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Corporate reorganization and the reallocation of labor in bankruptcy

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Abstract

We analyze how corporate reorganization and liquidation change labor reallocation during bankruptcy using randomized judge assignments and linked Portuguese employer-employee and firm data. Reorganization reduces the negative effect of bankruptcy on employee earnings, even with most workers leaving reorganized firms. We examine plausible mechanisms and find evidence that the retention of general skills and improved job-match quality contribute meaningfully to this effect. The average cost of labor misallocation caused by reorganization is small. However, for some workers in the least productive filers, this cost can be large, outweighing the effect on earnings.

JEL codes: G33, G38, J24, J63, K39

Keywords: corporate reorganization, bankruptcy

Non-technical summary

When a firm goes through a bankruptcy procedure, the workers of the firm will inevitably be affected. However, the consequences of such procedures for workers may widely differ depending on the design of the process. For instance, corporate reorganization may yield outcomes that differ from those of liquidation. However, empirically identifying the differences in worker outcomes between the two procedures is not straightforward.

We analyse these implications empirically, using corporate reorganization filings from Portugal between 2012 and 2016, along with administrative employer-employee and firm data. This setting offers unique advantages. The employer-employee data allow us to track workers from firms that file for reorganization when they move to other jobs. Firm data covers all firms operating in Portugal, including private firms and firms in non-manufacturing sectors. Linking employer-employee and firm data allows us to examine the effect of reorganization on earnings, and to study how reorganization affects the reallocation of workers to firms with different levels of profitability and productivity.

The choice between reorganization and liquidation is not random, implying that external factors might create a spurious relationship between reorganization and labour outcomes. We address this problem by exploring the random allocation of reorganization cases to judges in the same court, following the literature on random judge allocation. By exploring the fact that judges have different characteristics, with some being more lenient than others, their random allocation to cases allows for the causal identification of the relationship between reorganization and labour outcomes.

We find that reorganization has a material effect on job transitions because it changes when and how workers leave their jobs, when compared to liquidation. Workers from reorganized firms take on average one year more than workers from liquidated firms to leave their jobs. Five years after the filing, reorganized firms only keep about 20% of their workforce. Additionally, reorganization reduces the probability of layoff by 37 percentage points and increases the probability that workers quit by 9 percentage points. Reorganization reduces earnings losses in comparison to liquidation, even after workers from liquidated firms find new jobs. We provide evidence that reorganization improves earnings both for workers who stay in reorganized firms and for workers who leave for new employers, compared to the liquidation counterfactual.

After documenting this fact, we examine the main mechanisms that potentially determine workers' earnings and assess how these mechanisms influence the relationship between reorganization, liquidation, and earnings. First, we analyse the role of human capital. We show that firm-specific skills have a limited impact on the relationship between reorganization on earnings, whereas general skills have a significant impact. Second, we investigate the relationship between the impact of reorganization on earnings, job match quality, and bargaining dynamics. Reorganization improves matching by allocating workers to jobs at firms and occupations characterized by higher average earnings. This effect arises at least partially from the effect of reorganization on transitions to new jobs, since reorganization increases the probability that workers move to new employers with high average earnings. Our analysis does not yield substantial evidence supporting the notion that bargaining power contributes to explaining the influence of reorganization on earnings.

Policies that promote firm continuation, such as reorganization, might lead to an inefficient allocation of resources. We provide evidence that reorganized firms are less profitable and have lower labour gaps than the firms where workers who leave reorganized firms are employed. The effect of reorganization on labour reallocation is not unequivocally positive and provides evidence of potentially excessive retention in reorganized firms.

1 Introduction

In this paper we analyze the relationship between corporate reorganization and labor reallocation in the context of bankruptcy proceedings. Whereas existing research studies the impact of reorganization and liquidation on credit claims and asset allocation (e.g., Strömberg (2000), Bernstein et al. (2019b), Antill (2022)), labor reallocation follows different dynamics for two reasons. First, unlike assets, workers accumulate human capital (Becker (2009)). Second, firms lack the ability to contract on the human capital of workers (Titman (1984), Hart and Moore (1994)), but can contract on asset ownership. Creditors do not control workers' human capital, which means that workers from bankrupt firms are free to withdraw their human capital to other firms. Therefore, creditors have no incentive to consider the effect of choosing reorganization instead of liquidation on worker outcomes because they cannot capture the worker benefits of choosing one outcome over the other. In contrast, physical assets are included in the bankruptcy estate and the proceeds from their sale are used to reimburse creditors. This difference introduces unique implications for labor reallocation dynamics within corporate bankruptcy decisions.

We analyze these implications empirically, using corporate reorganization filings from Portugal between 2012 and 2016, along with administrative employer-employee and firm data. This setting offers unique advantages. The employer-employee data allow us to track workers from firms that file for reorganization when they move to other jobs. Firm data covers all firms operating in Portugal, including private firms and firms in non-manufacturing sectors. Linking employer-employee and firm data allows us to examine the effect of reorganization on earnings, and to study how reorganization affects the reallocation of workers to firms with different levels of profitability and productivity.

The choice between reorganization and liquidation is not random, implying that external factors might create a spurious relationship between reorganization and labor outcomes. We address this problem by exploring the random allocation of reorganization cases to judges in the same court, following the literature on random judge allocation (e.g., Chang and Schoar (2013), Bernstein et al. (2019a), Bernstein et al. (2019b), Antill (2022)).

Reorganization has a material effect on job transitions because it changes when and how workers leave their jobs, when compared to liquidation. We measure the effect of reorganization on labor reallocation in a five-year window after the filing. Workers from reorganized firms take on average one year more than workers from liquidated firms to leave their jobs. Five years after the filing, reorganized firms only keep about 20% of their workforce. Additionally, reorganization reduces the probability of layoff by 37 percentage points and increases the probability that workers quit by 9 percentage points. Reorganization reduces earnings losses in comparison to liquidation, even after workers from liquidated firms find new jobs. By the last year of the analysis, reorganization reduces earnings losses by 20% in the baseline, with estimates ranging from 12% to 22% under different assumptions. The intensive margin, i.e., workers from reorganized firms having higher-paying jobs than workers from liquidated firms, explains 14 percentage points of the baseline effect. We provide evidence that reorganization improves earnings both for workers who stay in reorganized firms and for workers who leave for new employers, compared to the liquidation counterfactual.

After documenting this fact, we examine the main mechanisms that potentially determine workers' earnings and assess how these mechanisms influence the relationship between reorganization, liquidation, and earnings. We start by analyzing the role of human capital. Following the seminal work by Becker (2009), we distinguish between firm-specific human capital and general human capital. First, the positive effect on labor outcomes could result from the valuation of firm-specific human capital within and outside the firm. By working at the same employer for a long period of time, workers acquire skills that cannot be transferred to new jobs. While liquidation renders firm-specific human capital valueless, reorganization can preserve its continuation value. Second, the valuation of general human capital may also contribute to explaining the results. Skilled workers are more likely to leave distressed firms (Baghai et al. (2021)), and managers need to offer higher earnings to retain them. At the same time, workers with high general human capital, such as highly educated individuals, are on average more productive (Becker (2009)), thereby making them more valuable to the reorganized firm. Outside of the firm, the reallocation of workers to lower-quality jobs leads to skill depreciation (Burdett et al. (2020)), possibly causing persistent earnings losses. We provide evidence that firm-specific skills have a limited impact on the relationship between reorganization on earnings, whereas general skills have a significant impact.

To uncover the role of human capital, we analyze the relationship between worker characteristics, the probability that reorganized firms retain workers, and earnings growth within reorganized firms after the filing. Tenure at the firm, an indicator of firm-specific skills (Topel (1991)), is the factor that is most strongly associated with retention in reorganized firms. However, workers with longer tenure at the firm do not have higher earnings growth. Additionally, we find strong evidence for the effect of reorganization on earnings among workers with short employment tenure.

We observe the opposite relationship with education, a commonly used proxy for general skills (e.g., Lise and Postel-Vinay (2020)). Education is not a meaningful determinant of retention in reorganized firms. However, highly educated workers have higher earnings growth than other workers in reorganized firms. This evidence is consistent with reorganized firms needing to compensate these skills to retain workers after the filing. Additionally, we provide evidence that reorganization reduces transitions to low-skill occupations and increases transitions to high-paying jobs for high-skill workers, which is consistent with lower skill depreciation. Overall, we observe a strong relationship between reorganization and earnings for high-skill workers, but not for low-skill workers.

Job-match quality and bargaining are two key determinants of earnings in labor markets (Rogerson et al. (2005)) that potentially affect the relationship between reorganization and earnings. Due to search frictions, reorganization might improve job-match quality by substituting job search during unemployment with on-the-job search, providing workers more time to find high-quality matches. Reorganization may also grant workers increased bargaining power. If workers have access to a job in the reorganized firm, they possess an outside option, empowering them when negotiating with employers.

We adapt the methodology employed by Nekoei and Weber (2017) to investigate the relationship between the impact of reorganization on earnings, job match quality, and bargaining dynamics. We utilize average earnings at both the firm and occupation levels as indicators of match quality, while the difference between a worker's earnings and these averages serves as a proxy for bargaining power. Reorganization improves matching by allocating workers to jobs at firms and occupations characterized by higher average earnings. This effect arises at least partially from the effect of reorganization on transitions to new jobs, since reorganization increases the probability that workers move to new employers with high average earnings.

We leverage the existence of heterogeneity in terms of job search frictions to rule out external factors, beyond better job matches, that could explain higher earnings. We use the number of workers per occupation and region (labor market thickness) as a proxy for search frictions, since one should expect to see few job postings for rare occupations. The effects of reorganization on earnings, on the probability that workers transition to high-paying jobs in new employers, and on job match quality are particularly large for workers in labor markets with strong search frictions. We separate the effect of search frictions from the effects of low demand by using outside employment options as a proxy for demand. We find evidence that reorganization also has a strong effect on labor markets with low demand. Nonetheless, the proxies for search frictions and demand have negative but small correlation.

Our analysis does not yield substantial evidence supporting the notion that bargaining power contributes to explaining the influence of reorganization on earnings. These results on bargaining power are consistent with our previous findings, as reorganized firms often lay off a large portion of their workforce, making it impossible to stay in reorganized firms for many workers.

Policies that promote firm continuation might lead to an inefficient allocation of resources (e.g., Caballero et al. (2008)). Since reorganization is a policy that promotes firm continuation, we use firm data to assess whether reorganization creates a trade-off between higher earnings and excessive firm continuation. We consider the retention of workers in firms with low EBITDA/assets ratio and low labor gaps (the difference between the marginal revenue product of labor and the cost of hiring one additional worker) as indicators of excessive firm continuation. This is a tentative partial equilibrium analysis, as it does not consider additional impacts such as aggregate demand or competition effects.

We provide evidence of potentially excessive retention in reorganized firms. The average EBITDA/assets and labor gap of reorganized firms gradually increases after the filing. However, firms employing workers who leave reorganized firms have, on average, higher EBITDA/assets and labor gaps. Leveraging on the fact that earnings and the labor gap can be expressed in the same currency units, we compute the benefit-to-cost ratio of reorganization. For the whole sample, reorganization has a positive, large, and statistically significant effect on earnings and a negative but statistically insignificant effect on the labor gap. The benefit-to-cost ratio is 4.47. However, there is considerable heterogeneity across subsamples. For some workers, the cost of being allocated to firms with smaller labor gaps outweighs the benefit of higher earnings. For example, costs outweigh benefits for workers with less than high school (low general skills), labor market thickness at or above the median (low search frictions), and small pre-filing labor gaps (ex-ante low productivity).

The paper contributes primarily to the literature on the impact of bankruptcy and distress on labor outcomes. The closest studies are by Araújo et al. (2023) and Graham et al. (2023).[1] While Graham et al. (2023) find that workers affected by bankruptcy have lower earnings than comparable unaffected workers, Araújo et al. (2023) find that employees in bankrupt firms with pro-continuation bias (due to reorganization, dismissal of liquidation cases, automatic stays, and deadline extensions) experience lower post-bankruptcy earnings. In our setting, reorganization also leads to firm continuation, but unlike Araújo et al. (2023), who attribute the negative earnings effect to imperfect information and adjustment costs, we find a positive effect. This paper makes three key contributions to the existing literature. First, we analyze the mechanisms underlying the positive earnings effect of reorganization, focusing on human capital, job match quality, and bargaining power. Second, we isolate the pure effect of reorganization from other factors like automatic stays by comparing its impact on labor outcomes to that of liquidation. Finally, we make use of comprehensive firm data encompassing public and private firms operating in both manufacturing and non-manufacturing sectors. This allows us to analyze firm profitability, resource misallocation, and the benefits and costs of labor reallocation, which is lacking in most existing studies.

The paper establishes key differences between corporate reorganization and job displacement (e.g., Jacobson et al. (1993), Neal (1995), Robinson (2018), Raposo et al. (2019), Burdett et al. (2020), and Jarosch (2023)). The displacement literature typically compares outcomes for separated workers (those who lose their jobs) with outcomes for workers who remain in the same firm or for workers who are employed in other firms. In our setting, reorganization differs from pure displacement in two key ways. First, it acts as a financial shock that affects firm operations by influencing how workers transition to new employers. This leads to distinct outcomes for workers

[1] Other studies on labor markets, corporate bankruptcy, and distress include Brown and Matsa (2016), Berton et al. (2018), Babina (2020), Baghai et al. (2021), and Grindaker et al. (2021).

from reorganized firms and liquidated firms, even within the group of separated workers. Second, reorganization significantly transforms the firm, changing employment conditions for retained workers in ways that pure displacement does not. Therefore, the mechanisms that explain the effect of reorganization on worker outcomes differ fundamentally from the mechanisms that explain the effect of displacement.

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, with Section 6 forming the conclusion.

2 The Portuguese bankruptcy system

Across most legal systems, reorganization and liquidation are the two procedures that regulate bankruptcy (Djankov et al. (2008)).[2] With liquidation, the court system liquidates assets and distributes the proceeds to claimants according to a legally established priority schedule. With reorganization, firms negotiate the reallocation of resources with creditors. Whereas firms may shed a large portion of their labor and capital, reorganization seeks to keep firms open.

In 2012, Portuguese lawmakers introduced the Portuguese corporate reorganization system that we analyze in this paper. We provide a brief explanation of the system in this section. Section A of the Internet Appendix discusses the historical background and provides a more detailed explanation. Figure 1 outlines the reorganization process. Only firms (debtors) may file for reorganization in Portugal. Firms must file petitions where they are headquartered or engage in most of their business. They have three months to agree upon a reorganization plan with a majority of creditors. Cases are randomly assigned to judges who work in the same court. Judges intervene in the reorganization process to recognize creditor claims and to accept or reject reorganization plans. Bankruptcy managers oversee negotiations between firms and creditors without the intervention of the judge. Judges intervene only once to approve or reject reorganization plans. Bankruptcy regu-

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

lations 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. If the reorganization filing is unsuccessful, then the case might be dismissed or converted into a liquidation case. If the case is dismissed, then the firm operates as a going concern. Firms cannot submit new plans after an unsuccessful filing for two years.

3 Data

We gather data on bankruptcy filings from *Citius*, a repository of court documents maintained by the Portuguese government. In Section A of the Internet Appendix we provide more details about the process used to collect and treat the data, data coverage, and variable definitions.

We use filings between 2012 and 2016 and court documents between 2012 and 2018, covering 6,680 cases. The dataset contains the cases of other institutions such as firms without employees, trusts, independent workers, households, associations, and condominiums. Even though these cases are not in the sample, we use them to estimate the tendency of judges to accept reorganization plans.

We use firm-level data from *Sistema de Contas Inegradas das Empresas* (SCIE), 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 active firms in Portugal. We use annual data between 2008 and 2018.

We obtain worker data from *Quadros de Pessoal*, an employer-employee matched dataset maintained by the Portuguese Ministry of Social Security, between 2008 and 2018. This dataset covers all employers regulated by the Portuguese labor code with at least one employee in October, the reference date of the administrative dataset. *Quadros de Pessoal* provides information on workers such as earnings, education, occupations, and hierarchies. We define these variables in Section B of the Internet Appendix.

Quadros de Pessoal does not cover the universe of jobs in Portugal. Specifically, it does not cover independent workers who are not owners of firms with at least one employee and covers government workers only partially. Additionally, the dataset only provides employee earnings. Therefore, it provides employment data, but not earnings data for employers, furloughed workers, and workers on leave. In Section 5 we bound estimates, considering other sources of earnings, and the possibility that workers switch to unobserved jobs. For that purpose, we use *Inquérito ao Emprego* (Labor Force Survey), an employment survey, to estimate the percentage of jobs not reported in the sample, and *Inquérito às Condições de Vida e Rendimento* (Statistics on Income and Living Conditions), a survey on living conditions, to estimate earnings for these jobs. Moreover, we use firm data from SCIE to estimate earnings for employers and data from previous years for furloughed workers and workers on leave. We provide more information about these datasets and about the procedure used to estimate employment and earnings in Section C.1 of the Internet Appendix.

We detect reporting gaps in the employer-employee matched dataset. First, some firms do not submit data to *Quadros de Pessoal* in the last period before the filing (9% of all observations), possibly because they are closed in the following year, when they should submit employee records. Additionally, some firms fail to report worker data after the filing but report having more than 100 workers in their financial statements (1% of all observations). We discuss the procedures used to address these reporting gaps in Section C.1 of the Internet Appendix.

We complement the analysis with information from the *Occupational Informational Network* (O*NET), a survey of occupation characteristics. With this data, and adapting the procedure employed by Lise and Postel-Vinay (2020), we rank occupations by cognitive, interpersonal, and manual skill requirements. We use this information to evaluate transitions to new occupations after reorganization and liquidation. We provide more information about O*NET, and about the methods used to construct the skill requirement variables and to link these variables to *Quadros de Pessoal* data in Section C.2 of the Internet Appendix.

The final dataset consists of matched employer-employee data between 2008 and 2018, and results from merging *Quadros de Pessoal*, firm data, O*NET, and the bankruptcy dataset retrieved from *Citius*.

3.1 Descriptive Statistics

Table 1 shows descriptive statistics. Column (1) depicts statistics for the whole sample, column (2) for reorganized firms, and column (3) for the remaining firms (liquidations and dismissed cases). The sample has 47,642 workers and 2,471 firms. We follow common practice in the literature (Couch and Placzek (2010)) and exclude workers who, before the filing, were less attached to firms or more likely to continue their education or retire early. Therefore, we exclude workers who were employed part time, earned less than the minimum wage, were foreign nationals, or were under 23 or over 50 years of age. We winsorize ratios and estimated quantities (e.g., labor gap) at the 5% level.

Panel A of Table 1 depicts reorganization case outcomes. The case outcome is reorganization when the reorganization filing is successful. This is the outcome for 59% of the cases. When firms are not reorganized, they are liquidated or the case is dismissed. We classify the case outcome as a liquidation if the firm files for liquidation or ceases to exist in the year of the filing or in the subsequent year.[3] In 36% of all cases firms are liquidated. Dismissal is the final outcome for less than 5% of all cases.

Panel B of Table 1 reports firm descriptive statistics. On average, firms with accepted reorganization plans are larger, have more workers, are more profitable, and are better capitalized. We also measure the labor gap for firms in the sample, given by the difference between the marginal revenue product of labor and the cost of hiring an additional worker. We provide a more detailed definition of this variable in Section B of the Internet Appendix. The procedure used to estimate the marginal revenue product of labor follows Lenzu and Manaresi (2019) and Gandhi et al. (2020), and is explained in Section D of the Internet Appendix.

In the absence of frictions, firms should hire workers up to the point where revenue equals cost. However, firms in the sample seem to be constrained, as the mean labor gap is positive. Reorganized firms have a slightly greater labor gap than other firms.

The second part of the table presents descriptive statistics for workers. Compared to workers from non-reorganized firms, those from reorganized ones have higher earnings, are less likely to be

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

female, are slightly younger, and have slightly shorter firm tenures. In Table A1 of the Internet Appendix we report the distribution of workers and firms that file for reorganization by industry.

4 Identification strategy

4.1 Empirical model

The baseline specification:

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

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

In Section E of the Internet Appendix, we estimate equation (1) using ordinary least squares (OLS). However, selection might affect the estimation of β because reorganization is a choice made by firms, creditors, and judges. Bias might run in both directions. Some firms might not be reorganized because they face poorer prospects. At the same time, reorganization is more prevalent among capital-intensive firms (Kermani and Ma (2020)), for which employees' earnings are more likely to be tied to capital-labor complementary (Fonseca and Van Doornik (2022)). Also, practitioners cite filing early as an important driver of a successful reorganization plan (Pinheiro (2013)). Earnings losses happen partially before bankruptcy (Graham et al. (2023)) and should affect more intensely firms that file for reorganization at a later date. These concurring effects are visible from descriptive statistics shown in Table 1. Reorganized firms are larger, more profitable and productive, and their workers have higher earnings.

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

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

$$Reorganization_{i,t} = \rho + \pi Z_{i,j,c,t} + \delta X_{e,i,t} + \delta_{c,t} + \xi_{e,i,t} \tag{2}$$

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

We include subscript i in variable $Z_{i,j,c,t}$ because the variable is a leave-one-out instrument. Therefore, we observe multiple values for Z for the same judge j in court c and year t. We compute judge j's tendency to accept reorganization plans at court c in a given year t with the following leave-one-out measure of judge leniency, as has been done previously in the literature (e.g., Dobbie and Song (2015), Gupta et al. (2016)):

$$\begin{aligned} Z_{i,j,c,t} = \frac{1}{n_{c,j,t}-1}\Big[\Big(\sum_{b=1}^{n_{cjt}} Reorganization_{b,t}\Big) - Reorganization_{i,t}\Big] - \\ \frac{1}{n_{c,t}-1}\Big[\Big(\sum_{b=1}^{n_{ct}} Reorganization_{b,t}\Big) - Reorganization_{i,t}\Big] \end{aligned} \tag{3}$$

Here, $n_{c,j,t}$ is the number of cases for court and judge identifiers c and j in year t. $\sum_{b=1}^{n_{cjt}} Reorganization_{b,j,c,t}$ stands for the total number of accepted reorganizations for court and judge identifiers c and j in year t. $n_{c,t}$ is the total number of filings for court identifier c and year t. $\sum_{b=1}^{n_{ct}} Reorganization_{b,t}$ is the total number of accepted reorganizations for court identifier c and year t. Intuitively, the formula computes the difference between the average reorganization rate for judge-court pairs c and t in year t, minus the average reorganization rate for court c in year t, excluding case i from the calculation.

Figure 2 depicts the distribution of $Z_{i,j,c,t}$. Our sample consists of cases filed in courts for which we can compute $Z_{i,j,c,t}$ for more than one judge identifier j. On average, in each year cases are distributed across 22 court and 129 judge identifiers.[4] Firms must file for reorganization in the jurisdiction where they are headquartered or conduct most of their business. We assume that leniency is equal to the estimated value at the court level whenever there are insufficient data to compute it. Alternatively, in Section E of the Internet Appendix we drop cases for which there are not enough data. We compute the instrumental variable for case i using cases filed both before and after the filing date of the case. Alternatively, we compute the instrumental variable by using only filings that occur before filing i in Section E of the Internet Appendix.

The second stage equation is given by the following expression:

$$Y_{e,i,\tau} = \alpha + \beta \widehat{Reorganization}_{i,t} + \gamma X_{e,i,t-1} + \delta_{c,t} + \epsilon_{e,i,\tau} \tag{4}$$

where $\widehat{Reorganization}_{i,t}$ gives the predicted values from equation (2). In all regressions, we cluster errors at the court-year level. Alternatively, we cluster errors at the court level in Section E of the Internet Appendix.

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

In some of the analysis, we create subsamples using time-varying worker characteristics observed after the reorganization filing. For example, we separately assess the effect of reorganization on workers who remain in the same firm and on workers who move to jobs in new employers after the filing. In such cases, the interpretation of the coefficients from equation (4) is not causal because the independence assumption of the instrumental variables model does not hold. The independence assumption requires that the assignment of the instrumental variable is as good as random. However,

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

with sample selection, we are comparing two groups that have a different response to treatment (reorganization). Consider the case where we compare workers from reorganized firms who stay in the same firm with workers from liquidated firms. The treatment group contains only the workers who stay in the same job in the presence of reorganization, whereas the control group contains workers who would stay in the same job and workers who would leave the firm in the presence of reorganization. The average treatment effect is different in the control group and in the treatment group because of selection, violating the independence assumption. In such cases, we estimate the reduced form relationship between the instrumental variable $Z_{i,j,c,t}$ and the outcome of interest $Y_{e,i,\tau}$ by estimating the following model:

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

where $Z_{i,j,c,t}$ is the instrumental variable obtained using the expression from equation (3).

Finally, we study the relationship between the effect of reorganization on labor outcomes and sample characteristics, estimating the following second-stage equation:

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

where $\widehat{Reorganization_i}$ and $\widehat{Reorganization_i \times} Condition_{k,c,t-1}$ are predicted using $Z_{i,j,c,t}$ (judge leniency) and $Z_{i,j,c,t} \times Condition_{k,c,t-1}$ as instrumental variables. The variable $Condition_{k,c,t-1}$ is an indicator equal to one if the worker observes a certain condition based on sample characteristics (e.g., having tenure at the firm at or above the median) in the year of the filing. k is equal to one if the condition is met and zero otherwise. We use $\delta_{k,c,t}$ fixed effects, i.e. fixed effects for each condition-court-year group. For example, if we split the sample at the median by labor market thickness then, for each court-year pair we have two separate dummy variables, one for individuals with labor market thickness below the median, and another one for individuals with labor market thickness at or above the median. By including $Condition_{k,c,t-1} \times X_{e,i,t-1}$, we allow the relationship between the dependent variable and controls to differ between workers who have $Condition_{k,c,t-1} = 1$ and the remaining workers.

4.2 First stage estimates

Table 2 presents results from estimating equation (2). 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 percentage point more lenient judge is associated with a 0.362 percentage point higher reorganization rate.

Following the literature (e.g., Bernstein et al. (2019a), Bernstein et al. (2019b)), we weight all regressions by the inverse of the number of workers in each firm, which ensures that results are not driven by some very large firms in the sample. Alternatively, in Section E of the Internet Appendix we estimate the model with unit sample weights instead of firm weights. The estimates are similar with firm weights and unit weights, but are statistically less significant with unit weights. This difference is expected because in the analysis with unit weights very large firms have more weight, and average judge leniency is less likely to be informative about very large cases, which are abnormal, than about regular cases.

We analyze the model's identification assumptions. The instrument F-statistic in Table 2 is 35 (34.99 without controls in column (1), and 35.16 with controls in column (2)), well above the oft-cited threshold of 10 for weak instruments (Staiger and Stock (1997)).

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

We provide additional evidence of random judicial assignment using the following equation:

$$Z_{i,j,c,t} = \alpha + \theta X_{e,i,t-1} + \varepsilon_{e,i,t} \tag{7}$$

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

Table 3 shows estimates for equation (7). In column (1) we include the set of firm and worker controls that we also use in equation (2). In columns (2) to (12) we do pairwise regressions of the instrument on each of the controls. Column (1) differs from columns (2) to (12) because in column (1) we regress the instrumental variable on all other variables together, while in columns (2) to (12) we regress the instrumental variable on each variable separately. At the bottom of column (1), we also report a joint significance test for the industry fixed effects. There is no evidence that the instrumental variable is correlated with worker and firm characteristics. None of the variables are statistically significant, and coefficients are small. In Table A2 of the Internet Appendix, we repeat the analysis for time-changing variables two and three years before the filing, and do not find evidence of pre-trends.

In the main analysis, we construct the instrumental variable using filings by firms that are not in the sample of firms included in Table 1 (e.g., firms without employees), and by other entities such as individuals. However, there are factors that judges might consider in these filings that they do not consider in the filings of firms included in the sample, making the instrument less informative. To make sure that the inclusion of these filings is not a concern, in Table 3 we present empirical evidence demonstrating the consistent behavior of judges across different types of cases. We include the variable *% firms in the sample* (columns (1) and (12)), representing the percentage of cases assigned to the judge in the year of the filing that are part of the sample of firms included in Table 1. Our findings indicate an absence of correlation between this variable and judge leniency. As an alternative approach, we compute leniency using equation (3) exclusively based on court cases involving firms within our sample. The correlation between the primary instrumental variable and the alternative instrumental variable stands at 70%. We present the results derived from this alternative instrumental variable in Section E of the Internet Appendix, revealing consistent outcomes.

Another potential limitation is that the instrument might be associated with improved labor outcomes because of factors that are not related to reorganization. First, the allocation of reorganization cases to more lenient judges may improve the quality of reorganization plans. Second, subsequent liquidation cases might be allocated to judges who supervised the previous reorganization case, and judicial decisions during the liquidation phase may affect worker outcomes. In Table A3 we estimate the relationship between filer and worker outcomes – firm survival, employment at firms that file for reorganization, and earnings growth – and judicial leniency, separately for reorganized firms and for firms that are not reorganized. In Panels A, B, and C we report coefficients for all firms, for reorganized firms, and for the remaining firms, respectively. In Panel A we see a positive and strong relationship between judge leniency and the number of years the firm stays alive, the number of years workers stay in firms that file for reorganization, and earnings growth. As expected, there is a strong and positive correlation between these outcomes and the instrument for the sample that has all firms. However, we do not observe these strong relationships in each of the subsamples. These results suggest that the random allocation of judges does not have a large impact on the quality of reorganization plans. These findings also suggest that the results are not driven by decisions taken in subsequent liquidation cases.

We must assume monotonicity to interpret our two-stage least square estimates as the local average treatment effect (LATE) of reorganization on worker outcomes. The monotonicity assumption requires that plans accepted by a strict judge would also be accepted by a more lenient judge. Likewise, plans rejected by lenient judges would also be rejected by a stricter judge. We provide evidence to support the monotonicity assumption by running the first-stage regression on subsamples of the data. Table A4 of the Internet Appendix shows estimates of first-stage coefficients for these subsamples. We split the data according to observable characteristics. We divide the sample into two groups, at the median, using worker and firm characteristics. Consistent with the monotonicity assumption holding, coefficients are positive and significant for each of the subsamples. The first stage coefficient is larger in subgroups that contain a larger percentage of compliers (e.g., Dahl et al. (2014)). We compare the first stage coefficient in the subsamples of Table A4 of the Internet Appendix. Coefficients are relatively similar across all subgroups, but compliers tend to have more years of education.

One additional concern is the possibility that firms predict the assignment of cases to judges, and time their filing to get a more lenient judge. An example of this concern affecting the validity of the instrument is the empirical setting described by Hüther and Kleiner (2022), who show that unsecured hedge fund creditors in the US predict the assignment of cases to judges and sway the timing of bankruptcy filings to get more reorganization-friendly judges. They propose a correction for this type of violation to random assignment, which we apply to our setting in Section E of the Internet Appendix. We find no evidence of a similar phenomenon in Portugal and provide alternative estimates using an empirical model similar to the one suggested by Hüther and Kleiner (2022).

In Section E of the Internet Appendix we also provide evidence that the findings are robust to alternative specifications: obtaining the instrumental variable using only cases that were filed before the case for which we are computing the instrumental variable; computing bootstrap standard errors that correct for the estimation error in judge leniency; obtaining the instrumental variable with a split sample to avoid mechanical correlations that could arise from the interaction between court fixed effects and the instrumental variable; averaging out worker-level variables and estimating the model at the firm level; defining the instrumental variable in equation (3) in absolute terms, instead of subtracting court-level leniency. All the exercises show that the results on the first stage are robust to alternative specifications.

5 Results

5.1 Labor outcomes

Table 4 shows estimates of the impact of reorganization on bankruptcy outcomes using the model from equation (4). As discussed in Section 2, liquidation and dismissal are complementary to reorganization. Table 1 shows that dismissal occurs in less than 5% of all cases. In Table 4, we demonstrate that liquidation, rather than dismissal, is the counterfactual outcome of reorganization. A 1 percentage point increase in the probability of reorganization leads to an approximate 1 percentage point decrease in the probability of liquidation (column (1)). In column (2) the effect of reorganization on dismissal is close to 0. Based on these estimates, we assume throughout the rest of the paper that liquidation is the counterfactual outcome of reorganization. Another alternative approach would be to exclude dismissals from the sample, as it has been done in other empirical studies of bankruptcy in the past (e.g., Bernstein et al. (2019b), Antill (2022)). In Section E of the Internet Appendix, we estimate the empirical model excluding dismissed cases and obtain similar results.

Table 4 also measures the effect of reorganization on the survival of the firms where workers are employed before the filing (columns (3) and (5)), and on the probability that workers stay in these firms (columns (4) and (6)). We measure outcomes in the short term and in the long term. We define the short term as the first year after the filing. For the long term, we define year 5 as the last year of the analysis, or the last observed year whenever the sample does not cover five years of data. In columns (3) and (4) we report estimates for the short term. In columns (5) and (6) we report estimates for the long term. Reorganization increases firm survival by 28 percentage points and retention in the same firm by 24 percentage points. These results are not surprising, as reorganization plans often mandate managers to keep firms open. The effect persists over time, as firm survival increases by 43 percentage points and worker retention by 29 percentage points in the long term. These results confirm that reorganization has a material and 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 percentage points.

Figure 3 shows how workers from reorganized firms compare with workers from similar liquidated firms and with workers from a benchmark sample in terms of job retention (Panel A) and firm survival (Panel B). We match each worker from the sample of reorganized firms with five years of data after the filing with a worker from the sample of liquidated firms and with a worker from a benchmark. This benchmark contains all workers in *Quadros de Pessoal* without employment records at firms included in the sample either before or after the filing. Concerning liquidated firms, we choose as the match the worker with the nearest pre-filing earnings, among firms that file for reorganization in the same year, are in the same industry, and are in the same EBITDA/assets quintile. For the benchmark, we choose the worker with the nearest pre-filing earnings, among firms that are in the same industry in the year before the filing. We drop workers from reorganized firms without matched workers in liquidated or benchmark firms. By year 5, only 23% of the workers from reorganized firms remain in the same job vs. 59% of the workers from the benchmark sample.

Likewise, the firm remains open for 52% of the workers from reorganized firms, less than the 85% firm survival rate for the benchmark. Figure A1 of the Internet Appendix shows that reorganized firms reduce their workforce on average by more than firms in the benchmark, and only in a few cases reorganized firms remain with the same workforce. In fact, only for 24% of the workers from reorganized firms we do not see employment reductions of the initial workforce by more than 50%. This value contrasts with 65% for the benchmark sample.

Firm outcomes influence when and how workers exit reorganized and liquidated firms. Table 5 establishes the causal effect of reorganization on job transitions using equation (4). In column (1) we show that workers stay in the same job by one more year because of reorganization. Reorganization might increase the number of years workers stay in the same job because workers do not leave reorganized firms, not because workers leave reorganized firms later. However, we provide evidence that it is not the case. In column (2) we estimate the effect of reorganization on the probability that workers leave their job in years 1-3, and in column (3) we estimate the probability that workers leave in year 4 or later. Reorganization reduces the probability that workers leave in the first three years, and increases the probability that they leave at a later period.

We categorize worker exits as layoffs, job quits, and establishment transfers. We do not observe these outcomes directly in the data, therefore we estimate them. We categorize an exit as being a layoff if the worker leaves the firm and the firm does not hire any worker in the same occupation after the filing. We classify an exit as being an establishment transfer if the establishment is transferred to another firm and the worker stays in that establishment at the time of the transfer. Job quits are exits not categorized as layoffs nor as establishment transfers. We provide more details about the construction of these outcomes in Section B of the Internet Appendix. In columns (4) to (6) we show that reorganization reduces the probability that workers are laid off by 37 percentage points and increases the probability that workers quit by 9 percentage points. We find a small and statistically insignificant effect on establishment transfers. Therefore, reorganization changes both when and how workers leave their job.

We illustrate exit patterns of workers from reorganized and liquidated firms, and compare them to the workers from the benchmark in Figure A2 of the Internet Appendix. Panel A depicts the relative frequency of firm exits by year. In the first two years after the filing, workers from reorganized firms are on average less likely to leave their job than workers liquidated firms, but more likely to leave than workers from the benchmark. In Panel B the layoff rate for workers from reorganized firms (56%) is lower than the rate for workers from liquidated firms (93%), but higher than the rate for workers from the benchmark (28%). The quit rate at reorganized firms (20%) is similar to the rate at benchmark firms (17%), and higher than the rate at liquidated firms (4%). Establishment transfers represent a small share of worker exits, even though many reorganized and liquidated firms engage in establishment transfers. [5] However, establishment transfers only affect 2% of the workers from reorganized and liquidated firms directly. These findings highlight an important difference between reorganization and displacement. Displaced workers leave the firm, and non-displaced workers mostly stay in the same job. When we compare reorganization with liquidation, we observe that few workers from reorganized firms stay in the same job. However, they leave their jobs differently from workers at liquidated firms, taking more time to leave and being less likely to be laid off.

Reorganization and liquidation also influence employee earnings. In Figure A3 of the Internet Appendix, we depict the difference between the earnings and employment trajectories of workers from reorganized and comparable liquidated firms, and the trajectories of the workers from the benchmark group. Panel A depicts average earnings for employed workers, and Panel B depicts the employment rate, i.e. the percentage of workers with records in *Quadros de Pessoal*. Workers from liquidated firms have lower employment and earnings growth than workers from reorganized firms, even though the employment gap narrows over time. Importantly, while employed workers from reorganized firms have lower earnings losses, they still earn 9% less than workers from the benchmark. Therefore, we speak of lower earnings losses instead of higher earnings caused by reorganization.

Table 6 establishes the causal effect of reorganization on earnings. Columns (1) to (3) provide outcomes for the short term, and columns (4) to (6) provide outcomes for the long term. We measure the effect of reorganization on earnings in columns (1) and (4), and split it into an extensive margin component (effect on employment) in columns (2) and (5), and an intensive margin component

[5] 22% of the workers from the reorganization sample and 9% of the workers from the liquidation sample come from firms with at least one establishment transfer.

(earnings of employed workers) in columns (3) and (6). Reorganization persistently reduces earnings losses. In the year of the filing, employees from reorganized firms have 16% lower earnings losses. However, estimates are not statistically significant for the first year, and employment represents 70% of this effect,[6] which suggests that employment is the driver of earnings growth in the short term.

In the long term, reorganization decreases earnings losses by 20% of pre-filing earnings. Most of the earnings effect in the long term arises from the intensive margin (14%). The effect of reorganization on employment in the long term is relatively small (6%) and statistically not significant. While in the long term workers from liquidated firms move to other jobs, they still have considerably lower earnings. In Figure A4 of the Internet Appendix, we repeat the analysis of Table 6 separately for each of the years that surround the reorganization event. As expected, before reorganization there is no economically meaningful relationship between reorganization and earnings. After reorganization, we obtain positive estimates consistently over the years. This effect remains even as workers find other jobs.

The main analysis uses earnings from *Quadros de Pessoal*. However, this dataset only covers compensation paid to employees and does not cover some types of employment, as explained at length in Section C.1 in the Internet Appendix. In the main analysis we assume that missing earnings are equal to 0. Table A5 of the Internet Appendix provides estimates with alternative assumptions, using procedures that are similar to those previously used in the literature (e.g., Walker (2013), Graham et al. (2023)). Using different assumptions, the effect of reorganization on earnings ranges from 12% to 22%.

In Panel A of Table A5 of the Internet Appendix, we replace earnings for workers with no job with either the last earnings recorded before the reorganization filing or earnings from the 20^{th} and 80^{th} percentiles of the distribution for workers who earn at least the minimum wage. As discussed in Section C.1 of the Internet Appendix, we only observe jobs in October each year, and this approach allows us to test the sensitivity of results to the possibility that workers without October records are employed at other times. We also replace missing earnings with estimates from other data sources.

[6] We divide the estimate from column (2), 0.112, by the estimate from column (3), 0.159, to obtain 70%.

For workers without records in *Quadros de Pessoal*, we use the *Inquérito ao Emprego* to estimate their employment status, and then the *Inquérito às Condições de Vida e Rendimento* (ICOR) and unemployment insurance rules to estimate earnings by employment outcome. Data from SCIE and previous periods in *Quadros de Pessoal* are used to obtain missing earnings for employers, furloughed workers, and workers on leave. We explain this procedure at length in Section C.1 of the Internet Appendix.

In Panel B of Table A5, we measure the effect of reorganization on the earnings of employed workers, correcting for selection into employment using the procedure described in Section F of the Internet Appendix. Additionally, we use hourly earnings instead of monthly earnings, and separate the effect of reorganization on earnings into a fixed component and a variable component, showing that higher earnings arise mostly from the fixed component.

Bernstein et al. (2019b) show that reorganization increases asset utilization by 32% compared to liquidation in the United States. Antill (2022) shows that allocating bankruptcy cases to the outcome that maximizes credit recovery, which mostly implies substituting liquidation with reorganization, increases credit recovery by 12 cents on the dollar. Our estimates are of the same order of magnitude as these estimates, implying that the effect of reorganization on labor outcomes has comparable quantitative relevance to the effects on asset reallocation and credit claims.

In Table A6 of the Internet Appendix we provide estimates for the effect of reorganization on cumulative earnings (earnings between the year of the filing and the last year of the analysis), transitions into new occupations and industries. We find that reorganization increases cumulative earnings mostly through the intensive margin. Reorganization increases the probability that workers stay in the same occupation and in the same industry.

We show that reorganization is associated with higher earnings both for workers who stay in the same job and for workers who move to a new job. In Table 7 we estimate equation (5) to measure whether reorganization is correlated with smaller earnings loss for all employed workers (columns (1) and (2)), for workers who stay in reorganized firms (columns (3) and (4)), and for workers who leave for new employers by the last year of the analysis (columns (5) and (6)). As we explain in Section 4, estimates from Table 7 are not causal because we use employment in the same firm as a condition to create subsamples of the data, and this condition is affected by reorganization (see Table 4). However, they are useful to understand earnings outcomes for workers who stay in reorganized firms and for workers who leave for new employers.

We establish that the instrument has a positive correlation with earnings for workers who remain in reorganized firms and for workers who leave to new employers, after reorganization, but not before. In columns (1), (3), and (5) of Table 7, we estimate the correlation between the instrument and earnings growth before the filing for employed workers, workers who remain at the same firm, and workers who leave to new employers. In columns (2), (4), and (6) we estimate the correlation between the instrument and earnings growth in the last year of the analysis. After reorganization, the instrument and earnings growth have a positive correlation in all three groups. Figure A5 in the Internet Appendix estimates equation (5) year-by-year, both for workers who stay in the same firm (Panel A) and for workers who leave to new employers (Panel B). Consistent with the findings from Table 7, we only obtain a positive relationship between the instrument and the earnings variable after the filing, but not before.

Table 7 provides suggestive evidence that reorganization reduces earnings losses for workers who move to new employers, but does not establish any causal link. We establish that causal relationship with Figure 4, where we depict coefficients from estimating the effect of reorganization on job transitions by earnings quintile using the model from equation (4). We compare the earnings of each worker who has a job after the reorganization filing against the earnings distribution for workers in Portugal who earn at least the minimum wage.[7] We create indicator variables $\{\mathbb{1}_e^{job,Q}, \mathbb{1}_e^{job\ new\ employer,Q}\}$ 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 earnings distribution. The variable $\mathbb{1}_e^{job,Q}$ is equal to 1 when worker e has a job in quintile Q of the earnings distribution. The interaction $\mathbb{1}_e^{job\ new\ employer,Q}$ is equal to 1 when worker e has a job in quintile Q of the earnings 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

[7]Note that we use earnings measured *after* the reorganization event, not before. Table A7 of the Internet Appendix provides point estimates for all employed workers (Panel A), for workers who stay in the same firm (Panel B), and for workers who move to a new employer (Panel C).

to new employers because some workers may choose to stay in reorganized firms. However, we find evidence that reorganization decreases transitions at low earnings quintiles but not at high quintiles. In fact, reorganization has a positive effect on the probability that workers find jobs with new employers in the highest earnings quintile. These findings reveal differences between reorganization and displacement. Displacement primarily determines earnings outcomes through separation from the firm. With reorganization, most workers still leave the firm at some point. However, these separations impact earnings differently in reorganized and liquidated firms.

5.2 Mechanisms

Why does reorganization lead to lower earning losses? To address this question, we investigate the factors that contribute to the positive effect of reorganization on employee earnings. We start by analyzing the role of human capital. Becker (2009) argues that human capital and employee earnings are positively related because earnings are tied to labor productivity, and human capital increases productivity. Following Becker (2009), we divide human capital into two forms: firm-specific human capital, or the human capital that workers cannot bring to other jobs, and general human capital, or the human capital that workers can use if they move to a job in a new firm. We use the number of years of employment at the firm (firm tenure) to measure firm-specific human capital, and education to measure general human capital. The usage of these variables to measure human capital is standard (e.g., firm tenure by Topel (1991) and education by Lise and Postel-Vinay (2020)), making it easier to contextualize the empirical analysis in the existing literature.

In Figure 5 we analyze the relationship between human capital accumulation, job retention and earnings growth in reorganized firms using the following equation:

$$y_{e,i,\tau} = \alpha_i + \gamma X_{e,i,t-1} + \epsilon_{e,i,\tau} \tag{8}$$

where $y_{e,i,\tau}$ is the dependent variable (an indicator variable set to 1 if the worker continues in the reorganized firm, or earnings growth for workers who remain in reorganized firms). $X_{e,i,t-1}$ is a vector of standardized worker characteristics akin to those in column (2) of Table 2. We standardize these independent variables to ensure comparability of the coefficient estimates portrayed in Figure 5. α_i are filing fixed effects, which absorb the contribution of firm characteristics to the determination of the dependent variable.

Panel A of Figure 5 depicts the relationship between worker characteristics and the probability that workers remain in reorganized firms. A one standard deviation increase in worker tenure corresponds to a substantial increase of 11 percentage points in the probability of workers remaining employed in reorganized firms. This coefficient substantially exceeds estimates associated with other worker characteristics. Consequently, this proxy for firm-specific capital emerges as a pivotal determinant of worker retention in reorganized firms. We do not see evidence that more educated workers, who have more general human capital, are more likely to stay in reorganized firms. In Table A8 of the Internet Appendix, we report descriptive statistics for the workers who remain in reorganized firms and for the workers who leave. Tenure at the firm also stands out as having different average values across the two subsamples. We do not find evidence of differences between the subsamples for education.

In Panel B of Figure 5, we analyze the relationship between earnings growth in reorganized firms and worker characteristics. We do not find evidence that longer-tenured workers have higher earnings growth. However, a one standard deviation increase in education is associated with a 3% increase in earnings. This evidence is consistent with reorganized firms needing to increase compensation to retain general skills, but not to retain firm-specific skills. It is also consistent with the setting proposed by Becker (2009), where firms remunerate firm-specific human capital to avoid job quits, as these quits imply investments in hiring and training new workers. When reorganized firms go through significant restructuring and layoff a large part of their workforce (Figure A2 of the Internet Appendix), there is little relative demand for firm-specific capital relative to supply, and firms do not need to provide additional compensation for it. General human capital is disputed by other firms, therefore reorganized firms must compensate for it.

In Figure 6 we explore further the relationship between education and earnings growth after reorganization. Light bars represent the difference between earnings growth for workers who quit and for workers who are laid off. Dark bars represent the difference between earnings growth for workers who remain in the same firm and workers who are laid off. If firms value general skills, and reorganized firms compete for them, then the earnings premium should increase with education. We observe this pattern both for workers who remain in reorganized firms and for workers who quit, corroborating the findings from Figure 5.

In Table 8 we split the sample by firm tenure (columns (1) and (2)) and by education (columns (3) and (4)).[8] We find that reorganization has a positive, large, and statistically significant effect on earnings growth for workers with firm tenure below the median (i.e., tenure of four years or less). The relationship between earnings and reorganization is weak for workers with tenure above the median. The weaker first stage for workers with high tenure and the existence of confounders of tenure such as age might influence the estimates. However, the strong relationship between earnings and reorganization for workers with low tenure, together with the evidence from Figure 5, imply that firm-specific capital is not the key factor that explains the relationship between reorganization and earnings. For education, we find that reorganization has a large and positive effect on earnings for workers with high school or more education, and a small and statistically insignificant effect for workers with less than high school, which is consistent with general skills contributing to the effect of reorganization on earnings.

In Table 8 we show that reorganization has a strong effect on the probability that workers transition to jobs in new employers in the highest quintile of the earnings distribution, among the workers who completed high school or more, but not for the remaining workers. We provide evidence that reorganization reduces the depreciation of human capital, contributing to this effect. We classify occupations by skill intensity using data from O*NET, and rank occupations in terms of cognitive, manual, and interpersonal skill requirements using the procedure described in Section C.2. Table A10 of the Internet Appendix shows the effect of reorganization on the probability that workers move to occupations that are higher or lower-ranked in comparison to the pre-filing occupation in terms of skill requirements. Reorganization reduces transitions to occupations with lower cognitive skill requirements. This effect could be expected because reorganization decreases the probability that workers move to different occupations, some more skill-intensive, others less.

[8] Table A9 of the Internet Appendix presents an alternative analysis to sample splits. This analysis estimates equation (6) with the variable used to split the sample as the interaction variable. Panel A differs from Panel B because the former interacts court-year fixed effects with the sample split variable, while the latter uses court-year fixed effects without interactions.

However, reorganization does not have a symmetric effect on decreasing transitions to more skill-intensive occupations. We verify similar effects for workers who completed high school or more. For workers who did not complete high school, the effect is symmetric: reorganization reduces transitions to occupations with both lower and higher skill requirements.

In Table A11 of the Internet Appendix, we measure the effect of reorganization on the ranking of the occupations in terms of skill requirements, i.e. we use a continuous variable instead of indicator variables. We obtain positive estimates for the effect of reorganization on the continuous ranking for the full sample, but estimates are noisy. Estimates for workers who completed high school (or more) are larger and statistically significant at the 10% level, whereas estimates for workers who did not complete high school are smaller and statistically insignificant.

In Table A12 of the Internet Appendix we split the sample by hierarchical levels. We explain how we obtain hierarchies in Section B of the Internet Appendix. We find that reorganization has a large and positive effect on earnings for workers at high hierarchical levels and a small and statistically insignificant effect on workers at low hierarchical levels. Figure A6 of the Internet Appendix depicts the difference between average earnings for workers who stay on the same firm or quit, and average earnings for workers who are laid off from reorganized firms. In general, higher hierarchical levels receive higher earnings premiums. We observe that middle managers and top managers have on average higher premiums for staying at reorganized firms or for quitting than professionals. Professionals have higher premiums than other employees. These findings are consistent with interpreting managerial skills as a form of general human capital, since reorganization increases earnings at higher hierarchical levels both for workers who quit and for workers who remain in reorganized firms.

Job-match quality and bargaining are other two crucial determinants of earnings in labor markets (Rogerson et al. (2005)). Search frictions in labor markets prevent immediate matching of demand and supply. Workers experience wait times for vacancies, which offer varying levels of match quality. Reorganization might influence the matching process by enabling workers to search on-the-job instead of during unemployment, potentially leading them to find higher-quality matches. Additionally, reorganization may enhance workers' bargaining power during negotiations with firms by providing them with a job in the reorganized firm as an outside option.

We use the methodology employed by Nekoei and Weber (2017) to test whether match quality or bargaining contribute to the effect of reorganization on earnings. In Table 9 we decompose earnings growth into a firm component and a residual. In column (1) the dependent variable is the average earnings at the firm where the worker is employed, over their pre-filing earnings. We find that reorganization has a positive effect on this earnings component, which is consistent with reorganization allowing workers to find firms with higher earnings. In column (2) the dependent variable is the difference between the worker's earnings and the average earnings at the firm level, divided by their earnings before the filing. This is a proxy for bargaining (Nekoei and Weber (2017)). The estimate in column (2) is smaller and noisier than the estimate in column (1), hence we do not find evidence that reorganization affects bargaining. These findings are consistent with the job search behavior of workers. Reorganization increases job quality because workers can search for new employment while employed on a job. However, reorganized firms go through extensive restructuring and are not long-term employment alternatives for most workers, hence not providing workers significant bargaining power when negotiating job offers.

The average earnings at the firm might not represent the typical earnings the worker would get at the firm, as jobs with different functions have different earnings. To address that problem, in column (3) of Table 9, we obtain the earnings of comparable workers at the firm by taking the average earnings of the workers that are in the same firm and occupation. In column (4) the dependent variable is the difference between earnings growth and the dependent variable from column (3). Reorganization has a positive effect on the average earnings premium measured at the firm/occupation level, but we find no evidence for the residual (column (4)), confirming the findings of columns (1) and (2). Table A13 in the Internet Appendix provides robustness analysis. In Panel A we correct for selection into employment using the procedure described in Section F of the Internet Appendix. In Panel B we exclude workers when computing the average earnings at their firm and firm/occupation pair. The results are consistent.

In Figure 7, we repeat the analysis from Figure 4, but use the average earnings at the firm level instead of the worker's earnings to compute earnings quintiles. We find that reorganization increases the probability that workers transition to jobs in new employers with average firm-level earnings in the highest quintile. These findings imply that transitions to new employers, and not the retention of workers in reorganized firms with high average earnings, explain the positive effect of reorganization on job match quality.

In Table 10 we provide evidence that reorganization has a larger effect on earnings and on job match quality in markets with stronger job search frictions. Here, we describe succinctly the variables use to split the sample, and provide formal definitions of these variables in Section B of the Internet Appendix. In columns (1) and (2) of Table 10, we split the sample by labor market thickness, i.e., the number of workers employed in the same occupation and region. Thinner labor markets have stronger search frictions because workers are less likely to receive similar job offers if their occupation is rare in the local labor market. We find that reorganization has a large and positive effect on earnings in thin labor markets, and a small and statistically insignificant effect in thick labor markets. We observe the same pattern for the firm component of earnings and for the probability that workers transition to high-paying jobs in new employers. This evidence is consistent with reorganization improving job match quality in markets with search frictions. In Table A14 of the Internet Appendix, we use two alternative definitions of labor market thickness: 1) the number of workers per occupation and county (a finer administrative division than regions); 2) the definition of thickness used by Bernstein et al. (2019b) adapted to labor markets, which takes into account the fact that workers might easily transition between similar occupations. We reach similar conclusions, even though the instrument is weaker if we use counties as administrative regions, since there are few judges in counties with few people.

The relationship between labor market thickness and earnings growth might be caused by demand, and not by search frictions. For example, there might be little demand for occupations with few workers. In such cases, reorganization would improve labor outcomes not by alleviating search frictions, but by creating temporary demand for such occupations. In columns (3) and (4) of Table 10, we split the labor market by outside options, i.e., the earnings that workers could obtain if they were not employed in their current job. Outside options are latent in the data, therefore we adapt the machine learning model employed by Jäger et al. (2022) to estimate them. We would expect reorganization to have a meaningful effect on labor outcomes in markets with few outside options because, by providing additional time to search for a new job, reorganization would protect workers from accepting jobs with significantly lower compensation. We find that reorganization reduces earnings losses significantly when workers have few outside options, but has no meaningful effect in labor markets with many outside options. The correlation between labor market thickness and outside options is -4%, meaning that the two variables measure different labor market characteristics.

5.3 Firm profitability and resource misallocation

We assess the impact of reorganization on worker reallocation across firms, considering both profitability and potential resource misallocation. Figure 8 presents the average EBITDA/assets ratio (a profitability proxy) and labor gap (a resource misallocation proxy) for firms employing workers after reorganization. Panel A displays average EBITDA/assets, while Panel B depicts the average labor gap. The sample is divided into two subsamples: workers remaining in reorganized firms and those leaving to find new employment. Each worker is assigned one value for both EBITDA/assets and labor gap, reflecting the observed values at their respective firm. The values shown in Figure 8 are the averages of these values within each subsample. On average, both average firm-level EBITDA/assets and the labor gap are lower for workers remaining in reorganized firms compared to those in other firms. However, the average for reorganized firms improves over time.

The improvement in average EBITDA/assets and labor gap over time reveals an important difference between reorganization and displacement. Displacement research typically studies earnings outcomes for laid-off workers (e.g., Jacobson et al. (1993)). With reorganization, it is crucial to study labor outcomes for both workers who leave and those who stay in reorganized firms because these firms undergo significant transformations after filing, potentially affecting workers' earnings.

In Table A15 of the Internet Appendix, we estimate the effect of reorganization on EBITDA/assets and on the labor gap. We obtain negative but statistically insignificant estimates for EBITDA. We obtain more negative and statistically significant estimates for the subsample of workers who remain in reorganized firms, but not for the workers who move to new employers. This evidence is consistent with reorganization leading to the retention of workers in reorganized firms, which have on average lower EBITDA/assets and labor gaps.

Based on these findings, we assess whether there exists a tradeoff between higher earnings and lower labor gaps. We can compare these variables with each other because they can be measured in the same units. In Table 11 we contrast how reorganization affects earnings and labor reallocation to firms with different labor gaps. Using this analysis, we compute the net benefit of reorganization as the sum of the estimates for the effect of reorganization on earnings and for the effect on the labor gap. This is a partial equilibrium analysis, as it does not consider additional impacts such as aggregate demand effects or competition effects. In column (1) we measure the effect of reorganization on annualized earnings (monthly earnings multiplied by 12). This variable differs from the previously used earnings variable because it is measured in thousand euros, instead of being measured as a percentage of pre-filing earnings. We use this alternative earnings variable to make it directly comparable to the labor gap, which we also measure in thousand euro. For the whole sample, we find evidence that the negative effect of reorganization on labor retention is small when compared to the positive effect on earnings. We find that reorganization reduces earnings losses by 3,618 euro. In column (2) we measure the effect of reorganization on the labor gap. Reorganization has a negative, but small (-810 euro) and statistically insignificant effect on the labor gap. Therefore, the benefit-to-cost ratio of reorganization for the whole sample is 4.47.[9]

However, in labor markets where the mechanisms that drive earnings during reorganization are not present, the negative effect of reorganization on reallocation might dominate the positive earnings effect. Columns (3) and (4) of Table 11 provide an example. The subsample used in these columns contains workers who observe the following conditions: 1) they are employed at firms whose labor gap is below the median before the filing; 2) their jobs before the filing have labor market thickness above or equal to the median; 3) they have less than high school. By conditioning the sample on the labor gap, we focus on the workers who are the most likely to remain in low-quality firms after reorganization. By conditioning on labor market thickness and education, we focus on the workers who are the least likely to benefit from the mechanisms that influence earnings

[9]The benefit-to-cost ratio of 4.47 results from dividing the increase in annual earnings (3.618) by the decrease in the labor gap (0.810)

after reorganization. In column (3) we obtain a negative, small (-287 euro), and statistically non-significant effect of reorganization on earnings for this group. However, in column (4) we find evidence of a negative effect on the labor gap with a large magnitude (-9,254 euro). These estimates show that the effect of reorganization on labor reallocation is not unequivocally positive. If the mechanisms that lead to higher earnings after reorganization are not present, and if reorganized firms have a small labor gap, then workers do not get higher earnings, and reorganization leads to the retention of labor in firms with lower labor gaps. In columns (5) and (6) we provide estimates for workers who are not in the subsample of columns (3) and (4). Reorganization has a positive effect on earnings and on the labor gap for these workers, even though the effect on the labor gap is not statistically significant.

5.4 Generalizability

We perform the analysis in the Portuguese labor market, which has two distinctive characteristics. First, Portugal has a high level of labor protection in comparison to other countries such as the US (Blanchard and Portugal (2001)). Second, the analysis period corresponds to a time of slow economic growth, as the European sovereign debt crisis led to a major recession in Portugal between 2010 and 2013 (Reis et al. (2023)).

We do not find evidence that labor market rigidity affects the extrapolation of our findings to other settings with lower levels of rigidity. For instance, if labor market rigidity contributes to the effect of reorganization on earnings, we would expect that workers with longer tenure, who generally stay longer in the same job because of labor protections (Centeno and Novo (2012)), would have higher earnings after reorganization. However, in Table 8, we do not find evidence that reorganization has a stronger effect on earnings for workers with long tenure at the firm. Also, in Section E of the Internet Appendix, we show that reorganization has a strong and positive effect on worker outcomes for workers with temporary job contracts. Based on these findings, we would expect to observe economically meaningful effects of reorganization on worker outcomes in labor markets that are less rigid.

Concerning the role of economic growth, in columns (3) and (4) of Table 10, we show that reorganization has a large effect for the subsample with outside employment options below the median, but a small and statistically insignificant effect in the subsample with outside options at or above the median. Therefore, in aggregate, we would expect the effect of reorganization on earnings to be smaller in high-growth economies, where workers have more outside options. In such settings, reorganization might have a negative net effect on labor reallocation because of the retention of resources in inefficient firms documented in Table 11.

6 Conclusion

In this paper, we analyze the effect of corporate reorganization on the reallocation of labor in bankruptcy. We use the Portuguese labor market as a laboratory to analyze this question. The Portuguese setting offers some unique advantages. First, we track workers even as they leave firms that file for reorganization. Second, we use comprehensive firm and worker data to study the mechanisms that contribute to the effect of reorganization on earnings. Finally, we use the random assignment of reorganization cases to judges to create exogenous variation in the probability of reorganization.

We show that reorganization reduces earnings losses by 20% of pre-filing earnings up to five years after the reorganization filing. In the long term, most of the earnings premium arises from the intensive margin, meaning that workers from reorganized firms have better-paying jobs. Transitions to high-paying jobs in new employers contribute to this effect.

We discuss how four different mechanisms contribute to the effect of reorganization on earnings: firm-specific skills, general skills, job match quality, and bargaining. Reorganized firms are more likely to retain workers with firm-specific skills, but they do not pay a premium to retain these workers. Reorganization increases the probability that workers use their general skills, and the effect of reorganization on earnings is larger for workers with more general human capital. Contrary to what happens with firm-specific skills, we provide evidence that reorganized firms pay to retain workers with more general skills. Reorganization also increases the quality of matches between workers and firms, as it increases the probability that workers move to firms and occupations with higher average earnings. We find no evidence that reorganization increases earnings by improving workers' bargaining power.

We discuss the relationship between reorganization and the quality of the firms where workers are employed. We provide evidence that reorganized firms are less profitable and have lower labor gaps than the firms where workers who leave reorganized firms are employed. The effect of reorganization on labor reallocation is not unequivocally positive. If we consider the effect of earnings as a benefit and the effect on the labor gap as a cost, then the benefit-to-cost ratio for the whole sample is 4.47. However, when workers are not exposed to the mechanisms that drive the positive effect of reorganization on earnings, and reorganized firms have low labor productivity, reorganization simultaneously does not reduce earnings losses and retains workers in firms with significantly lower labor gaps.

Tables

Table 1: Descriptive statistics

	All firms (1)	Reorganized (2)	Not reorganized (3)	Difference (4)
Panel A: outcomes				
Reorganization	59.166	100.000	0.000	
Liquidation	35.977	0.000	88.107	
Dismissal	4.856	0.000	11.893	
Panel B: firm characteristics				
Assets (€ Million)	6.513	7.444	5.165	2.278**
	(28.379)	(29.979)	(25.840)	
Workers†	30.491	33.140	26.654	6.485**
	(72.325)	(80.408)	(58.497)	
EBITDA/Assets (%)	-5.327	-4.277	-6.849	2.573***
	(15.056)	(14.134)	(16.185)	
Equity ratio (%)	-7.391	-4.881	-11.027	6.145***
	(50.642)	(49.116)	(52.589)	
Labor gap (€ thousand)	12.397	13.077	11.411	1.666*
	(21.281)	(21.402)	(21.076)	
Panel C: worker characteristics				
Age (years)	37.446	37.398	37.534	-0.136*
	(7.52)	(7.50)	(7.55)	
Female (%)	40.894	39.513	43.385	-3.872***
	(49.164)	(48.889)	(49.562)	
Schooling (years)	9.441	9.422	9.475	-0.053
	(3.888)	(3.917)	(3.834)	
Tenure (years)	7.430	7.333	7.606	-0.274***
	(7.597)	(7.437)	(7.874)	
Earnings (€)	1092.472	1105.325	1069.293	36.032***
	(866.098)	(896.008)	(808.878)	
Observations (firms)	2,471	1,462	1,009	
Observations (workers)	47,462	30,532	16,930	

Notes. The table shows descriptive statistics for firms and workers in the sample. Column (1) reports statistics for the whole sample, column (2) for reorganized firms, and column (3) for firms that are not reorganized. Column (4) reports the difference between Columns (2) and (3). Standard deviations are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. We define variables in Section B of the Internet Appendix. † This value is greater than the number of workers in the worker sample because we follow the literature and exclude workers who are under 23 or over 50 years old, part-time workers, and foreign nationals from the worker sample (Couch and Placzek (2010)).

Table 2: First stage

	Reorganization	
Variable	(1)	(2)
Instrument	0.361***	0.362***
	(0.061)	(0.061)
Log assets		-0.006
		(0.017)
Log workers		0.029**
		(0.012)
Labor gap		0.0001
		(0.000)
Equity ratio		0.001
		(0.001)
EBITDA/Assets		0.002***
		(0.001)
Age		-0.0002
		(0.001)
Tenure		-0.0002
		(0.001)
Female		-0.032**
		(0.014)
Years schooling		0.003
		(0.002)
Log earnings		0.005
		(0.018)
Instrument F-stat	34.99	35.16
Observations	47,462	47,462
R-squared	0.077	0.095

Notes. The table reports first stage results. The dependent variable is an indicator variable equal to 1 if the reorganization filing is accepted. We define the variables in Section B of the Internet Appendix. Column (1) contains court-year fixed effects, and column (2) contains court-year fixed effects, industry fixed effects firm and worker-level controls. The standard errors, shown in parentheses, are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Random judge assignment

Variable	Instrumental variable											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Log assets	0.007	0.007										
	(0.007)	(0.005)										
Log workers	-0.002		-0.001									
	(0.005)		(0.004)									
Labor gap	0.0002			0.0003								
	(0.0002)			(0.0002)								
Equity ratio	0.0001				0.00002							
	(0.0001)				(0.0001)							
EBITDA/Assets	-0.0004					-0.0002						
	(0.0003)					(0.0003)						
Age	0.0004						0.0002					
	(0.0004)						(0.0003)					
Tenure	0.0002							0.0002				
	(0.0005)							(0.000)				
Female	0.002								0.006			
	(0.006)								(0.006)			
Years schooling	0.001									0.001		
	(0.001)									(0.001)		
Log earnings	-0.006										0.003	
	(0.007)										(0.007)	
% firms with employees	-0.020											-0.022
	(0.046)											(0.046)
R-squared	0.011	0.0008	0.00001	0.0007	0.00003	0.0003	0.00004	0.0001	0.0002	0.0005	0.00003	0.0003
F-statistic	1.011											
Observations	47,462	47,462	47,462	47,462	47,462	47,462	47,462	47,462	47,462	47,462	47,462	47,462

Notes. This table reports randomization tests to illustrate the random assignment of reorganization to judges within a court. The dependent variable is the instrumental variable, as defined in equation (2). Column (1) reports coefficients from regressing the instrumental variable on all variables. Columns (2) to (12) show pairwise regressions for each variable. We define variables in Section B of the Internet Appendix. The F-statistic corresponds to a joint significance test for industry dummies, using the list of industries from Table A1 of the Internet Appendix. The standard errors in parentheses are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Reallocation outcomes

Variable	Bankruptcy outcome: Liquidation (1)	Bankruptcy outcome: Dismissal (2)	Short term (year 1): Firm survival (3)	Short term (year 1): Job same firm (4)	Long term (year 5): Firm survival (5)	Long term (year 5): Job same firm (6)
Reorganization	-1.015***	0.015	0.284**	0.244***	0.429***	0.289***
	(0.068)	(0.068)	(0.111)	(0.092)	(0.130)	(0.071)
Observations	47,462	47,462	47,462	47,462	47,462	47,462
R2	0.808	0.052	0.178	0.126	0.204	0.109

Notes. This table shows the effect of reorganization on the reallocation of workers in the short term and the long term using equation (4). Short-term outcomes are measured in the year of the filing (year 1). Long-term outcomes are measured in year 5 or in the last year of the sample when there are less than five years of data after the filing. *Liquidation* is equal to 1 if the firm is liquidated. *Dismissal* is equal to 1 if the case is dismissed. *Firm survival* is an indicator equal to 1 if the firm remains open. *Job same firm* is an indicator equal to 1 if the worker remains employed at the firm that files for reorganization. All specifications contain the controls used in column (2) of Table 2. The standard errors in parentheses are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Job transitions

Variable	Time to leave (years) (1)	Leave Years 1-3 (2)	Leave Year 4+ (3)	Transition outcome: Layoff (4)	Transition outcome: Quit (5)	Transition outcome: Transfer (6)
Reorganization	1.094***	-0.371***	0.081**	-0.373***	0.092*	-0.008
	(0.250)	(0.077)	(0.039)	(0.102)	(0.052)	(0.033)
Observations	47,462	47,462	47,462	47,462	47,462	47,462
R-squared	0.174	0.129	0.065	0.117	0.086	0.039

Notes. This table shows the effect of reorganization on the transition of workers to jobs in new firms using equation (4). *Time to leave (years)* is the number of years between the year before the filing and the year when the worker leaves the job. This variable is equal to the number of years between the filing and the last year of the analysis when the worker remains in the same job. *Leave Years 1-3* is an indicator variable equal to 1 if the worker leaves the firm between years 1 and 3. *Leave Year 4+* is an indicator variable equal to 1 if the worker leaves in Year 4 or later. *Layoff* is an indicator equal to 1 if the worker is laid off. *Quit* is an indicator equal to 1 if the worker quits. *Transfer* is an indicator equal to 1 if the worker stays in the same establishment, and the establishment is transferred to another firm. Transition outcomes are not observed directly in the data. We explain the procedure used to estimate them in Section B of the Internet Appendix. All specifications contain the controls used in column (2) of Table 2. The standard errors in parentheses are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Earnings outcomes

Variable	Short term (year 1) Earnings growth (1)	Short term (year 1) Employment (extensive margin) (2)	Short term (year 1) Intensive margin growth (3)	Long term (year 5) Earnings growth (4)	Long term (year 5) Employment (Extensive margin) (5)	Long term (year 5) Intensive margin growth (6)
Reorganization	0.159	0.112	0.071	0.204**	0.063	0.140***
	(0.097)	(0.086)	(0.077)	(0.091)	(0.076)	(0.052)
Observations	47,462	47,462	32,873	47,462	47,462	47,462
R-squared	0.08	0.08	0.091	0.061	0.048	0.076

Notes. This table shows the effect of reorganization on earnings using equation (4). We measure short-term outcomes in the year of the filing (year 1), and long-term outcomes in year 5 or in the last year of the analysis when there are less than five years of data after the filing. *Earnings growth* is the ratio between the earnings after the filing and the earnings before the filing. *Employment (extensive margin)* is an indicator equal to 1 if the worker has a job in the employer-employee matched dataset. *Intensive margin growth* is the difference between earnings growth and the employment dummy. The standard errors in parentheses are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Relationship between judge leniency and earnings by worker group

Dependent variable	Employed workers Before filing (1)	Employed workers After filing (2)	Same firm Before filing (3)	Same firm After filing (4)	New employer Before filing (5)	New employer After filing (6)
Reorganization	0.012	0.080***	0.052	0.074**	-0.002	0.099***
	(0.027)	(0.024)	(0.033)	(0.036)	(0.032)	(0.028)
Observations	33,117	33,117	19,289	19,289	25,123	25,123
R-squared	0.086	0.161	0.101	0.163	0.105	0.173

Notes. This table shows the relationship between the instrumental variable and earnings growth for workers who stay in the same firm and for workers who find jobs with new employers using equation (5). Columns (1), (3), and (5) show coefficients for the year before the filing. Columns (2), (4), and (6) show coefficients for the last year of the analysis. Columns (1) and (2) show values for all employed workers. In Columns (3) and (4) we exclude workers from reorganized firms who stay in the same job. In Columns (5) and (6) we exclude workers from reorganized firms who move to other employers. The standard errors in parentheses are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Human capital

Variable	Tenure		Education	
	< median (1)	≥ median (2)	< high school (3)	≥ high school (4)
Instrument	0.412***	0.299***	0.313***	0.414***
	(0.071)	(0.069)	(0.067)	(0.075)
Job same firm	0.277***	0.245*	0.228**	0.345***
	(0.067)	(0.126)	(0.097)	(0.082)
Earnings growth	0.317***	0.055	0.001	0.377***
	(0.107)	(0.156)	(0.108)	(0.140)
Job (extensive margin)	0.174**	-0.094	-0.06	0.132
	(0.085)	(0.131)	(0.105)	(0.099)
Earnings growth (intensive margin)	0.143**	0.149	0.062	0.245***
	(0.066)	(0.091)	(0.078)	(0.081)
Earnings quintile 5, new employer	0.075**	0.073	0.038	0.116**
	(0.030)	(0.048)	(0.036)	(0.047)
Observations	22,414	25,048	29,344	18,118

Notes. This table estimates equation (4) for subsamples of the data by levels of human capital. We use tenure at the firm to measure firm-specific human capital and education to measure general human capital. In columns (1) and (2) we split the sample at the median by firm tenure. In columns (3) and (4) we split the sample by the level of education. The standard errors in parentheses are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

Table 9: Decomposition of the earnings effect

Variable	Average earnings (firm) (1)	Earnings - Average earnings (firm) (2)	Average earnings (firm+occupation) (3)	Earnings - Average earnings (firm+occupation) (4)
Reorganization	0.176***	0.042	0.201***	0.016
	(0.057)	(0.077)	(0.049)	(0.051)
Observations	33,117	33,117	33,107	33,107
R-squared	0.287	0.091	0.456	0.100

Notes. The table shows the effect of reorganization on the firm and firm-occupation components of the worker's earnings. *Average earnings (firm)* is the average earnings at the firm where the worker is employed, divided by the worker's pre-filing earnings. *Earnings - average earnings (firm)* is the difference between the worker's earnings and their firm's average earnings, divided by the worker's pre-filing earnings. *Average earnings (firm+occupation)* is the worker's earnigns, divided by the worker's pre-filing earnings. *Earnings - average earnings (firm+occupation)* is the difference between the worker's earnings and their firm's average earnings, divided by the worker's pre-filing earnings. The standard errors in parentheses are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 10: Job search frictions

	Thickness		Outside options	
	$<$ median	$\geq$ median	$<$ median	$\geq$ median
Variable	(1)	(2)	(3)	(4)
Instrument	0.263***	0.491***	0.355***	0.401***
	(0.064)	(0.078)	(0.079)	(0.073)
Job same firm	0.418***	0.167***	0.327***	0.243***
	(0.128)	(0.063)	(0.094)	(0.090)
Earnings growth	0.406**	0.039	0.248**	0.158
	(0.194)	(0.115)	(0.119)	(0.125)
Job (extensive margin)	0.194	-0.049	0.044	0.068
	(0.131)	(0.086)	(0.099)	(0.098)
Earnings growth	0.212*	0.089	0.204**	0.089
(intensive margin)	(0.113)	(0.059)	(0.080)	(0.058)
Earnings quintile 5,	0.117*	0.04	0.108**	0.038
new employer	(0.070)	(0.042)	(0.049)	(0.030)
Average earnings	0.273**	0.132*	0.272***	0.061
(firm)	(0.119)	(0.075)	(0.098)	(0.072)
Observations	23,685	23,777	23,731	23,731

Notes. This table shows the effect of reorganization on worker outcomes for data subsamples created using proxies for job search frictions. In columns (1) and (2) we split the sample by the median using the variable *thickness* from Section B in the Internet Appendix, which implies splitting by the number of workers employed at the occupation-region pair, excluding the workers from the firm that files for reorganization. In columns (3) and (4) we split the sample by outside options, i.e., workers' earnings if they were not employed in the current job. We explain the machine learning model used to obtain this variable in Section B of the Internet Appendix. The variable *average earnings (firm)* only includes employed workers. The standard errors in parentheses are clustered at court-year level. *, **, *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

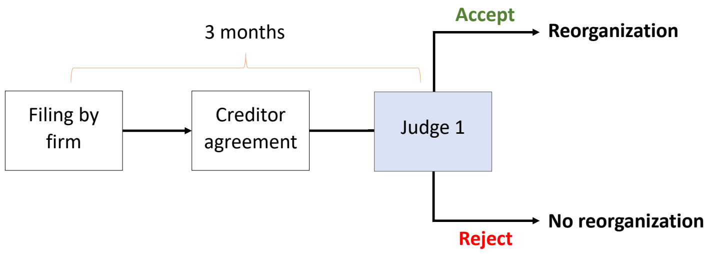
Table 11: Net benefit of reorganization

	All workers		Labor gap < median Thickness ≥ median < high school		Other workers	
Variable	Annual earnings (1)	Labor gap (2)	Annual earnings (3)	Labor gap (4)	Annual earnings (5)	Labor gap (6)
Reorganization	3.618***	-0.810	-0.287	-9.254***	4.497***	1.07
	(1.174)	(2.778)	(1.142)	(3.475)	(1.551)	(3.552)
Observations	47,462	47,462	8,394	8,394	39,068	39,068
R-squared	0.113	0.088	0.149	0.019	0.102	0.084

Notes. This table estimates the effect of reorganization on the net benefit of reorganization. Columns (1), (3), and (5) measure the effect of reorganization on annualized earnings, measured in thousand euro. Columns (2), (4), and (6) measure the effect of reorganization on the labor gap, also measured in thousand euro. We obtain estimates using equation (4). In columns (1) and (2) we use the whole sample. In columns (3) and (4) we use the subsample of workers who are employed at a firm with pre-filing labor gap below the median, have labor market thickness at or above the median, and have less than high school. In columns (5) and (6) we use the sample of workers that are not included in the sample from columns (3) and (4). We cluster standard errors in parentheses at the court-year level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

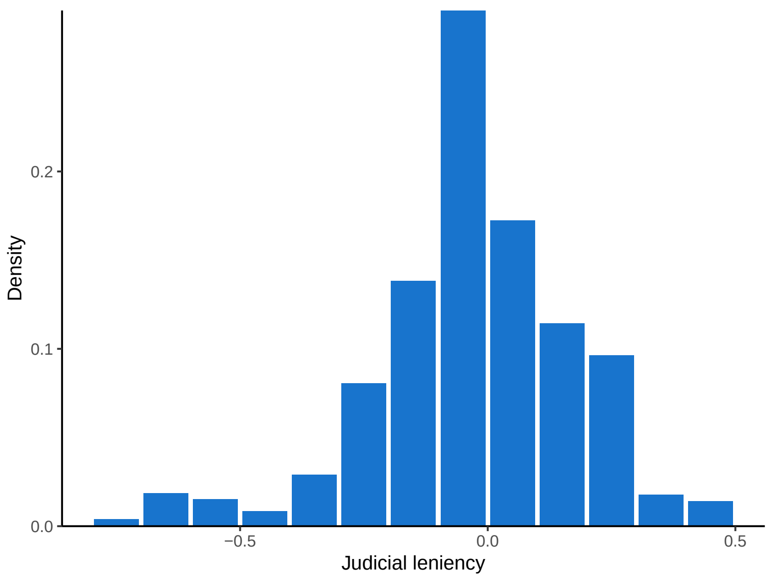
Figures

Figure 1: Reorganization in the Portuguese Bankruptcy Code



Notes. This figure depicts the Portuguese corporate reorganization system.

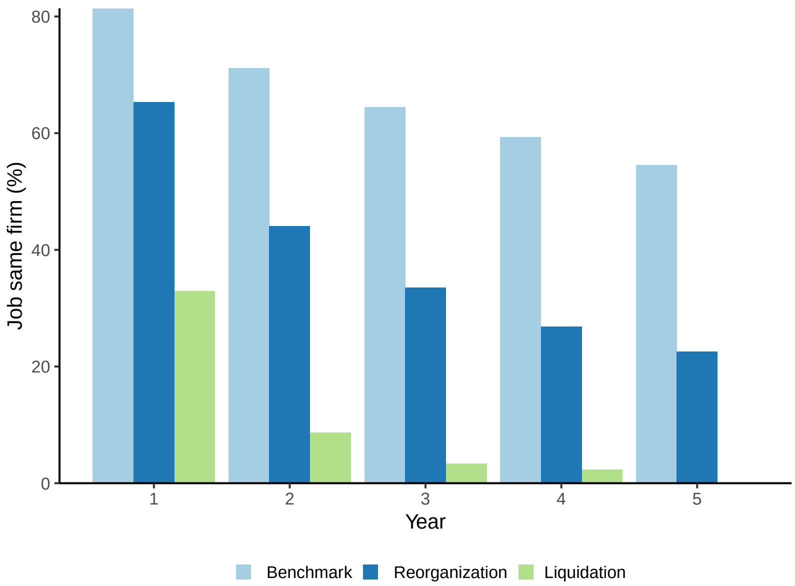
Figure 2: Judicial leniency distribution



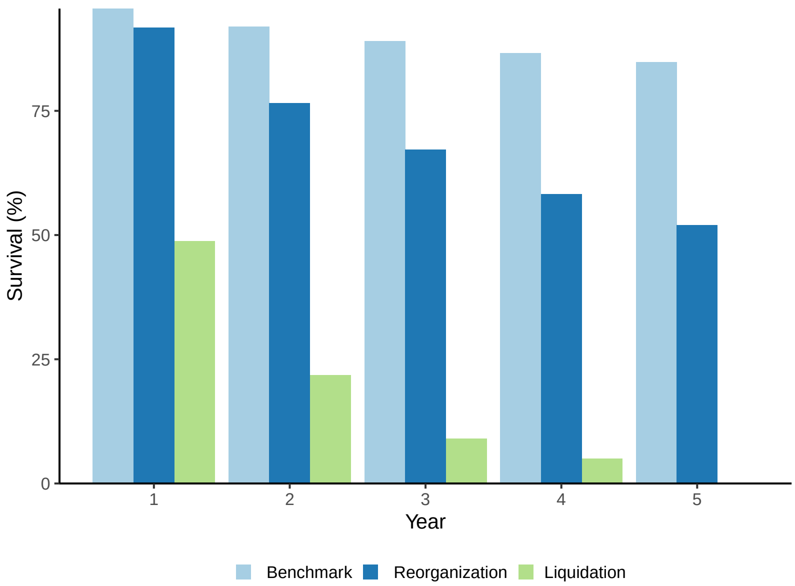
Notes. This figure depicts the distribution of the instrumental variable obtained with equation (2).

Figure 3: Worker reallocation over time

(a) Worker retention

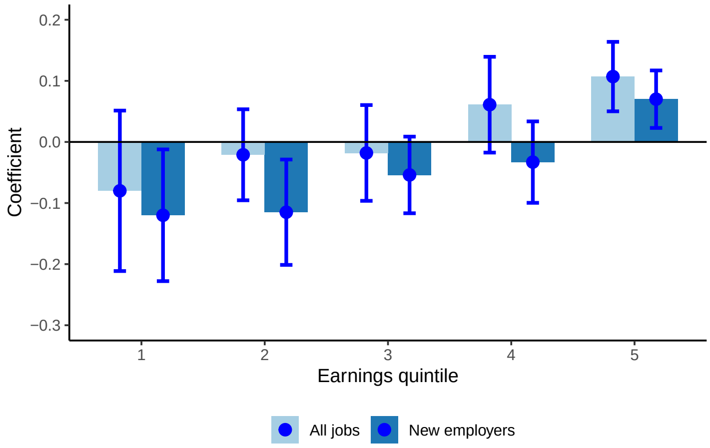


(b) Firm survival



Notes. Panel A depicts the percentage of workers who stay in reorganized firms. We restrict the sample to firms that have five years of data after the filing. Panel B depicts the percentage of workers from firms that survive up to five years after the reorganization filing. We use the procedure described in Section 5.1 to obtain the sample.

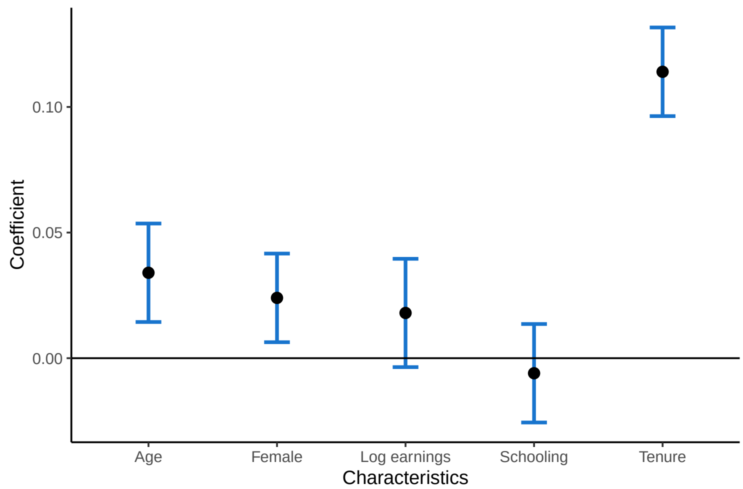
Figure 4: Job transitions and earnings quintiles



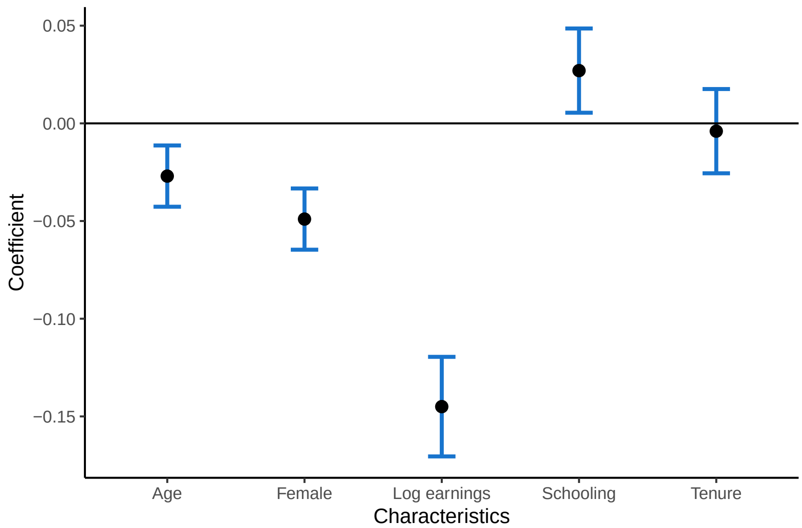
Notes. The figure depicts estimates for the effect of reorganization on employment transitions by earnings quintile using equation (4). We compute earnings quintiles using the earnings distribution of a sample of employed workers earning at least the minimum wage. *All jobs* refers to the probability that workers have a job in each of the five quintiles of the earnings distribution. *New employers* refers to the probability that workers switch to a job in a new firm, and this job is in one of the five quintiles of the earnings distribution. Error bars denote 95% confidence intervals.

Figure 5: Employment and earnings growth in reorganized firms

(a) Employment

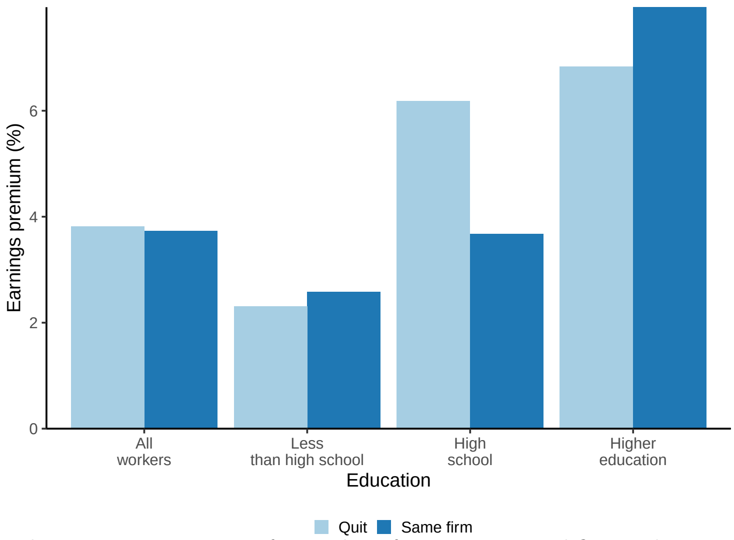


(b) Earnings growth



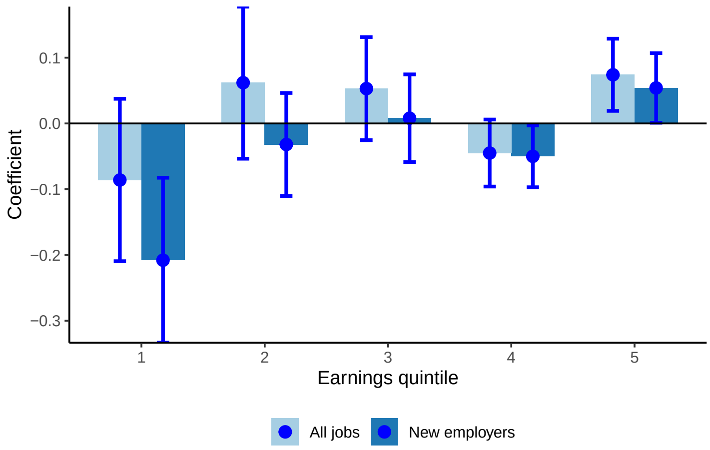
Notes. The figure depicts the relationship between worker characteristics observed before the filing and worker retention and earnings growth in reorganized firms using equation (8). In Panel A the dependent variable is an indicator variable equal to 1 if the worker stays employed in the reorganized firm by the last year of the analysis. In Panel B the dependent variable is the ratio of earnings after the filing over earnings before the filing. Panel A contains all workers employed in reorganized firms before the filing (column (2) of Table 1), and Panel B contains workers who remain in reorganized firms by the last year of the sample Independent variables, described in Section B of the Internet Appendix, are standardized to allow for the comparison of magnitudes.

Figure 6: Earnings premiums at reorganized firms



Notes. The figure depicts the earnings premiums for workers from reorganized firms who remain in these firms or quit. Light bars depict the difference between the average earnings growth for workers from reorganized firms who quit and the average earnings growth for workers from reorganized firms who are laid off. Dark bars depict the difference between the average earnings growth for workers from reorganized firms who stay in the same firm and the average earnings growth for workers from reorganized firms who are laid off. We measure earnings growth in the last year of the analysis.

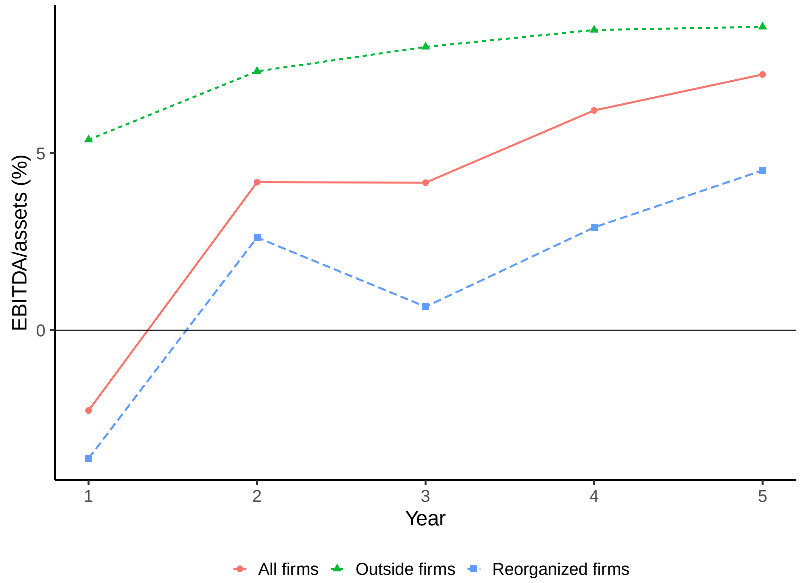
Figure 7: Job transitions and earnings quintiles: firm component



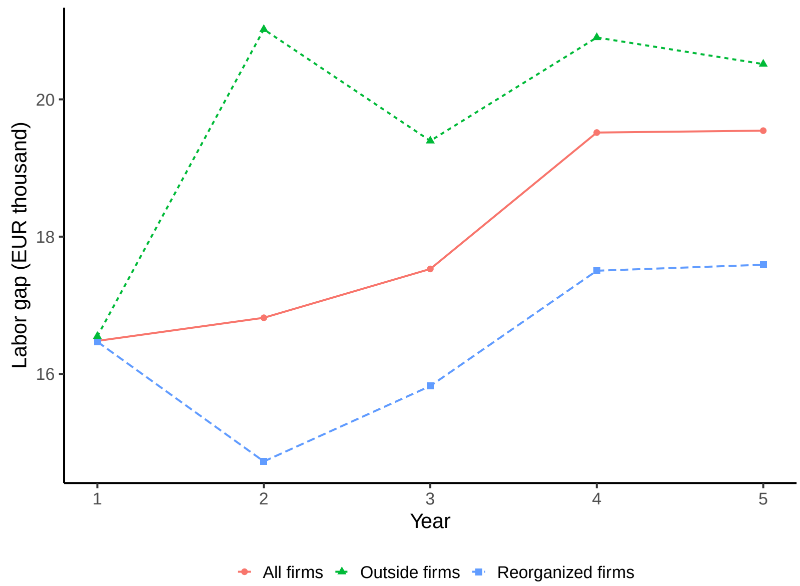
Notes. The figure depicts estimates for the effect of reorganization on employment transitions by quintile of average earnings at the firms where workers are employed, using the average earnings for workers that never worked at firms with reorganization filings to obtain the distribution of earnings. *All jobs* refers to the probability that workers have a job in each of the five quintiles of the earnings distribution. *New employers* refers to the probability that workers have a job in each of the five quintiles of the earnings distribution, and this job is not in the firm that files for reorganization. Error bars denote 95% confidence intervals.

Figure 8: Employment and earnings growth in reorganized firms

(a) EBITDA/assets



(b) Labor gap



Notes. This figure presents the average labor gap and EBITDA/assets ratio of firms where workers who were originally employed in reorganized firms find jobs after the filing.

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Internet Appendix

Appendix Tables

Table A1: Industries

Industry (1)	Workers (2)	Firms (3)
Manufacure of food products	1,333	61
Manufacture of textiles	1,986	56
Manufacture of wearing apparel	2,807	76
Manufacture of leather and related products	1,499	54
Manufacture of wood and related products	1,045	53
Manufacture of fabricated metal products, except machinery and equipment	1,946	102
Manufacture of furniture	954	55
Construction of buildings	4,201	225
Civil engineering	1,804	61
Specialized construction activities	2,456	133
Wholesale and retail trade and repair of motor vehicles and motorcycles	1,284	97
Wholesale trade, except of motor vehicles and motorcycles	3,472	299
Retail trade, except of motor vehicles and motorcycles	5,783	270
Land transport and transport via pipelines	906	66
Food and beverage service activities	1,035	97
Other industries	14,951	766
Total	47,462	2,471

Notes. The table reports the distribution of workers and firms that file for reorganization by industry. Column (1) reports the industry name. Columns (2) and (3) show the number of workers and firms from each industry in the sample. The designation of the industries follows EU NACE rev2 2-digit industry codes. The industry "other industries" aggregates industries with fewer than 50 firms in the sample.

Table A2: Instrument robustness: pre-trends

Variable	Instrumental variable 2 years before filing (1)	 3 years before filing (2)
Log assets	0.004	0.002
	(0.004)	(0.003)
Log workers	0.000	-0.001
	(0.003)	(0.002)
Labor gap	0.0001	-0.0001
	(0.0002)	(0.0002)
Equity ratio	0.004	-0.007
	(0.011)	(0.011)
EBITDA/Assets	0.023	-0.035
	(0.034)	(0.037)
Log wage	0.0002	-0.0002
	(0.0017)	(0.0010)
Observations	47,462	47,462

Notes. This table reports randomization tests to illustrate the random assignment of reorganization to judges within a court, two and three years before filing. The dependent variable is the instrumental variable, as defined in equation (3). We assume that the values are equal to zero when values are missing. Columns (1) and (2) show pairwise regressions for each variable. We provide variable definitions in Section B of the Internet Appendix. 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: Worker outcomes within filers

(a) All firms

Variable	Years firm survival (1)	Cumulative employment (2)	Earnings growth (3)
Instrument	0.735***	0.505***	0.074**
	(0.205)	(0.120)	(0.033)
Observations	47,462	47,462	47,462
R-squared	0.147	0.114	0.073

(b) Reorganized firms

Variable	Years firm survival (1)	Years employed at firm (2)	Earnings growth (3)
Instrument	0.227	0.220	0.072
	(0.194)	(0.141)	(0.045)
Observations	30,532	30,532	30,532
R-squared	0.231	0.152	0.081

(c) Other firms

Variable	Years firm survival (1)	Years employed at firm (2)	Earnings growth (3)
Instrument	-0.106	0.001	0.043
	(0.278)	(0.149)	(0.057)
Observations	16,930	16,930	16,930
R-squared	0.182	0.147	0.110

Notes. The table reports 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 between year 1 and the last year of the analysis. *Years employed at firm* is the number of years the worker stays in the firm. *Earnings growth* is earnings growth measured at the last year of the analysis. In Panel A we include all firms. In Panel B we include only firms with reorganized firms. In Panel C we include the remaining firms.

Table A4: First stage by group

Variable	< median (1)	≥ median† (2)	Above median - below median (3)
Assets	0.380***	0.449***	0.069
	(0.063)	(0.142)	(0.155)
Workers	0.362***	0.462**	0.1
	(0.063)	(0.208)	(0.217)
Equity ratio	0.451***	0.280***	-0.171
	(0.078)	(0.077)	(0.110)
Labor gap	0.434***	0.281***	-0.153
	(0.065)	(0.088)	(0.109)
EBITDA/Assets	0.406***	0.343***	-0.063
	(0.079)	(0.078)	(0.111)
Age	0.308***	0.398***	0.09
	(0.066)	(0.069)	(0.095)
Tenure	0.412***	0.299***	-0.113
	(0.071)	(0.069)	(0.099)
Education	0.221***	0.429***	0.208**
	(0.080)	(0.059)	(0.099)
Female	0.324***	0.436***	0.112
	(0.066)	(0.069)	(0.095)
Log earnings	0.338***	0.377***	0.039
	(0.067)	(0.071)	(0.098)

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 (2) 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. † Since fewer than 50% of the workers in the sample are female, in the case of this variable column (1) contains workers who are below or at the median, and column (2) the workers who are above the median.

Table A5: Additional earnings outcomes

(a) Missing earnings

Variable	Earnings growth (1)	Missing earnings = pre filing (2)	Missing earnings = 20th percentile (3)	Missing earnings = 80th percentile (4)	Missing earnings = Survey estimates (5)
Reorganization	0.204**	0.140***	0.154***	0.117**	0.119**
	(0.091)	(0.052)	(0.054)	(0.059)	(0.054)
Observations	47,462	47,462	47,462	47,462	47,462

(b) Other earnings outcomes

Variable	Earnings employed workers (1)	Earnings employed workers (selection) (2)	Hourly earnings growth (3)	Hourly earnings growth (selection) (4)	Fixed component (5)	Variable component (6)
Reorganization	0.218***	0.212**	0.180*	0.164**	0.163**	0.04
	(0.075)	(0.087)	(0.093)	(0.073)	(0.077)	(0.032)
Observations	33,117	33,117	47,462	33,117	47,462	47,462

Notes. This table provides additional earnings outcomes. In Panel A we provide additional evidence concerning missing earnings. In column (1) we replace missing earnings by 0. In column (2) we bound estimates by replacing wages for workers with no employment data by earnings recorded before the reorganization filing. In columns (3) and (4) we replace missing earnings for workers with no employment data with changes by the 20th and 8th percentiles in terms of earnings growth. In column (5) we replace missing earnings by estimated earnings using survey data, as explained in Section C of the Internet Appendix. In Panel B we provide additional earnings-related outcomes. In column (1) we drop workers with no job. In column (2) we perform a selection correction of the earnings process by following the procedure from Section F of the Internet Appendix. In column (3) we use hourly earnings instead of earnings. In column (4) we measure the effect of reorganization on hourly earnings for employed workers, performing the selection correction procedure from Section F of the Internet Appendix.In columns (5) and (6) we separate earnings after the filing into a fixed component and a variable component, and divide these earnings by pre-filing earnings. We cluster errors at the court-year level. In column (2) of Panel B we compute cluster bootstrap standard errors. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels.

Table A6: Additional labor outcomes

Variable	Earnings growth (1)	Employment (extensive margin) (2)	Intensive margin growth (3)	Same occupation (4)	Same industry (5)	Same industry (5 digits) (6)
Reorganization	0.859***	0.394	0.465***	0.189**	0.005	0.132*
	(0.283)	(0.242)	(0.144)	(0.074)	(0.079)	(0.076)
Observations	47,462	47,462	47,462	47,462	47,462	47,462
R-squared	0.140	0.156	0.105	0.065	0.068	0.090

Notes. This table shows the effect of reorganization on additional labor outcomes using estimates for equation (4). In column (1) the dependent variable is equal to cumulative earnings earned after the filing over pre-filing earnings. In column (2) the dependent variable is equal to the cumulative number of years of employment. In column (3), the numerator is equal to the difference between columns (1) and (2). In column (4) the dependent variable is an indicator equal to 1 if the worker remains in the same occupation after 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 industries from Table A1 of the Internet Appendix and column (6) for 5-digit industries. These industry codes are from *Classificação Portuguesa das Atividades Económicas* (CAE), which are harmonized with European NACE rev2 codes. All specifications contain the controls used in column (2) of Table 2, including court-year and industry fixed effects. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A7: Employment transitions by earnings quintile

(a) All employed workers

	Job new employer = 1				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
	(1)	(2)	(3)	(4)	(5)
Reorganization	-0.120**	-0.115***	-0.054*	-0.033	0.070***
	(0.055)	(0.044)	(0.032)	(0.034)	(0.024)
Observations	47,462	47,462	47,462	47,462	47,462
R-squared	0.040	0.018	0.024	0.043	0.085

(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)
Reorganization	0.04	0.094***	0.037	0.093***	0.037*
	(0.036)	(0.031)	(0.032)	(0.026)	(0.022)
Observations	47,462	47,462	47,462	47,462	47,462
R-squared	0.073	0.027	0.059	0.029	0.070

(c) Workers who move to new employers

	Job = 1				
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
	(1)	(2)	(3)	(4)	(5)
Reorganization	-0.080	-0.021	-0.018	0.061	0.107***
	(0.067)	(0.038)	(0.040)	(0.040)	(0.029)
Observations	47,462	47,462	47,462	47,462	47,462
R-squared	0.062	0.046	0.030	0.047	0.132

Notes. The table reports estimates for the effect of reorganization on employment transitions by earnings quintile using the model from equation (4). We compute earnings 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 earnings 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 earnings in quintile Q. In Panel C the dependent variable is an indicator equal to 1 if the worker moves to a new employer and has a job with earnings in quintile Q. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A8: Characteristics of workers from reorganized firms

	Stay (1)	Leave (2)	Difference (3)
Age (years)	38.501	37.007	1.495***
	(7.247)	(7.546)	(0.417)
Female (%)	41.319	38.872	2.446***
	(49.244)	(48.747)	(0.543)
Schooling (years)	8.919	9.600	-0.681
	(3.814)	(3.938)	(0.111)
Tenure (years)	10.113	6.346	3.767***
	(7.965)	(6.979)	(0.085)
Earnings (€)	1051.453	1124.433	-72.98
	(730.298)	(947.128)	(14.070)
Number of workers	7,994	22,538	

Notes. The table reports descriptive statistics for workers who stay in reorganized firms and for workers who leave these firms. In column (1) we show the average and standard deviation (in parentheses) of each variable for workers who stay in reorganized firms. In column (2) we show the same descriptive statistics for workers who leave reorganized firms. Column (3) shows the difference between the averages from columns (1) and (2), and reports standard errors in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A9: Interaction between reorganization and sample characteristics

(a) Main specification

Variable	Tenure (1)	Education (2)	Thickness (3)	Outside options (4)	Labor gap < median Thickness ≥ median < high school (5)
Instrument	-0.113*	0.101	0.228***	0.046	0.238**
	(0.065)	(0.069)	(0.077)	(0.083)	(0.105)
Job same firm	-0.032	0.117	-0.251*	-0.084	-0.166
	(0.132)	(0.115)	(0.131)	(0.126)	(0.133)
Earnings growth	-0.261	0.376**	-0.367	-0.09	-0.397**
	(0.180)	(0.168)	(0.237)	(0.170)	(0.174)
Job (extensive margin)	-0.268*	0.193	-0.243	0.025	-0.318**
	(0.150)	(0.133)	(0.151)	(0.129)	(0.141)
Earnings growth (intensive margin)	0.006	0.184	-0.124	-0.115	-0.079
	(0.112)	(0.117)	(0.135)	(0.089)	(0.097)
Earnings quintile 5, new employer	-0.002	-0.122	-0.077	-0.07	-0.04
	(0.059)	(0.098)	(0.100)	(0.061)	(0.043)
Average earnings (firm)	-0.018	0.177	-0.142	-0.211	-0.014
	(0.178)	(0.148)	(0.151)	(0.132)	(0.130)
Earnings quintile 5 (firm), new employer	0.019	0.112**	-0.037	-0.031	-0.023
	(0.040)	(0.057)	(0.028)	(0.024)	(0.017)
Annualized earnings	-1.702	0.03	-4.798	-1.81	-4.784**
	(2.531)	(0.165)	(3.446)	(2.166)	(2.042)
Labor gap	-2.694	0.116	-1.479	3.444	-10.325**
	(5.543)	(0.147)	(5.428)	(4.289)	(5.109)
Observations	47,462	47,462	47,462	47,462	47,462

Table A9: Interaction between reorganization and worker characteristics

(b) Specification without interactions

Variable	Tenure (1)	Education (2)	Thickness (3)	Outside options (4)	labor gap < median Thickness ≥ median < high school (5)
Instrument	-0.091	0.098	0.215***	0.043	0.246**
	(0.066)	(0.066)	(0.075)	(0.081)	(0.096)
Job same firm	-0.059	0.142	-0.277**	-0.161	-0.198
	(0.136)	(0.121)	(0.135)	(0.143)	(0.134)
Earnings growth	-0.331*	0.439**	-0.412*	-0.111	-0.417**
	(0.182)	(0.184)	(0.241)	(0.165)	(0.182)
Job (extensive margin)	-0.320**	0.204	-0.284*	-0.02	-0.324**
	(0.145)	(0.141)	(0.154)	(0.127)	(0.138)
Earnings growth (intensive margin)	-0.01	0.236*	-0.129	-0.091	-0.093
	(0.116)	(0.122)	(0.139)	(0.087)	(0.112)
Earnings quintile 5, new employer	-0.023	-0.149	-0.092	-0.08	-0.055
	(0.056)	(0.100)	(0.098)	(0.062)	(0.046)
Average earnings (firm)	-0.058	0.22	-0.122	-0.188	-0.047
	(0.170)	(0.147)	(0.150)	(0.134)	(0.142)
Annualized earnings	-2.731	0.07	-5.43	-2.472	-5.119**
	(2.404)	(0.169)	(3.460)	(2.193)	(2.218)
Labor gap	-2.184	0.174	-2.057	2.349	-8.986*
	(5.464)	(0.161)	(5.677)	(4.760)	(5.001)
Observations	47,462	47,462	47,462	47,462	47,462

Notes. This table depicts the relationship between the effect of reorganization on labor outcomes and worker characteristics. We display estimates from equation (6). In column (1) the interaction variable is an indicator equal to 1 for workers with firm tenure at or above the median. In column (2) the interaction variable is an indicator equal to 1 for workers with high school or more education. In column (3) the interaction variable is an indicator variable equal to 1 for workers with labor market thickness equal or above the median, as defined in Equation 6. In column (4) the interaction variable is an indicator equal to 1 if the worker has outside options at or above the median. We explain the model used to estimate outside options in Section B of the Internet Appendix. In column (5) the interaction variable is an indicator equal to 1 if the worker has pre-filing labor gap below the median, thickness at or above the median, and less than high school education. In Panel A we interact the court-year fixed effects with the interaction variable. In Panel B we use court-year fixed effects without interactions, i.e. in equation (6) we use $\delta_{c,t}$ instead of $\delta_{k,c,t}$. All specifications contain the controls used in column (2) of Table 2, including court-year and industry fixed effects. The variable *average earnings (firm)* only includes employed workers. Standard errors clustered at the court-year level are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A10: Transition to new occupations

(a) All workers

Variable	Lower skill requirement = 1 Cognitive (1)	Interpersonal (2)	Manual (3)	Higher skill requirement requirement = 1 Cognitive (4)	Interpersonal (5)	Manual (6)
Reorganization	-0.184***	-0.170**	-0.142***	-0.064	-0.078	-0.106**
	(0.065)	(0.066)	(0.051)	(0.057)	(0.054)	(0.053)
Observations	47,462	47,462	47,462	47,462	47,462	47,462
R-squared	0.01	0.014	0.018	0.037	0.033	0.035

(b) Less than high school

Variable	Lower skill requirement = 1 Cognitive (1)	Interpersonal (2)	Manual (3)	Higher skill requirement requirement = 1 Cognitive (4)	Interpersonal (5)	Manual (6)
Reorganization	-0.156*	-0.225**	-0.082	-0.156**	-0.086	-0.229**
	(0.091)	(0.096)	(0.070)	(0.074)	(0.079)	(0.090)
Observations	29,344	29,344	29,344	29,344	29,344	29,344
R-squared	0.038	0.015	0.04	0.021	0.039	0.013

Table A10: Transition to new occupations

(c) High school or more

	Lower skill requirement = 1			Higher skill requirement requirement = 1		
Variable	Cognitive (1)	Interpersonal (2)	Manual (3)	Cognitive (4)	Interpersonal (5)	Manual (6)
Reorganization	-0.201**	-0.111	-0.204***	0.023	-0.066	0.026
	(0.096)	(0.085)	(0.073)	(0.091)	(0.069)	(0.074)
Observations	18,118	18,118	18,118	18,118	18,118	18,118
R-squared	0.007	0.031	0.017	0.06	0.06	0.042

Notes. This table shows the effect of reorganization on the skill requirements of the last occupation held by workers by the last year of the analysis. In columns (1)-(3) the dependent variable is equal to 1 if the skill requirement the occupation decreases, and in columns (4)-(6) the dependent variable is equal to 1 if the skill requirement of the occupation increases. We consider three skill categories: cognitive (columns (1) and (4)), interpersonal (columns (2) and (5)), and manual (column (3) and (6)). Skill requirements are obtained using the procedure described in Section C.2 of the Internet Appendix. When the worker is not employed, we use the skill requirements of the last known occupation. Panel A depicts estimates for all workers, Panel B for workers with less than high school, and Panel C for workers with high school or more. We display estimates from equation (4). 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 A11: Transition to new occupations (continuous variable)

(a) All workers

Variable	Cognitive (1)	Interpersonal (2)	Manual (3)
Reorganization	0.034	0.033	-0.001
	(0.033)	(0.031)	(0.024)
Observations	47,462	47,462	47,462
R-squared	0.018	0.014	0.02

(b) Less than high school

Variable	Cognitive (1)	Interpersonal (2)	Manual (3)
Reorganization	-0.032	0.019	-0.054
	(0.043)	(0.048)	(0.045)
Observations	29,344	29,344	29,344
R-squared	0.021	0.025	0.02

Table A11: Transition to new occupations

(c) High school or more

Variable	Cognitive (1)	Interpersonal (2)	Manual (3)
Reorganization	0.102*	0.049	0.059*
	(0.057)	(0.043)	(0.032)
Observations	18,118	18,118	18,118
R-squared	-0.005	0.028	0.013

Notes. This table shows the effect of reorganization on the skill requirements of the last occupation held by workers by the last year of the analysis. The dependent variable is the rank of the occupation in terms of skill requirements. We explain the procedure used to obtain this rank in Section C.2 of the Internet Appendix. We display estimates from equation (4). 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 A12: Additional sample splits: hierarchy

	Hierarchy	
Variable	Entry level (1)	Higher levels (2)
Instrument	0.335***	0.374***
	(0.080)	(0.063)
Job same firm	0.174	0.330***
	(0.126)	(0.073)
Earnings growth	0.149	0.228**
	(0.157)	(0.097)
Job (extensive margin)	0.094	0.051
	(0.126)	(0.084)
Earnings growth (intensive margin)	0.055	0.177***
	(0.071)	(0.058)
Earnings quintile 5, new employer	-0.019	0.101***
	(0.031)	(0.033)
Observations	14,401	33,061

Notes. This table shows the effect of reorganization on worker outcomes for data subsamples created using the hierarchical levels defined in Section B of the Internet Appendix. Column (1) includes workers at the base of the firm's hierarchy. Column (2) includes higher-skilled professionals, supervisors, team leaders, intermediary executives, and top executives. 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. The variable *average earnings (firm)* only includes employed workers. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A13: Decomposition of the earnings effect: robustness

(a) Selection into employment

Variable	Firm (1)	Earnings - firm (2)	Firm/ occupation (3)	Earnings - firm/ occupation (4)
Reorganization	0.209**	0.002	0.223***	-0.011
	(0.081)	(0.103)	(0.064)	(0.075)
Observations	33,117	33,117	33,107	33,107

(b) Excluding the worker from the average

Variable	Firm (1)	Earnings - firm (2)	Firm/ occupation (3)	Earnings - firm/ occupation (4)
Reorganization	0.138**	0.07	0.180***	0.003
	(0.066)	(0.090)	(0.068)	(0.079)
Observations	32,727	32,727	27,701	27,701
R-squared	0.297	0.088	0.229	0.048

Notes. The table shows the effect of reorganization on the firm and firm-occupation components of earnings, as defined in Section B of the Internet Appendix. In Panel A we measure the effect of reorganization on earnings components for employed workers. In Panel B we remove the worker when compution firm, occupation, and firm-occupation earnings components. We display 2SLS estimates from Equation 4 for all dependent variables. In Panel A we perform a selection correction of the earnings process by following the procedure described in Section F of the Internet Appendix. 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 A14: Additional sample splits: thickness

Variable	County level		Bernstein et al. (2019)	
	< median (1)	≥ median (2)	< median (3)	≥ median (4)
Instrument	0.264***	0.497***	0.303***	0.435***
	(0.068)	(0.072)	(0.066)	(0.071)
Job same firm	0.355***	0.247***	0.366***	0.239***
	(0.126)	(0.060)	(0.098)	(0.082)
Earnings growth	0.275	0.168	0.376**	0.07
	(0.175)	(0.104)	(0.153)	(0.116)
Job (extensive margin)	0.143	0.031	0.222*	-0.053
	(0.133)	(0.078)	(0.119)	(0.093)
Earnings growth (intensive margin)	0.132	0.137**	0.155**	0.123*
	(0.092)	(0.057)	(0.079)	(0.075)
Earnings quintile 5, new employer	0.077	0.059**	0.085	0.056
	(0.064)	(0.027)	(0.057)	(0.035)
Average earnings (firm)	0.193*	0.176***	0.180*	0.147*
	(0.100)	(0.060)	(0.097)	(0.076)
Observations	23,678	23,784	23,728	23,734

Notes. This table shows the effect of reorganization on worker outcomes for data subsamples created using alternative definitions of market thickness. In columns (1) and (2) we compute labor market thickness at the county level using the variable *thickness* provided in Section B in the Internet Appendix, but using data at the county level instead of using data at the region level. In columns (3) and (4) we use the variable *thickness (Bernstein et al. (2019))* defined in Section B in the Internet Appendix. For each subsample we obtain estimates from equation 4. 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. The variable *average earnings (firm)* only includes employed workers. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

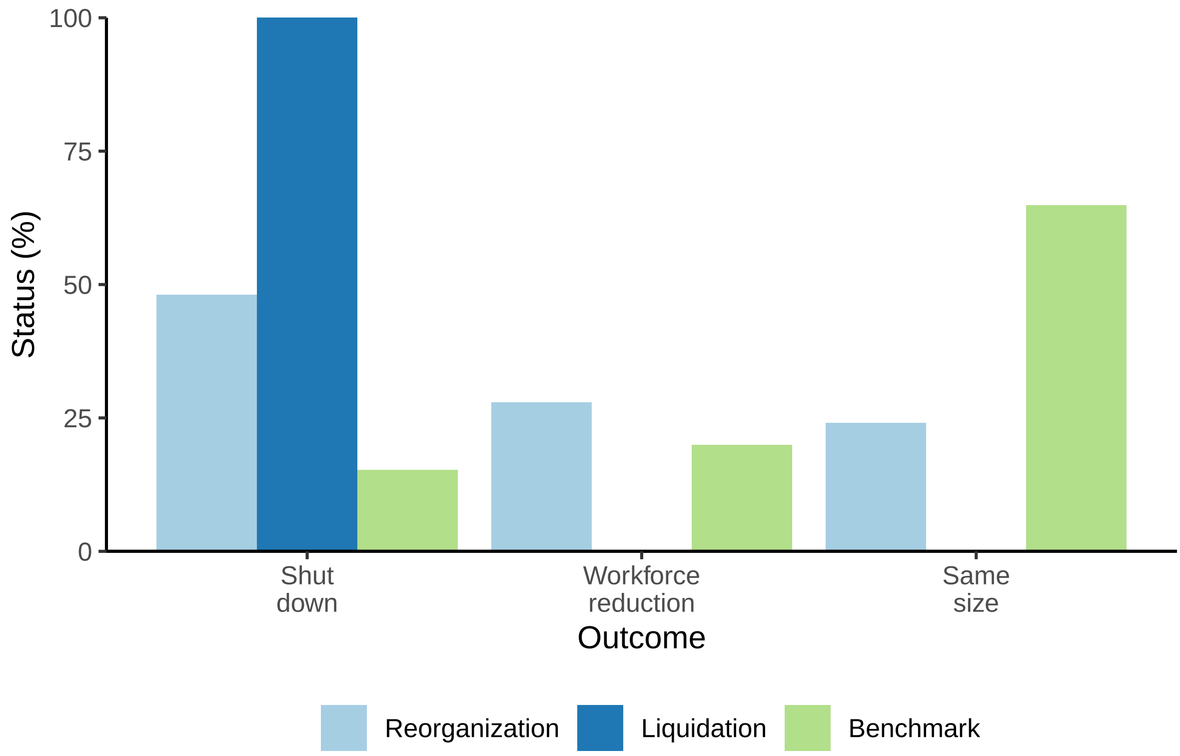
Table A15: Firm characteristics

	Effect of reorganization			Correlation with instrument			
				Same firm		New employer	
Variable	Labor gap (1)	EBITDA/ Assets (2)	EBITDA/ Assets (selection) (3)	Labor gap (4)	EBITDA/ Assets (5)	Labor gap (6)	EBITDA/ Assets (7)
Reorganization	-0.810	-0.060*	-0.058				
	(2.778)	(0.034)	(0.044)				
Instrument				-2.086	-0.043***	-0.596	-0.007
				(1.834)	(0.014)	(1.280)	(0.011)
Observations	47,462	31,892	31,892	19,289	18,740	25,123	23,898

Notes. This table shows the effect of reorganization on the characteristics of firms where workers are employed. In columns (1) to (3), we estimate the effect of reorganization on firm characteristics using Equation (4). In columns (4) and (5), we estimate the reduced-form relationship between these variables and the instrumental variable (5) in a dataset containing workers from reorganized firms who stay in those firms and workers from firms that are not reorganized. In columns (6) and (7), we estimate the reduced-form relationship between the instrumental variable and firm characteristics from equation (5) in a dataset containing workers from reorganized firms who move to new employers and workers from firms that are not reorganized. In columns (1), (4), and (6), the dependent variable is the labor gap, as defined in Section D of the Internet Appendix. In columns (2), (3), (5), and (7), the dependent variable is the firm's EBITDA/assets ratio. In column (3) we perform correct for selection by following the procedure described in Section F of the Internet Appendix. All specifications contain the controls used in column (2) of Table 2, including court-year and industry fixed effects. We display clustered standard errors at the court-year level in parentheses. In column *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Figures

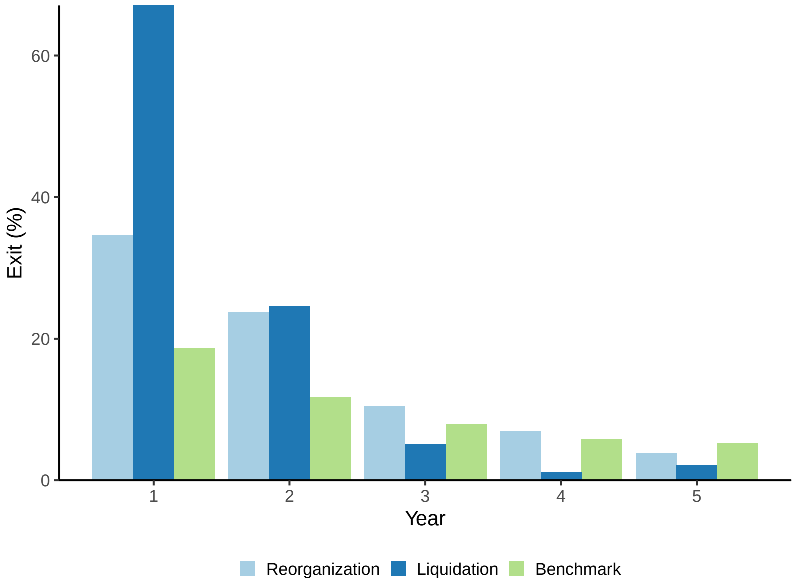
Figure A1: Firm outcomes



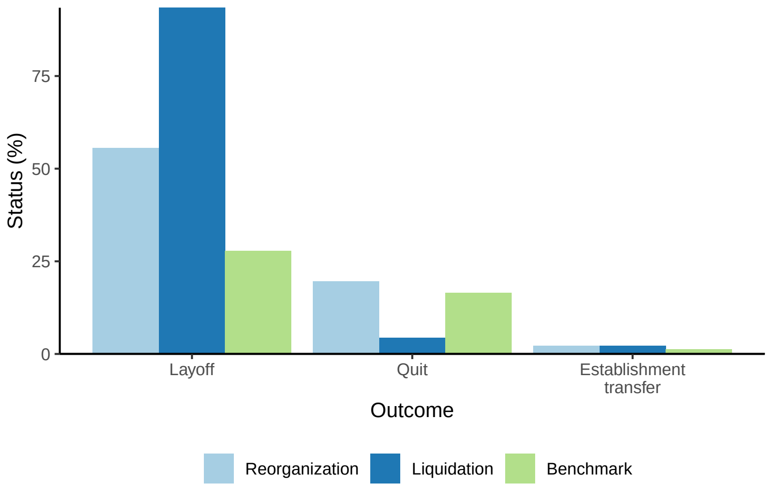
Notes. The figure depicts the final outcome of the bankruptcy process of the firms in the sample. We compare each worker from reorganized firms with a worker from comparable liquidated firms and a worker from a benchmark. We explain the process used to construct the sample in Section 5.1. We consider three outcomes: *shut down* if the firm shuts down, *workforce reduction* if more than 50% of the workers in the sample leave the firm, and *same size* if at most 50% of the workers in the sample leave the firm. These categories sum up to 100%.

Figure A2: Worker exits

(a) Exit year



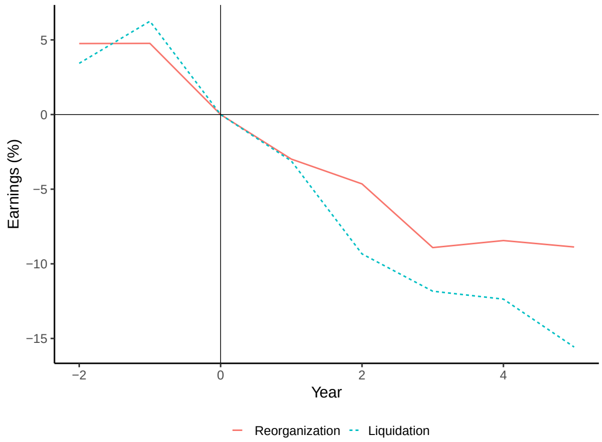
(b) Outcome



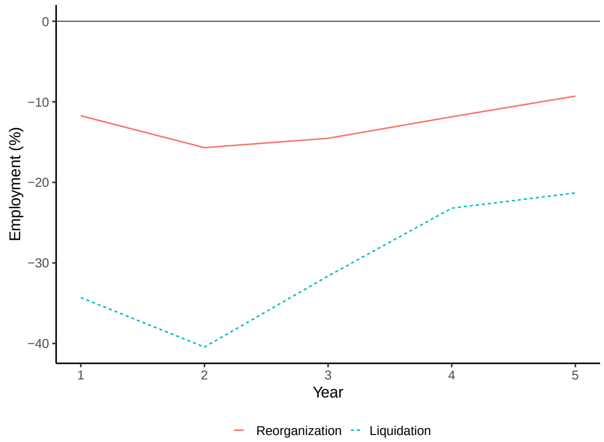
Notes. The figure depicts transition outcomes for reorganized firms, comparable workers from liquidated firms, and workers from a benchmark. We explain the process used to construct the sample in Section 5.1. Panel A depicts the percentage of workers in the sample who exit in years 1-5. Panel B depicts transition outcomes for workers who leave their initial job. We estimate layoffs, quits, and establishment transfers, as explained in Section B of the Internet Appendix.

Figure A3: Earnings and employment paths

(a) Earnings (employed workers)

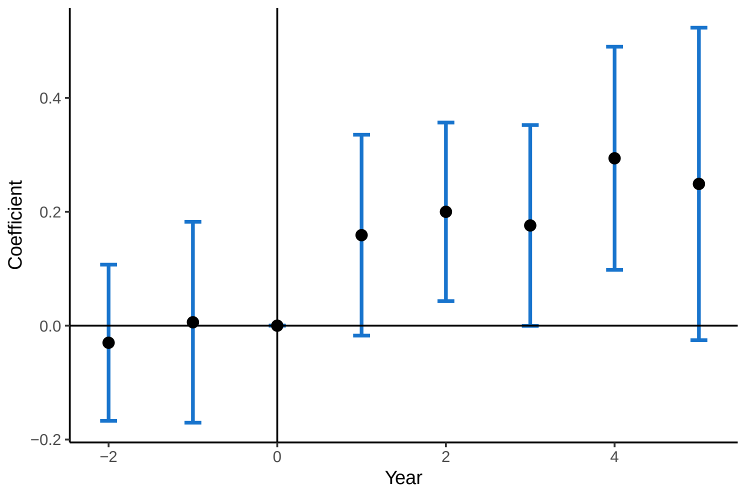


(b) Employment



Notes. Panel A depicts earnings paths for workers who were employed in reorganized and comparable liquidated firms before the filing. The straight line depicts the average earnings for workers who were employed in reorganized firms before the filing minus the average earnings for workers in the benchmark. The dashed line depicts the average earnings for workers who were employed in comparable liquidated firms before the filing minus the average earnings for workers in the benchmark. Earnings are scaled by the average earnings in the year before the filing. Panel B depicts employment paths for workers who were employed in reorganized and liquidated firms before the filing. The straight line depicts the average employment (measured as being in the employer-employee matched dataset) for workers who were employed in reorganized firms before the filing, minus the average employment rate for workers in the benchmark. The dashed line depicts the average employment rate for workers who were employed in liquidated firms before the filing minus the average employment rate for workers in the benchmark. We obtain comparable workers from liquidated firms and the benchmark using the procedure described in Section 5.1.

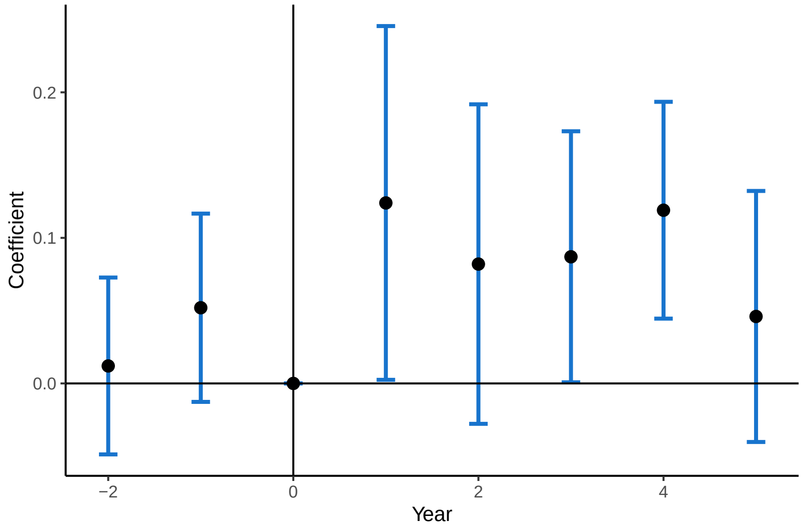
Figure A4: Year-by-year second stage results



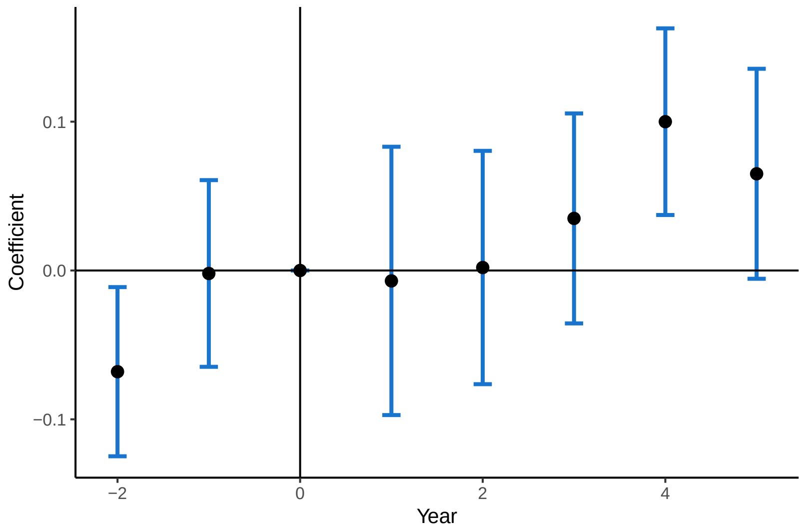
Notes. This figure depicts 2SLS estimates for the effect of reorganization on earnings up to five years after reorganization. We estimate equation (4) separately for each year. All specifications contain the controls used in column (2) of Table 2, including court-year and industry fixed effects. Standard errors are clustered at the court-year level. Error bars denote 95% confidence intervals.

Figure A5: Earnings growth and judge leniency: year-by-year results

(a) Workers who stay in the same firm

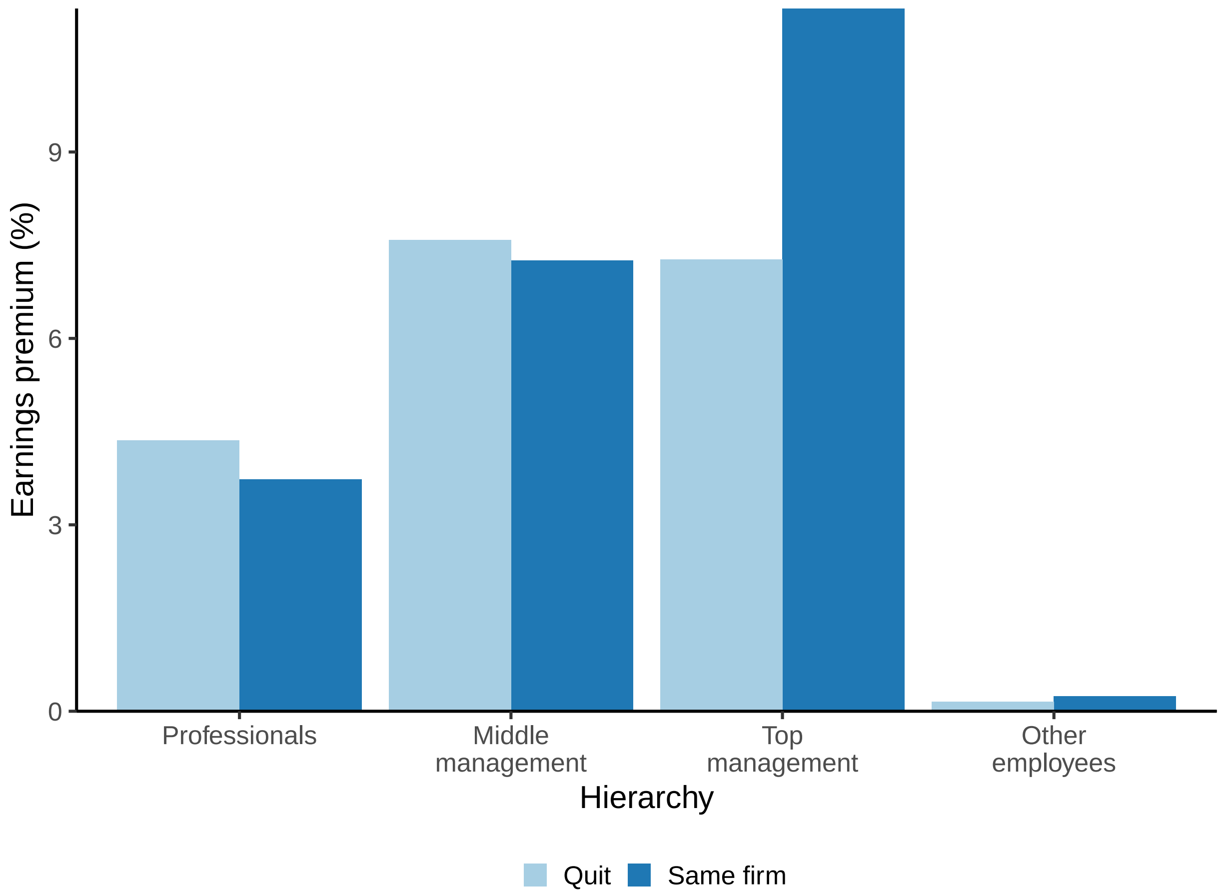


(b) Workers who leave to new employers



Notes. In this figure we estimate the relationship between the instrumental variable and earnings growth. We estimate equation 5 each year between year -2 and year 5. In Panel A the sample contains workers from reorganized firms who remain in the same job by the end of the sample period and employed workers from firms that are not reorganized. In Panel B the sample contains workers from reorganized firms who move to a new employer by the end of the sample period and employed workers from firms that are not reorganized. 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 A6: Earnings premiums at reorganized firms: by hierarchy



Notes. The figure depicts the earnings premiums for workers from reorganized firms who remain in these firms or quit, by the hierarchical level of the worker before the filing. Light bars depict the difference between the average earnings growth for workers from reorganized firms who quit and the average earnings growth for workers from reorganized firms who are laid off. Dark bars depict the difference between the average earnings growth for workers from reorganized firms who stay in the same firm and the average earnings growth for workers from reorganized firms who are laid off. We measure earnings growth in the latest year of the analysis. We derive layoffs, quits, and establishment transfers, as explained in Section 5. We define hierarchical levels in Section B of the Internet Appendix.

A The Portuguese bankruptcy system

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

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

With the implementation of the Law *Lei n.º 16/2012* in May 2012, Portugal added a separate chapter on reorganization to the bankruptcy code. This new reorganization system was based on Chapter 11 from the US bankruptcy code and shared many characteristics of US reorganization law. In this system debtors had the right to file for reorganization. They had a 3-month period to negotiate a bankruptcy plan with creditors. During this period, they retained possession of the business and were protected from creditor claims by automatic stay provisions. Reorganization plans had to be approved by a majority of creditors and by a judge. The bankruptcy code underwent some additional changes between 2012 and 2017. In 2015 the Decree-Law *Dec. Lei n.º 26/2015* introduced voting rules that made it easier to approve reorganization plans. In 2017 Decree-Law *Dec. Lei n.º 79/2017* created a separate reorganization system for individuals, which allowed the establishment of separate jurisprudence for individuals and firms. This decree-law also required the certification of reorganization petitions by an authorized accountant. This requirement had the purpose of reducing petitions from economically non-viable firms.

The Portuguese reorganization system In this section we expand the description of the Portuguese reorganization system provided in Section 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*[10]. Cases are distributed automatically twice per day.

In the first hearing after the filing the judge of the case starts the reorganization process and

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

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

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

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

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

The reorganization process is closed when firms are not reorganized (art.º 17-G of CIRE). After the process is closed the bankruptcy case might be dismissed or attached to a liquidation filing (i.e., a filing under the original bankruptcy system set up by Decree-law *Dec. Lei n.º 53/2004*). The bankruptcy manager submits a liquidation filing if the firm is "insolvent" at the end of the reorganization process. Aside from bankruptcy managers, the debtor and creditors may also submit a subsequent liquidation filing.

B Variable Definitions

% firms in the sample: Percentage of cases associated with a given judge ID, within the court and year of the filing, that are included in the final sample of firms that file for reorganization from Table 1. This value is not 100% because judges also get cases that are not included in the sample (e.g., individuals).

7-day acceptance rate: Average of the instrumental variable in the court in the seven days prior to the filing date, as defined in Equation 15.

Age: Age of the worker in years.

Assets: Total assets (in euro).

Court: Court identifier, as defined in Section C.3 of the Internet Appendix.

Dismissal: Reorganization filing that is not accepted, and the firm does not file for liquidation or cease to exist in the year of the filing or in the subsequent year.

Earnings: Sum of the following components from *Quadros de Pessoal* pertaining to total earnings obtained in October: base earnings (*remuneração base paga*); recurring pecuniary supplements (*prestações regulares*), non-recurring pecuniary supplements, including bonuses (*prestações irregulares*). Earnings include earnings from all jobs, including part-time jobs, if a worker holds multiple positions. We assume that earnings are 0 when there is no data available.

Earnings (annualized): *Earnings* variable multiplied by 12.

EBITDA/Assets: Earnings before interest, tax, depreciation, and amortization (EBITDA), divided by average assets (average of total assets in the current year and in the previous year).

Employment: Indicator variable equal to 1 if the worker has at least one employment record in *Quadros de Pessoal*. *Quadros de Pessoal* includes employment records for workers whose jobs are regulated by the Portuguese labor law (*Código do Trabalho*) or for employers and unpaid family workers of firms with at least one job regulated by the Portuguese labor law, encompassing both full-time and part-time workers. We provide more information about reported and unreported jobs in *Quadros de Pessoal* in Section C.1 of the Internet Appendix.

Equity ratio: Total current period equity over current period assets.

Establishment transfer: *Quadros de Pessoal* contains establishment identifiers, however these identifiers change when an establishment transfers to a new firm. Therefore, we categorize a job exit as an establishment transfer if the worker moves to an establishment at a different firm, the establishment operates in the same industry and parish[11] as the establishment where the worker was previously employed, the establishment commences operations in the same year as the worker's move, and the establishment where the worker was previously employed closes in the same year. A firm engages in an establishment transfer if at least one worker's job exit falls into this category. This category coexists with other categories in Figure A1 of the Internet Appendix. For instance, a firm can simultaneously cease operations and transfer establishments to other firms.

Exit: Indicator variable equal to 1 if the worker leaves the firm.

Female: Indicator variable equal to 1 if the worker is female.

Fixed component (earnings): Base earnings (*remuneração base paga* in *Quadros de Pessoal*). We sum up earnings from all jobs if the worker has more than one job.

Hierarchy: Rank of the worker in the firm's hierarchy. Workers are ranked based on their hierarchical level using a procedure similar to the one employed by Caliendo et al. (2020). We establish four hierarchical layers using the variable for the qualification level (*nível de qualificação*) in *Quadros de Pessoal*. Layer 3 comprises 'top executives (top management)'. Layer 2 includes 'intermediary executives (middle management)' and 'supervisors, team leaders'. Layer 1 consists of 'higher-skilled professionals', and layer 0 encompasses the remaining workers.

Hourly earnings: Monthly earnings (as defined in the *earnings* variable) divided by hours worked. We replace the hourly wage by zero when the value is missing.

Hours worked: sum of regular working hours (*horas mensais remuneradas - normais (horas)*) and overtime (*horas mensais remuneradas - suplementares (horas)*). We impute hours worked by the last available hours worked when hours are missing and for furloughed workers and workers on leave.

Industry: Industry from the list of industries from Table A1 of the Internet Appendix.

Industry (5 digits): 5-digit industry using the Portuguese *Classificação das Atividades Económicas* (CAE) rev3 codes. These codes are the Portuguese adaptation of EU NACE rev2 industry

[11] The parish designation corresponds to *freguesia*, the lowest administrative subdivision available in the data from *Quadros de Pessoal*. A *município*, the Portuguese counterpart to US counties, contains multiple parishes.

codes. The Portuguese and European industry classifications differ because the Portuguese version has one more industry at the 4-digit level (it separates hotels with and without restaurants) and because it has industries disaggregated at the 5-digit level.

Interest coverage ratio: EBIT (earnings before interest and tax) over gross interest expense.

Labor gap: See the definition for *marginal labor revenue product-cost gap.*

Layoff: Indicator variable equal to 1 if the worker leaves the firm and the firm does not hire other workers in the same occupation after the reorganization filing.

Liquidation: Reorganization filing that is not accepted, and the firm files for liquidation or ceases to exist in the year of the filing or in the subsequent year.

Marginal labor revenue product-cost gap: difference between revenues and costs generated by hiring an additional worker. We use the following expression to estimate it empirically:

$$\tau_{it} = MRP_{it}^{L} - w_{it} \tag{9}$$

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 (*gastos com o pessoal*) divided by the number of workers (*pessoal ao serviço*) reported in SCIE.

Occupation: *Quadros de Pessoal* includes a variable with the occupation of each worker. The occupation categories we use in the analysis follow the 4-digit CPP-2010 classification (*Classificação Portuguesa das Profissões*), which is the Portuguese version of the 4-digit ISCO-08 classification (*International Standard Classification of Occupations 2008*). CPP-2010 differs from ISCO-08 because it includes additional occupation codes for the military. Otherwise, CPP-2010 codes are equal to ISCO-08 codes.

Outside options: We estimate the value of outside employment options in the year before the reorganization filing for workers in the sample by adapting the machine learning model employed by Jäger et al. (2022). We estimate a lasso regression to predict changes in earnings for workers affected by firm closures. We exclude all workers with at least one employment record at any firm that files for reorganization in the sample period. The sample includes all workers who were affected by firm closure and found a new job in the subsequent year. The dependent variable is the change in log earnings. The independent variables are all variables included in column (2) of Table 2, excluding court-year fixed effects. We perform 10-fold cross-validation to select λ.

Quit: Indicator variable equal to 1 if the worker leaves the firm and they are not laid off or their establishment is not transferred to other firm.

Reorganization: Reorganization filing that ends with an accepted reorganization plan.

Sales: Total sales and services rendered by the firm.

Tenure: Number of years at the firm retrieved from the variable *antiguidade* in *Quadros de Pessoal*.

Thickness: measure of labor market thickness based on the thickness variable used by Gavazza (2011). We define labor market thickness as:

$$Thickness_{i,m,r,t-1} = \ln\left(1 + N_{i,m,r,t-1}\right) \tag{10}$$

Where $N_{i,m,r,t-1}$ is the number of workers in occupation m in NUTS 3 region r in the year before the filing,[12] excluding the workers from the firm that files for reorganization (firm i). We exclude the workers from the firm that files for reorganization and use data for the year before the filing to assuage concerns that bankrupt firms endogenously affect the structure of the labor market. In very thin labor markets, there might be no workers in the same occupation and county outside of the firm. Therefore, we use the logarithm of 1 plus the number of workers instead of the logarithm of the number of workers.

Thickness (Bernstein et al. (2019)): measure of labor market thickness based on the thickness variable used by Bernstein et al. (2019). We define labor market thickness as:

$$Thickness_{i,m,r,t-1} = \sum_{l} \tau_{m,l} s_{l,r,t-1} \tag{11}$$

where $\tau_{m,l,t-1}$ is the percentage of individuals who transition from occupation m to occupation l in the year before the filing and $s_{l,c,t-1}$ is the percentage of workers employed in occupation l in county r in the year before the filing, divided by the total number of workers in county r in the year before the filing. We exclude the workers from the firm that files for reorganization and use

[12] In Table A14 of the Internet Appendix, we use counties instead of NUTS 3 regions.

data for the year before the filing to assuage concerns that bankrupt firms endogenously affect the structure of the labor market.

Variable component (earnings): Recurring pecuniary supplements (variable *prestações regulares* in *Quadros de Pessoal*) and non-recurring pecuniary supplements, including bonuses (variable *prestações irregulares* in *Quadros de Pessoal*).

Years schooling: Estimated number of years of schooling. We derive this variable from the variable *habilitações literárias* in *Quadros de Pessoal.* The variable is equal to 0 if the worker has less than elementary schooling (*ensino primário*), 4 if the worker completed elementary school, 9 if the worker completed middle school (*ensino básico*), 12 if the worker completed high school (*ensino secundário*), 13 if the worker completed a post-secondary non-higher education course, 15 if the worker has a bachelor's degree (*bacharelato* or *licenciatura*), 17 if the worker has a master's degree (*mestrado*), and 21 if the worker has a PhD (*doutoramento*).

C Data appendix

C.1 *Quadros de Pessoal*

Coverage. *Quadros de Pessoal* is an employer-employee matched dataset that includes jobs regulated by the Portuguese labor law (*Código do Trabalho*), entrepreneurs, and family workers at firms that have at least one job contract regulated by the Portuguese labor law. The dataset only contains employment data for the month of October. Given the reporting requirements, the dataset does not contain government jobs that are regulated by *Lei do Trabalho em Funções Públicas* (LTFP), the labor law applied to civil servants,[13] and independent workers who have no employees. Additionally, the dataset does not contain individuals who are not employed (e.g., unemployed and inactive individiuals).

The dataset reports earnings paid to employees (*trabalhadores por conta de outrém*). Earnings data is missing for the remaining categories of workers: employers (*empregadores* and *membros ativos de cooperativas de produção*) and unpaid family workers (*trabalhadores familiares não remunerados*). *Quadros de Pessoal* reports zero base earnings (*remuneração base paga*) and zero regular hours worked (*horas mensais remuneradas - normais (horas)*) for for individuals on leave, and for furloughed individuals with delayed earnings (Branstetter et al. (2019)).[14]

In Table A16 of the Internet Appendix, we estimate labor outcomes for workers in the sample without employment data in *Quadros de Pessoal*. We utilize the *Inquérito ao Emprego*, an employment survey, to calculate the likelihood of workers transitioning from jobs documented in *Quadros de Pessoal* to jobs that are not documented. *Inquérito ao Emprego* is a quarterly survey using a representative sample of Portuguese residents. It asks respondents about their current and previous year's employment status. There is no identifier that allows us to link workers in the two surveys.

We obtain the number of workers by year and industry who lack employment data in *Quadros de Pessoal*. For each year and industry, we use *Inquérito ao Emprego* to calculate one-year transition outcomes for employees aged 23 to 50 in the previous year who worked in the industry but no longer

[13] Not all government jobs are regulated by LTFP. For example, LTFP typically does not cover workers employed in regulators.

[14] For furloughed workers, the maximum period of delay is two months. We cannot distinguish furlough from leave with the available data.

do. We determine the number of workers by their employment status and exclude those we would expect to observe in the employer-employee matched dataset as per the *Quadros de Pessoal* reporting rules, i.e., employees from all sectors except for "Public Administration and Defense, Compulsory Social Security", "Education", or "Human Health and Social Security", and independent workers who employ at least one person. Then, we compute the distribution of transition outcomes (e.g., unemployment) by industry and region.[15] We estimate that 60% of the workers with missing wages are unemployed, 17% leave the labor force, 15% have a job not reported in *Quadros de Pessoal*, and 6% pursue further education, and 1% have other outcomes (either retire or become homemakers).

Table A16: Estimated outcomes for workers outside of the sample

Transition	% workers in the survey (1)
Unemployment	60%
Inactivity	17%
Self employment	9%
Government	6%
Education	6%
Other	1%
Observations (Quadros de Pessoal)	14,345
Observations (Employment Survey)	1,163

Notes. The table reports estimated labor reallocation outcomes for the outcomes of workers who have no employment in *Quadros de Pessoal*.

In Panel A of Table A5, we estimate the effect of reorganization on earnings using survey estimates when employment data is unavailable. We estimate the probability of each unobserved employment outcome for workers without employment data, conditionally on their industry before the filing and on the year.[16] We replace missing earnings for each employment category with

[15] We consider the seven regions from the Nomenclature of Territorial Units for Statistics (NUTS 2) by Eurostat: Norte, Algarve, Centro, Área Metropolitana de Lisboa, Alentejo, Região Autónoma da Madeira, Região Autónoma dos Açores. This is the most disaggregate administrative subdivision that is available simultaneously in all datasets used in the analysis.

[16] If there is no employment survey data for any given industry-region pair, then we use aggregate survey data at the industry level. If there is no data at the industry level, then we use aggregate data at the region level.

earnings estimates from *Inquérito às Condições de Vida e Rendimento* (ICOR). This survey provides yearly earnings data for workers that are not documented in *Quadros de Pessoal*. We assume that the earnings of civil servants are equal to the average earnings for employees in the "Public Administration and Defense, Compulsory Social Security", "Education", or "Human Health and Social Security" sectors in ICOR. The earnings of self-employed workers is equal to the average earnings reported by self-employed workers without employees in ICOR. Concerning the remaining employment categories, we assume that unemployed workers earn 65% of their pre-filing earnings, aligning with the legal unemployment insurance level (Segurança Social (2022)). Retired workers have earnings equal to pre-filing earnings. Inactive individuals, students and domestic workers have no earnings.

The ICOR dataset available for research at Statistics Portugal does not provide region data before 2018. We use region-level earnings estimates in 2018. Before 2018, we obtain nationwide earnings estimates, and then multiply these estimates by the ratio between the earnings estimate for each region in 2018 and the national estimate in 2018.

We use data from *Sistema de Contas Integradas das Empresas* (SCIE) to estimate the earnings of employers. If the worker has an employment record as employer, then earnings will be equal to the remuneration of corporate bodies of the firm (*remunerações dos órgãos sociais*), divided by the number of employers of the firm, and then divided by 12. If the worker has an employment record as an unpaid family worker, or if the worker has a record as a laid-off worker or as being on leave, then we replace total earnings for that worker by total earnings from the previous year.

Individual-year panel. The *Quadros de Pessoal* data file contains job-level data, meaning that if in any given year a worker has more than one job, then the dataset contains multiple observations in that year. We create a panel dataset with one observation for each year. For each worker-year pair, we order jobs by earnings in descending order. We sum up the variables that can be summed up across jobs (e.g., earnings and hours worked). For the remaining variables (e.g., years of schooling), we keep the value associated to the job with the highest earnings.

Reporting gaps. As discussed in Section 3, data for firms that file for reorganization in *Quadros de Pessoal* has two reporting gaps: 1) some firms do not submit data to *Quadros de Pessoal* in the last period before the filing (9% of all observations) apparently because they are closed in the following year, when they should submit employee records; 2) some firms fail to report worker data after the filing but report having more than 100 workers in their financial statements (1% of all observations).

To address the first reporting gap, for all workers in the sample we replace data in the year before the filing with data from the previous year, whenever such data are available. To address the second reporting gap, we select the workers who were employed at firms with missing data before the filing and impute this missing data with data from previous years. In Section E of the Internet Appendix, we repeat the main analysis by measuring the effect of reorganization on worker outcomes for workers who are employed at filers two years before the filing. In this alternative analysis we use the second year before the filing as the reference year and do use these procedures to address reporting gaps. The alternative approach avoids the selection issue caused by only some firms reporting data in *Quadros de Pessoal* in the year before the filing by only using data from two years before the filing. Additionally, it does not use imputed worker data for firms that report SCIE data but not *Quadros de Pessoal* data.

C.2 O*NET

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

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

Occupations in *O*NET* are classified according to the SOC system, while our employer-employee matched dataset uses the *Classificação Portuguesa das Profissões* (CPP-2010). CPP-2010 uses the same occupation codes as the *International Standard Classification of Occupations* (ISCO-08), but includes additional military occupations that do not exist in ISCO-08. To merge O*NET with *Quadros de Pessoal*, we exclude these military occupations from the analysis, and merge occupation codes with the SOC/ISCO crosswalk maintained by the US Bureau of Labor Statistics, taking score averages when ISCO codes have more than one SOC code correspondence.

C.3 Bankruptcy data

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

We collect information for cases that were filed between May 2012 (inception of the reorganization system) and December 2016. For each reorganization case we collect all records (*Atos*) dated between the filing date and December 2018. Figure A7 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*)[17]; type of case (*Espécie*), e.g. reorganization; 4) record date (*Data*); 5) original case filing date (*Data de propositura da acção*); 6) debtor designation and unique tax ID (*Requerente* or *Devedor* or *Insolvente*); 7) Trustee ID (*Administrador Insolvência*); 8) Creditor names (*Credor*) and tax IDs (*NIF/NIPC*).

Some records have an associated PDF file with additional information (under *Ver mais* from Figure A7 of the Internet Appendix). Figure A8 of the Internet Appendix shows the PDF file associated with the record from Figure A7 of the Internet Appendix. 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 A7.
- Tax ID: tax ID of the debtor obtained from field *NIF/NIPC*. Some cases do not have an associated tax ID. In these situations we use the reported company name to search for the tax ID. Some documents have incorrect tax IDs, often because debtors show up as creditors and vice-versa. We verify tax IDs using *Informação Empresarial Simplificada* (a dataset with financial statements for non-financial corporations available at Banco de Portugal) and *SPAI* (a firm register kept by Banco de Portugal containing data on firms' sector of activity). We

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

create a list of cases containing the filings of the tax IDs that are the largest in terms of assets and filings from firms that operate in the financial sector. We check whether these firms actually filed for reorganization or are just creditors of the filer. Whenever we find an incorrect tax ID we search for the name of the debtor to obtain the correct one. We also correct for inconsistencies that we detect by comparing extractions of the same data in Figure A7 from the Internet Appendix performed at different points in time. We use the tax ID to merge the bankruptcy dataset with the employer-employee dataset and firm financial statements described in Section 3.

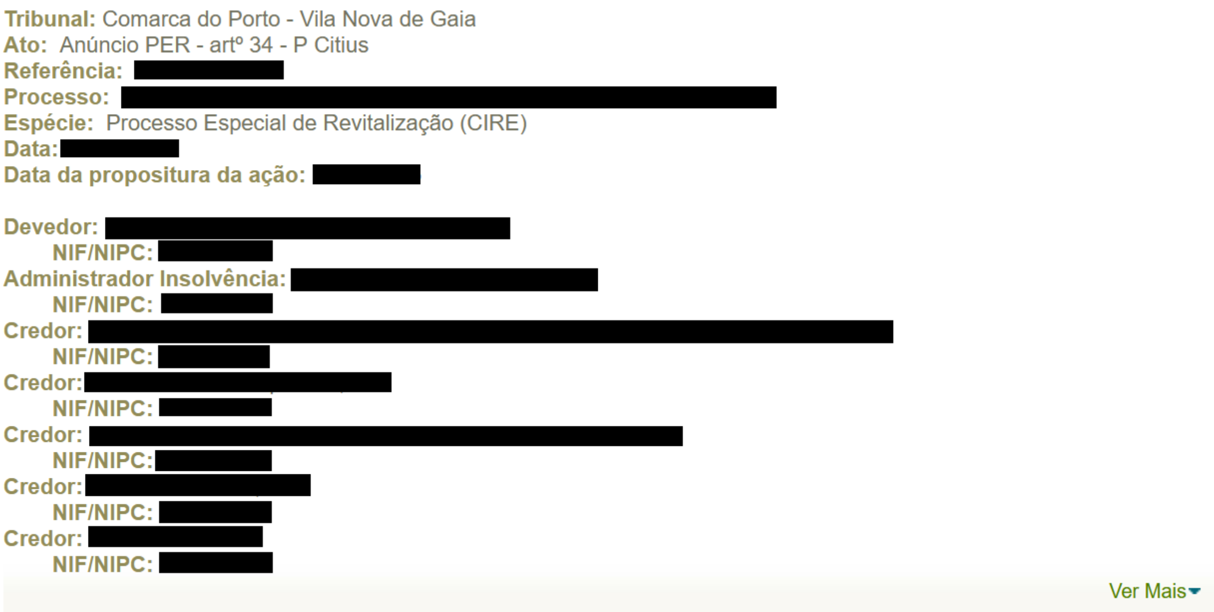
- Training sample: indicator variable that is equal to 1 for entities outside the scope of the paper. The case is outside the scope of the paper if it does not satisfy at least one of these conditions: 1) there is financial statement data for the debtor in the SCIE dataset described in Section 3; 2) the firm has more than one employee in *Quadros de Pessoal* (i.e., not a firm without employees or independent workers). We do not include cases from the training sample in the analysis but use them to obtain the instrumental variable.

- Court ID: court identification number generated from court names reported in field *Tribunal* from Figure A7 of the Internet Appendix. Portuguese courts are organized in districts (*comarcas*). In 2014 Decree-law *Dec. lei n.º 49/2014* reformed the Portuguese court map. This law changed court names, extinguished some courts, reallocated other courts to new districts, and created court sub-units to handle special types of case. The court ID variable reflects the last name of each court. We obtain this name by establishing a correspondence between court names before and after the reform. We create a list of all cases that are transferred between old and new court names. For each old court we associate the new court name that has the most transfers. In Section E of the Internet Appendix we create alternative court fixed effects using the original court names from the field *Tribunal* in Figure A7 of the Internet Appendix, adding the sub-unit from the field *Processo* to the court name whenever the case is assigned to one of these sub-units.

- Filing date: date when the case was filed by the debtor, reported as *Data de propositura da ação* in Figure A7 of the Internet Appendix.

- Year: filing year, generated from the filing date.
- Judge ID: judge identification number generated from judge names reported in PDF files (Figure A8 of the Internet Appendix). Judges are allocated to courts annually by the institution that ensures the self-management of the judiciary (*Conselho Superior de Magistratura*, CSM). The allocation process is regulated by Decree-law *Dec. lei n.º 49/2014* since September 2014. Previously the process was regulated by *Law Lei n.º 3/99.* Some documents do not have a judge name assigned to it. In such cases we order documents by date and impute judge names from the previous document. Some cases are allocated to more than one judge. In the main analysis we create a separate ID for these situations within each court-year pair. Alternatively, in Section E of the Internet Appendix, we use the ID available in the most recent document.
- Case outcome: dummy variable that is equal to 1 for cases that end with an accepted reorganization plan. We create this variable using *Ato* from Figure A7.

Figure A7: 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 A8: 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.

D Estimating production functions

In order to compute the marginal revenue product of labor used in Equation 9, we need to estimate firms' output elasticity of labor θ_L. We estimate the following second-order translog revenue production function at the firm level:

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

with:

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

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

Our baseline estimates follow the estimation procedure from Lenzu and Manaresi (2019) and Gandhi et al. (2020). We deflate nominal variables using a procedure that is similar to the one used by Blattner et al. (2023). We retrieve price indices for Portugal from Eurostat. We obtain real output and intermediate inputs by deflating variables with 2-digit or 3-digit industry price indices. In industries without price indices we use the agricultural price index, service price index, or consumer price index, depending on the industry. We deflate capital using the capital goods price index.

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

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

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

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

Table A17 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 A17: Output elasticity estimates

	All firms (1)	Reorganization (2)	No reorganization (3)
θ_k	0.094	0.098	0.089
	(0.069)	(0.071)	(0.066)
θ_l	0.434	0.438	0.428
	(0.165)	(0.164)	(0.167)
θ_m	0.539	0.533	0.548
	(0.172)	(0.171)	(0.174)
Sum	1.067	1.069	1.065
	(0.078)	(0.079)	(0.077)

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

E Alternative empirical models

In this section we provide robustness tests using alternative empirical models. Table A18 depicts outcomes split into two panels (Panel A and Panel B) to fit all the analyses.

2-year lag: As explained in Section C.1 of the Internet Appendix, we measure the effect of reorganization on labor outcomes for workers who are employed at filers two years before the filing, use the second year before the filing as the reference year, and do not do not impute data for firms that report no data in *Quadros de Pessoal* but report data firm data in SCIE.

Absolute leniency: We estimate judge leniency excluding the second term from equation (2) and including the court case acceptance rate as an additional explanatory variable to correct for exclusion bias (Fafchamps and Caeyers (2020)).

Bootstrap: 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.

Court-level clustering: We cluster standard errors at the court level instead of clustering at the court-year level.

Drop cases: We drop cases for which we do not have enough data to obtain the instrumental variable using equation (3).

Drop dismissals: We drop the cases categorized as dismissals (see Table 1) from the sample.

Firm aggregation: We aggregate worker-level data at the case level by averaging out worker-level variables, and then estimate the empirical model at the case level instead of estimating it at the worker level.

Firms in the sample: We estimate the instrumental variable from equation (3) only using the reorganization cases included in the sample. With this restriction, we exclude other cases such as cases of firms without employees and cases of individuals.

Hüther and Kleiner (2022): Hüther and Kleiner (2022) argue that unsecured hedge fund creditors can predict the assignment of cases to judges and influence the timing of bankruptcy filings to secure more reorganization-friendly judges. Their proposed mechanism works as follows: unsecured hedge funds in the United States prefer reorganization to liquidation because the recovery rate for unsecured debt is higher under reorganization. US judges have the discretion to convert or not convert corporate reorganization cases into liquidation cases. Courts strive to maintain a balanced workload across judges. Therefore, a judge who receives a large corporate reorganization case at a given time is less likely to receive other reorganization cases in the following seven days. If creditors wish to obtain a more reorganization-friendly judge, they incentivize debtors to file for reorganization after liquidation-friendly judges receive large cases. Hüther and Kleiner (2022) provide evidence that unsecured hedge funds engage in this practice. If courts assign large cases to more liquidation-friendly judges in the previous seven days, these judges will be busy, and courts will assign new cases to more reorganization-friendly judges. This behavior creates a negative relationship between the average conversion rate for current cases and the average conversion rate for cases assigned in the previous seven days, as reorganization-friendly judges are more likely to receive cases after liquidation-friendly judges. Hüther and Kleiner (2022) suggest that researchers should residualize the judge leniency variable using the average conversion rate of judges who received previous cases in the same court within a short timeframe (e.g., seven days) as an explanatory variable. If unsecured creditors exploit the same mechanism in Portugal, they will incentivize filings after less reorganization-friendly get cases, so that they are assigned a more reorganization-friendly judges. Therefore, the relationship between the instrumental variable for case i and the average instrumental variable for the cases filed in the same court in the previous seven days will be negative. We test for this hypothesis using the following equation:

$$Z_{i,j,c,t} = \alpha + \beta S_{,c,t} + \delta X_{e,i,t-1} + \delta_{c,t} + \epsilon_{e,i,t} \tag{15}$$

$Z_{i,j,c,t}$ is the instrumental variable obtained with equation (3). $S_{c,t}$ is the average instrumental variable for cases filed in the same court in the previous seven days. We report a positive and statistically insignificant estimate for β in the first row of Table A18. Therefore, we find no evidence for the mechanism proposed by Hüther and Kleiner (2022) in the Portuguese setting. In Table A18 we re-estimate the effect of reorganization on labor outcomes, including $S_{c,t}$ as an additional explanatory variable. The estimates are similar to our main estimates.

Judicial decision: we use the judge name from the most recent document with an associated judge name to create the judicial allocation variable.

Non-permanent workers: we exclude workers with open-ended contracts (*contrato sem termo*) from the sample.

Old court FE: we create court identifiers using the field *Tribunal* in Section C.3 of the Internet Appendix, together with the court subdivision name available in the field *Processo*. We do not perform any treatment to harmonize court identifiers before and after the change in the court map in 2014.

OLS: We estimate equation 1 using OLS.

Past cases: We only use the cases that were filed before case i when obtaining the instrumental variable using equation (3).

Sample split: For each court-year pair we order cases by the filing date. We place odd-numbered cases in one subsample and even-numbered cases in the other subsample. For each court-year-subsample pair we obtain the instrument with the other subsample and use court-year-subsample fixed effects.

Unit weights: We estimate the empirical model from Section 4 using equal weights for all workers. This model differs from the main model because in the main model we use as sample weights the inverse of the number of workers in the sample from the same reorganization case.

Table A18: Alternative specifications

(a) Part 1

	Base (1)	2-year lag (2)	Absolute leniency (3)	Bootstrap (4)	Court-level clustering (5)	Drop cases (6)	Drop dismissals (7)	Firm aggregation (8)	Firms in the sample (9)
Instrument	0.362***	0.344***	0.269***	0.362***	0.362***	0.341***	0.388***	0.363***	0.279***
	(0.061)	(0.063)	(0.056)	(0.058)	(0.077)	(0.062)	(0.064)	(0.063)	(0.056)
Job same firm	0.289***	0.238***	0.341***	0.289***	0.289***	0.296***	0.300***	0.286***	0.442***
	(0.071)	(0.073)	(0.105)	(0.083)	(0.086)	(0.077)	(0.069)	(0.072)	(0.108)
Earnings growth	0.204**	0.217**	0.267**	0.204*	0.204**	0.204**	0.232***	0.196**	0.420***
	(0.091)	(0.096)	(0.133)	(0.116)	(0.087)	(0.098)	(0.085)	(0.093)	(0.136)
Job (extensive margin growth)	0.063	0.132*	0.072	0.063	0.063	0.056	0.094	0.059	0.219**
	(0.076)	(0.073)	(0.106)	(0.087)	(0.084)	(0.081)	(0.069)	(0.078)	(0.102)
Earnings growth (intensive margin)	0.140***	0.085	0.195**	0.14**	0.140**	0.148***	0.138***	0.137**	0.201***
	(0.052)	(0.055)	(0.082)	(0.067)	(0.056)	(0.057)	(0.047)	(0.053)	(0.070)
Earnings other firm	0.099***	0.066**	-0.114	0.099***	0.099***	0.097***	0.103***	0.050*	0.096***
	(0.028)	(0.032)	(0.369)	(0.033)	(0.030)	(0.029)	(0.029)	(0.025)	(0.026)
Time to leave (years)	1.094***	0.815***	1.240***	1.094***	1.094***	1.104***	1.048***	1.090***	1.470***
	(0.250)	(0.253)	(0.360)	(0.316)	(0.288)	(0.268)	(0.258)	(0.257)	(0.322)
P(job quintile 5)	0.070***	0.058*	0.097***	0.07	0.070***	0.072***	0.068***	0.068***	0.111***
	(0.024)	(0.033)	(0.036)	(0.043)	(0.020)	(0.026)	(0.023)	(0.024)	(0.042)
Earnings growth (low tenure)	0.317***	0.363***	0.412***	0.317**	0.317***	0.319***	0.330***	0.279**	0.662***
	(0.107)	(0.118)	(0.155)	(0.147)	(0.113)	(0.116)	(0.106)	(0.117)	(0.179)
Earnings growth (high tenure)	0.055	0.049	0.059	0.055	0.055	0.049	0.112	0.130	0.039
	(0.156)	(0.168)	(0.232)	(0.194)	(0.175)	(0.166)	(0.139)	(0.123)	(0.183)
Earnings growth (less than HS)	0.001	0.101	-0.020	0.001	0.001	-0.012	0.039	0.012	0.225*
	(0.108)	(0.132)	(0.160)	(0.149)	(0.110)	(0.114)	(0.100)	(0.114)	(0.117)
Earnings growth (HS or higher)	0.377***	0.274**	0.486**	0.377**	0.377***	0.402**	0.402***	0.358**	0.699***
	(0.140)	(0.120)	(0.204)	(0.182)	(0.127)	(0.156)	(0.141)	(0.157)	(0.269)
Earnings growth (thin markets)	0.406**	0.276*	0.647	0.406	0.406**	0.440*	0.411**	0.425**	0.646**
	(0.194)	(0.154)	(0.403)	(0.363)	(0.176)	(0.226)	(0.198)	(0.170)	(0.278)
Earnings growth (thick markets)	0.039	0.170	0.039	0.039	0.039	0.038	0.100	0.043	0.236
	(0.115)	(0.137)	(0.140)	(0.129)	(0.114)	(0.120)	(0.093)	(0.121)	(0.156)
Firm component	0.176***	0.115	0.240***	0.176*	0.176***	0.184***	0.193***	0.175***	0.279**
	(0.057)	(0.070)	(0.087)	(0.107)	(0.059)	(0.062)	(0.055)	(0.057)	(0.115)
Bargaining component	0.042	0.019	0.051	0.042	0.042	0.039	0.022	0.004	0.012
	(0.077)	(0.064)	(0.104)	(0.086)	(0.077)	(0.082)	(0.064)	(0.071)	(0.095)
P(firm quintile 5)	0.058**	0.042	0.083**	0.058	0.058**	0.061**	0.061**	0.057**	0.090**
	(0.026)	(0.029)	(0.037)	(0.043)	(0.025)	(0.028)	(0.025)	(0.026)	(0.037)
Annual earnings (low labor gap, thickness, education)	-0.287	1.682	-0.547	-0.287	-0.287	-0.163	0.596	1.288	-0.237
	(1.142)	(1.832)	(1.363)	(1.746)	(0.947)	(1.188)	(1.112)	(1.291)	(1.718)
Labor gap (low labor gap, thickness, education)	-9.254***	-4.479	-10.840**	-9.254*	-9.254***	-9.668***	-8.798***	-6.369*	-11.157**
	(3.475)	(4.066)	(4.394)	(4.979)	(2.458)	(3.631)	(3.331)	(3.705)	(4.371)
Annual earnings (other workers)	4.497***	4.054***	6.307**	4.497*	4.497***	4.742***	4.589***	4.306***	6.107**
	(1.551)	(1.445)	(2.570)	(2.363)	(1.648)	(1.711)	(1.470)	(1.364)	(2.372)
Labor gap (other workers)	1.070	2.495	1.132	1.07	1.070	1.428	3.378	1.655	6.988
	(3.552)	(3.316)	(5.266)	(4.522)	(3.947)	(3.782)	(2.907)	(2.931)	(4.739)
Observations	47,462	50,834	47,462	47,462	47,462	44,700	45,396	2,471	47,462

(b) Part 2

	Base (1)	Hüther and Kleiner (2022) (2)	Judicial decision (3)	Non-permanent workers (4)	Old court FE (5)	OLS (6)	Past cases (7)	Sample split (8)	Unit weights (9)
7-day acceptance rate		0.054							
		(0.068)							
Instrument	0.362***	0.426***	0.219***	0.469***	0.323***		0.283***	0.283***	0.326***
	(0.061)	(0.069)	(0.055)	(0.076)	(0.067)		(0.049)	(0.057)	(0.113)
Job same firm	0.289***	0.185***	0.367***	0.253***	0.291***	0.174***	0.378***	0.412***	0.277*
	(0.071)	(0.070)	(0.132)	(0.076)	(0.081)	(0.011)	(0.097)	(0.103)	(0.157)
Earnings growth	0.204**	0.177**	0.316*	0.293**	0.306**	0.053***	0.184*	0.216*	0.143
	(0.091)	(0.089)	(0.168)	(0.125)	(0.120)	(0.017)	(0.106)	(0.126)	(0.114)
Job (extensive margin growth)	0.063	0.108	0.184	0.087	0.098	0.044***	0.097	0.106	0.021
	(0.076)	(0.078)	(0.135)	(0.099)	(0.092)	(0.013)	(0.093)	(0.095)	(0.076)
Earnings growth (intensive margin)	0.140***	0.069*	0.132	0.206***	0.209***	0.009	0.086*	0.110*	0.122
	(0.052)	(0.041)	(0.094)	(0.079)	(0.068)	(0.008)	(0.050)	(0.066)	(0.081)
Earnings other firm	0.099***	0.065**	0.066**	0.172***	0.116***	0.017	0.050*	0.049*	0.073**
	(0.028)	(0.029)	(0.033)	(0.057)	(0.031)	(0.013)	(0.026)	(0.029)	(0.030)
Time to leave (years)	1.094***	0.900***	0.941**	0.780***	1.056***	0.779***	1.220***	1.285***	1.213**
	(0.250)	(0.264)	(0.396)	(0.290)	(0.284)	(0.044)	(0.307)	(0.331)	(0.484)
P(job quintile 5)	0.070***	0.062**	0.042	0.112***	0.094***	-0.005	0.072**	0.039	0.093
	(0.024)	(0.029)	(0.048)	(0.042)	(0.033)	(0.005)	(0.031)	(0.041)	(0.075)
Earnings growth (low tenure)	0.317***	0.336***	0.379*	0.311**	0.401***	0.059***	0.245	0.270*	0.113
	(0.107)	(0.104)	(0.209)	(0.121)	(0.152)	(0.023)	(0.162)	(0.151)	(0.079)
Earnings growth (high tenure)	0.055	-0.011	0.289	-0.072	0.203	0.049**	0.119	0.127	0.256
	(0.156)	(0.143)	(0.274)	(0.604)	(0.180)	(0.022)	(0.163)	(0.190)	(0.264)
Earnings growth (less than HS)	0.001	0.007	0.119	-0.031	0.100	0.057***	-0.055	-0.028	0.070
	(0.108)	(0.111)	(0.190)	(0.150)	(0.135)	(0.021)	(0.145)	(0.165)	(0.140)
Earnings growth (HS or higher)	0.377***	0.358***	0.599*	0.466**	0.475**	0.048**	0.363**	0.412**	0.251**
	(0.140)	(0.130)	(0.328)	(0.186)	(0.186)	(0.024)	(0.173)	(0.183)	(0.112)
Earnings growth (thin markets)	0.406**	0.429**	0.287	0.637**	0.651**	0.058**	0.297	0.327	0.230
	(0.194)	(0.174)	(0.248)	(0.321)	(0.314)	(0.023)	(0.193)	(0.218)	(0.248)
Earnings growth (thick markets)	0.039	0.024	0.330*	0.127	0.107	0.049*	0.055	0.180	0.082
	(0.115)	(0.124)	(0.183)	(0.141)	(0.128)	(0.026)	(0.138)	(0.158)	(0.076)
Firm component	0.176***	0.091	0.093	0.277***	0.235***	-0.004	0.077	0.097	0.260
	(0.057)	(0.064)	(0.111)	(0.091)	(0.083)	(0.013)	(0.065)	(0.077)	(0.178)
Bargaining component	0.042	0.052	0.088	0.005	0.082	0.024**	0.054	0.047	-0.083
	(0.077)	(0.077)	(0.145)	(0.084)	(0.089)	(0.012)	(0.086)	(0.068)	(0.087)
P(firm quintile 5)	0.058**	0.059*	0.020	0.121***	0.080**	-0.002	0.038	0.055	0.132
	(0.026)	(0.030)	(0.048)	(0.039)	(0.035)	(0.006)	(0.035)	(0.034)	(0.090)
Annual earnings (low labor gap, thickness, education)	-0.287	-0.664	1.125	0.053	0.090	0.878***	-2.151	1.965	0.314
	(1.142)	(1.379)	(2.204)	(1.723)	(1.165)	(0.329)	(2.700)	(1.886)	(0.923)
Labor gap (low labor gap, thickness, education)	-9.254***	-13.508**	-0.263	-9.037*	-8.817**	-0.679	-8.804	-10.031**	-14.123**
	(3.475)	(5.427)	(5.925)	(4.634)	(3.809)	(0.708)	(5.572)	(4.974)	(5.891)
Annual earnings (other workers)	4.497***	3.936**	5.685**	6.379***	6.739***	0.641**	3.891**	3.441	4.229
	(1.551)	(1.532)	(2.841)	(2.077)	(2.328)	(0.300)	(1.616)	(2.127)	(2.881)
Labor gap (other workers)	1.070	5.474	2.444	1.768	0.611	0.704	4.819	5.575	-0.950
	(3.552)	(3.562)	(5.927)	(3.943)	(4.586)	(0.469)	(4.526)	(4.275)	(5.328)
Observations	47,462	33,755	47,462	12,323	47,462	47,462	47,462	47,462	47,462

Notes. The table reports robustness checks described in Section E from the Internet Appendix. We include the set of control variables from column (2) of Table 2. Each line depicts estimates for the effect of reorganization on an outcome under different assumptions. We estimate these outcomes using equation (4), except in the robustness tests where we spectify a different equation in Section E of the Internet Appendix. Standard errors, clustered at the court-year level, are shown in parentheses. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

F Selection into employment

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

$$selection\ dummy_{e,\tau} = \beta Z_{i,j,c,t} + \lambda I_{e,t}^{\geq 45yo} + \gamma X_{e,i,t-1} + \delta_{c,t} + \epsilon_{e,\tau} \tag{16}$$

where $selection\ dummy_{e,\tau}$ 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 $X_{e,i,t-1}$, being over 45 years old should not affect labor outcomes for employed workers. Empirically, we find a strong negative relationship (significant at the 1% level) between being at least 45 years old and having a job contract, but no statistically significant relationship between being at least 45 years old and earnings growth for workers with jobs, conditional on control variables used throughout the analysis.

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

$$Y_{e,\tau} = \alpha + \beta.\widehat{Reorganization}_{i,t} + \gamma X_{e,i,t-1} + \delta_{c,t} + IMR_{e,\tau} + \epsilon_{e,\tau} \tag{17}$$

where $IMR_{e,\tau}$ is the Inverse Mills Ratio computed using estimates from Equation 16. The remaining variables come from Equation 4. We compute cluster bootstrap standard errors at the court-year level to account for the fact that the Inverse Mills Ratio is estimated.

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