K_{i,t-1}$ is deflated capital from the previous period, $I_{i,t}$ is CAPEX, and def_t is the capital goods deflator.

We estimate output elasticities using the two-stage estimation procedure from Gandhi et al. (2020). Inputs might be pre-determined (chosen at $t-1$), or flexible (chosen at t), dynamic (value at t is affected by value at $t-1$), or static (value at t is not affected by value at $t-1$). Capital is pre-determined and dynamic, labor is flexible and dynamic, and intermediate goods are flexible and static. We use capital as an instrument for itself, and labor in period $t-1$ as an instrument for labor in period t.

Table A11 provides estimated output elasticities. Our estimates seem to be reasonable, as the average sum of the estimates is close to 1, suggesting constant returns to scale.

Table A11: Output elasticity estimates

	All firms (1)	Reorganization (2)	Liquidation (3)
$theta_k$	0.091 (0.071)	0.095 (0.073)	0.085 (0.068)
$theta_l$	0.441 (0.162)	0.445 (0.162)	0.434 (0.162)
$theta_m$	0.542 (0.174)	0.535 (0.172)	0.552 (0.176)
Sum	1.074 (0.079)	1.075 (0.080)	1.072 (0.077)

Notes. The table shows production function elasticity estimates. θ^K, θ^L, θ^M stand for capital, labor, and intermediate good elasticity, respectively. Sum is the sum of the three elasticity estimates. The procedure we use to estimate these parameters is described in the text.

C Selection into employment

We use the two-step correction method from Heckman (1979) to correct for selection into employment. We estimate the following probit selection equation:

$$selection\ dummy_{e,\tau} = \beta Z_{i,j,c,t} + \lambda I_{e,t-1}^{\geq 45yo} + \gamma X_{e,i,t-1} + \delta_{c,t} + \epsilon_{e,\tau} \tag{15}$$

where $selection\ dummy_{e,\tau}$ is an indicator variable equal to 1 for workers who are selected into employment, and $I_{e,t-1}^{\geq 45yo}$ is an indicator variable that is equal to 1 for workers who are at least 45 years old. We use $I_{e,t-1}^{\geq 45yo}$ as an instrument in the selection equation because workers receive considerably more advantageous unemployment benefits if they are at least 45 years old. For identification, we assume that after controlling for age and tenure at the firm in $X_{e,i,t-1}$, being over 45 years old should not affect labor outcomes for employed workers. Empirically, we find a strong negative relationship (significant at the 1% level) between being at least 45 years old and having a job contract, but no statistically significant relationship between being at least 45 years old and wage growth for workers with jobs, conditional on control variables used throughout the analysis.

We use the following second-stage equation to estimate the effect of reorganization on labor outcomes:

$$Y_{e,\tau} = \alpha + \beta.\widehat{Reorganization}_{i,t} + \gamma X_{e,i,t-1} + \delta_{c,t} + IMR_{e,\tau} + \epsilon_{e,\tau} \tag{16}$$

where $IMR_{e,\tau}$ is the Inverse Mills Ratio computed using estimates from Equation 15. The remaining variables come from Equation 5. We compute cluster bootstrap standard errors at the court-year level to account for the fact that the Inverse Mills Ratio is estimated.

D Occupation premium

We wish to estimate the wage premium associated with occupations. However, omitted variable bias may affect these estimates because occupational choice is correlated with other factors that also influence wages.

Starting with the seminal work of Abowd et al. (1999), many papers empirically investigate various explanations for wage differences between workers. For example, wages may vary because of intrinsic worker and employer characteristics (Abowd et al. (1999)), the quality of worker-firm matches (Card et al. (2013)), or age and job ladder effects (Burdett et al. (2020)).

Taking into account factors from the literature that could influence wage determination, we adapt the empirical model of Card et al. (2013) and estimate the following wage equation for workers using data from *Quadros de Pessoal* between 2010 and 2018:

$$y_{i,t} = \alpha_i + \psi_{J(i,t)} + \phi_{W(i,t)} + x'_{i,t}\beta + r_{i,t} \tag{17}$$

$y_{i,t}$ is the log wage, α_i is the person fixed effect, $\psi_{J(i,t)}$ is the firm fixed effect, $\phi_{W(i,t)}$ is the occupation-year fixed effect[9], and $x'_{i,t}$ is a vector of time-varying worker characteristics that includes quadratic terms for age and tenure at the firm.

Adapting Card et al. (2013), we assume that the error term $r_{i,t}$ can be decomposed in three separate random effects: a match component, a unit root component, and a transitory component. The match component $\eta_{i,j,w}$ is the idiosyncratic wage premium earned by worker i at firm j and occupation-year w relative to the baseline wage $\alpha_i + \psi_j + \phi_w$. We assume that $\eta_{i,j,w}$ has mean 0 within each i, j, w pair. $\zeta_{i,t}$ is a unit root with mean zero for each worker i. $\epsilon_{i,t}$ is the transitory component with mean zero for each worker. We rewrite $r_{i,t}$ as the sum of the three components:

$$r_{i,t} = \eta_{i,j,w} + \zeta_{i,t} + \epsilon_{i,t} \tag{18}$$

[9]We include occupation-year fixed effects to guarantee that the occupation premium is not mismeasured because of occupations that become relatively less valued over time (e.g., Deming (2017))

We use OLS to estimate Equation 17. Card et al. (2013) discuss at length the conditions that must hold for OLS to identify the parameters in Equation 17. In our version of the model it is key that combinations of firm and occupation-year indicators are orthogonal to the error term. For that condition to hold, it is sufficient to assume a strict exogeneity condition with respect to the error term:

$$P(J(i,t) = j \wedge W(i,t) = w|r) = P(J(i,t) = j \wedge W(i,t) = w) = G_{jwt}(\alpha_i, \psi_1, ..., \psi_J, \phi_1, ..., \phi_W), \forall i, t \tag{19}$$

where the employment probability functions G_{jwt} sum to 1 for every worker in every period. Card et al. (2013) discuss three forms of endogenous job changes that violate the condition in Equation 19. First, workers may select jobs according to their match component. If this happens, trend-adjusted wage gains for workers who move from a firm-occupation-year pair to another should be considerably different from losses for workers who make the opposite move. As Table A12 shows, we do not find much empirical evidence of such behavior. We track wages for workers in the sample who transition to a new firm and/or occupation and have observable wage data between two years before and two years after the transition. We classify transitions by whether the average wage of other workers in the same firm or occupation is below or above the median. Wage gains and losses are relatively symmetric for job transitions in opposite directions.

Second, a drift in the unit root component $\zeta_{i,t}$ may predict job changes. This pattern may overestimate the effect of occupations on wages if wages rise more when workers move to higher-paying occupations than to lower-paying occupations. We do not observe such systematic trends in Table A12.

Third, the transitory error may be systematically associated with job changes to higher or lower wage firms and occupations. As Table A12 shows, the evidence does not suggest a systematic relationship between transitory wage fluctuations and job changes to new firms and occupations.

Table A12: Mean log wages before and after job change, by occupation and firm quantile of co-workers' average wage

Quantiles					Log wage	Log wage	Log wage	Log wage	Change	Change
Firm before	Occupation before	Firm after	Occupation after	Obs. (1)	$t-1$ (2)	t (3)	$t+1$ (4)	$t+2$ (5)	(raw) (6)	(adjusted) (7)
1	1	1	1	198,566	6.291	6.323	6.354	6.395	0.104	0.000
1	1	1	2	31,982	6.443	6.479	6.569	6.620	0.176	0.072
1	1	2	1	32,362	6.410	6.467	6.670	6.732	0.322	0.218
1	1	2	2	17,153	6.402	6.464	6.809	6.890	0.488	0.384
1	2	1	1	26,960	6.472	6.496	6.478	6.529	0.056	-0.048
1	2	1	2	51,606	6.626	6.651	6.671	6.705	0.079	0.000
1	2	2	1	7,588	6.534	6.580	6.725	6.795	0.260	0.181
1	2	2	2	29,504	6.685	6.723	6.959	7.023	0.338	0.259
2	1	1	1	18,624	6.642	6.672	6.451	6.514	-0.129	-0.215
2	1	1	2	4,976	6.727	6.759	6.584	6.654	-0.072	-0.159
2	1	2	1	46,072	6.737	6.766	6.788	6.823	0.086	0.000
2	1	2	2	29,785	6.900	6.938	7.004	7.059	0.159	0.073
2	2	1	1	9,492	6.814	6.855	6.445	6.531	-0.283	-0.369
2	2	1	2	19,873	7.006	7.035	6.734	6.785	-0.221	-0.307
2	2	2	1	23,179	6.972	7.004	6.981	7.034	0.062	-0.025
2	2	2	2	224,818	7.334	7.372	7.398	7.439	0.105	0.000

Notes. This table reports the average logarithm of wages for workers in *Quadros de Pessoal* who move to a new job. Workers move to a new job when they switch to a new firm or to a new occupation. Workers are in quantile 1 (2) if the average wage of co-workers in the same occupation or firm is below (above) the median.