Corporate Reorganization as Labor Insurance in Bankruptcy

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Abstract

How does corporate reorganization affect labor reallocation in bankruptcy? In this paper, we provide evidence that reorganization is an important source of labor insurance against bankruptcy shocks, including for workers who eventually move to other firms. We measure the effect of reorganization on labor outcomes using data from Portugal and the random allocation of corporate reorganization cases to judges, which creates exogenous variation in the probability of reorganization. Reorganized firms only keep about 20% of their workforce five years after reorganization. We also find no evidence that reorganization affects the reallocation of workers to efficient or profitable firms. However, reorganization has a positive and persistent effect on wages. In the short term (first year after reorganization), workers are more likely to have jobs. In the longer term (subsequent five years), workers have higher paying jobs. Consistent with the literature on the scarring effect of negative production shocks, reorganization reduces labor transitions to less skill intensive occupations with lower wage premiums. Finally, we show that reorganization provides labor insurance to workers who move to new employers. Reorganization reduces the probability that workers move to low-paying jobs and increases the probability that they find high-paying jobs in new employers.

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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 existing 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 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. (2019a), Bernstein et al. (2019b), Araujo et al. (2020)).

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 persistent costs of job loss, which may come from different sources (Neal (1995), Barlevy (2002), Kambourov and Manovskii (2009), Raposo et al. (2019), Burdett et al. (2020)). At the extensive margin, workers who lose their job contracts may not find a new job easily in illiquid labor markets. At the intensive margin, workers may get lower-paying jobs because of foregone human capital accumulation, sorting into low-paying firms, or worse worker-firm matches. 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.

We test this explanation for the effect of reorganization on labor outcomes using data from Portugal and the random allocation of corporate reorganization cases to judges who have the power to accept or reject reorganization plans. We use the Portuguese setting for three reasons. First, 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 whether firms are reorganized or not. Second, all firms in Portugal must report financial statements to the government annually. We use this data to estimate firm production functions, measure firm profitability and productivity, and test the effect of reorganization on labor reallocation to more efficient firms. Finally, we link this data to a rich employer-employee dataset that contains both wage data and detailed job descriptors (Portugal (2020)). We use this information to investigate the sources of wage variation that may explain the effect of reorganization on wages.

This paper has three main empirical findings. First, there is extensive labor reallocation even in reorganized firms. Five years after filing, the firm survival rate is almost 50 p.p. higher for reorganized firms than for firms with rejected plans. However, only about 20% of all workers stay in reorganized firms five years after reorganization. We identify firm production functions using financial statements (Lenzu and Manaresi (2019), Gandhi et al. (2020)) and test the effect of reorganization on the quality of firms where workers are employed. We do not find evidence that reorganization moves workers to firms with higher or lower marginal labor revenue-cost gaps. Additionally, reorganization does not significantly affect the transition of workers to profitable firms.

Second, reorganization has a positive and persistent effect on labor outcomes. Reorganization increases wages by 16 p.p. in the short term (first year after reorganization) and 17 p.p. in the longer term (up to five years after reorganization). In the short term, the effect of reorganization on wages is explained by lower employment rates. In the longer term, the difference in employment between workers from firms with accepted and rejected reorganization plans is small but the effect on wages persists.

We establish a relationship between reorganization and the literature on resource reallocation after firm failure. Liquidations affect resource reallocation more strongly when markets are thinner (Gavazza (2011), Bernstein et al. (2019b)). We show that reorganization has a stronger positive effect on wages and employment for workers who face thinner labor markets. Additionally, liquidations may have a scarring effect on labor outcomes by persistently reallocating workers to low-quality jobs (Barlevy (2002)). We provide evidence that reorganization reduces this deterioration in job quality. 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 reorganization has a positive 2 p.p. effect on the wage premium associated with occupations.

Third, we investigate the relationship between reorganization and labor outcomes for workers who stay in reorganized firms and for workers who leave for other jobs. We examine the effect of reorganization on the amount of time that workers have to search for a new job. The literature suggests that workers are more likely to find higher-paid jobs when they search for work while still working for another employer rather than while unemployed (e.g., Rogerson et al. (2005), Lazear (2009)). Reorganization adds one year to the average time it takes to leave a firm that files for reorganization. It also results in a 20 p.p. decrease in the probability that workers will leave in the first year after the filing and a 6 p.p. increase in the probability that they will leave after four or more years.

We examine the relationship between judge leniency and worker outcomes for the subset of workers from reorganized firms who find work with new employers. Consistent with reorganization improving labor outcomes for workers who leave for new employers, judge leniency is not correlated with wage growth for these workers before reorganization but it is positively correlated afterward. 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 wage distribution by 6 p.p.

This paper 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), Araujo et al. (2020)). 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.

We establish a relationship between the literature on optimal bankruptcy design and the literature on the costs of job loss. Rogerson et al. (2005) review the theoretical literature on search frictions in labor markets. 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 costs faced by workers who want to switch jobs. Burdett et al. (2020) show that long-term wage losses after job loss can be explained by foregone human capital acquired on the job. In this paper, we establish that corporate reorganization provides labor with 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 both for workers who stay in reorganized firms and for workers who leave for new employers.

The existing literature shows that displacement through mass layoffs causes long-term wage losses (Jacobson et al. (1993)), particularly 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 the 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 job functions.

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

2 The Portuguese bankruptcy system

Across most legal systems, bankruptcy is regulated by two procedures: reorganization and liquidation (Djankov et al. (2008)). These two legal systems 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 let businesses remain open.

Until 2012, the Portuguese bankruptcy code did not have a separate codified reorganization system. Many distressed firms would rely on private workouts to restructure. In 2012, the bankruptcy code was amended to include a new chapter on reorganization.

Figure 1 depicts a diagram of the Portuguese corporate reorganization system in the bankruptcy code. We also provide a more detailed description of the Portuguese bankruptcy code in Appendix A. Under the new bankruptcy system, firms can file petitions where they are headquartered or engage in most of their business. The subsequent legal framework is simple. 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. The random assignment of reorganization cases is a key component of our identification strategy. Judges only intervene 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 off on account of reorganization. After reorganization is rejected, the reorganization case may be converted into a liquidation case. Liquidation might be requested by the debtor, the bankruptcy manager, or creditors. This subsequent liquidation filing should be added to the court's distribution schedule as a separate case.

[Figure 1]

3 Data

3.1 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 the data, data coverage, and variable definitions.

The dataset covers a total of 6,619 cases but we only use 2,554 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. Our sample contains filings between 2012 (when reorganization was introduced) and 2016.

3.2 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 all firms operating in Portugal. We merge the firm dataset with the employer-employee matched dataset using the firm tax ID number.

3.3 Worker-Level Data

We use *Quadros de Pessoal*, an employer-employee matched dataset maintained by the Portuguese Ministry of Social Security. 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 firms and individuals misreporting 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 47,632 workers employed at firms that filed for reorganization. We follow common practice in the literature (Couch and Placzek (2010)) and exclude part-time workers, foreign nationals, and employees under 23 and over 50 years of age, who are less attached to firms or are more likely to continue their education or retire early. We detect some reporting differences between datasets. First, some firms do not submit data to *Quadros de Pessoal* in the last period before the filing, apparently because they are liquidated in the year of the filing. Additionally, we detect some cases in which firms remain operational but fail to report firm or worker data. We perform two actions to address these reporting differences. Whenever there is available data, we replace worker data from the year before the filing by data from the previous year. Additionally, we address reporting gaps by replacing missing data by data from adjacent years. Alternatively, we do not any replacements and drop observations with missing data. We obtain similar estimates.

3.4 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 in 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 social 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.

3.5 Descriptive Statistics

Table 1 shows descriptive statistics for the final sample, which includes 2,554 firms and 47,632 workers. We winsorize ratios and estimated quantities (e.g., labor gap) at the 5% level . For 58% of the firms and 63% of the workers, reorganization ends with an accepted plan.

[Table 1]

The first section of the table describes firms. The marginal revenue product-cost gap of labor 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 €14 thousand , higher than the average for Portuguese firms (€8 thousand). Firms with accepted reorganization plans are larger, more profitable, and have considerably higher book equity ratios than firms with rejected reorganization plans.

The second part of the table shows descriptive statistics for workers. Workers employed at firms 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 A1 we depict the distribution of workers and firms that file for reorganization by industry.

In Section 5, we establish a relationship between the intensity of search frictions in labor markets and the effect of reorganization on labor outcomes. We follow the literature (e.g., Gavazza (2011), Bernstein et al. (2019b)) and use market thickness to estimate the intensity of search frictions, which are less intense in thicker labor markets. We define labor market thickness as $Thickness_{ot} = \frac{workers_{ot}}{workers_t}$, where $workers_{ot}$ is the number of workers in occupation o at time t and $workers_t$ is the total number of workers at time t.

4 Empirical strategy

Our baseline specification of interest is:

$$Y_{e,i,t+k} = \alpha + \beta Reorganization_{i,t} + \gamma X_{e,i,t} + \epsilon_{e,i,t+k} \tag{2}$$

e is the worker identifier, i is the firm identifier, t is the year of the reorganization filing, and k measures the number of years post-filing (up to 5 years). Following Bernstein et al. (2019b), we categorize the year of the filing as the first year post-filing. $Y_{e,i,t+k}$ is the outcome variable of interest. $Reorganization_{i,t}$ is equal to 1 if firm i reorganizes, and $X_{e,i,t}$ is a vector of firm and worker-level controls. We want to estimate β, the effect of reorganization on worker outcomes.

Selection might affect the estimation of β in equation 2 because reorganization is a choice of firms, creditors and judges. Bias might run in both directions. Some firms might not be reorganized because they face worse 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 later. 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 significant leeway to reject reorganization plans (see Appendix A). As judges' interpretations of the law vary significantly, we use the tendency to accept reorganization plans to instrument for reorganization.

Our final sample consists of cases filed in courts that have at least 25 filings. On average, in each year cases are distributed across 26 court and 143 judge identifiers.[1] 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.

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

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

$Reorganization_{i,t}$ is a dummy equal to 1 for firms with accepted reorganization plans, $Z_{i,j,c,t}$ is

[1] Some courts were renamed or merged in 2014 after a court reform. We use the last denomination of each court. Appendix A provides more detail about variable definitions. In Table A2 we repeat the analysis using original court names.

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 it has been done previously in the literature (e.g., Dobbie and Song (2015), Gupta et al. (2016)):

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

Figure 2 depicts the distribution of $Z_{i,j,c,t}$. The first term of $Z_{i,j,c,t}$ is the average of $Reorganization_{i,t}$ 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 is not enough data to compute it. Alternatively, in Table A2 we drop cases for which there is not enough data.

[Figure 2]

The second stage equation is given by the following expression:

$$Y_{e,i,t+k} = \alpha + \beta \widehat{Reorganization}_{i,t} + \gamma X_{e,i,t} + \delta_{c,t} + \epsilon_{e,i,t+k} \quad (5)$$

where $\widehat{Reorganization}_{i,t}$ 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 only measures 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 thickness. We estimate the second stage equation:

$$\begin{aligned} Y_{e,i,t+k} = \alpha + \beta_1.\widehat{Reorganization_{i,t}} + \beta_2 \widehat{Reorganization_{i,t}} \times Thickness_{e,t} + \\ \beta_3 Thickness_{e,t} + \gamma X_{e,i,t} + \delta_{c,t} + \epsilon_{e,i,t+k} \end{aligned} \tag{6}$$

$\widehat{Reorganization_{i,t}}$ and $\widehat{Reorganization_{i,t}} \times Thickness_{e,t}$ are predicted using $Z_{i,j,c,t}$ (judge leniency) and $Z_{i,j,c,t} \times Thickness_{e,t}$ as instrumental variables.

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

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

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

4.1 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 p.p. more lenient judge is associated with a 0.402 p.p. 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. In Section 6 we use alternative specifications for the first stage regression. For example, we compute the instrument restricting the training sample to cases that precede the filing, estimate the first stage regression with unit sample weights, and use bootstrap standard errors to account for the fact that the instrumental variable is estimated. In these cases, first and second stage coefficients are similar to the main specification.

We analyze the model's identification assumptions. The instrument F-statistic at the end of column (2) in Table 2 is 38.02 , 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 2, judges are randomly assigned to bankruptcy cases and only intervene in reorganization procedures to approve or reject plans. Under reorganization, judges end their oversight over cases after approving or rejecting plans.

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:

$$Z_{i,j,c,t} = \alpha + \theta . X_{e,i,t} + \varepsilon_{e,i,t} \tag{8}$$

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

Table 3 shows estimates for Equation 8. 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. At the end of column (1) we also provide 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 A3 we repeat the analysis for time-changing variables in years -2 and -3. We do not find evidence of pre-trends.

[Table 3]

Judge leniency might be associated with improved labor outcomes because the allocation of reorganization cases to more lenient judges may improve improve the quality of reorganization plans. In this case, judge leniency would affect worker outcomes by improving reorganization plans and not by increasing the probability of reorganization. In Table A4 we estimate the relationship between filer outcomes and judge leniency, separately for firms with accepted and rejected reorganizaiton plans. In Panels A, B, and C we depict 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, and the cumulative wage workers get from firms that file for reorganization. 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.

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 sub-samples 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]

In the main first stage regression (Table 2), the first stage coefficient is 0.402 . The average probability of reorganization increases by 40.2 p.p. when firms move from the strictest judge to the most lenient judge. The first stage coefficient should be larger in subgroups that are more sensitive to judicial discretion. We compare the first stage coefficient of the main sample with estimates for subsamples in Table 4. In Column (3), we calculate the difference between these estimates. Coefficients are relatively similar accross all subgroups. We observe the largest difference in coefficients between firms with high and low equity ratios. When firms are more capitalized, judges are less likely to change reorganization outcomes. This result is intuitive: more senior creditors are less likely to be exposed to losses in better capitalized firms, hence they have fewer incentives to contest reorganization. We also find that workers with education above or at the median and workers with wages at or above the median are more sensitive to judge discretion.

5 Results

5.1 Worker reallocation

We document the effect of reorganization on the reallocation of workers. Without frictions, stakeholders of distressed firms (i.e., managers, workers, creditors, suppliers, etc.) should be able to privately contract the optimal allocation of resources with each other (Coase (1960), Strömberg (2000)). However, these companies are subject to bankruptcy frictions (e.g. Gertner and Scharfstein (1991)). In such cases, the design of reorganization mechanisms may affect the reallocation of workers.

In Figure 3a, we compare survival rates for firms that file for reorganization against a benchmark firm where the average worker in Portugal is employed in 2011. Five years post-filing, about 50% of all firms that reorganize survive. This value is inferior to 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 20% of their workers, while an average company keeps approximately 55% of its employees after five years. Visually inspecting 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 (less than 5%).

Table 5 shows estimates of the impact of reorganization on firm survival and on the probability that workers stay in the same job using the 2SLS model from Equation 5. In Columns (1) and (2), we report estimates for the short term (first year post-bankruptcy). In Columns (3) and (4), we report estimates for the long term. Long-term results are measured in the fifth year post-filing. We use data for the last observed period when there are fewer than five years of available data after the filing. From 2SLS estimates, we see that reorganization increases firm survival by 27 p.p. and retention in the same firm by 21 p.p. These results were expected, as reorganization plans often mandate managers to keep firms open. This effect lasts in the long term, as firm survival increases by 46 p.p. and worker retention by 27 p.p. 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 p.p.

[Table 5]

We investigate how reorganization affects the characteristics of firms where workers are employed. Reorganization may reduce the destruction of the least productive and profitable firms (Caballero and Hammour (1996), Caballero et al. (2008)). Creditors might use reorganization as an opportunity to reduce accounting loan losses and give interest rate subsidies to reorganized companies, which allows them to remain in business with negative operating profits. Workers stay in these jobs instead of moving to more efficient firms. In this case, we would expect workers to stay in firms with lower profitability and labor marginal productivity - cost gaps.

In Table 6 we report the effect of reorganization on the characteristics of firms where workers are employed. In Column (1), the dependent variable is the last recorded labor marginal labor revenue-cost gap. In Column (2), we measure the effect of reorganization on the marginal labor revenue-cost gap for workers who are employed. We correct for selection into employment using the procedure from Appendix C. In Column (3) the dependent variable is an indicator variable equal to 1 for workers employed at firms with EBITDA coverage ratio greater than 1 (meaning that EBITDA is more than sufficient to cover interest expenses). 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.

[Table 6]

5.2 Labor Outcomes

Table 7 measures the impact of reorganization on the probability that workers have jobs and on wages. Whenever no wages are reported, we assume that workers receive zero wages. In the short term, employees from reorganized firms are 12.7 p.p. more likely to have a job. Reorganization increases wages by 15.7 p.p. , which suggests that employment is the driver of wage growth in the short term.

[Table 7]

In the long term, we do not find a statistically significant relationship between reorganization and the probability that workers have job contracts. Nonetheless, wages of workers from reorganized firms are higher by 16.6 p.p. . While in the long term workers from firms that do not reorganize move to other jobs, they still have considerably lower wages. In Table A6 we report the effect of reorganization on cumulative wages and employment.

In Figure 4 we repeat the analysis of Table 7 separately for each of the years that surround the reorganization event. We repeat Equation 5 for each $k \in \{-2, -1, ..., 4, 5\}$. 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]

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. Column (2) of Table A5 presents results in which we have replaced wages for workers with no job by 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.

Table 8 shows the effect of reorganization on the probability that workers move to occupations with different levels of skill intensity.[2] 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 3. Workers move to a more skill-intensive occupation when their current job is at a higher skill level than their previous job. In the absence of reorganization, 20% and 18% of the workers move to less cognitive skill-intensive and interpersonal skill-intensive occupations, respectively. 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. 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.

[2] In Table A6, we report the effect of reorganization on the probability that workers switch to new industries or occupations.

[Table 8]

Can the reallocation of workers to more skill-intensive occupations explain the effect of reorganization on wages? We show that the variation wages caused by reorganization can partially be 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 Table A9 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 8 We find that reorganization has a positive effect on the occupation premium of about 2 p.p.

We analyze the relationship between the effect of reorganization on labor outcomes and the intensity of labor search frictions. We estimate the intensity of labor market frictions using labor market thickness, defined in Section 3. Our approach follows previous research on the reallocation of assets in distressed markets (Gavazza (2011) and Bernstein et al. (2019b)).

In Table 9, we report the relationship between labor market thickness and the effect of reorganization on labor outcomes. Consistent with reorganization having a stronger effect on labor outcomes in thinner markets, there is a negative relationship between labor market thickness and the effect of reorganization on wages and on the occupation premium.

[Table 9]

5.3 Transition to New Jobs

In Section 5.1, we show that reorganization has a relatively small but positive effect on the retention of workers in reorganized firms. In this section, we show that reorganization also affects the transition of workers to new employers.

Workers are more likely to find better jobs when they search for a new job while on their previous job than when they search while unemployed (Lazear (2009)). In this section we provide empirical evidence that reorganization increases the amount of on-the-job search time given to workers. In Figure 5 we depict annual attrition rates at firms with accepted and rejected reorganization plans, and compare them with the attrition rate for the average worker in Portugal. After the filing, both workers from firms with accepted and rejected reorganization plans have higher attrition rates than the average worker in Portugal. However, workers from firms with rejected reorganization plans have much higher attrition rates than workers with accepted reorganization plans, particularly in the first year post-filing. These results suggest that workers from accepted reorganization plans have more time to search for a new job.

[Figure 5]

In Table 10 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 have to search for a new job before leaving their initial job. In Columns (2)-(5) the dependent variable is an indicator equal to 1 if workers leave their inital job k years after the filing (1,2,3 or 4+ years). Reorganization increases search time by approximately 1 year. Workers are less likely to leave firms in the first year post-filing by 20 p.p. and are more likely to leave firms 4 years post-filing or later by 6 p.p.

[Table 10]

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 instrumental variables 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.

Table 11 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 judge leniency two years before reorganization. Before reorganization, judge leniency does not correlate with wage growth in the three groups. In Columns (2), (4), and (6), we estimate the correlation between wage growth and judge leniency five years after reorganization. After reorganization, judge leniency and wage growth have a positive correlation in all three groups.

[Table 11]

In Figure 6, we estimate Equation 7 for each year between two years pre-filing to five years post-filing, both for workers who stay in reorganized firms and for workers who find jobs with new employers. 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.[3] 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

[3] Note that we use wages measured *after* the reorganization event, not before. Table A7 provides point estimates.

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 effect should be smaller for 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]

6 Robustness

In Table 12, we report OLS coefficients and use different definitions of the instrumental variable. Column (1) reports OLS coefficients. In Column (2) we compute the instrumental variable $Z_{i,j,c,t}$ from Equation 3 using only reorganization filings that happened before filing i. In Column (3) we split the sample in two. For each court-year-subsample pair, we use judge leniency estimated with the other subsample and use court-year-subsample fixed effects. In this way, we avoid mechanical correlations that could arise from the interaction between court dummy variables and the instrumental variable. In Column (4), we repeat the first stage by giving unit weights to all observations, instead of weighing each observation with the inverse of the number of workers in the firm. Column (5) presents bootstrap standard errors that correct for estimation error in the judge leniency measure. 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 (6), we use court and year fixed effects separately. In Column (7), we estimate the model at the firm level. In Column (8), we estimate judge leniency in absolute terms by excluding second term of Equation 3.

[Table 12]

7 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 Portugal to study this research question, exploiting random variation in the probability of corporate reorganization due to the fact that some judges are more likely than others to approve reorganization plans. Using judge leniency to generate random variation in the probability of reorganization allows us to separate the effect of reorganization from other confounding factors that may affect workers' job trajectories. We use detailed panel data on workers and firms to assess labor outcomes, even as workers leave distressed firms.

We document three facts about the effect of reorganization on the reallocation of workers in bankruptcy. First, there is significant labor reallocation even in reorganized firms. In five years, only 20% of the workers initially employed in reorganized firms stay in the same firm. We do not find evidence that reorganization systematically retains workers in less efficient firms, as measured by marginal labor revenue-cost gaps.

Second, we show that reorganization provides labor insurance in bankruptcy. Reorganization has a positive and persistent effect on wages. Workers are less likely to transition to less skill-intensive occupations that have lower wage premia. Consistent with reorganization reducing labor search frictions in bankruptcy, we show that the effect of reorganization on labor outcomes is negatively correlated with labor market thickness.

Third, we show that reorganization positively affects reallocation outcomes for workers who leave for new employers. Reorganization increases the amount of available time to search for new jobs. Reorganization is associated with higher wages, both for workers who stay in reorganized firms and for workers who move to new employers. We show that reorganization increases the probability that workers find jobs in other firms at the highest quintile of the wage distribution.

Tables

Table 1: Descriptive statistics

	All firms (1)	Accepted reorganization (2)	Rejected reorganization (3)	Difference (4)
Firm variables				
Assets (€ M)	6.37	7.34	5.01	2.32**
	(0.55)	(0.77)	(0.77)	(1.09)
Workers†	29.99	32.43	26.57	5.85**
	(1.39)	(2.02)	(1.78)	(2.69)
EBITDA/Assets (%)	-5.06	-4.08	-6.45	2.37***
	(0.25)	(0.31)	(0.41)	(0.52)
Equity ratio (%)	-3.85	-1.90	-6.58	4.69***
	(0.76)	(0.97)	(1.22)	(1.56)
Labor gap (€ th)	14.41	14.92	13.70	1.22
	(0.38)	(0.50)	(0.59)	(0.77)
Worker variables				
Age (years)	37.51	37.48	37.56	-0.07
	(0.03)	(0.04)	(0.06)	(0.07)
Female (%)	40.90	39.50	43.40	-3.9***
	(0.23)	(0.28)	(0.38)	(0.47)
Schooling (years)	8.52	8.47	8.60	-0.12***
	(0.02)	(0.03)	(0.03)	(0.04)
Tenure (years)	7.47	7.34	7.72	-0.38***
	(0.03)	(0.04)	(0.06)	(0.07)
Wage (€)	1095.54	1108.55	1072.91	35.64***
	(4.02)	(5.25)	(6.17)	(8.10)
Number of firms	2,554	1,490	1,064	
Number of workers	47,632	30,241	17,391	

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 errors 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)
Judge leniency	0.401***	0.402***
	(0.066)	(0.065)
Log assets		0.003
		(0.017)
Log workers		0.028**
		(0.011)
Labor gap		0.015
		(0.034)
Equity ratio		0.0004
		(0.001)
EBITDA/Assets		0.299***
		(0.087)
Age		-0.001
		(0.001)
Tenure		-0.001
		(0.001)
Female		-0.024*
		(0.014)
Years schooling		-0.002
		(0.001)
Log wage		0.024
		(0.017)
Instrument F-stat	36.45	38.02
Observations	47,632	47,632
R2	0.081	0.104

Notes. The table reports first stage results. The dependent variable is a dummy equal to one if the reorganization filing is accepted. Judge leniency 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

	Judge leniency										
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Log assets	0.004	0.003									
	(0.007)	(0.005)									
Log workers	0.0001		-0.0001								
	(0.005)		(0.003)								
Labor gap	-0.001			-0.003							
	(0.014)			(0.011)							
Equity ratio	-0.00002				0.00004						
	(0.000)				(0.000)						
EBITDA/Assets	-0.016					-0.021					
	(0.039)					(0.034)					
Age	0.0001						0.0002				
	(0.0004)						(0.0003)				
Tenure	0.001							0.001			
	(0.001)							(0.0004)			
Female	0.006								0.007		
	(0.005)								(0.006)		
Years schooling	0.001									0.001	
	(0.001)									(0.001)	
Log wage	-0.001										0.002
	(0.007)										(0.007)
Industry F-stat	1.34										
Observations	47,632	47,632	47,632	47,632	47,632	47,632	47,632	47,632	47,632	47,632	47,632

Notes. This table reports randomization tests to illustrate the random assignment of reorganization to judges within a court. The dependent variable is judge leniency, as defined in Equation 3. All regressions are at the worker level. Column (1) reports coefficients from regressing judge leniency on all variables. Columns (2)-(11) 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 4: First stage by group

Variable	Below median (1)	Above median (2)	Above median - below median (3)
Assets	0.394***	0.428***	0.034
	(0.069)	(0.115)	(0.134)
Workers	0.402***	0.551***	0.149
	(0.066)	(0.164)	(0.177)
Equity ratio	0.474***	0.293***	-0.181
	(0.085)	(0.075)	(0.113)
Labor gap	0.458***	0.322***	-0.136
	(0.073)	(0.099)	(0.123)
EBITDA/Assets	0.453***	0.360***	-0.093
	(0.086)	(0.080)	(0.117)
Age	0.344***	0.443***	0.099
	(0.068)	(0.073)	(0.100)
Tenure	0.447***	0.348***	-0.099
	(0.075)	(0.071)	(0.103)
Education	0.312***	0.456***	0.144
	(0.082)	(0.065)	(0.105)
Log wage	0.380***	0.424***	0.044
	(0.070)	(0.077)	(0.104)
Thickness	0.361***	0.441***	0.080
	(0.077)	(0.068)	(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

	Short-term (year 1)		Long-term (year 5)	
	Firm survival	Job same firm	Firm survival	Job same firm
Dependent variable	(1)	(2)	(3)	(4)
Reorganization	0.270***	0.201**	0.457***	0.270***
	-0.099	-0.085	(0.127)	(0.070)
Observations	47,632	47,632	47,632	47,632
R2	0.184	0.129	0.198	0.118

Notes. This table shows the effect of reorganization on the reallocation of workers in the short and long term. Short-term outcomes are measured in the first year post-filing. Long-term outcomes are measured in the fifth year post-filing. When there are fewer than five years of available data, we measure outcomes in the last year with available data. *Firm survival* is an indicator equal to 1 if the firm 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: Labor marginal revenue-cost gaps and profitability

Dependent variable	Labor gap (1)	Labor gap (2)	EBITDA coverage > 1 (3)
Reorganized	-1.927	0.972	0.054
	(3.071)	(4.008)	(0.076)
Observations	47,632	47,632	47,632
R-squared	0.227	0.156	0.044

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 last observed marginal labor revenue-cost gap of firms where workers are employed. *Labor gap* is the last reported labor marginal revenue-cost gap, as defined in Appendix B. For observations with no labor gap or profitability data, we use the last observed data point. In Column 3, 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 restrict the sample to workers who have a job after reorganization and correct for selection into employment. The correction procedure is explained in 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) 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 7: Wages and employment

	Short term (year 1)		Long term (up to year 5)	
	Job	Wage growth	Job	Wage growth
Dependent variable	(1)	(2)	(3)	(4)
Reorganized	0.127*	0.157*	0.045	0.166**
	(0.076)	(0.085)	(0.063)	(0.068)
Observations	47,632	47,632	47,632	47,632
R-squared	0.087	0.058	0.056	0.074

Notes. This table shows the effect of reorganization on wages and the probability of having a job. Short-term outcomes are measured in the first year post-filing. Long-term outcomes are measured in the fifth year post-filing. When there are fewer than five years of available data, we measures outcomes in the last year with available data. *Wage growth* is defined as $\Delta\%\ Wage = \frac{Wage_{t+k}}{Wage_t}$, where $Wage_{t+k}$ is the wage in year $t+k$. *Job* is an indicator equal to 1 if the worker has a job contract. We display 2SLS 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 8: Transition to new occupations

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

	Lower skill percentile = 1			Occupation
	Cognitive	Manual	Interpersonal	premium
Variable	(1)	(2)	(3)	(4)
Reorganized	-0.199***	-0.109**	-0.176***	0.019**
	(0.059)	(0.049)	(0.066)	(0.009)
Observations	47,632	47632	47632	47,632
R-squared	0.009	0.024	0.016	0.084

(b) Transition to more skill-intensive occupations

	Higher skill percentile = 1		
	Cognitive	Manual	Interpersonal
Variable	(1)	(2)	(3)
Reorganized	-0.006	-0.095*	-0.029
	(0.054)	(0.053)	(0.057)
Observations	47,632	47,632	47,632
R-squared	0.041	0.037	0.035

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 from Section 3. 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 9: Reorganization outcomes and labor market thickness

Variable	Job contract (1)	Wage growth (2)	Higher skill percentile = 1: Cognitive (3)	Manual (4)	Interpersonal (5)	Occupation premium (6)
Reorganized	0.148**	0.275***	-0.219***	-0.135**	-0.202**	0.033***
	(0.068)	(0.089)	(0.073)	(0.062)	(0.080)	(0.011)
Reorganized * Thickness	-0.132***	-0.143**	0.024	0.022	0.031	-0.017**
	(0.040)	(0.057)	(0.036)	(0.030)	(0.038)	(0.007)
Observations	47,632	47,632	47,632	47,632	47,632	47,632
R-squared	0.030	0.052	0.006	0.036	0.011	0.063

Notes. This table shows the relationship between the effect of reorganization on labor outcomes and labor market thickness. *Reorganization* is an indicator variable equal to 1 for firms with accepted reorganization plans. *Thickness* is the ratio between the number workers employed in occupation m where worker i is employed and the total number of employed workers. Dependent variables are defined in the text. 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.

Table 10: Reallocation to new firms

Variable	Search time (years) (1)	Leave firm = 1: $k = 1$ (2)	$k = 2$ (3)	$k = 3$ (4)	$k \geq 4$ (5)
Reorganized	0.939***	-0.198**	-0.148**	0.014	0.063*
	(0.234)	(0.084)	(0.074)	(0.052)	(0.038)
Observations	47,632	47,632	47,632	47,632	47,632
R-squared	0.179	0.135	0.024	0.034	0.072

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. *Search time (years))* is the number of years between the filing year 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 post-filing of the last observation. *Leave firm* is an indicator variable equal to 1 when workers leave the firm k years after the filing. 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 11: Relationship between judge leniency and wages by worker group

	Employed workers		New job		Job same firm	
	(1)	(2)	(3)	(4)	(5)	(6)
Reorganized	0.018	0.071***	0.033	0.058*	0.033	0.087***
	(0.024)	(0.023)	(0.030)	(0.031)	(0.027)	(0.025)
Observations	32,953	32,953	19,073	19,073	25,416	25,416
R-squared	0.458	0.17	0.419	0.175	0.500	0.176

Notes. This table shows the relationship between judge leniency and wage growth for workers who find jobs at new employers and for workers who stay in the same firm. Columns (1), (3), and (5) show coefficients 2 years before the reorganization event. Columns (2), (4), and (6) show coefficients five years after the reorganization event, or in the last period observed after reorganization. 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.

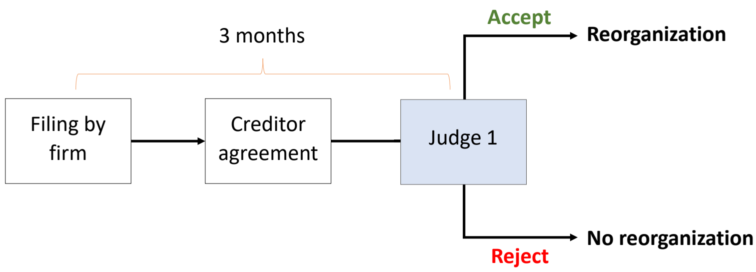
Table 12: Alternative empirical models

Variable	OLS (1)	Past cases (2)	Sample split (3)	Worker weights (4)	Bootstrap (5)	Court+ Year FE (6)	Firm aggregation (7)	Absolute leniency (8)
Instrument		0.307***	0.334***	0.381***	0.371***	0.386***	0.405***	0.306***
		(0.053)	(0.050)	(0.113)	(0.074)	(0.095)	(0.068)	(0.065)
Job same firm	0.174***	0.256***	0.242***	0.299**	0.248***	0.277***	0.264***	0.305***
	(0.011)	(0.079)	(0.077)	(0.142)	(0.082)	(0.015)	(0.071)	(0.103)
Job	0.050***	0.019	0.064	0.071	0.057	0.056	0.042	0.038
	(0.013)	(0.075)	(0.079)	(0.071)	(0.074)	(0.048)	(0.065)	(0.089)
Wage growth	0.061***	0.162**	0.182**	0.151*	0.191**	0.193***	0.160**	0.209**
	(0.016)	(0.078)	(0.090)	(0.082)	(0.090)	(0.061)	(0.070)	(0.098)
Labor gap	-0.759	1.643	-2.8	-5.045	-1.044	-0.794	-2.008	-1.953
	(0.519)	(3.360)	(3.851)	(3.965)	(4.865)	(1.878)	(3.110)	(4.237)
Profitability	0.026**	0.051	0.056	0.057	0.069	0.073	0.063	0.079
	(0.013)	(0.089)	(0.086)	(0.117)	(0.090)	(0.078)	(0.073)	(0.099)
Cognitive skill	-0.063***	-0.130**	-0.072	-0.163***	-0.18**	-0.191***	-0.198***	-0.246***
	(0.009)	(0.060)	(0.065)	(0.053)	(0.072)	(0.023)	(0.060)	(0.087)
Manual skill	-0.030***	-0.102*	-0.006	-0.112*	-0.071	-0.082	-0.106**	-0.137**
	(0.010)	(0.059)	(0.062)	(0.058)	(0.066)	(0.064)	(0.049)	(0.069)
Interpersonal skill	-0.058***	-0.170**	-0.029	-0.162***	-0.151**	-0.163***	-0.176***	-0.219**
	(0.010)	(0.069)	(0.067)	(0.056)	(0.072)	(0.058)	(0.067)	(0.097)
Occupation premium	0.007***	0.015	0.021*	0.017	0.019*	0.019***	0.018*	0.023*
	(0.002)	(0.011)	(0.012)	(0.012)	(0.011)	(0.007)	(0.009)	(0.013)
Wages same firm	0.030*	0.073**	0.05	0.035	0.059*	0.064*	0.024	0.057*
	(0.015)	(0.029)	(0.031)	(0.031)	(0.033)	(0.033)	(0.033)	(0.031)
Wages other firms	0.020*	0.076***	0.080***	0.052**	0.087***	0.093***	0.057**	0.088***
	(0.011)	(0.023)	(0.029)	(0.022)	(0.029)	(0.027)	(0.023)	(0.025)
Search time	0.776***	0.554*	0.832***	1.382***	0.955***	1.016***	0.926***	1.016***
	(0.044)	(0.312)	(0.267)	(0.371)	(0.317)	(0.208)	(0.239)	(0.331)
Job at new employer - quantile 5	-0.004	0.080***	0.073**	0.057	0.077	0.071***	0.062***	0.089**
	(0.005)	(0.029)	(0.030)	(0.057)	(0.040)	(0.013)	(0.024)	(0.036)
Observations	47,632	47,632	47,632	47,632	47,632	47,632	2,554	47,632

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 split the sample in two equal parts. In each case, we estimate judge leniency using data only from the other sub-sample. In Column (4), we repeat the first stage by giving unit weights to all observations, instead of weighing each observation with the inverse of the number of workers in the firm. In Column (5), 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 (6), we use court and year fixed effects separately. In Column (7), we estimate the aggregate model at the firm level. In Column (8), 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)). 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.

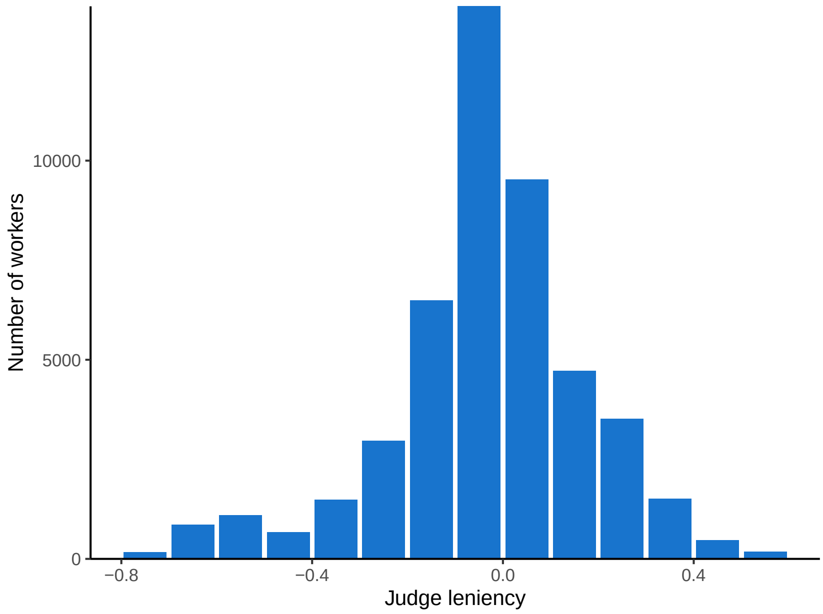
Figures

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.

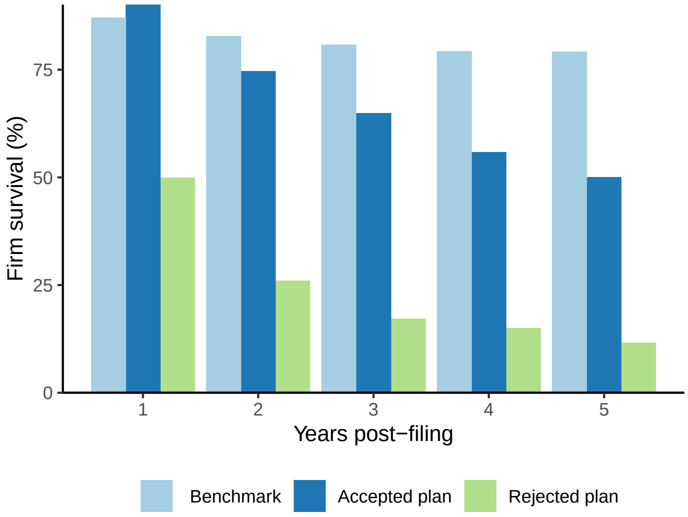
Figure 2: Judge leniency distribution



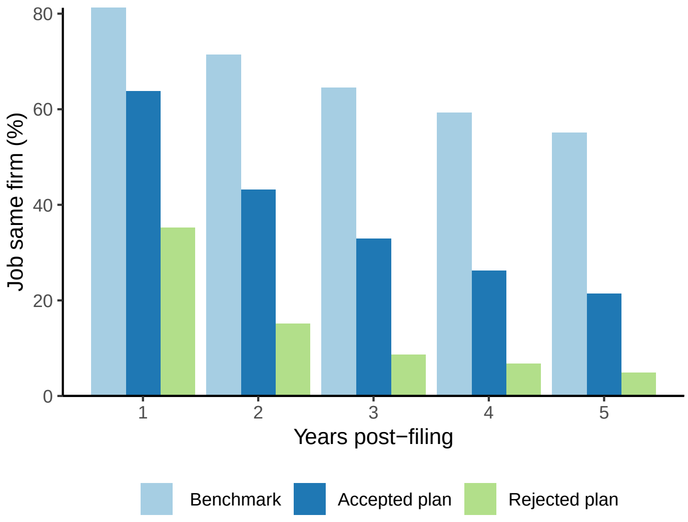
Notes. This figure depicts the distribution of the instrumental variable obtained with Equation 3.

Figure 3: Worker reallocation over time

(a) Firm survival

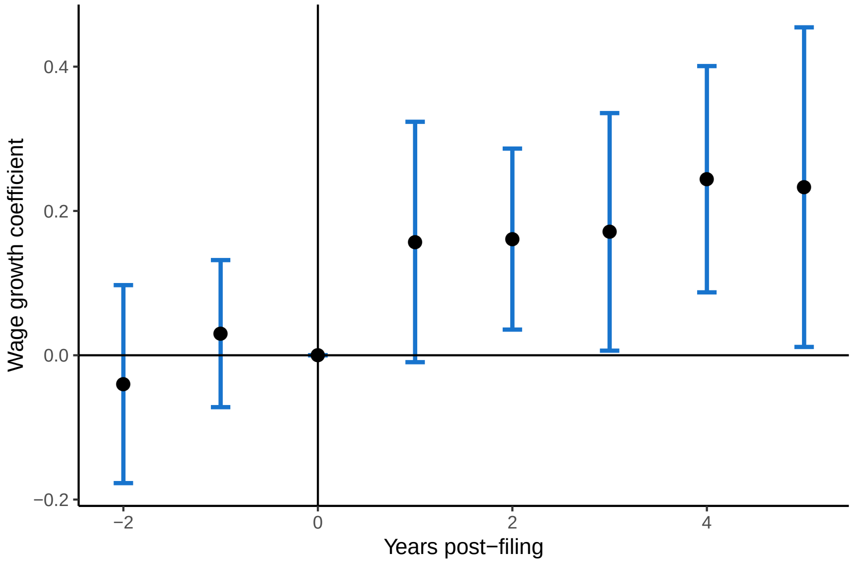


(b) Worker retention



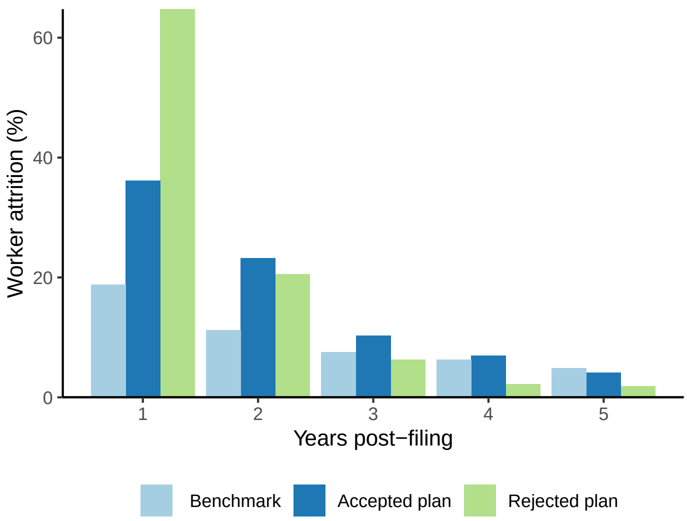
Notes. Panel A depicts the percentage of workers from firms that survive up to five years after reorganization. Panel B depicts the percentage of workers who stay in reorganized firms. We restrict the sample to firms that have five years of data post-filing. We obtain the benchmark sample using a random sample of 1% of the workers with similar characteristics who were employed in Portugal in 2011.

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, from 2 years pre-filing to 5 years post-filing. 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.

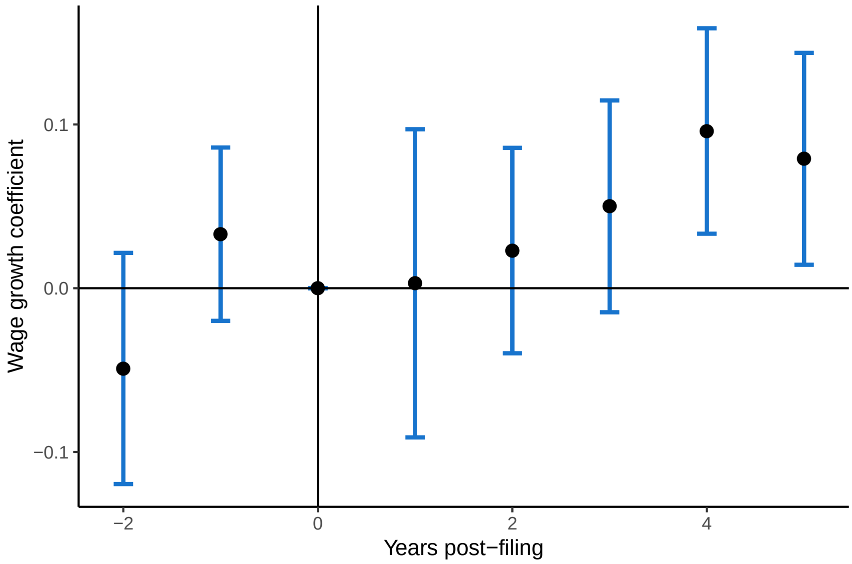
Figure 5: Worker attrition



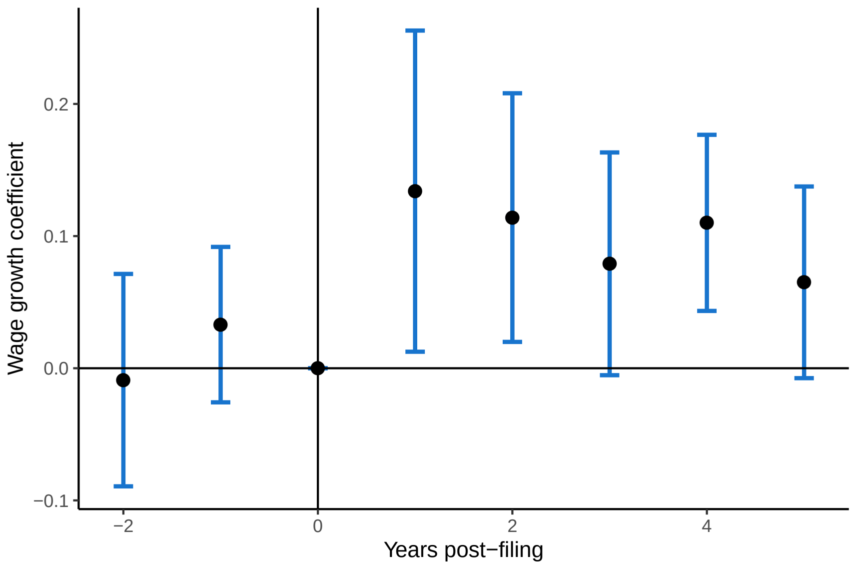
Notes. This figure shows the percentage of workers who leave firms. Dark blue bars (green bars) depict annual attrition rates for workers employed at firms with accepted (rejected) reorganization plans. We restrict the sample to firms with five years of data post-filing. We compute the benchmark attrition rate using a random sample of 1% of the workers with similar characteristics who were employed in Portugal in 2011.

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

(a) Workers who leave to new employers

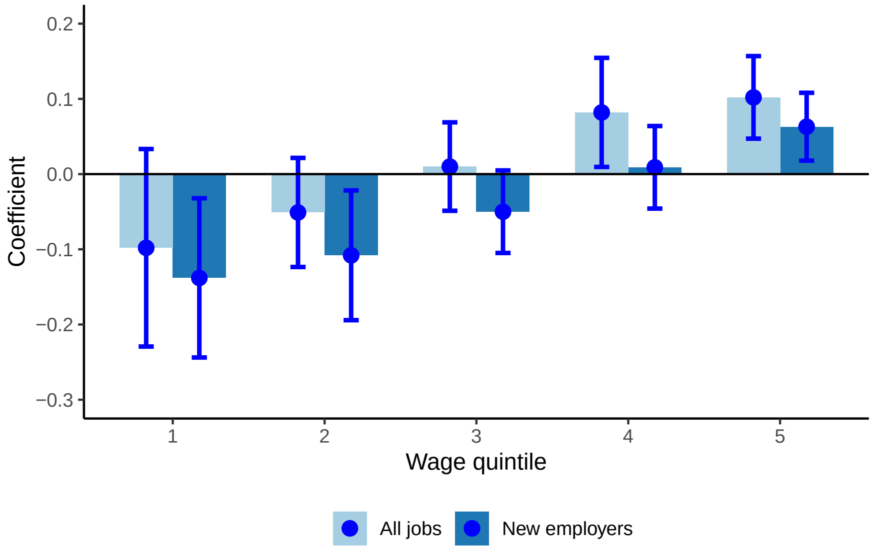


(b) Workers who stay in the same firm



Notes. In this figure, we estimate the relationship between wage growth and judge leniency. We estimate Equation 7 each year, from 2 years pre-filing to 5 years post-filing. 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: Industries

Industry	Workers	Firms
Manufacure of food products	1,446	66
Manufacture of textiles	2,133	57
Manufacture of wearing apparel	2,780	76
Manufacture of leather and related products	1,524	55
Manufacture of wood and related products	1,204	57
Manufacture of fabricated metal products, except machinery and equipment	2,049	110
Manufacture of furniture	867	56
Construction of buildings	4,290	229
Civil engineering	1,799	60
Specialized construction activities	2,514	143
Wholesale and retail trade and repair of motor vehicles and motorcycles	1,315	102
Wholesale trade, except of motor vehicles and motorcycles	3,511	310
Retail trade, except of motor vehicles and motorcycles	5,135	279
Land transport and transport via pipelines	919	70
Accommodation	1,277	50
Food and beverage service activities	1,042	99
Other industries	13,827	735
Total	47,632	2,554

Notes. The table depicts the distribution of workers and firms that file for reorganization by industry.

Table A2: Additional empirical models (1/2)

	Drop cases (1)	Old court FE (2)
Instrument	0.369*** (0.067)	0.402*** (0.066)
Job same firm	0.291*** (0.079)	0.252*** (0.067)
Job	0.045 (0.070)	0.034 (0.062)
Wage growth	0.170** (0.076)	0.153** (0.069)
Labor gap	-1.39 (3.436)	0.044 (2.471)
Profitability	0.069 (0.078)	0.04 (0.072)
Cognitive skill	-0.199*** (0.066)	-0.168*** (0.056)
Manual skill	-0.114** (0.053)	-0.102** (0.052)
Interpersonal skill	-0.182** (0.073)	-0.150** (0.062)
Occupation premium	0.019* (0.010)	0.011 (0.008)
Observations	44,856	47,734

Table A2: Additional empirical models (2/2)

Variable	Drop cases (1)	Old court FE (2)
Wages same firm	0.053*	0.049
	(0.031)	(0.034)
Wages other firms	0.083***	0.090***
	(0.025)	(0.026)
Search time	1.051***	0.997***
	(0.260)	(0.241)
Job at new employer - quantile 5	0.070***	0.074***
	(0.026)	(0.026)
Observations	44,856	47,734

Notes. The table reports robustness checks for alternative empirical models. The regressions are estimated on the sample as described in Table 2. The dependent variable is listed in each row. In Column (1) we drop cases for which there is not enough data to compute $Z_{i,j,c,t}$ from Equation 4. In Column (2) we use alternative court identities described in Section 4. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A3: Instrument robustness: pre-trends

	Judge leniency	
	Year -2	Year -3
Variable	(1)	(2)
Log assets	0.0002	-0.0004
	(0.001)	(0.001)
Log workers	0.001	-0.001
	(0.002)	(0.002)
Labor gap	0.0001	-0.0002
	(0.0002)	(0.0002)
Equity ratio	0.00003	-0.0001
	(0.0002)	(0.0002)
EBITDA/Assets	-0.0002	-0.001
	(0.001)	(0.001)
Log wage	0.002	0.0004
	(0.002)	(0.001)
Observations	47,632	47,632

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 judge leniency, 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 A4: Worker outcomes within filers

(a) All firms

Variable	Years firm survival (1)	Years employed at firm (2)	Cumulative wages (3)
Instrument	0.831***	0.525***	0.546***
	(0.222)	(0.142)	(0.141)
Observations	47632	47632	47632
R-squared	0.16	0.117	0.109

(b) Firms with accepted reorganization plans

Variable	Years firm survival (1)	Years employed at firm (2)	Cumulative wages (3)
Instrument	0.074	0.151	0.211
	(0.231)	(0.162)	(0.165)
Observations	30241	30241	30241
R-squared	0.274	0.16	0.15

(c) Firms with rejected reorganization plans

Variable	Years firm survival (1)	Years employed at firm (2)	Cumulative wages (3)
Instrument	-0.008	-0.011	0.013
	(0.294)	(0.182)	(0.198)
Observations	17391	17391	17391
R-squared	0.178	0.141	0.129

Notes. The table depicts estimates for the effect of reorganization on worker outcomes at firms that file for reorganization using the model from Equation 5. *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. *Cumulative wages* is the total wages obtained by the worker at the firm. 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 A5: Missing wages

	Wage growth (%)			
Depedendent variable	(1)	(2)	(3)	(4)
Reorganized	0.166**	0.121***	0.173***	0.180***
	(0.068)	(0.047)	(0.065)	(0.083)
Observations	47,632	47,632	32,953	32,953

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. 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 A6: Additional worker outcomes

	Cumulative wage effect			Same occupation	Same industry (2-digit)	Same industry (5-digit)
	Total	Extensive margin	Intensive margin			
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Reorganized	0.646***	0.374*	0.272*	0.205***	0.131*	0.197***
	(0.210)	(0.215)	(0.149)	(0.068)	(0.073)	(0.073)
Observations	47,632	47,632	47,632	47,632	47,632	47,632
R-squared	0.134	0.158	0.122	0.051	0.085	0.092

Notes. This table shows the effect of reorganization on additional 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. 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 A7: Employment transitions by wage quintile

(a) All employed workers

	Job = 1				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
	(1)	(2)	(3)	(4)	(5)
Reorganized	-0.098	-0.051	0.01	0.082**	0.102***
	(0.067)	(0.037)	(0.030)	(0.037)	(0.028)
Observations	47,632	47,632	47,632	47,632	47,632
R-squared	0.044	0.042	0.027	0.036	0.13

(b) Workers who stay in the same firm

	Job same firm = 1				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
	(1)	(2)	(3)	(4)	(5)
Reorganized	0.04	0.057***	0.061**	0.073***	0.039**
	(0.038)	(0.022)	(0.027)	(0.023)	(0.020)
Observations	47,632	47,632	47,632	47,632	47,632
R-squared	0.064	0.046	0.044	0.034	0.067

(c) Workers who move to new employers

	Job new employer = 1				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
	(1)	(2)	(3)	(4)	(5)
Reorganized	-0.138***	-0.108**	-0.050*	0.009	0.063***
	(0.054)	(0.044)	(0.028)	(0.028)	(0.023)
Observations	47,632	47,632	47,632	47,632	47,632
R-squared	0.028	0.024	0.021	0.034	0.087

Notes. The table depicts 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 job with wage in quintile Q.

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. We use Kalil (2017), Vasconcelos et al. (2017), and Simões (2019) as references. They 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 debtor recovery, both in the case of firms (mainly through Chapter 11) and househods (mainly through Chapter 13). The Portuguese system discouraged firm recovery in bankruptcy, particularly 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 only propose recovery plans 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 stayed in 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 suffered 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 2. 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*[4]. 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 onwards, 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. Aftewards, 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 cast votes are for the approval of the plan, and one half of the cast votes 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 cast votes 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 (i.e., a filing under the original bankruptcy system set up by Decree-law *Dec. Lei n.º 53/2004*).

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

The bankruptcy manager submits a liquidation filing if the firm is "insolvent" at the end of the reorganization process. Besides bankruptcy managers, the debtor and creditors may also submit a subsequent liquidation filing. This filing should be added to the court's distribution schedule and lead to the opening of a new case.[5]

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 (Ministério da Justiça). 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 2019. 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"); type of case ("Espécie"), e.g. reorganization; 4) record date ("Data"); 5) original case filing date ("Data de propositure 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 A2. 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

[5] In the past, litigants argued that filings by bankruptcy managers should not lead to the opening of new cases. However, according to common practice and jurisprudence (*1520/14.5TBSTS-A.P1*) these filings should lead to the opening of new cases

associated tax ID. In these situations we use the reported company name to search for the tax ID. We use the tax ID to merge the bankruptcy dataset with the employer-employee dataset and firm financial statements described in Section 3.

- 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) the filing is classified as a firm filing by *Citius* (and not as a household); 2) the firm has more than one employee in Quadros de Pessoal (i.e., not a firm without employees or an independent worker); 3) the firm reports firm financial statistics (IES);

- 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 and reallocated other courts to new districts. 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 with 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.

- 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 an administrative agency (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 cases are allocated to more than one judge. We create separate judge IDs for these situations.

- 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. We verify the outcome of each case manually by reading PDF files (Figure A2).

Figure A1: Example of court record from Citius

Tribunal: Comarca do Porto - Vila Nova de Gaia
Ato: Anúncio PER - artº 34 - P Citius
Referência: [redacted]
Processo: [redacted]
Espécie: Processo Especial de Revitalização (CIRE)
Data: [redacted]
Data da propositura da ação: [redacted]

Devedor: [redacted]
NIF/NIPC: [redacted]
Administrador Insolvência: [redacted]
NIF/NIPC: [redacted]
Credor: [redacted]
NIF/NIPC: [redacted]
Credor: [redacted]
NIF/NIPC: [redacted]
Credor: [redacted]
NIF/NIPC: [redacted]
Credor: [redacted]
NIF/NIPC: [redacted]
Credor: [redacted]
NIF/NIPC: [redacted]

Ver Mais

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:

$$q_{i,t} = (\omega_{i,t} + \epsilon_{i,t}) + f(k_{i,t}, l_{i,t}, \gamma) \tag{9}$$

With:

$$f(k_{i,t}, l_{i,t}, \gamma) = \gamma_K k_{it} + \gamma_L l_{i,t} + \gamma_M m_{i,t} + \gamma_{KK} k_{i,t}^2 + \gamma_{KL} k_{i,t} l_{i,t} + \gamma_{KM} k_{i,t} m_{i,t} + \gamma_{LL} l_{i,t}^2 + \gamma_{LM} l_{i,t} m_{i,t} + \gamma_{MM} m_{i,t}^2 \tag{10}$$

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. In subsequent years, we update capital using the equation:

$$K_{it} = (\delta_{i,t} K_{i,t-1} + \frac{I_{i,t}}{def_t}) \tag{11}$$

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 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 A8 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 A8: Output elasticity estimates

Variable	All firms (1)	Reorganized (2)	Not reorganized (3)
θ^K	0.096	0.099	0.091
	(0.069)	(0.070)	(0.067)
θ^L	0.463	0.466	0.46
	(0.156)	(0.157)	(0.155)
θ^M	0.516	0.51	0.523
	(0.162)	(0.161)	(0.163)
Sum	1.075	1.076	1.074
	(0.081)	(0.083)	(0.078)

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

$$selection\ dummy_{e,t+k} = \beta Z_{i,j,c,t} + \lambda I_{e,t}^{\geq 45yo} + \gamma X_{e,i,t} + \delta_{c,t} + \epsilon_{e,t+k} \tag{12}$$

Where $selection\ dummy_{e,t+k}$ is an indicator variable equal to 1 for workers who are selected into employment, and $I_{e,t}^{\geq 45yo}$ is an indicator variable that is equal to 1 for workers who are at least 45 years old. We use $I_{e,t}^{\geq 45yo}$ 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 $X_{e,i,t}$, 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:

$$Y_{e,t+k} = \alpha + \beta.\widehat{Reorganization}_{i,t} + \gamma X_{e,i,t} + \delta_{c,t} + IMR_{e,t+k} + \epsilon_{e,t+k} \tag{13}$$

Where $IMR_{e,t+k}$ is the Inverse Mills Ratio computed using estimates from Equation 12. 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 analyze empirically 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)), 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:

$$y_{i,t} = \alpha_i + \psi_{J(i,t)} + \phi_{W(i,t)} + x'_{i,t}\beta + r_{i,t} \tag{14}$$

$y_{i,t}$ 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[6], 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:

$$r_{i,t} = \eta_{i,j,w} + \zeta_{i,t} + \epsilon_{i,t} \tag{15}$$

[6]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 14. Card et al. (2013) discuss at length the conditions that must hold for OLS to identify the parameters in Equation 14. 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:

$$P(J(i,t) = j \wedge W(i,t) = w|r) = P(J(i,t) = j \wedge W(i,t) = w) = G_{jwt}(\alpha_i, \psi_1, ..., \psi_J, \phi_1, ..., \phi_W), \forall i, t \quad (16)$$

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 16. 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 do the opposite move. As Table A9 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 A9.

Third, the transitory error may be systematically associated with job changes to higher or lower wage firms and occupations. As Table A9 shows, the evidence does not suggest a systematic relationship between transitory wage fluctuations and job changes to new firms and occupations.

Table A9: 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 depicts 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.