Corporate Bankruptcy and the Reallocation of Managerial Talent*

Morten Grindaker† Andreas R. Kostøl‡ Kasper Roszbach§

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Abstract: Corporate bankruptcy epitomizes the process of creative destruction, but how costly it is for top managers depends on whether their skills are firm-specific and if it taints their resumés. In theory, such career costs can distort incentives to undertake risky projects and the allocation of managerial talent. However, existing empirical evidence is hampered by a potential bias from adverse selection of top managers into bankruptcy-prone firms. This paper overcomes the identification challenge and offers causal evidence on the career outcomes of chief executive officers (CEOs) using a unique combination of quasi-experimental variation in bankruptcy and rich administrative data from Norway. Our contribution builds on the random assignment of bankruptcy petitions to judges who differ systematically in their tendency to liquidate a firm. We use judges' liquidation rate as an instrument for bankruptcy and estimate the impacts of corporate bankruptcy using administrative records on CEOs' new employers, job titles, income and wealth. We draw three main conclusions from our empirical analysis. First, CEOs of liquidated firms are quickly employed in new firms but are 30 percentage points less likely to hold the top executive position than CEOs of firms where the petition is dismissed. This displacement causes a decline in the net present value of their future capital income, equal to about 60 percent of annual pre-bankruptcy income. In contrast, we do not detect any statistically significant effect on wage income. Second, our evidence shows that managerial skills are highly portable. Displaced CEOs are more likely to move to new industries and geographic areas and find jobs at higher-quality firms but in positions lower in the corporate hierarchy. Third, average impacts mask substantial heterogeneity across industry conditions. We show that the displacement effects are significantly stronger when industry conditions are good, suggesting that marginal bankruptcy cases are (erroneously) used as a signal of managerial ability. Our findings are consistent with information frictions that entail important social costs that can impede aggregate entrepreneurship and job growth.

Keywords: Occupational Choice; Bankruptcy; CEO; Organizations; Executive Compensation.
JEL codes: G33, J24; K22, L29, M12

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†Norwegian Business School; Norges Bank. E-mail: morten.grindaker@bi.no
‡Arizona State University, W.P. Carey School of Business; Norges Bank; NBER; IZA. E-mail: andreas.r.kostol@gmail.com
§Norges Bank; University of Groningen. E-mail: kasper.roszbach@norges-bank.no

1 Introduction

Economists often view corporate bankruptcy as part of the process of creative destruction.[1] But how costly is corporate bankruptcy to top managers' careers? The answer depends in part on whether the bankruptcy is perceived as a signal of organizational failure or lack of managerial qualifications (e.g., Gibbons and Katz, 1991), and the extent to which compensation reflects specific skills valued less by outside employers (e.g., Becker, 1962 and Oi, 1962). The consequences of these career costs can extend beyond the executive labor market – with potential implications for aggregate growth and employment, for example by distorting the allocation of talent or inducing chief executive officers (CEOs) to undertake fewer risky projects than what is socially desirable (e.g., Jensen and Meckling, 1976, Murphy et al., 1991, Eckbo and Thorburn, 2003, and Gormley and Matsa, 2016). In spite of these potential implications, empirical evidence on the career costs of corporate bankruptcy and the channels through which they operate remains scarce.[2]

The knowledge gap has proven difficult to bridge, in part because corporate bankruptcy typically doesn't occur randomly. Instead, bankruptcy decisions are confounded by unobserved heterogeneity and selection of low-ability managers into bankrupt firms. Bankrupt CEOs may, on average, have poorer outside options because of unobserved traits or because they tend to operate in declining industries.[3] Without addressing this selection problem, an observational analysis would be biased towards concluding that CEOs suffer financially from bankruptcy. On top of this identification challenge, examining CEOs' income and career trajectories requires large-scale firm-manager panel data to measure executive remuneration components and follow job positions, business start-ups and industry affiliation before and after the firm's default.

In this paper, we take a first step to fill this gap in the literature by identifying the causal effects of corporate bankruptcy – i.e., liquidation – on the income and careers of CEOs. To do so, we combine an experimental setting with a rich data set with administrative information on all Norwegian CEOs. We overcome the identification challenges by exploiting a feature of the Norwegian judicial system that randomizes the assignment of bankruptcy petitions to district judges. Judges declare a firm bankrupt when they deem the company insolvent. While this condition applies equally to all firms, judges need to apply judgment and can vary in their interpretation of the insolvency condition. This scope for discretion is a source of significant variation in judges' bankruptcy rates. We use this exogenous variation in the likelihood of firms being declared bankrupt – i.e., the firm being liquidated through a cash auction and the CEO's contract being terminated – to study how corporate bankruptcy affects CEOs' careers.[4]

[1] See, e.g., Fama and Jensen, 1983, Aghion and Howitt, 1992 and Thesmar and Thoenig, 2000, where creative destruction entails organizational, technological and product-market innovation.

[2] Eckbo et al. (2016) study large listed corporations reorganized under Chapter 11 in the US and find that bankruptcy is associated with substantial costs for the CEO. While this personal cost may affect the CEO's willingness to take risks in the run-up towards bankruptcy, several forces, potentially working in opposite directions, are at work when a firm is declared bankrupt. Hurst and Pugsley (2011) discusses small businesses and the role of government policies aiming to stimulate entrepreneurship and growth.

[3] This selection contrasts with a stigma effect of bankruptcy that would arise if executive labor markets use managing a bankrupt firm as a signal of low managerial ability. Using corporate bankruptcy as a signal of manager ability is analogous to employers using layoffs and unemployment duration as signals of worker ability. Gibbons and Katz (1991) shows that asymmetric information in layoffs can result in lower wages and longer unemployment spells for laid-off workers. Using an audit study, Kroft et al. (2013) show that employers are less likely to invite applicants with unemployment in strong labor markets to interview consistent with informational stigma.

[4] Whereas reorganization (c.f. , Chapter 11 in the U.S.) is available as a formal proceeding in Norwegian bankruptcy law, it is virtually nonexistent in the data. In rare cases, firms are sold as a "going concern" whereby new owners may prefer to keep the old

Our empirical strategy draws on a unique dataset we construct by combining six administrative registers. These enable us to identify CEOs of all private sector businesses, match them with firm characteristics and obtain granular information on earnings, employment, and occupational histories. Our final sample consists of bankruptcy filings of 7,605 unique firms, of which 5,038 are declared bankrupt by a judge. This unique combination of data and an instrumental variable (IV) approach allow us to estimate the average career impacts of bankruptcy-liquidation among CEOs who lost their job because their firm was assigned to a stringent - rather than a lenient judge. We implement this design using two-stage least squares regressions where the first stage equation instruments the indicator variable for whether the firm is liquidated. The second stage identifies the effect of bankruptcy – the difference in CEO career outcomes between cases of court-mandated liquidation and situations where firms' petitions are dismissed so that CEOs have the opportunity to continue in their position. Importantly, the research design holds the quality of the CEOs and their financially distressed firms constant while experimentally varying the extent to which marginal firms are declared bankrupt.

Three broad conclusions emerge from our empirical analysis. First, corporate bankruptcy has a long-lasting impact on CEOs' careers. CEOs whose firms are declared bankrupt are 30 percentage points more likely to exit the executive workforce. Displaced CEOs find new employment quickly but do so by moving to lower-ranked positions in new firms. Bankruptcy also has an economically significant impact on CEO remuneration; we document an annual fall in capital income equal to about five percent of annual gross income. While the net present value of the average decline in capital income over the remainder of a CEO's working-age career is equal to 60 percent of pre-bankruptcy annual income, we find no enduring effect on CEOs' labor income after five years. Second, our evidence shows managerial skills are highly portable. Displaced CEOs are more likely to find jobs at bigger firms that pay higher wages, in positions lower down in a firm's hierarchy and reallocate to new industries and geographic areas. Third, the displacement effects of bankruptcy are significantly stronger when default rates in an industry are low. Exploring heterogeneity in impacts by quartiles of industry-specific variation in bankruptcy, we find that CEOs are five times more likely to be displaced from their position when bankruptcy rates are low instead of high. By contrast, we find no evidence of variation in wage income driven by industry conditions.

We interpret our findings in the light of empirical evidence documenting CEOs' wide discretion over firms' policies (e.g., Bertrand and Schoar, 2003, Jenter and Lewellen, 2015). If this discretion affects a firm's likelihood of bankruptcy, avoiding liquidation may signal to the executive labor market that the manager is of high ability. Conversely, future employers may infer that bankrupt CEOs are of lower quality and offer them less responsibility in their next job. Our finding of smaller occupational displacement effects when industry conditions are poor is consistent with differential strengths of an informational signal (e.g., Gibbons and Katz and Bernstein et al., 2022). In related work, Kroft et al. (2013) performed an experimental audit study and found that unemployment on the resume has a smaller impact on callback rates when aggregate unemployment is high. Our evidence complements their findings.[5] We also document that CEOs with

CEO – which we show only occurs in two percent of all cases in Appendix C.

[5]Our paper also relates to a long literature studying the consequences of job loss – typically documenting larger and more persistent earnings losses when job loss occurs in recessions (see, e.g., Davis and von Wachter, 2011 and Huckfeldt, 2016). Taken together, our findings indicate that labor demand and mismatch can affect workers' outcomes. In contrast, high-skilled workers'

longer tenure in the bankrupt firm are more likely to leave the executive labor market. This supports our interpretation as longer-tenured CEOs are more likely to be held accountable for the firm's performance.

Why does not the loss of executive position lead to persistent wage losses? Our evidence suggests several potential explanations. First, CEOs of small and medium-sized enterprises (SMEs) are fundamentally different from large listed corporations studied in earlier research. Second, CEOs in our data are on average 43 years old, almost ten years younger than in large listed firms. As older CEOs are more likely to have accumulated more firm-specific human capital, the displacement effect of bankruptcy is likely to be higher than for younger CEOs. Our evidence is consistent with older CEOs experiencing higher career costs of bankruptcy. Younger CEOs, i.e., aged 44 and below, are less likely to leave the executive labor market following bankruptcy and do not suffer from a reduction in total income. Third, the evidence on higher industry mobility and movement to more profitable firms suggests that CEOs have highly portable labor market skills (e.g., Gathmann and Schonberg, 2010), which mitigates career costs.[6] Fourth, the impacts on the market income are predominantly driven by changes in variable income components – indicating that some CEOs experience a reduction in remuneration from falling off the corporate career ladder, possibly due to incentive schemes tied to job hierarchies (e.g., Lazear and Rosen, 1981).

Our study advances the understanding of CEO remuneration and executive labor markets (see, e.g., Frydman and Jenter, 2010 for a review). We complement Eckbo et al. (2016), who document the earnings decline for CEOs in 322 large US firms filing for Chapter 11 bankruptcy. They show that two-thirds of their sample of CEOs exit the executive labor market after bankruptcy and that the median fall in remuneration equals five times the annual pre-bankruptcy income over the remainder of their career.[7] Our results are derived in a different context, for predominantly small and medium-sized firms and in a strict experimental setting. Our empirical analysis offers, to the best of our knowledge, the first experimental evidence of CEO careers, allowing us to overcome the fundamental econometric identification challenge of separating the causes from selection into bankruptcy. While we lack the precision to draw firm conclusions in some heterogeneity dimensions, our evidence indicates that younger CEOs go through bankruptcy unscathed while older CEOs incur substantial costs. Our main contribution, however, is to document important heterogeneity in displacement effects by industry conditions – highlighting an important role for informational frictions in executive labor markets.

Our work contributes to the literature that studies the consequences of - and implications of frictions associated with corporate bankruptcy.[8] Shleifer and Vishny (1992) demonstrate that default costs are higher when assets are specific to an industry and the market is thin. Consistent with this, Bernstein et al. (2019a) find that straight bankruptcy (c.f. Chapter 7 in the US) often leads to an inefficient allocation of assets, es-

outcomes, such as CEOs, are shaped by informational frictions rather than aggregate demand factors.

[6] Blanchard and Katz (1992) discuss and document the slow reallocation of workers to cities with higher wages, where costly relocation slows down the reallocation and imposes a cost on CEOs. Mortensen and Pissarides (1999) review the literature on search frictions.

[7] Eckbo and Thorburn (2003) show that median income falls by 40% in the Swedish bankruptcy system, where a going-concern auction is mandatory. By contrast, Colonnelli et al. (2022) find little, if any, evidence on career effects for CEOs whose firms close down due to corruption.

[8] Our paper extends studies of labor market frictions by offering evidence from settings with high-skilled labor and high stakes (see, e.g., Ridder and Van den Berg, 2003 for a survey of literature).

pecially when search frictions are large and credit supply is low.[9] In a recent study, Bernstein et al. (2022) document that CEOs exhibit fear of reputational stigma stemming from bankruptcy. Our work complements theirs by showing that the career costs for CEOs are smaller when a bankruptcy is less salient. Chang and Schoar (2013) find that debtor-friendly judges lead to higher firm exit and lower sales after Chapter 11 bankruptcy, and argue that misaligned incentives between owners and managers of distressed firms are a potential explanation. In a recent paper, Graham et al. (2019) establish that employees' annual earnings fall by 67 percent over seven years after their employer files for bankruptcy. Araujo et al. (2021) document large earnings losses among employees following bankruptcy using randomly assigned judges in the formal Brazilian reorganization system, while Bonfim and Nogueira (2021) provide evidence that corporate reorganization partially insures workers, even those who move to other firms, against bankruptcy shocks. We complement these studies by offering causal evidence on CEOs careers, validating the role of reputational stigma in the executive market, and highlighting the transferability of skills across industries.[10]

Methodologically, our work is related to studies that exploit the random assignment of judges to court cases. Our IV strategy is similar to that used by Bhuller et al. (2020) to estimate the impact of incarceration in Norway as well as Kling (2006), French and Song (2013), Di Tella and Schargrodsky (2013), Dahl et al. (2014), Aizer and Doyle Jr (2015), Mueller-Smith (2015), Dobbie et al. (2018), Autor et al. (2019) and van Dijk et al. (2022), for applications of the judge instrumental variables in various settings. We implement the standard judge IV design, using a just identified model and a leave-one-out liquidation rate as an instrument for bankruptcy, similar to the Jackknife estimator in Angrist et al. (1999). However, as judges can only process a limited number of cases per year, the ratio of instruments to the sample size can be large — raising concerns over weak instruments and biased 2SLS estimates. We perform the pre-test suggested by Mikusheva and Sun (2022), which allows for multiple instruments and a variance estimator that accounts for heteroskedasticity. Reassuringly, we find that even when the ratio of instruments over observations equals about one to ten, the pre-test statistic strongly rejects the weak instrument hypothesis.

Finally, our findings have implications for the executive labor market and the economy. First, they inform the debate on the consequences of conflicting interests between CEOs and shareholders (Edmans et al., 2008, Page, 2018). We confirm the presence of a misalignment, albeit weaker than previously reported, and highlight an incentive for CEOs to delay default. CEOs may, for example by rolling over debt or strategically delaying a bankruptcy filing, extend a marginal firm's survival to avoid incurring greater personal costs from stigma when industry conditions are good – which in turn can lower creditors' recovery rates due to thinner markets. Second, we perform a simple accounting exercise that suggests bankruptcy stigma reduces the pool of potential CEOs by up to 13 percent relative to a counterfactual where stigma is eliminated. Next, assuming that CEOs and start-ups search in a standard matching framework and using data on annual vacancy postings for new CEO positions, we calculate that the stigma lowers the annual hiring rate by 6 percent.

[9] Bernstein et al. (2019b) highlight a negative externality of bankruptcy through higher unemployment in the immediate neighborhood of liquidated firms. Antill (2020) demonstrates that over 20 percent of all Chapter 11 bankruptcies reduce creditor recovery.

[10] Our findings are also connected to the literature documenting how CEOs affect firm performance. Bertrand and Schoar (2003) document that manager characteristics affect firms' investment, finances, and organizational practices. Gennaioli et al. (2013) point to the role managerial human capital plays in improving firm productivity. Bandiera et al. (2017) provide evidence that professional CEOs work more than family CEOs and that this explains part of the performance gap between these firms. Bernile et al. (2017) show that CEOs who go through disasters without exceptionally adverse consequences lead more aggressive firms, while those who experience the downsides of disasters lead more cautiously.

The remainder of this paper is organized as follows. In Section 2, we explain the bankruptcy process in Norway and provide extensive details on the institutional background. In Section 3, we describe the data, variables and how we construct the sample. Section 4 outlines the identification challenges and our research design. Section 5 presents our causal evidence on CEOs' career outcomes and possible mechanisms driving our results. Section 6 concludes.

2 Background

This section describes the institutional features of the labor market and the bankruptcy process in Norway.

2.1 The Labor Market in Norway

The Norwegian labor market can be characterized by a combination of institutional regulation and flexibility. Institutional features of the labor market are governed by the Working Environment Act (Ministry of Labour and Social Affairs, 2005) and the Labor Market Act (Ministry of Labour and Social Affairs, 2004). Firms can hire employees on either fixed-term or permanent contracts and fire workers who under-perform relative to their peers or when operating at a loss. Collective bargaining agreements cover nearly all private-sector jobs, and wages and working hours are typically set per collective agreements between unions and employer associations. Union membership is relatively high at 53 percent, compared to other OECD countries, but down from 58 percent in 1992 (OECD, 2020). A two-tier wage setting framework is considered a key reason for Norway's compressed wage structure (Barth et al., 2014). In the first step, average wage growth is set centrally at the industry level, and in the second step local adjustments at the firm level create wage drift within firms. Unemployment benefits in Norway replace 62.4 percent of previous income, approximately the OECD average of 60 percent, for a maximum period of two years. The income base for unemployment benefits is capped at six times the National Insurance Scheme's basic amount, which is NOK 8,500 (about USD 1,000) per month in 2021. As Table 1 shows, the average CEO of a non-distressed firm would experience an income loss of 76 percent per month spent unemployed.

2.2 The Norwegian Court System

The Norwegian court system is organized into three levels: the district court, the court of appeals and the supreme court. As in most court systems, most cases are dealt with at the lowest level. Bankruptcy cases are almost always settled in one of the 60 district courts. All but one of them have more than one judge, while the largest district court has 63 judges. Two types of professional judges work in the district courts, regular judges and deputy judges. In contrast to criminal cases, deputy judges and regular judges are assigned to the same caseload of bankruptcy cases.

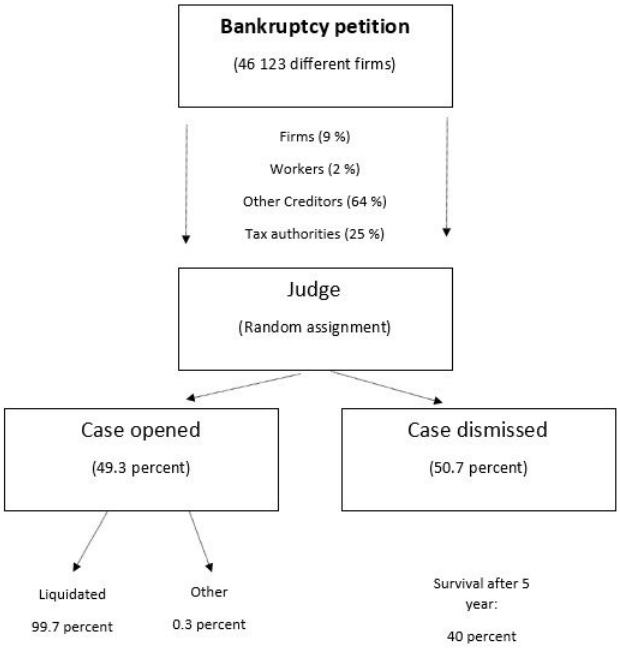
Regular judges are appointed permanently and covered by strong employment protection. Among them, the chief judge oversees the administration of the district court. Deputy judges are also law school graduates, but are appointed to a court for a maximum period of three years (five years in Oslo). We do not have access to data that enables us to distinguish between deputy and regular judges. Bhuller et al. (2020) characterize

the judges in the district courts and report that 370 of the judges were full-time regular judges. Their average age was 53 years and 62 percent were male.

2.3 The Bankruptcy Process in Norway

The resolution of financial distress of companies in Norway is regulated in the Bankruptcy Act (Ministry of Justice and Public Security, 1984). Distress can be resolved through a debt renegotiation or within the court system through a bankruptcy procedure. Opening bankruptcy proceedings in practice means that a firm will be liquidated with near certainty. Creditors can negotiate a voluntary debt settlement outside the court system or reach a compulsory settlement through the district court. In the case of a bankruptcy process, proceedings take place in the district court. The path to bankruptcy in Norway formally begins by filing a petition for protection or an unsecured creditor filing for bankruptcy to cover its losses. The filing in district court may be initiated by the cessation of payments and unsuccessful debt enforcement during the preceding three months, or by non-payment of undisputed due debt following the service of a bankruptcy notice by the creditor. A filer must document that the firm is illiquid and insolvent. Insolvency requires that liabilities exceed assets plus the net present value of continuing the firm (Konkursradet, 2012). Figure 1 visualizes the process for a corporation from financial distress to a bankruptcy petition as well as the possible outcomes in the district court. About 90 percent of all bankruptcy petitions are filed by creditors or the tax authorities, while nine percent is filed by the firm itself and two percent by its employees. Once bankruptcy proceedings are opened by a district court judge, liquidation follows in 99.7 percent of all cases.

Figure 1: **Bankruptcy Process**



Notes: The figure depicts the different steps of the bankruptcy process in Norway and the share of bankruptcy petitions that was opened or dismissed in the period 2004-2018.

When the district court considers an application for bankruptcy at the request of a petitioner, it assesses whether or not the company is insolvent. If the judge finds the firm insolvent, s/he "approves" a bankruptcy filing and "opens bankruptcy proceedings", i.e., a firm is declared bankrupt, and appoints a trustee. The trustee's task is to carry out the bankruptcy proceedings, manage the bankrupt firm's assets and raise funds from the sale of these assets to cover as much of the bankrupt firm's debt as possible. The Norwegian Labour and Welfare Administration's (NAV) wage guarantee scheme covers one month of employees' wages. This includes the wages of a CEO who owns less than 20 percent of the distressed firm. Claims are limited to twice the annual basic amounts, or about NOK 200,000. After one month, the normal unemployment insurance scheme automatically covers lost income. The Coverage Act of 1986 governs certain exceptions. Shareholdings in excess of 20 percent will typically not be considered to have significant influence if there are bigger shareholders.

The trustee can sell the assets either individually or together, as a going concern, whichever generates most income for the creditors in a private sale or a public auction. The district court terminates the administration proceedings once the available funds have been distributed among the creditors. In practice, bankruptcy procedures in Norway consist of liquidation through a cash auction, while all debt renegotiation essentially occurs out-of-court. The trustee also informs the court if there are reasons to impose a bankruptcy quarantine on board members or the CEO. [11]

Norway's bankruptcy system in an international context

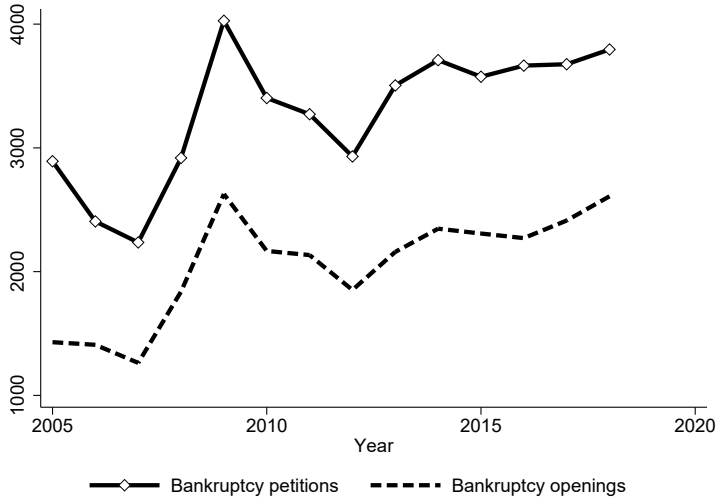
While Chapter 11 bankruptcy is more prevalent among larger firms in the US, Chapter 7 is more common among smaller firms (Bris et al., 2006). In 2019, for example, 14,524 firms filed for Chapter 7 while 5,814 firms filed for Chapter 11 bankruptcy. The structure of the bankruptcy process and the related priority order of claimants likely influence managers' stakes in firms and thereby their compensation structure. Bris et al. (2006) show that Chapter 7 bankruptcy is associated with lower equity shares held by managers than in reorganizations under Chapter 11. In Arizona and New York State, for example, the equity share held by managers was 32 percent in Chapter 7 bankruptcies compared with 43 percent in Chapter 11 cases.[12]

The Norwegian bankruptcy system resembles that of the US in several ways. In both countries, for example, the bankruptcy process's administrative expenses are paid first, before unpaid taxes or debts to government agencies, and wage claims have priority over secured creditors. The evolution of bankruptcies also displays a similar pattern. Figure 2 shows that the number of bankruptcies in Norway rose by about 50 percent from 2007 to 2009. Chapter 7 bankruptcies in the US rose more sharply in the same period,

[11] Bankruptcy quarantine may be imposed if there is a suspicion of a criminal act in connection with the bankruptcy or a person is deemed unfit to establish a new company or be a board member or managing director of such a company due to improper business conduct. If the court decides to place, for example, the CEO in bankruptcy quarantine, then s/he cannot take on new positions on a board or as a CEO of a limited liability company and can even be removed from already held positions. A quarantine lasts for two years and is registered in the Bankruptcy Register. In our estimation sample, only 0.05 percent of CEOs are placed in quarantine. In a robustness check (available upon request), we removed these CEOs from our sample and find the estimates are quantitatively unchanged. The effect on wages in the treatment group rise by less than half a percent.

[12] In addition to human capital and reputational scarring, career costs of bankruptcy can be temporary, including temporary income losses due to the time-consuming executive search process, partly offset by unemployment benefits. CEOs without significant ownership in the firm (i.e., below 20%) are eligible for a wage guarantee, covering up to six months of unpaid wage income up to a ceiling of about USD20,000.

Figure 2: **Bankruptcy Petitions in Norway**



Notes: Figure plots the number of new bankruptcy petitions and the number of straight bankruptcy openings (e.g., Chapter 7 in the US context) in Norway in the period 2005-2018. Dismissed cases do not meet the insolvency criteria. Only the first petition for each firm is included in the figure. There were about 300,000 firms in Norway in 2018.

but subsequently experienced a gradual decline. The Norwegian system also has some distinguishing features. The average ownership share in Norwegian bankruptcies is about 50 percent, i.e., about 20 percentage points higher than in US Chapter 7 cases, as is the fraction of managers with a positive ownership share. Reorganization as a court-steered mechanism to deal with financial distress is less common than in the US. The risk of liquidation during the renegotiation or reorganization process may explain this. As a result, both renegotiation and reorganization primarily occur outside the court-run bankruptcy system, prior to a possible bankruptcy filing.

2.4 CEO Characteristics

Next, we compare the characteristics of CEOs in the overall sample in 2015 to CEOs of firms being petitioned for bankruptcy during 2015-2018. Total compensation consists of several fixed and variable components. In our administrative data, described in more detail in the next section, we observe fixed wages, bonus payments, overtime pay, severance pay and non-standard wage components, which include, among other things, taxable benefits, such as certain business travel-related cost reimbursements, and sold options. The data also allows us to measure the ownership structure and whether the CEO is hired externally or internally.

Table 1 displays summary statistics of pay, workforce and ownership characteristics, measured in 2015. The first row shows that CEOs in distressed firms are compensated 51 percent less than their CEO peers in the overall population of firms. Nearly 75 percent of this compensation difference can be ascribed to lower bonus payments, about 20 percent to differences in fixed wages while the remaining part stems from other components, including taxable benefits, the sale of options and various irregular supplements linked to a position. Not surprisingly, CEO's in firms that are near bankruptcy receive little bonus pay, while their peers in non-distressed firms earn about 40 percent of their total compensation through bonuses. Column

(1) documents that capital income, and in particular stock income, constitutes a large share of total CEO remuneration in non-distressed firms, where capital income accounts for about 35 percent of total compensation. In contrast, column (3) shows the corresponding number is only four percent in distressed firms. In terms of ownership, the rate is 12 percent in distressed firms, about twice as large as their peers in non-distressed firms. However, the fraction of total ownership shares is somewhat smaller in distressed firms (2.2 vs 2.8 percent) – showing that ownership-based control and daily management in the average firm are clearly separated. Regarding number of employees, CEOs of healthy businesses manage more than three times as many employees as their distressed peers. The within-firm wage dispersion between CEOs and employees is relatively low compared to the US (Lazear et al., 2015). Including bonuses, CEOs of non-distressed firms receive 2.8 times the wage of an average employee, compared with 2.3 times in distressed firms. Taken together, comparing compensation components and characteristics across the two groups, the t-statistics in column (5) show that the differences between CEOs in distressed and non-distressed firms are all statistically significant.

Table 1: **Summary Statistics for CEO Pay**

	(1)	(2)	(3)	(4)	**(5)**
	Non-Distressed		**Financially Distressed**		**Test of equal means**
	Mean	Std. Dev.	Mean	Std. Dev.	t-stat
Total monthly CEO wage compensation	11,983	[728,282]	6,173	[8,122]	-2.70
Fixed wage (USD)	6,730	[25,440]	5,627	[6,581]	-8.15
Bonus pay (USD)	4,639	[703,493]	289	[3,157]	-2.62
Overtime pay (USD)	61.859	[828.437]	33.837	[609.935]	-2.09
Total market income (monthly)	16,618	[35,568]	7,569	[7,909]	-52.75
Capital Income (USD)	5,673	[32,279]	324	[5,190]	-40.99
Stock Income (USD)	4,863	[29,988]	316	[2,581]	-45.86
Manager hired externally	0.222	[0.416]	0.216	[0.411]	0.63
Share of CEOs with ownership	0.061	[0.240]	0.118	[0.323]	10.22
Ownership share	0.028	[0.137]	0.022	[0.122]	2.71
Number of Full-time Employees	22.0	[240.9]	6.3	[37.1]	16.49
Ratio of CEO / Average Employee Wage	2.812	[5.127]	2.302	[3.680]	6.67
Observations	114,730		3,422		

Notes: This table reports descriptive statistics for the sample of CEOs and employees with a regular wage contract paying at least NOK1000 per month in 2015. Firms in distress are observed in the bankruptcy process during the period 2005-2017 and include non-bankrupt and bankrupt firms. Total wage compensation includes all wage components, including bonus, overtime, non-standard wage components, and severance payments. Managers hired externally are observed in an external firm the year prior to being assigned the CEO position of the current firm. The number of managers with ownership includes incorporated and non-incorporated firms, the ownership share is the average ownership share of managers in incorporated firms. The ratio of CEO pay over average employee wages is right-censored at 100. Market income includes wage, capital and business income and is converted to monthly compensation adjusting for the average number of working days within a year.. Nominal values are deflated to 2015 using the average wage growth (base amounts) and converted to US dollars. The average exchange rate in 2020 is about NOK/USD = 9. Standard deviations [in square brackets].

Competition for managerial talent and changes in the demand for general skills can influence the level

and growth rate of CEO pay. Bertrand (2009) documents the evolution of CEO recruitment, work and compensation for the US. A lower fraction of externally hired CEOs could, for example, be suggestive of a downward pressure on CEO compensation. Our data show that the fraction of externally hired managers in Norway is comparable to findings in other studies. In our data, 22 percent of the CEOs have been hired externally, well within the 17-22 percent range reported for the 2500 largest companies (PwC Strategy&, 2018). The average Norwegian firm thus relies primarily on the internal labor market, investing in core business skills specific to the firm, similar to large international firms.

3 Data, Sample Construction and Descriptive Evidence

This section describes our data sources and how we construct our CEO-firm-judge dataset starting from the full Norwegian population.

3.1 Data Sources

We employ data from six different registers provided by three different agencies, Statistics Norway (SSB), the Norwegian Courts Administration (NCA), and the Brønnøysund Register Center (BRREG). The latter is a government agency responsible for the management of many public registers and governmental systems for digital information exchange. The datasets can be linked using unique and anonymized identifiers for each employee, firm, judge and for each stakeholder in a firm, i.e. owners, managers and board members. We combine the data sources in three steps.

In the first step, we obtain data on all employment relationships from 1999 to 2018 from SSB's Register of Employers and Employees. These data enable us to construct time series for monthly earnings and to identify employees' transitions between firms and occupations. The Business and Enterprise Register identifies CEOs, board members, and shareholders for the population of firms over the period 2004 to 2018, as well as basic longitudinal features of the firms. The register contains a separate entry for owner-CEOs. In our sample of firms, about 600 are registered with a owner-CEO which we exclude from our final sample. Having identified the CEOs for all businesses and their full employment history, we then append the relevant shareholdings and the related dividend payments to CEOs from the SSB Shareholder Register. We also attach information on the income and wealth, location and other socioeconomic characteristics of CEOs from longitudinal administrative registers provided by SSB – covering every Norwegian resident from 1967 to 2018 and contain individual demographic information such as gender, age, residential municipality, and education.

In the second step, we add detailed information on all corporate bankruptcy cases that we obtain from BRREG. These data cover every bankruptcy case filed in Norway between 2004 and 2018 and contain the district court case number, the organization number of the distressed firm, the date on which the case entered the legal system, relevant decisions and dates and information establishing if the judge dismissed a bankruptcy filing because the insolvency criterion was not met. We supplement this information with firms' income and balance sheet data.

In the last step, we link the firm-level bankruptcy information with the anonymized ID for the assigned

district court judge from the NCA. We combine this firm-bankruptcy-judge data set with the above CEO-firm level data using the anonymized individual and firm identifiers. Appendix B provides further details on the data sources we use.

3.2 Sample Description

After completing these steps and dropping petitions filed after 2013, our sample of CEOs consists of individuals who we can follow for at least four years before and after their employer's bankruptcy petition was registered by the district court. [13] We exclude all cases handled by judges with fewer than 20 bankruptcy cases between 2004 and 2018. This choice reduces the noise in our estimate of judge stringency, which we measure as the bankruptcy rate in all cases except the current case – i.e., as a leave-out-mean. We drop CEOs in companies without registered employees in the matched employee-employer register and focus on CEOs of limited companies (AS). To avoid endogenous sample selection, we only use the first observed case for firms and CEOs that are observed with more than one bankruptcy filing, treating subsequent petitions as part of their outcomes.

Treatment in our setting means that bankruptcy is opened, which usually implies the firm is liquidated in the proceedings. In contrast, the control group constitutes CEOs of firms petitioned but where bankruptcy is not opened. These initially untreated CEOs may nevertheless experience subsequent bankruptcy petitions in the years after the initial filing was dismissed, which may affect their likelihood of remaining a CEO in later years. The first column of Table 2 reports the fraction remaining in the CEO role 1-4 years after the first bankruptcy case, separately for the treatment and control groups. Among the treated CEOs, 56 percent remained a CEO four years after the bankruptcy petition. In comparison, nearly 80 percent of the CEOs in the control group remain in the CEO position four years after – which is remarkably close to the average survival rate for all CEOs (column 4). We still see that nearly 50 percent of firms in the control group petitioned for bankruptcy again after the initial case. Notably, only 13 percent of the CEOs initially employed in these firms have left the executive labor market four years after the initial case.[14]

[13] The youngest CEOs in your sample are 23. We do not have any upper age limit.

[14] Some bankrupt firms may re-appear in the data with a new identity if the firm was either sold as a going concern with the same CEO or because the CEO starts up as a restructured entity but with the original workforce. To explore the empirical importance of this phenomenon, we follow the approach of Hethey-Maier and Schmieder (2013) who identify implicitly reorganized firms using information from matched employer-employee data. We describe this in more detail in Appendix section C

Table 2: **Descriptive Statistics: CEO Survival Rate and Bankruptcies**

Years after initial case	(1) Being CEO	(2) Bankruptcy	(3) Firm liquidated & not CEO	(4) Survival rate all CEOs
	A. Bankrupt (first case)			
Year 1	0.63	1	0.35	0.94
Year 2	0.60	1	0.39	0.90
Year 3	0.57	1	0.46	0.86
Year 4	0.56	1	0.46	0.83
	B. Non-Bankrupt (first case)			
Year 1	0.89	0.32	0.06	0.94
Year 2	0.83	0.39	0.10	0.90
Year 3	0.80	0.43	0.12	0.86
Year 4	0.77	0.46	0.13	0.83

Notes: This table reports the probability of holding a CEO position, and the probability that a firm experiences a bankruptcy opening in the years after the initial case. We also report the joint probability that the firm experience a bankruptcy opening and the CEO is no longer registered as a CEO. The survival rate among all CEOs is estimated on a sample of all registered CEOs in 2009 and reports the probability that they remain a CEO in the subsequent four years. The sample of petitioned firms includes bankrupt petitioned firms with a unique identified CEO from 2005-2013 that managed a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded.

3.3 Observational Challenges

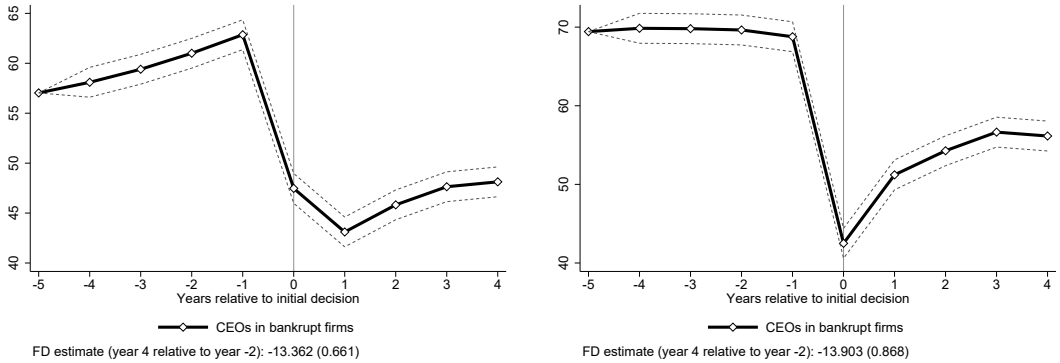
The main challenge in estimating bankruptcy costs in observational data is that we only observe a CEO in either a bankrupt or a surviving firm, and that bankruptcy is likely correlated with unobserved factors that also determine career outcomes. Suppose, for example, that CEOs with poor outside options populate firms in declining industries. The potential outside job offers would likely be CEO positions in the same industry where firms, again, are more likely to be petitioned by creditors and to default. Therefore, these CEOs would have lower earnings growth even when they escape the initial bankruptcy. This selection on potential outcomes leads to biased estimates of career costs from OLS regressions.

The income losses associated with corporate bankruptcy have so far been estimated by comparing the full remuneration of CEOs before and after bankruptcy (see, e.g. Eckbo et al., 2016). This comparison, or first-difference estimator, gives a consistent estimate of the career costs of bankruptcy under the assumption that compensation would have remained stable in the absence of bankruptcy. In our setting, this approach captures the compensation loss associated with holding the CEO position in a firm that ends up in bankruptcy. However, it does not isolate the career effects of bankruptcy from the effect of being a CEO in a poorly performing firm.

To illustrate this point, we start by describing the evolution of wage income and total CEO remuneration before and after bankruptcy in our sample. Figure 3 shows that wage income grows at an annual rate of 1.7 percent, from $57,000 to about $63,000 over the five years leading up to the bankruptcy petition. Wage income subsequently drops by $20,000 in the year after the bankruptcy petition. If we suppose wage income, in the absence of the bankruptcy event, would have continued to rise along a linear trend, then wage income would have reached $68,000 four years after. This comparison suggests that bankruptcy leads to a four-year drop in wage income of about $21,000, or more than 30 percent relative to the counterfactual of no

bankruptcy. However, wage income only accounts for a fraction of the total remuneration of the CEO. The right panel of Figure 3 shows that including other sources of market income, such as dividend and business income, flattens the trend, indicating that a decline in other sources of income offsets the gains in wage income. The before and after comparison of total market income indicates bankruptcy is associated with a fall in market income of about $15,000, which is close to 20 percent of pre-bankruptcy income.[15]

Figure 3: **Income Trends for CEOs in Bankrupt Firms**



(a) Wage Income (1000 USD)

(b) Market Income (1000 USD)

Notes: The left panel plots the evolution of CEOs' wage income and the right panel plots the evolution of market income five years prior to the case is received by the bankruptcy court to four years after, conditional on case year fixed effects and age fixed effects. Time is normalized to zero at the year the bankruptcy petition was filed. The sample includes all identified CEOs of liquidated firms in the period 2005-2013 that managed a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. Wage income is winsorized at the 99th percentile. Market income includes capital income, business income and wage income and is winsorized at the 99th percentile. Both measures are deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. 95% confidence intervals are represented by dashed lines.

Our goal is to identify the career costs of corporate bankruptcy, operating via search frictions or a stigma, leading to potentially long-lasting adverse career effects or loss of firm- or industry-specific rents. These figures illustrate that separating the effect of bankruptcy from being in a poorly performing firm requires strong parametric assumptions on the trends in income components. Similarly, CEOs of non-bankrupt may also suffer substantial income losses, for example, if their remuneration is performance-related, or the board may seek to find a replacement CEO due to poor performance. We examine whether these CEOs constitute a natural control group by examining pre-trends in earnings and market income. Unfortunately, the two groups display diverging trends before the petition, which poses a challenge for disentangling correlation and causation.[16] The following section outlines how our research design intends to isolate the effects of bankruptcy while keeping the firm's quality constant.

[15] In Appendix Figure A.1, we use the overall population of CEOs as a control group and estimate the difference in income growth between CEOs in bankrupt firms relative to the control group. The estimated income drop remains roughly the same.

[16] Appendix Figure A.2 plots the difference in wage growth and market income growth between bankrupt and non-bankrupt CEOs in our sample. Four years after the case, there are no significant differences between the market income of bankrupt CEOs relative to non-bankrupt CEOs. This clearly shows that the non-bankrupt CEOs in our sample also suffer income losses. At the same time, this comparison highlights the potential confounding selection effects that would arise from a simple comparison of bankrupt and non-bankrupt CEOs. The wage evolution before bankruptcy differs significantly between bankrupt and non-bankrupt CEOs, as do other observable characteristics. We document this in the left panel of Appendix figure A.2 and in Appendix Table A.1 that displays averages of background characteristics and pre-bankruptcy economic variables.

4 Experimental Research Design

This section describes our experimental setting and research design.

4.1 Experimental Setting

Our contribution stems in part from a quasi-experimental setting that provides a natural treatment and control group consisting of CEOs work in firms that were granted or declined bankruptcy by a district court judge. The Norwegian court system randomly assigns cases to judges with different tendencies to open bankruptcy proceedings, generating random variation in bankruptcy among financially distressed firms. This allows us to hold the quality of CEOs and the firms they operate fixed while exploiting this assignment to identify the causal effects of bankruptcy on CEOs.

Random Assignment of Bankruptcy Cases to Judges

The Norwegian judicial system builds on two important, closely related principles: the randomness principle and the generalist principle. The generalist principle implies that judges should have general expertise and be able to deal with a broad range of cases. Judges, for example, rotate between criminal and civil cases. The randomness principle imposes that cases must be assigned to judges through a randomization procedure. The Courts of Norway explain the randomness principle as follows: "The decision on which judge is to handle a case shall be made on the basis of a principle of random distribution. The main purpose of the principle of randomness is to prevent outsiders from influencing the choice of judges in the individual case" (Courts of Norway, 2021). This ensures that the allocation of cases to justices is carried out without consideration of the potential outcome of the case. In practice, the chief judge of a district court mechanically assigns each incoming case to the next judge on a list with fixed sequencing based on the date of the case and without knowledge of the case's content (Bohn, 2000).[17]

Essential for our research design is that the assignment of judges is random and that some judges are more likely than others to open bankruptcy proceedings, i.e., declare a firm bankrupt. In our empirical implementation, we use a judge's bankruptcy declaration rate in *other* randomly assigned cases they handled from 2004 up to and including 2018 to estimate a judge's stringency. This leave-one-out mean thus includes both past and future cases and not just the bankruptcy cases we use in our CEO sample. We include, for example, cases where CEOs are older than in our estimation sample because we do not need data on their earnings after the bankruptcy event. Similarly, we include firms without sales or employees, such as holding companies. We estimate judge strictness for 879 judges who presided over a total of 84,061 cases, i.e., an average of 96 cases each. Of the 60 district courts, 59 have more than one judge. The three largest courts cover nearly one-third of our baseline sample of bankruptcy cases while the twenty smallest courts stand for eight percent of the sample. We remove fully interacted year and district court fixed effects from our stringency measure as the randomization of cases is conducted among the pool of *available* judges, i.e.,

[17] Certain severe crimes or cases involving juveniles are exempt from the randomness principle. These exemptions, however, do not apply to bankruptcy cases

within each district court, and to control for any differences in bankruptcy rates over time across district courts.

To verify that the assignment of bankruptcy filings to judges is indeed random and not systematically related to firm or CEO characteristics, we regress a dummy indicating whether bankruptcy proceedings were opened on a set of CEO characteristics, pre-determined outcomes for CEOs, pre-determined firm characteristics and fully interacted court and year fixed effects. The second column of Table 3 shows that nearly all CEO demographic background characteristics and economic conditions as well as past firm characteristics are good predictors of a firm's probability of bankruptcy. We also reject the null hypothesis that the variables are jointly unrelated to whether the judge opens bankruptcy or not.

Next, we apply a test that is similar to the one done to verify the random assignment in a randomized controlled experiment by investigating if the same CEO and firm characteristics can predict the stringency of their assigned judge. Column 4 reports coefficient estimates and F-test, and column 5 reports standard errors. The regression model of judge stringency on the same CEO characteristics and pre-determined economic outcomes and firm characteristics are not jointly significant (p=.23). Moreover, we run separate regressions of judge stringency on each characteristic and find all are insignificant at conventional levels. These tests provide strong support for the notion that district courts follow procedure by randomly assigning judges to bankruptcy cases, conditional on the included court and year fixed effects.

Although the drivers of differences in judge stringency are statistically insignificant and irrelevant to our analysis provided cases are randomly assigned, understanding why judges who are expected to be generalists differ in their tendency to declare bankruptcy, i.e., assess that firms are illiquid and insolvent, is interesting in itself. Unfortunately, we lack the data to study if the personal characteristics of judges or other conditions can explain these differences. We can however calculate the experience of the judge and examine how a judge's experience is related to average stringency. To do this, we run a separate regression on of judge stringency on the total number of cases a judge has handled and report the estimates in Table 3. In this setting, we do not find any statistically significant association between experience and stringency.

Table 3: **Testing for Random Assignment of Cases to Judges**

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:		**Firm Liquidated**		**Judge stringency**	
	Average	coef.	s.e.	coef.	s.e.
		A. Pre-determined characteristics of manager			
Age (at the time of decision)	43.476	-0.0007	(0.0005)	-0.0000	(0.0001)
Female	0.213	0.1610***	(0.0123)	0.0043	(0.0029)
Married	0.675	0.0219*	(0.0117)	-0.007	(0.0025)
Foreign born	0.104	-0.0483***	(0.0131)	0.0024	(0.0035)
Less than high school	0.354	-0.0121	(0.0115)	-0.0016	(0.0027)
High school	0.426	0.0256**	(0.0110)	0.0005	(0.0024)
Any college	0.220	-0.0204	(0.0136)	0.00150	(0.0028)
		B. Pre-determined Economic Outcomes (USD 1,000)			
Labor income	65.953	-0.0007***	(0.0001)	0.0000	(0.0000)
Capital income	-4.231	-0.0003*	(0.0001)	-0.0000	(0.0000)
Business income	2.435	-0.0008**	(0.0000)	0.0000	(0.0001)
Dividend income	2.772	-0.0004**	(0.0000)	0.0000	(0.0000)
Bank deposit holding	16.913	0.0001	(0.0001)	0.0000	(0.0000)
Total personal debt	277.386	-0.0001***	(0.0000)	-0.0000	(0.0000)
Entrepreneur in any firm	0.444	-0.0400***	(0.0075)	-0.0016	(0.0025)
Board member in any firm	0.454	-0.0121	(0.0110)	0.0004	(0.0023)
		C. Pre-determined Firm Variables (mill. USD)			
Revenue	0.783	-0.0079***	(0.0019)	0.0002	(0.0002)
Employees	7.985	-0.0007*	(0.0004)	0.0000	(0.0001)
Value added	0.348	-0.0194***	(0.0056)	0.0000	(0.0003)
Output per worker	0.038	-1.4547***	(0.1086)	-0.0201	(0.0163)
Current ratio	1.029	0.0012***	(0.0003)	0.0001	(0.0002)
Interest coverage ratio	-27.862	0.0000	(0.0000)	0.0001	(0.0001)
Debt to equity	0.522	-0.0001	(0.0001)	-0.0000	(0.0000)
Total debt	0.437	-0.0138***	(0.0028)	-0.0001	(0.0002)
Equity	0.045	-0.0089***	(0.0031)	0.0003	(0.0004)
Net financial costs	-0.012	0.0975***	(0.0293)	-0.0000	(0.0039)
Labor costs	0.215	-0.0404***	(0.0118)	-0.0002	(0.0006)
Judge experience	238.588	-0.0001***	(0.0000)	0.0000	(0.0000)
F-statistic		13.8735		1.2002	
[p-value]		0.00		0.23	
Observations		7,586		7,586	

Notes: This table reports separate and joint tests of whether the court complied with the random allocation procedure described in Section 4. The sample includes all identified CEOs of bankrupt petitioned firms from 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded. Pre-determined variables and outcomes are measured the year before the case was received in the bankruptcy court. There are 845 unique judges. Characteristics and outcomes are measured a year prior to the case was received in the court. Column 1 reports averages pre-determined characteristics of CEOs (Panel A), pre-determined economic outcomes, and corporate roles of the CEO (Panel B) and pre-determined firm-variables (Panel C). Columns 2-5 report OLS regressions of manager characteristics on (column 2) a dummy variable for whether the firm was liquidated and (column 3) our measure of judge stringency. F-statistics are obtained from OLS estimation on the combined set of manager characteristics. Each regression controls for fully interacted year and court dummies. Groups with only one observation within the fixed effects (19 singleton groups) are dropped. Characteristics of managers are measured prior to the court case. Variable definitions are as follows: any college is equal to one if a person has some college education or has a college degree. All pre-determined variables are measured one year before the court. Nominal values are divided by 1,000 and deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

4.2 IV model

Our primary objective is to estimate the effect of bankruptcy on CEOs' remuneration, job choices and career development. To achieve this, we will estimate the following structural model,

$$y_{i,t+s} = \alpha_s + \beta_s B_{i,t} + x'_{i,t}\Gamma_s + u_{i,t} \tag{1}$$

which relates career outcome $y_{i,t+s}$ in period s to the bankruptcy experience $B_{i,t}$, an indicator variable equal to 1 if CEO i works in a firm that is declared bankrupt at time t. The vector of control variables, $x_{i,t}$, always includes a full set of year by district-court dummies, and may include firm and CEO characteristics in alternative specifications.[18] In our setting, given that firms fail only once and CEOs in practice only experience one bankruptcy, we can interchangeably use i as an index for either the CEO or the firm.

Our parameter of interest β_s, is the effect of corporate bankruptcy on CEO career outcomes – i.e., the difference in potential outcomes between a CEO who experiences bankruptcy and one who does not. Our design takes advantage of the fact that judges are assigned randomly to bankruptcy cases and vary in their stringency. This feature allows us to instrument the bankruptcy variable $B_{i,,t}$ using each district court judge's stringency and to estimate the impact of bankruptcy among CEOs using two-stage least squares (2SLS). Provided judge stringency is relevant and satisfies the assumptions stated below, we can interpret β_s as the causal effect of experiencing bankruptcy for CEOs of firms on the margin of survival – i.e., whose bankruptcy outcome depends on the random variation in judge stringency. Under heterogeneous treatment effects, this estimand is usually termed the local average treatment effect (LATE) and can be estimated by indirect least squares, where CEO outcomes are regressed on judge stringency and the vector of controls. This reduced form coefficient is then divided by the estimated effect of judge leniency on the bankruptcy probability.

We estimate the effects directly using 2SLS, where the second stage is captured by equation (1), and the the first stage equation equals

$$B_{i,t} = \gamma Z_{j(i),t} + x\prime_{i,t}\delta + e_{ii} \tag{2}$$

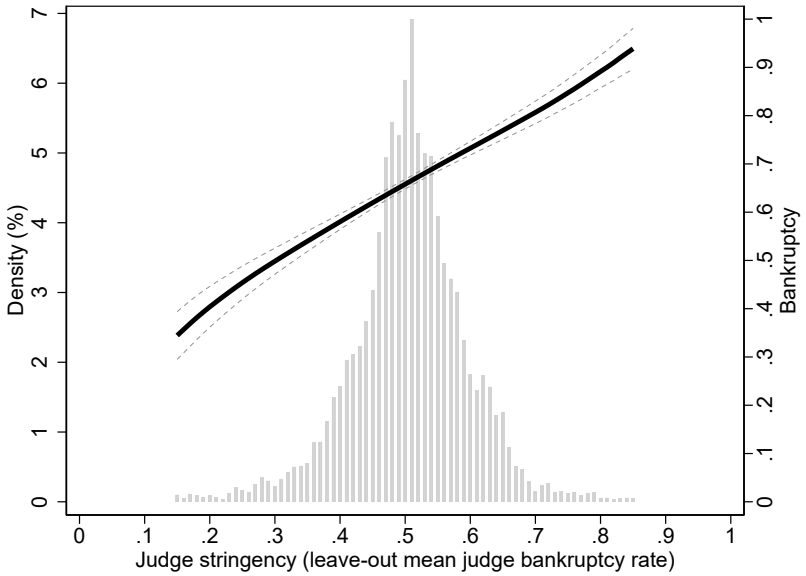
where, $Z_{j(i),t}$ is the stringency measure for judge j to which firm/CEO i's was assigned in bankruptcy court at time t. We will also decompose our estimates of β into the potential outcomes of treated and untreated marginal firms (see Imbens and Rubin, 1997 and Dahl et al., 2014 for continuous instruments). Throughout our empirical analysis, we will report the 2SLS estimate, the overall and the non-bankrupt averages among marginal firms. We will interchangeably refer to complier and marginal firms as the firms whose outcomes depend on the stringency of the judge.

[18] Our baseline specification of no control variables except for the fully interacted year and court dummies is consistent with a local average treatment effect (LATE) interpretation (see, e.g., Blandhol et al., 2022). We nevertheless show that including the individual characteristics does not change the main findings or give statistically significant different estimates from the baseline specification in Appendix Tables R.1 and R.2. Implementing a separate regression marginal treatment effect model with linear judge instruments as proposed by Brinch et al. (2017). we can neither reject the hypothesis of no treatment effect heterogeneity. These results are available upon request.

4.3 Relevance and Identifying Assumptions

Relevance Figure 4 provides a graphical representation of the first stage of our IV model. The background of the figure contains a histogram that displays the density of the judge stringency measure (controlling for fully interacted year and district-court dummies). The mean of our leave-out measure of judge stringency is 0.5, with a standard deviation of 0.125. The histogram shows that there is a substantial amount of heterogeneity among judges in terms of their stringency. The solid line in the figure illustrates the relationship between judge stringency and the likelihood of a firm being declared bankrupt by a judge. The plot is the output of a flexible analog to the first stage equation (2), where we estimate a local linear regression of firm bankruptcy on judge stringency and fully interacted court and year dummies.[19] The probability of a firm being declared bankrupt by a judge increases monotonically, almost linearly, in our stringency instrument. Judges in the top of the stringency distribution open bankruptcy proceedings in 90 percent or more of their cases compared with 40 percent or less by their colleagues with the lowest liquidation propensities. Table 4 reports the parameter estimates of the first stage regression. A 10 percentage point increase in the judge's bankruptcy rate in other cases is associated with an approximately eight percentage point increase in the probability of the firm going bankrupt.

Figure 4: **Effect of Judge stringency on Firm Bankruptcy**



Notes: This figure displays the effect of judge stringency on bankruptcy, conditional on fully interacted year and district court dummies. 95% confidence intervals are represented by dashed lines. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. The solid line plots a local linear regression of bankruptcy on judge stringency with a bandwidth of 0.2. The histogram of judge stringency is shown in the background of both figures.

Before turning to the regression counterpart, we perform the pre-test suggested by Mikusheva and Sun (2022). This test replaces the conventional F-test for single instruments. It extends existing tests by allowing the number of instruments to grow proportionally with the sample size and heteroskedastic residuals. In our setting, the ratio of instruments over observations is about one over ten. When implementing the pre-test,

[19] For the local linear regression, we use a uniform kernel with a bandwidth of 0.2

we find a test statistic of 145, well above the critical value of 4.4 – suggesting identification is strong in our setting.[20] Following the recommendation by Mikusheva and Sun (2022), we perform standard inference using our leave-one-out instrument and examine the linearity assumption in Section 5.1.

Identifying Assumptions For our judge stringency instrument to identify the career costs of bankruptcy, it needs to satisfy three conditions: 1) exogeneity, 2) the exclusion restriction and 3) monotonicity. For *exogeneity*, judge stringency must be uncorrelated with pre-determined observed and unobserved characteristics that may be relevant for whether bankruptcy proceedings are opened or not. In Table 3 in Section 4.1 we provided evidence consistent with random assignment of bankruptcy filings to district court judges. As a complementary check, columns (2), (3) and (4) in Table 4 display whether our first-stage estimates are sensitive to including multiple control variables. Given the earlier evidence, including additional pre-determined control variables should not change the coefficient estimate. Consistent with this notion, the first-stage coefficient on judge stringency barely moves when we include CEO, firm or judge control variables. This evidence offers strong support for the interpretation of the first stage and reduced form estimates as the impacts of being assigned to a judge who declares more firms bankrupt.

Table 4: **First Stage Regression Estimate of Judge Stringency on Bankruptcy**

Control variables	**Baseline**	**CEO controls**	**Firm controls**	**Judge controls**
	(1)	(2)	(3)	(4)
Judge Stringency (γ)	0.808***	0.792***	0.806***	0.811***
	(0.080)	(0.079)	(0.079)	(0.081)
Mean of dependent variable	0.662	0.662	0.662	0.662
Number of observations	7,586	7,586	7,586	7,586

Note: This table reports the estimated coefficient γ on our judge stringency measure for the first stage equation (2) with corporate bankruptcy as the dependent variable, for different sets of control variables. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. "Baseline" controls in column (1) are fully interacted year and court dummies, but the regressions include no other control variables. "CEO controls" in column (2) include fully interacted year and court dummies, CEO age, education, marital status and whether the CEO is foreign-born or not. "Firm controls" in column (3) include revenue, value added, number of employees in the firm, equity and labor costs. "Judge controls" in column (4) are the number of cases a judge has handled, the number of judges per court as well as fully interacted year and court dummies. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Interpreting our 2SLS estimates as the causal effect of bankruptcy requires judge stringency to satisfy the exclusion restriction. This excludes judge stringency from the structural equation 1, and requires stringency to only affect outcomes through the initial bankruptcy decision, not directly or through other channels. One concern could be that judges take multidimensional decisions, as in criminal cases, that have both an extensive margin in sentencing and an intensive margin decision, e.g., the duration of a prison sentence. Decisions in corporate bankruptcy filings, however, are in principle binary and one-dimensional. An alternative concern could be that judge leniency may be positively correlated (unobserved) with skills to forge relationships between firms and creditors. Thiswould generate an independent effect on firm outcomes, operating in parallel with bankruptcy. While we do not have data on judges' backgrounds and education to test this formally,

[20] To implement this test, we must impose the sample restriction of at least five judges per year-court combination.

we can include the number of cases a judge has handled as a proxy for a judge's quality. Reassuringly, as we document in the next section, including judge experience as an additional control does not change our estimates appreciably, providing additional support for the exclusion restriction.

Under the assumption of constant treatment effects among marginal firms, no additional assumptions are required to identify the average career costs of bankruptcy. The 2SLS would simply identify a population-wide average treatment effect. Should the effects vary by unobserved characteristics of the bankruptcy case, then we require an additional assumption, namely that, for each firm, the probability of bankruptcy is at least as high if assigned to a stricter judge (high Z) as when assigned to a lenient judge (low Z). Since no firm can be assigned to two different judges simultaneously, we cannot directly test this monotonicity assumption. However, the monotonicity assumption does have some testable implications. One is that judges who are stricter towards one subset of CEOs (e.g., younger) are also stricter towards other subgroups of CEOs (e.g., older). We implement this test by splitting the data into two subsets and recalculating a subsample's own stringency instrument as a judge's stringency for all cases outside the subsample. When assessing the effect of judge stringency on bankruptcy among firms with a young CEO, we calculate judge stringency using only decisions among older CEOs. Column (2) of Appendix Table R.3 reports these re-estimated first stage results. All estimates using this alternative instrument are positive and statistically significant, consistent with the notion that stricter judges in one set of bankruptcy cases are also stricter for other groups of bankruptcy cases. It is also possible to jointly test the exclusion restriction and monotonicity assumption. Suppose our instrument satisfies exogeneity. Then the exclusion restriction and monotonicity assumption implies that expected outcomes (at the judge level) are a continuous and monotone function of the judge's treatment propensity. Moreover, Frandsen et al. (2019) show that the slope of this function, representing the treatment effect, must be bounded by the support of the outcome variable.[21] In Appendix Table R.4, we implement this test for our main outcome variable – which lends further support to the exclusion restriction and monotonicity assumption. [22]

4.4 The Impact of Judges' Bankruptcy Decisions on Firm Outcomes

To support the interpretation of our research design, we begin by examining the effects of judges' bankruptcy decisions on firms' long-run outcomes. Our experimental setting is only meaningful to the extent that judges' decisions to open bankruptcy proceedings affect firm outcomes. As Norwegian bankruptcy proceedings consist of liquidation through a cash auction, we expect to see a termination of the activities in the corporation following a bankruptcy. Table 5 presents the reduced form estimates of our IV model for firm outcomes. All regressions in Table 5 control for fully interacted year and court fixed effects. Panel A reports our estimates of the impact of judge stringency on the likelihood that a firm will report zero sales one to four years after the bankruptcy declaration. This outcome effectively measures whether a firm terminates its activities

[21] The test consists of two stages. The first part of the test is generalization of the Sargan-Hansen overidentification test. The second part assesses if the implied treatment effects are possible given the support of the outcome.

[22] Specifically we cannot reject these assumptions even with a wide range of assumptions on the functional relationship between the outcome and the judge's treatment propensity and with different sets of included covariates. Frandsen et al. (2019) use regression splines to approximate the functional relationship between the outcome and the judge's treatment propensity. We show that the test supports the identifying assumption for a wide range of different splines. The test is also robust to including different sets of covariates that are used to estimate judge's treatment propensity.

following the bankruptcy decision. All estimates are highly significant and the declining effects over time reflect that an increasing share of the marginal but non-bankrupt firms eventually go out of business due to voluntary closures and, possibly at a later date, defaults as well. Table A.2 presents the 2SLS estimates. One year after the bankruptcy decision, treated firms are 72 percentage points more likely to be out of business than untreated. Summing the untreated mean and the treatment effect shows, as expected, that bankruptcy immediately terminates all sales activity. Two, three and four years after the court decision, the share of non-bankrupt but marginal firms that terminates activities rise to .50, .58 and .60, which reduces the treatment effect to .39.

Table 5: **Reduced Form Estimates of Judge Stringency on Firm Outcomes**

	Dependent variable				
Panel A:	y_{it} : **Reporting No Sales**				
Years after decision (t):	0	1	2	3	4
Judge stringency	0.048	0.585***	0.407***	0.334***	0.318***
(standard error)	(0.038)	(0.068)	(0.061)	(0.058)	(0.057)
Overall mean:	0.149	0.766	0.841	0.870	0.891
Panel B:	y_{it} : **Firm Employment**				
Years after decision (t):	0	1	2	3	4
Judge stringency	-2.519**	-3.025***	-2.388***	-2.261**	-2.342***
(standard error)	(1.063)	(0.780)	(0.779)	(0.907)	(0.801)
Overall mean:	4.602	2.080	1.438	1.314	1.147
Number of observations:	7,586	7,586	7,586	7,586	7,586

Note: This table reports the reduced form effects using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. Firm employment includes all workers registered with wage payments in the firm as registered in the Norwegian Register of Business Enterprises. Reported sales is from the same register. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Panel B reports our evidence on the reduced form employment effects of bankruptcy. There is a significantly negative persistent effect on firm employment. One year before the bankruptcy filing, the average (median) sample firm had 8.6 (5.0) employees (not shown in the table), and two-thirds of all firms employed up to 9 employees. Appendix Table A.2 presents the 2SLS estimates and the potential outcomes of untreated marginal firms (i.e., compliers). One year after the petition, the non-bankrupt marginal firms retain about two thirds of their employees. Four years after, this fraction has fallen to a third, implying that about 30 percent of the employees in firms that are denied bankruptcy retain their jobs. Marginal firms that go bankrupt evolve as expected. They do not necessarily register zero staff instantly following bankruptcy because of the ongoing liquidation process. Two years after being declared bankrupt, however, the firms no longer have any employees on their payrolls.

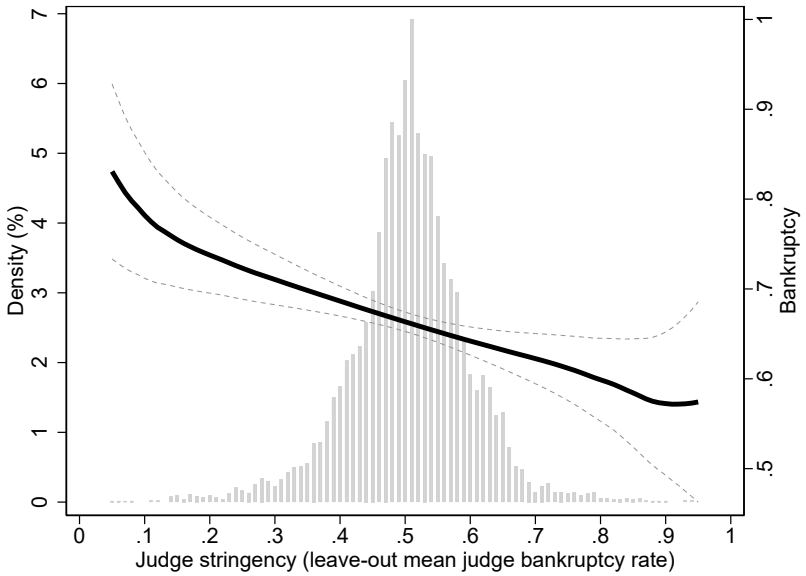
5 Experimental Findings

In this section, we presents our empirical analysis and discuss the main findings of our paper.

5.1 The Effects of Bankruptcy on CEOs' Careers

We begin by examining the effects of bankruptcy on CEOs' likelihood of continuing in the top executive role, having regular employment, and on the different remuneration components.

Figure 5: **Effect of Judge Stringency on Holding the CEO Position**



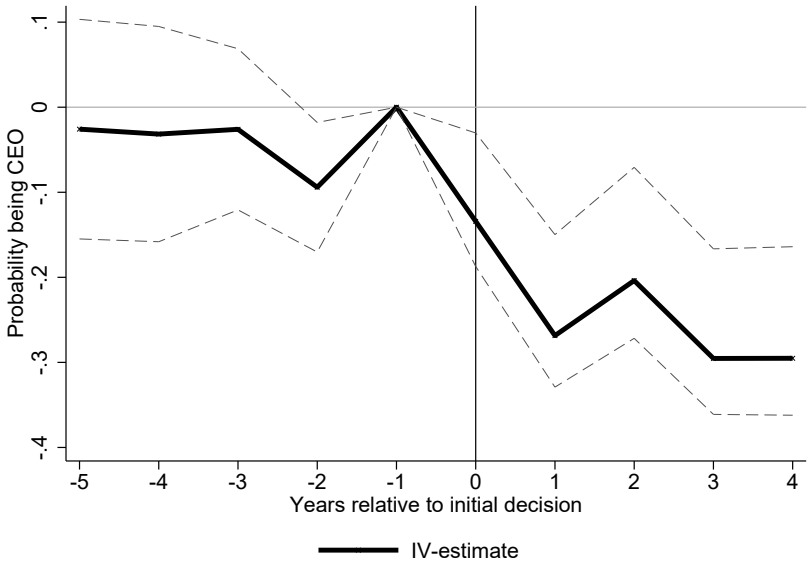
Notes: This figure displays the effect of judge stringency on bankruptcy, conditional on fully interacted year and district court dummies. 95% confidence intervals are represented by dashed lines. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that managed a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. The solid line plots a local linear regression of bankruptcy on judge stringency with a bandwidth of 0.2. The histogram of judge stringency is shown in the background of both figures. The lowest and top five percentiles are excluded.

5.1.1 Executive Positions and Regular Employment

Our first object of interest is whether bankruptcy affects the likelihood of continuing in a CEO role. We begin by examining the relationship between *judge stringency* and the likelihood of keeping the CEO role four years after the bankruptcy petition. Figure 5 provides a graphical representation. The plot is the output of a local linear regression of the an indicator for whether the person holds the CEO role on judge stringency and fully interacted court and year dummies. To obtain the implied IV estimate, we would have to divide the percentage change in the probability of being CEO from Figure 5 by the percentage change in the likelihood of bankruptcy from Figure 4 at a given level of judge stringency. The monotonic, close to linear and decreasing relationship suggests that CEOs who are assigned to a strict judge are less likely to stay in the executive labor market. Comparing a case assigned to a judge at the 10th percentile with a judge at the 90th percentile lowers the likelihood of maintaining a CEO role by about 10 percentage points.

Turning to the second stage equation, Figure 6 and Panel A of Table 6 report our 2SLS estimates of the impact of *bankruptcy* on holding a CEO position. We see that bankruptcy increases the likelihood of exiting the executive labor market by 20 to 30 percentage points. In the last row in Panel A and Appendix Figure A.5, we display the estimated potential outcome for untreated CEOs.The majority of CEOs transition into new CEO positions, irrespective of whether their employers are declared bankrupt or not. Among untreated marginal firms, between 83 and 95 percent of all CEOs occupy a CEO position one to four years after the bankruptcy filing, compared to 54 to 68 percent of treated CEOs. By contrast, Eckbo et al. (2016) document that merely 21 percent of CEOs of large firms hold executive positions following Chapter 11 bankruptcy.

Figure 6: **Effect of Bankruptcy on Holding the CEO Position**



Notes: This figure displays the second stage coefficients of equation 1 using judge stringency as an instrument. 95% confidence intervals are represented by dashed lines. The sample includes all identified CEOs of bankrupt petitioned firms from 2005-2013 that managed a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and district court dummies.

Having shown that bankruptcy has an important effect on CEO careers, we first explore whether displacement from the CEO position reflects a reduction in these CEOs' likelihood of employment. Our outcome of interest is an indicator for whether a CEO is registered with an employment contract in the employer-employee register. Panel B of Table 6 shows that bankruptcy lowers the likelihood of a CEO being employed, but only temporarily. The estimated average impact is significant only in the year the petition is received and quickly levels off. One possible interpretation is that the initial effect in the year of the bankruptcy petition is uncertainty about whether the firm will be sold as a going concern and whether potential new owners would want the CEO to continue. Another interpretation is that CEOs direct their search toward similar management positions and that searching for these positions is time-consuming. Already a year after the bankruptcy petition, however, the employment rate for CEOs who experience bankruptcy is equal to that of their peers.[23]

[23]If we instead use "having wage income above the substantial gainful activity level (SGA) level" as an alternative measure of employment, the results remain unchanged. The outcome variable for our alternative employment measure is an indicator equal to one if wage income is above the substantial gainful activity (SGA) level. The SGA is used by social security administrations to

Table 6: **Main Effects of Bankruptcy on CEO Careers**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			y_{it} **: Being a CEO**			
Bankruptcy (β)	-0.134**	-0.268***	-0.204***	-0.295***	-0.295***	-0.241***
	(0.053)	(0.061)	(0.068)	(0.066)	(0.067)	(0.052)
Overall mean:	0.868	0.720	0.674	0.647	0.631	0.710
Untreated marginal firm:	0.990	0.946	0.865	0.866	0.834	0.904
Panel B.			y_{it} **: Regular Employment**			
Bankruptcy	-0.127**	-0.076	-0.082	0.005	-0.006	-0.057
(standard error)	(0.059)	(0.064)	(0.063)	(0.062)	(0.066)	(0.045)
Overall mean:	0.741	0.641	0.671	0.685	0.689	0.685
Untreated marginal firm:	0.852	0.727	0.731	0.701	0.716	0.745
Number of Observations:	7,586	7,586	7,586	7,586	7,586	7,586

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms from 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. Regular employment is defined as being registered with an employment contract in the employer-employee register. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

5.1.2 Remuneration

We now turn to the potentially opposing effects of bankruptcy on CEOs' income. On the one hand, bankruptcy can create an informational stigma, where the executive labor market views the bankrupt firm as a signal of low ability and low CEOs' earning capacity (Eckbo and Thorburn, 2003). Moreover, a CEO's human capital could be tied to the firm or declining industry, where outside employers value their skills less and, therefore, offer a lower wage than their incumbent firm. These mechanisms predict that CEOs who have gone through bankruptcy are likelier than their peers to take jobs with lower wages and switch to non-executive positions. On the other hand, managing a financially distressed firm can generate valuable management experience that increases CEOs' ability to make good business choices (e.g., Bertrand, 2009 and Bandiera et al., 2017). A growing line of empirical research on CEO characteristics confirms this view (e.g., Custodio and Metzger, 2013, Custodio et al., 2013 and Custodio and Metzger, 2014). Finally, important non-pecuniary benefits, such as work flexibility or corporate perks (Jensen and Meckling, 1976), intrinsic motivation (Edmans et al., 2021) or personal traits, such as overconfidence (Malmendier and Tate, 2015), could tie a person to a CEO position even though better-paid job opportunities exist.

We examine the net effect of these mechanisms in Table 7. Panel A shows that CEO wage income initially falls, but the downward effect levels out two years after the bankruptcy filing. During the first two years, the wage income of CEOs who experience bankruptcy is approximately 15 percent lower than the average among CEOs of untreated marginal firms. Over time, however, the gap shrinks and becomes

determine eligibility for various types of social security benefits and calculate contributions to retirement accounts, and provides a strong incentive to earn income above the threshold (see, e.g., Kostol and Mogstad, 2014). For our purposes, we consider having an income above the SGA as an alternative way of establishing if a CEO whose employer filed for bankruptcy is in fact working after a court decision. The SGA equals the basic amount, see Section 2, and corresponds to about ten hours of work per week with an hourly wage rate of NOK 180 (USD20). Appendix Table A.3 reports these alternative results.

statistically indistinguishable from zero. Because executives typically receive other compensation on top of regular wages, we also examine the role of high-powered incentives, such as equity holdings, by measuring the effects of bankruptcy on stock income, i.e., dividends. Panel B presents the effect on stock income and shows that while the estimates are initially noisy, the average effect on stock income over the years 0 to 4 is about minus USD 2,100 among bankrupt CEOs. The results in Panel B also imply that CEOs who experience bankruptcy on average have no stock income four years after the bankruptcy filing. This suggests that small business CEOs also rely on income components with high-powered incentives and that displacement following bankruptcy is associated with the loss of this variable income component.

Table 7: **Effects of Bankruptcy on CEO Remuneration**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			y_{it} **: Wage Income (USD 1,000)**			
Bankruptcy (β)	-11.847**	-10.756*	-4.598	-3.167	-6.913	-7.456
	(5.147)	(5.638)	(5.983)	(5.881)	(6.257)	(5.092)
Overall mean:	51.459	46.750	47.926	48.858	49.377	48.874
Untreated marginal firm:	63.225	57.766	53.654	52.008	56.380	56.608
Panel B.			y_{it} **: Stock Income (USD 1,000)**			
Bankruptcy (β)	-2.191	-1.990	-3.256**	-0.035	-3.256**	-2.145**
	(1.337)	(1.215)	(1.586)	(1.491)	(1.410)	(1.058)
Overall mean:	1.098	1.116	1.273	1.529	1.759	1.355
Untreated marginal firm:	3.171	1.956	3.506	2.336	3.783	2.951
Panel C.			y_{it} **: Total Market Income (USD 1,000)**			
Bankruptcy	-17.693***	-10.581	-11.204	-5.736	-13.798*	-11.803*
(standard error)	(6.837)	(6.538)	(7.779)	(7.467)	(8.118)	(6.070)
Overall mean:	50.366	54.793	57.112	58.492	59.010	55.955
Untreated marginal firm:	69.121	65.841	66.371	60.705	70.156	66.439
Number of observations:	7,586	7,586	7,586	7,586	7,586	7,586

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. Market income includes wage, dividend and business income. Variables are winsorized at the 99th percentile levels, deflated to 2015 using the average wage growth (base amounts) and converted to (1000) USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

In Panel C, we consider the impact on total market income, i.e., the sum of wage income, capital, stock gains, dividends and other business income. Our 2SLS estimates make clear that once other variable compensation components are included, treated CEOs experience a greater income loss than their non-bankrupt peers, in particular the year of the bankruptcy petition. Relative to CEO *wage* income, *market* income displays a more pronounced decline initially and remains lower relative to the control group throughout our observation period. However, the coefficient estimates are not sufficiently precise to draw a firm conclusion about the size of the impact beyond the first year. In Appendix Table A.4, we further break down the results into estimates for the different components of market income, i.e., stock income, interest income, and capital gains from the sale of real estate and securities – clearly illustrating that falling stock income explains the

difference between the evolution of CEO wage income and total income.

To compare our estimated career costs with evidence from earlier studies, we calculate the present value (PV) of the loss in stock income for the average treated CEO in our sample. The PV of stock income losses equals about $40,000, or about 60 percent of the market income of the untreated CEOs This likely represents a lower-bound as capital income starts declining before a bankruptcy petition is filed for a firm. Table 1 shows that the monthly capital income of distressed CEOs is less than 10 percent of that of non-distressed CEOs. To compute the PV of the loss in market income, we assume the average five-year effect remains constant for the remainder of CEOs' careers – likely representing an upper bound of the career cost. The PV amounts to $223,000 or 3.3 times the market income of untreated CEOs.[24] By contrast, Eckbo et al. (2016) find an income loss of up to 4.6 times pre-bankruptcy compensation ($ 7.2 million) for bankrupt CEOs in large US companies. While the loss we estimate differs substantially from Eckbo et al. (2016) in absolute terms, the upper bounds of PV losses are comparable in relative terms although the CEOs in our sample are on average 12 years younger and employed at small and medium-sized firms.

5.1.3 Robustness and Sample Composition

We perform a range of specification checks in Appendix Tables R.1 and R.2. We begin by demonstrating that the main findings remain unchanged when we include CEO characteristics as controls.[25] We then show that including judge experience as a proxy for procedural quality does not significantly change our results in Panel C of R.1, supporting the exclusion restriction and causal interpretation of our findings. Next, we allow for the possibility that heterogeneous treatment effects are erroneously given equal weights by the linear first and second stages. We allow for different weights for different latent CEO types by discretizing the instrument by each judge stringency quintile as a separate instrument. In Panel D of R.1, we show that our evidence on the likelihood holding the CEO position remains very similar – indicating that effect heterogeneity is not a key feature of our setting.[26] We also show that measurement error in our judge leniency measure does not appear to bias our result by excluding judges who handle fewer than 30 cases in Panel E.

As an additional robustness check, we investigate if our findings are contaminated by new owners reestablishing firms with the same CEOs after a bankruptcy. As we show in section C, the continuation share among bankrupt firms is very small, between 0.6 and 1.1 percent depending on how we define continuation. In Appendix Tables A.17 and A.18**,** we show that our main results are not significantly affected when we exclude continuing firms from our sample. If anything, the effects on total market income are slightly stronger, reflecting lower income losses among bankrupt CEOs who resume their job once their firm is reestablished. Finally, as we document in section 3, nearly 50 percent of the firms in the control group experienced bankruptcy four years after the initial case. To assess how these liquidations affect our results, we estimate the effect on our main outcome variables using as the control group only CEOs of firms that

[24] To convert the estimated remuneration changes in Table 7 we assume a normal retirement age of 67. As the average CEO in our sample is 43 years old, the loss accumulates over 24 years. We use a discount rate of 2 percent to .

[25] While the effects on wage income are generally imprecise, the sharp effect in the first year is stable across our main specification checks. Moreover, Appendix Figure A.3 shows that the difference in pre-trends between bankrupt and non-bankrupt compliers is flat.

[26] The effects on wage income do change using the discretizing instrument. This indicates heterogeneous wage effects among CEOs of marginal firms, which can explain some of the variances in the wage estimates.

were not subsequently liquidated. These estimates are reported in Appendix Table A.14 and A.13. The main results do not change significantly when excluding this group.

Finally, we turn to the role of sample composition. We begin by noting that the firms in our data are predominantly small and medium-sized enterprises (SMEs). These firms' CEOs are likely to be different from the managers of large listed corporations, such as those examined by Eckbo et al. (2016). For example, CEOs in our data are, on average, 43 years old, almost ten years younger than in Eckbo et al. (2016). This difference is important as older CEOs are more likely to have accumulated specific human capital. They also have a shorter time horizon for their remaining careers, negatively affecting them in the executive search market if hiring firms are looking for a long-term commitment. To examine the role of CEO age, we split our sample at age 45, slightly above the mean of 43 years, and run separate second-stage regressions for each sub-sample. The evidence is reported in Appendix Table A.5. On average, younger CEOs experience no loss of stock income, whereas older CEOs experience a loss of about USD 4,600. The wage results are relatively imprecise, and we do not have sufficient statistical precision to conclude that the changes in wage income differ between younger and older CEOs. Consistent with the notion that older CEOs are likely to be more impacted, we find that the effect on remaining in a CEO position is stronger than among those aged 44 or younger. Overall, these results suggest that the negative career costs of bankruptcy we identify are primarily driven by older CEOs.

5.2 Skill Specificity

An influential strand of the labor economics literature has studied the role of specific and general skills in shaping wage growth over the course of job careers. Neal (1999) describes job mobility as a two-stage process in which people search for a match with both an occupation and a suitable firm. Over time, people become attached to the firm, either due to firm-specific wage policies that favor tenure, or because of learning and firm-specific human capital. This theory predicts that short-tenured workers are more likely to switch careers because their skill portfolio comprises relatively more general human capital than long-tenured workers. Similarly, Gathmann and Schonberg (2010) study the portability of labor market skills and find that workers move between occupations that use similar skills and tasks. Relatedly, Nakamura et al. (2021) find evidence that the gains of involuntary moving may be large due to location-dependent mismatching.[27]

We examine the importance of the specificity of CEOs' skills in two ways. First, we examine whether displacement due to bankruptcy induces CEOs to search for an employer in different industries. We interpret the mobility rate as an indicator of CEO skills' specificity to their past industry. Panel A of Table 8 shows that treated CEOs are 20 percentage points more likely to switch industries than non-bankrupt CEOs in the control group, who have a 32 percent probability of switching sectors after the filing. This increase in professionally more distant job switches supports the notion that CEO skills are portable across sectors.[28] Our second inquiry concerns the extent to which CEOs are tied to geographic locations. In Panel B of Table

[27] Their evidence suggests that careers may be tied to a location, either due to the concentration of industry or due to locational preferences.

[28] Note that our sample has changed to individuals with a registered employment contract, i.e., who are employed.

8, we analyze the tendency of CEOs to move to a new commuting zone after going bankrupt. Large effects would indicate a geographic mismatch, where moving to more productive locations could offset the negative effects of bankruptcy on wages. The evidence displays a weakly positive, but only temporary effect on geographic mobility, consistent with moving costs being relatively low compared to gains.

Table 8: **Effects of Bankruptcy on Industry and Geographic Mobility**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : Switching Industry (Two-digit)					
Bankruptcy (β)	0.225***	0.265***	0.238***	0.278***	0.260***	0.190***
	(0.060)	(0.075)	(0.079)	(0.067)	(0.064)	(0.055)
Overall mean:	0.312	0.515	0.593	0.643	0.686	0.490
Untreated marginal firm:	0.163	0.335	0.438	0.465	0.498	0.326
Panel B.	y_{it} : Move to new commuting zone					
Bankruptcy (β)	0.070	0.140**	0.104*	0.075	0.028	0.089**
	(0.043)	(0.057)	(0.054)	(0.055)	(0.062)	(0.040)
Overall mean:	0.076	0.136	0.150	0.163	0.166	0.120
Untreated marginal firm:	0.076	0.076	0.133	0.110	0.148	0.096
Number of observations:	5,589	4,819	5,059	5,167	5,199	5,589

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Industries are classified by its two-digit NACE code and commuting zones are measured according to firm location using the Bhuller (2009) classification. We can only observe the industry and commuting zone of CEOs that are registered as employed. The estimation sample is limited to this sample. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table 9: **Productivity of CEOs' Employer**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A:			y_{it} **: Output per Worker (1000 USD)**			
Bankruptcy (β)	-6.888	39.257***	32.206***	17.041	34.909***	15.297*
	(11.581)	(11.316)	(11.526)	(12.727)	(12.906)	(7.843)
Overall mean:	25.603	43.076	52.234	57.479	61.119	41.338
Untreated marginal firm:	23.773	26.808	44.211	44.803	38.297	32.106
Panel A:			y_{it} **: Poaching Index**			
Bankruptcy (β)	-0.045	0.110*	0.069	0.081	0.096*	0.027
	(0.051)	(0.060)	(0.057)	(0.054)	(0.056)	(0.045)
Overall mean:	0.603	0.698	0.707	0.711	0.715	0.655
Untreated marginal firm:	0.607	0.664	0.684	0.686	0.652	0.642
Number of observations:	5,602	4,833	5,071	5,179	5,202	5,602

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Value added is measured as revenue minus intermediates, output per worker is measured as value added over the number of employees. The variable is winsorized at the 99th percentile levels. Nominal values are divided by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. The poaching index is measured as the share of workers the firm recruits from other firms relative to workers it recruits out of unemployment. The measure is only available for the subsample of CEOs that are registered as employed and estimation is done for this subsample. Due to different number of singleton groups for the estimated average effect and the year specific effects, the estimated average effect deviates somewhat from the average of the coefficients in columns 0-4. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Next, we turn to the quality of new employment relationships. Our inquiry starts with an examination of average firm productivity, measured as the value-added per worker, i.e., firm revenue minus intermediaries. Panel A of Table 9 demonstrates that CEOs that experience displacement move to firms with substantially higher output per worker. The value-added per employee is almost 50 percent higher than at the employers of their peers. We also measure firms' overall attractiveness, reflecting wages and other amenities they offer, such as hours flexibility, employee benefits or location by means of a poaching index. The index is based on the poaching rank of Bagger and Lentz (2019). Firm A will be ranked higher than firm B if the former poaches more workers from other firms than firm B. Panel B of Table 9 shows that displaced CEOs move to more attractive firms. While the evidence is somewhat less precise, it suggests a pattern of CEOs moving to firms that rank around 10 percent higher than their non-bankrupt peers. Moreover, the average new firm in the sample ranks at 0.65, meaning that these firms on average hire 65 percent of their workers from other firms. This places these firms well above the average ranked firm.[29] An alternative way to measure the quality of new employers is their average wages or size.[30] Consistent with the evidence on productivity, Panel A of Table A.8 demonstrates that bankrupt CEOs move to higher-quality firms (e.g., with higher-paid employees). Panel B shows that these new employers are, on average, located 10 percent higher in the firm

[29] The average ranked firm hires about 40 percent of its workers from other firms. However, a large share of the firms in the overall sample are not registered with any poached workers. This might be because we do not place any restriction on firm size or type when estimating the index. Bagger and Lentz (2019) use a cutoff of 15 workers to avoid the poach rank picking up extreme realizations due to small firm size. The average firm size in our sample of bankrupt petitioned firms does not allow for such a cutoff.

[30] These rankings can proxy for firm quality and productivity and are easily obtained by sorting firms along these dimensions relative to the full population of firms (e.g., Herkenhoff et al., 2016).

size distribution than the employers of CEOs of non-bankrupt, marginal firms. [31]

Having documented that displaced CEOs move to higher-quality employers, we next examine their relative position in the firms' hierarchy. We define corporate management as either having the CEO position or any other lower management position using occupational codes in the employment register. Given the higher exit rates from the executive labor market, we expect bankruptcy to increase the likelihood of moving to lower-ranked jobs. In Table 10, we show that the reduction in the likelihood of being registered in a CEO occupation in Table 6 mirrors the shift in the probability of having a non-management job. We find no effect on the probability of being employed in mid-level management positions, suggesting that many displaced CEOs may commence a new climb on the corporate ladder starting from non-management positions in new firms. Our estimates of potential outcomes of managers in untreated marginal firms show that some of these CEOs also end up in a non-management positions, but at a much lower rate, ranging from 4 to 16 percent. Hence, treated CEOs are more than twice as likely to take non-management jobs. Next, we inspect the relative income position of CEOs, capturing the rank of individuals (e.g., a displaced CEO) relative to other employees in the new firm. Our 2SLS estimates show that bankrupted CEOs move to new positions located approximately 10-15 percent lower in the within-firm wage distribution than their non-bankrupt peers. This finding indicates that wage losses may be driven by CEOs being at the top of the hierarchical structure of internal labor markets – where the drop in wage income reflects the loss of the wage premium associated with the highest rank (see, e.g., Lazear and Rosen 1981). However, by moving to higher-paying firms, CEOs partly offset some of this downward pressure on total compensation.

Finally, we consider whether the relative decline in the probability of remaining in the executive workforce reflects a shift towards new business start-ups. To this end, we measure if a CEO is subsequently registered as the founder of a new business. Table 10 shows that bankrupted CEOs do not substitute more into (complementary) entrepreneurship than their non-bankrupt peers. Rather, we find weak evidence - though not statistically significant at conventional levels - that bankruptcy reduces the probability of being registered as the founder of a business by 20 percent four years after.

[31] Appendix Table A.6 and A.7 shows that displaced CEOs become employed at firms that have higher value added, higher earnings, lower leverage and a better liquidity position than the employers of non-bankrupt peers.

Table 10: **Changes in Management Positions Caused by Bankruptcy**

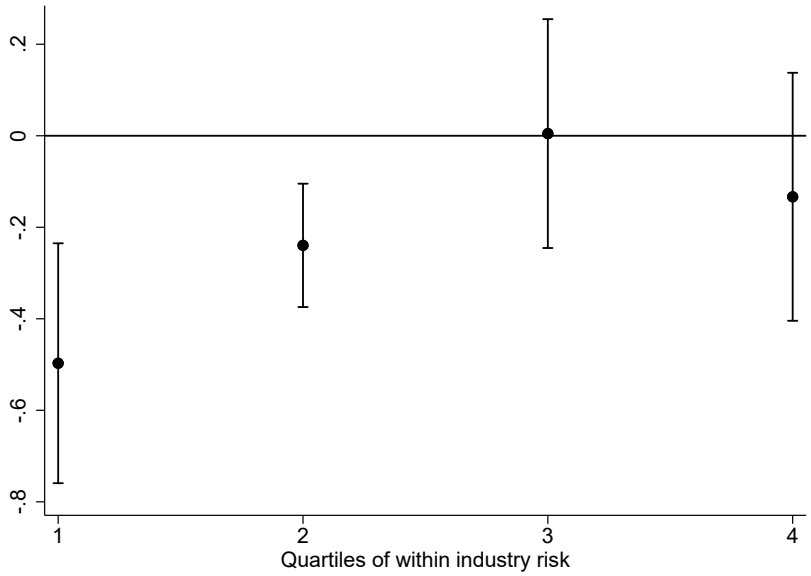
Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : No Management Position					
Bankruptcy (β)	0.147***	0.270***	0.216***	0.293***	0.316***	0.249***
	(0.048)	(0.059)	(0.065)	(0.060)	(0.062)	(0.049)
Overall mean:	0.129	0.280	0.321	0.346	0.358	0.287
Untreated marginal firm:	0.038	0.070	0.148	0.151	0.163	0.114
Panel B.	y_{it} :Lower / Mid Management Position					
Bankruptcy (β)	0.001	0.015	0.010	0.032	0.005	0.013
	(0.028)	(0.027)	(0.030)	(0.036)	(0.035)	(0.026)
Overall mean:	0.037	0.041	0.045	0.049	0.053	0.045
Untreated marginal firm:	0.040	0.033	0.028	0.020	0.042	0.032
Panel C.	y_{it} : Top Management Position					
Bankruptcy (β)	-0.192***	-0.296***	-0.229***	-0.311***	-0.322***	-0.270***
	(0.056)	(0.062)	(0.066)	(0.064)	(0.066)	(0.053)
Overall mean:	0.842	0.696	0.652	0.625	0.608	0.685
Untreated marginal firm:	0.968	0.909	0.824	0.830	0.800	0.866
Panel D.	y_{it} : Entrepreneur					
Bankruptcy (β)	-0.034	-0.066	-0.102	-0.134*	-0.120*	-0.081
	(0.070)	(0.069)	(0.074)	(0.073)	(0.073)	(0.065)
Overall mean:	0.478	0.472	0.473	0.471	0.469	0.468
Untreated marginal firm:	0.510	0.534	0.547	0.551	0.526	0.520
Number of observations:	7,586	7,586	7,586	7,586	7,586	7,586

*Note:*This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

5.3 Heterogenety by Industry Conditions

Finally, we assess how career costs of bankruptcy vary with industry conditions. To examine this question, we measure conditions as the *within-industry* average bankruptcy rate. Thus, subtracting an industry's average bankruptcy rate, we can estimate career costs of bankruptcy at different times within a sector rather than across industries with permanent differences in bankruptcy rates. After quantifying the industry conditions, we divide our sample into four quartiles by within industry bankruptcy rates (using the firms' petition date) and estimate the IV model separately for each quartile.

Figure 7: **Effect of Bankruptcy on CEO Probability by Industry Condition**



Notes: This figure displays the effect of bankruptcy on CEO-probability (second stage coefficients of equation 1) using judge stringency as an instrument by quartiles of industry bankruptcy risk. We measure industry bankruptcy risk as the variation within each industry to facilitate comparison s across industries with different permanent bankruptcy rates. Industries are classified by its two-digit NACE code. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that managed a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies, age dummies, education level, gender and an indicator for whether the manager is foreign-born. CEO probability is measured as the average probability from the case year to four years after. 95% confidence intervals are represented by spikes.

Figure 7 plots the estimates of the avergage CEO displacement effects with 95 percent confidence intervals. The pattern is monotone from the first to the third quartile, showing that the negative effect decays when industry-conditions worsen and bankruptcy rates are high. Table 11 reports the corresponding point estimates and standard errors. We perform inference on the average estimates over years zero to four and reject the hypothesis of equal effects by comparing the first and third quartiles using analytical standard errors and by boostrapping the standard error on their differences.[32] Appendix Table A.9 confirms the significant differences in the effects of bankruptcy on the probability of being a CEO by instead splitting the sample of CEOs at the median bankruptcy rate. By contrast, Appendix Table A.10 shows that we do not detect significant differences in the effect of bankruptcy on *wages* for the four subsamples.

[32]Using bootstrapped standard errors for the difference of the estimated effect between the different quartiles, we are able to reject that the effects are equal for all quartiles using 200 replications, see Appendix Table (A.11).

Table 11: **Effect of Bankruptcy on CEO Probability: By Industry Conditions**

Years after decision (t): Average from 0-4				
	y_{it} **: Holding the CEO Position**			
Industry condition/Bankruptcy risk:	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
	(1)	(2)	(3)	(4)
Bankruptcy (β)	-0.500***	-0.233***	0.005	-0.141
	(0.144)	(0.074)	(0.137)	(0.136)
Overall mean:	0.709	0.730	0.714	0.690
Untreated marginal firm:	0.923	0.844	0.879	0.936
Mean industry condition/Bankruptcy risk	0.032	0.047	0.062	0.084
Number of observations:	1,782	1,855	1,874	1,823

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. by quartiles of industry bankruptcy risk. We measure industry bankruptcy risk as the variation within each industry to facilitate comparison s across industries with different permanent bankruptcy rates. Industries are classified by its two-digit NACE code. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that managed a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies, age dummies, education level, gender and an indicator for whether the manager is foreign-born. The CEO position is measured as the average years a person held the position from the case year to four years after. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

5.4 Interpretation of the Evidence

We have thus far established that bankruptcy causes a significant reduction in CEOs' market income, primarily from a reduction in capital income, and a significant exit from the executive labor market. In terms of wage income, CEO with bankruptcy experience seem to offset the potential loss by moving to higher-quality firms, and exhibit portable skills, employable across industries. In particular, we see that displaced CEO move to lower-ranked positions in firms that rank above the average firm in the overall economy. These findings indicate that CEOs who go bankrupt due to the stringency of the judge posses skills that are valued by the labor market.

What do higher occupational displacement effects in favorable industry conditions tell us about possible mechanisms? In an influential audit study, Kroft et al. (2013) show that "unemployment scars" on resumés have smaller impacts on callback rates when aggregate unemployment is high. The evidence is consistent with screening models in which unemployment spell length is used as a signal of unobserved productivity – recognizing the signal is less informative in weak labor markets than in strong labor markets (e.g., Gibbons and Katz, 1991). Our evidence across industry conditions is consistent with this interpretation – that the executive market is less likely to hold the CEO accountable for the firm's performance when bankruptcy rates are higher and the signal-to-noise ratio of bankruptcy is weaker.[33] Following Kroft et al. (2013), we also examine how the displacement effects vary across regions with different local unemployment rates. Appendix Table A.12 documents similar effects, but the evidence is less pronounced. These findings go against the idea that poor labor market conditions worsen the employment opportunities of CEOs – as suggested by the

[33]Suppose these effects were driven by learning about comparative advantages or mismatch due to high switching costs (e.g., Nakamura et al., 2021). If that was the case, we expect overall and labor income to increase as the mismatched CEOs move to occupations that better match their abilities. This is not what we find when comparing wage impacts across industry-conditions (see Appendix Table A.10).

literature on job loss (e.g., Davis and von Wachter, 2011). In contrast, high-skilled workers' outcomes, such as CEOs, are more likely to be shaped by informational frictions rather than aggregate demand factors. To further assess this mechanism, we split our sample based on how long the CEOs had been employed at the firm before bankruptcy. CEOs with longer tenure are likely to be held more responsible for firm performance than CEOs appointed close to the bankruptcy date. Appendix Table A.15, shows how the main effect differs between CEOs with short tenure (less than three years) relative to CEOs with longer tenure in the firm. Longer tenured CEOs experience an average drop in the probability of remaining a CEO after bankruptcy by about 30 percentage points — twice the effect for the lower tenured CEOs.[34]

5.5 Potential Implications

What are the potential implications of our findings for the executive labor market and the economy? Suppose a bankruptcy stigma drives the CEO displacement effect. Then, making judges more stringent in normal times (when industry conditions are good) can introduce an informational stigma that lowers the chance that otherwise talented CEOs return to the executive labor market. In a simple simulation exercise, we use an accounting framework in combination with our IV estimates from Table A.9 to illustrate this point. Specifically, we assume that the differential re-entry rates reflects the bankruptcy stigma inferred from hiring firms. We then use CEOs' overall annual survival rate and the changes in bankruptcy probability for CEOs in our sample. We think of this channel as a shift in the matching efficiency between start-up firms and CEOs. This allows us to quantify the potential effect of stigma on the pool of CEOs in the economy. Our calculation implies that bankruptcy stigma can reduce the steady-state pool of available CEOs by up to 13 percent. Appendix Section D provides details on our calculations.

A smaller pool of potential CEOs implies a slower start-up growth rate. We quantify this effect via the probability of filling a vacant CEO position, v, assuming a constant return to scale matching function, m.[35] The total number of matches can be written as the sum of matches from the pool of regular applicants ceo_n and from the pool of applicants with bankruptcy experience ceo_s:

$$M = A_n m(v, ceo_n) + A(s) m(v, ceo_s)$$

where $A(s)$ captures the effect of stigma. We think of stigma as an additional informational friction – potentially arising due to a more time-consuming vetting process of managers that previously managed a bankrupt firm – leading to a lower hiring rate than without it. We quantify the change in annual total matches and hiring probability using data on the annual number of CEO vacancies from official vacancy data and an online job portal (Bhuller et al., 2021). This back-of-the-envelope calculation suggests that stigma effects can lower the hiring probability by 6 percent, leading to fewer matches and increase the start-up costs of businesses.[36]

[34] In contrast, we find larger impacts on market income among short-tenured CEOs than long-tenured ones. These results are available upon request.

[35] Constant returns to scale means that adding one vacancy and one applicant will increase the hiring by one. The empirical evidence is independent of the particular model structure. Under the (strong) assumption that the variation in CEOs' reentry rate from section 5.3 can be attributed to bankruptcy stigma, the estimates are informative of matching probabilities.

[36] Our simple calculation ignores any endogenous responses from firms' vacancy posting and assumes that firms cannot direct their search towards specific submarkets of CEOs.

6 Discussion

Corporate bankruptcy epitomizes the process of creative destruction's final stage, but how does it shape the CEOs' post-bankruptcy careers? As CEOs influence a wide range of corporate decisions, such as investment, financial, and organizational practices, policymakers have long been concerned about the potential negative effects of bankruptcy stigma on CEOs and business dynamics. Fear of reputational scarring caused by a firm's bankruptcy could lead managers to take less risk than desired by owners, which could manifest in lower performance and lower rates of entrepreneurship and job growth (e.g., Eckbo and Thorburn, 2003). Extant research has documented that CEOs in *large* US firms filing for Chapter 11 exit the executive labor market in two out of three cases and suffer median remuneration losses of five times their annual pre-bankruptcy income over the course of their career. While existing evidence suggests economically significant negative effects of corporate bankruptcy, these events seldom occur randomly and can be confounded by unobserved heterogeneity, leading to a bias towards concluding that CEOs suffer severe losses from bankruptcy.

Our analysis attempts to fill a gap in the literature by identifying the career costs of corporate bankruptcy, i.e., liquidation, in the Norwegian setting. Our empirical contribution builds on two key institutional features. The first is the requirement that bankruptcy petitions are randomly assigned to judges with different degrees of strictness in approving bankruptcy filings. Importantly, this generates exogenous variation in firms' likelihood of being declared bankrupt that we exploit to study how corporate bankruptcy affects CEOs. The second key feature is rich register data set with all Norwegian CEOs that enables us to track their corporate responsibilities, job titles, and remuneration before and after the filings.

Our paper offers, to the best of our knowledge, the first causal evidence on the consequences of straight bankruptcy (c.f. US Chapter 7) on CEOs' careers. We establish three new empirical facts about the career costs of corporate bankruptcy for top management. First, although a substantial share of bankrupt CEOs is displaced from the top executive position, we find that CEOs who experience corporate bankruptcy quickly find new employment. Bankruptcy is followed by a short-lived drop in wage income and a persistent fall in stock income, equivalent to about five percent of total remuneration. Bankrupt CEOs' total income falls by approximately 15 percentage points relative to non-bankrupt peers over the four years following a bankruptcy filing.

Second, we show that CEO skills are highly portable. Bankruptcy causes increased rates of mobility across industries. We also show that bankrupted CEOs are likelier to move to higher-productivity firms with a better-paid workforce than CEOs of non-bankrupt firms. These effects are particularly strong for younger managers. Moreover, when examining heterogeneity in impacts by the age of CEOs, we find that the career costs are primarily driven by older CEOs, i.e., aged 45 and older. Younger CEOs, in contrast, exit the executive labor market less and experience no reduction in total income following the bankruptcy. We argue that this is likely driven by the accumulation of firm- or industry-specific human capital throughout a CEO's career.

Third, we show that the displacement effects of bankruptcy are significantly stronger when default rates in an industry are low. Exploring differential impacts of bankruptcy by industry-specific variation in bankruptcy frequency, we find that CEOs are five times more likely to be displaced from their CEO position when bankruptcy rates are in the upper quartile of the distribution. CEOs who exit the executive labor

market shift to non-management positions in larger, more productive firms, in a lower position in the wage hierarchy. By contrast, we find no evidence of variation in wage income owing to industry conditions – suggesting that wages are not tied to industry demand.

Our work complements the literature on CEO remuneration but differs from earlier work in a few ways. Our data consists mainly of CEOs in small and medium-sized enterprises with substantially higher bankruptcy rates than large listed corporations. This may affect the weight the labor market assigns bankruptcy as a signal of unobserved managerial ability. It can also shape the adverse effects of bankruptcy on CEO remuneration, such as the lost value of stock options and ex-ante compensations for career risk. CEOs in SMEs are, on average, also ten years younger than in large listed firms, which creates greater scope for them and their employers to capitalize on their experience and supports mobility between industries.

Finally, our evidence has broader implications for debt and executive labor markets. First, in environments with conflicting interests between an agent, e.g., the CEO, and the principal, e.g., the firm's creditors, our findings highlight an incentive for CEOs to delay default to mitigate the career costs of bankruptcy. Such strategic behavior can, in turn, increase the cost of credit, where creditors experience a lower recovery rate when assets are sold in thinner markets. While outside the scope of this paper, our evidence also indicates that CEOs may want to start fewer risky projects to hedge against personal career costs in the event of default. Second, our evidence suggests that informational frictions can reduce the pool of potential CEOs and lower aggregate start-up rates by increasing recruiting costs. Our calculations indicate that the bankruptcy stigma can reduce the potential pool of available CEOs by up to 13 percent. Using data on the annual number of vacant CEO positions, our calculations further indicate that informational frictions can increase start-up costs by slowing down the hiring rates of CEOs by six percent.

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A Appendix: Additional Tables

A.1 Robustness

Table R.1: **Robustness: Holding the CEO Position**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			Baseline Estimate			
Bankruptcy (β)	-0.134**	-0.268***	-0.204***	-0.295***	-0.295***	-0.266***
	(0.053)	(0.061)	(0.068)	(0.066)	(0.067)	(0.058)
Dependent overall mean	0.868	0.720	0.674	0.647	0.631	0.670
Panel B.			Adding Individual Characteristics			
Bankruptcy (β)	-0.123**	-0.243***	-0.175***	-0.269***	-0.274***	-0.243***
	(0.052)	(0.061)	(0.068)	(0.067)	(0.068)	(0.058)
Dependent overall mean	0.868	0.720	0.674	0.647	0.631	0.670
Panel C.			Adding Judge Characteristics			
Bankruptcy (β)	-0.134***	-0.251***	-0.178***	-0.275***	-0.277***	-0.248***
	(0.050)	(0.059)	(0.067)	(0.067)	(0.068)	(0.058)
Dependent overall mean	0.868	0.720	0.674	0.647	0.631	0.670
Number of observations:	7,586	7,586	7,586	7,586	7,586	7,586
Panel D.			Multiple Instruments (five quintiles)			
Bankruptcy (β)	-0.155***	-0.270***	-0.173**	-0.268***	-0.293***	-0.252***
	(0.056)	(0.067)	(0.072)	(0.073)	(0.078)	(0.064)
Dependent overall mean	0.868	0.720	0.674	0.647	0.631	0.670
Number of observations:	7,013	7,013	7,013	7,013	7,013	7,013
Panel E.			Excluding Judges With Less Than 30 Cases			
Bankruptcy (β)	-0.138**	-0.239***	-0.174**	-0.240***	-0.241***	-0.227***
	(0.056)	(0.063)	(0.071)	(0.070)	(0.073)	(0.061)
Dependent overall mean	0.869	0.723	0.678	0.651	0.634	0.673
Number of observations:	6,420	6,420	6,420	6,420	6,420	6,420

Note: This table reports the second stage coefficients of equation 1, overall and non-bankrupt complier averages. The baseline sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 879 unique judges. Each regression controls for fully interacted year and court dummies, but no other control variables. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table R.2: **Robustness of Wage Result**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			Baseline Estimate			
Bankruptcy (β)	-11.847**	-10.756*	-4.598	-3.167	-6.913	-4.069
	(5.147)	(5.638)	(5.983)	(5.881)	(6.257)	(5.105)
Dependent overall mean	51.459	46.750	47.926	48.858	49.377	48.228
Panel B.			Adding Individual Characteristics			
Bankruptcy (β)	-9.252*	-8.302	-2.066	-0.684	-5.250	-4.069
	(4.983)	(5.458)	(5.677)	(5.544)	(5.789)	(5.105)
Dependent overall mean	51.459	46.750	47.926	48.858	49.377	48.228
Panel C.			Adding Judge Characteristics			
Bankruptcy (β)	-10.997**	-9.981*	-3.076	-1.694	-6.102	-5.206
	(4.991)	(5.300)	(5.612)	(5.601)	(5.807)	(5.079)
Dependent overall mean	51.459	46.750	47.926	48.858	49.377	48.228
Number of observations	7,586	7,586	7,586	7,586	7,586	7,586
Panel D.			Multiple Instruments (five quintiles)			
Bankruptcy (β)	-6.157	-3.608	5.372	4.864	-2.163	1.138
	(5.338)	(6.539)	(6.131)	(6.349)	(6.143)	(5.683)
Dependent overall mean	51.459	46.750	47.926	48.858	49.377	48.228
Number of observations:	7,013	7,013	7,013	7,013	7,013	7,013
Panel E.			Excluding Judges With Less Than 30 Cases			
Bankruptcy (β)	-7.473	-7.232	-1.623	0.453	-3.929	-3.066
	(4.977)	(5.560)	(5.915)	(5.682)	(5.913)	(5.250)
Dependent overall mean	51.708	46.907	48.086	49.002	49.522	48.379
Number of observations:	6,420	6,420	6,420	6,420	6,420	6,420

Note: This table reports the second stage coefficients of equation 1, overall and non-bankrupt complier averages. The baseline sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 879 unique judges."Baseline" controls are fully interacted year and court dummies, but the regressions include no other control variables. individual characteristics include fully interacted year and court dummies, CEO age, education, marital status and whether the CEO is foreign-born or not. Judge characteristics are the number of cases a judge has handled, the number of judges per court as well as fully interacted year and court dummies. Nominal values are divided by 1,000 and deflated to 2015 using the average wage growth (base amounts). To translate Norwegian kroner to US dollars, the average exchange rate in 2020 is set at about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table R.3: **Sub-Sample First Stage Estimates**

	Baseline instrument		**Reverse-sample instrument**	
	(1)		(2)	
Dependent variable	*Pr*(*Allow*)		*Pr*(*Allow*)	
Young managers (below 45)	.601	Dep. mean: .669	.556	Dep. mean: .611
	(.169)	N: 4522	(.154)	N: 1237
Older managers (aged 45 or above)	.340	Dep. mean: .651	.447	Dep. mean: .642
	(.178)	N: 3083	(.152)	N: 1277
Female managers	.453	Dep. mean: .789	.439	Dep. mean: .770
	(.153)	N: 1622	(.134)	N: 836
Male managers	.608	Dep. mean: .628	.412	Dep. mean: .579
	(.241)	N: 5983	(.321)	N: 706
Foreign-born managers	.551	Dep. mean: .669	.467	Dep. mean: .617
	(.137)	N: 5133	(.119)	N: 1136
Native-born managers	.526	Dep. mean: .647	.618	Dep. mean: .625
	(.156)	N: 2472	(.136)	N: 1161
Married managers	.726	Dep. mean: .619	.844	Dep. mean: .586
	(.157)	N: 793	(.148)	N: 513
Non-married managers	-.05	Dep. mean: .667	.054	Dep. mean: .515
	(.331)	N: 6812	(.148)	N: 535
Less than high school	.493	Dep. mean: .654	.539	Dep. mean: .628
	(.159)	N: 2690	(0)	N: 1162
High school or higher	.640	Dep. mean: .666	.629	Dep. mean: .635
	(.125)	N: 4915	(.119)	N: 1276
High wage	.446	Dep. mean: .627	.332	Dep. mean: .630
	(.161)	N: 3802	(.160)	N: 1403
Low wage	.327	Dep. mean: .697	.430	Dep. mean: .643
	(.158)	N: 3803	(.124)	N: 1242
High debt	.616	Dep. mean: .616	.753	Dep. mean: .597
	(.153)	N: 3802	(.169)	N: 1364
Low debt	.337	Dep. mean: .708	.410	Dep. mean: .655
	(.169)	N: 3803	(.126)	N: 1311
Large firm	.434	Dep. mean: .638	.300	Dep. mean: .609
	(.190)	N: 4375	(.139)	N: 1402
Small firm	.460	Dep. mean: .695	.506	Dep. mean: .667
	(.149)	N: 3230	(.135)	N: 1250

Notes: This table reports heterogeneity in first stage estimates using the baseline instrument (1) and the reverse-sample instrument (2). The first stage specification in (1) corresponds to subsample estimates of the first stage using our baseline judge stringency measure (including cases outside of our estimation sample). The reverse-sample instrument (2) is constructed by calculating judge stringency using all cases *except* for those in the specified subsample (e.g., judge stringency for the subsample of older applicants is constructed using judges' decisions for younger applicants). We create the baseline and the reverse sample instrument based on all managers and all years where we measure manager characteristics to maximize the sample size. Our baseline instrument in the main analysis is based on all cases, including cases with missing characteristics (e.g., foreigners and non-residents). ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table R.4: **Joint test of Monotonicity and Exclusion Restriction**

	5 knots	10 knots	15 knots	20 knots	25 knots
Panel A:	Covariates: Court and year FE				
Test statistic (χ^2)	0.69	0.01	0.94	0.02	0.06
P-value	1.00	1.00	1.00	1.00	1.00
Panel B:	Covariates: Judge and Individual Characteristics and FE				
Test statistic (χ^2)	8.67	9.11	9.17	7.76	5.86
P-value	1.00	1.00	1.00	1.00	1.00

Notes: This table displays the test statistics and p-values from a joint test of the monotonicity and exclusion restriction proposed by Frandsen et. al 2022. The outcome we consider is the average number of years a person holds the CEO position over the years t=0 to t=4 after the case date. Panel A reports the test statistic and p-values where we only control for interacted court and case year fixed effects. Panel B reports the test statistic and p-values when we include judge characteristics and individual controls, including judge experience, number of judges per court, CEO age, gender, education, marital status and a dummy for whether the CEO is foreign born. We set the fit component of the test to one as our estimation relies on a sample of many different judges. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded.

A.2 Additional results

Table A.1: **Summary Statistics of Bankrupt vs. Non-Bankrupt Firms**

	Non-Bankrupt Firms		Bankrupt Firms		Test of equal means
	Mean	Std. Dev.	Mean	Std. Dev.	t-stat
	A. Pre-determined CEO Characteristics				
Age (at the time of decision)	43.69	[10.02]	43.37	[10.35]	-1.29
Female	0.13	[0.34]	0.25	[0.44]	13.29
Married	0.66	[0.47]	0.68	[0.47]	1.88
Foreign-born	0.12	[0.32]	0.10	[0.30]	-2.65
Less than high school	0.36	[0.48]	0.35	[0.48]	-1.06
High school	0.41	[0.49]	0.44	[0.50]	2.34
Any college	0.23	[0.42]	0.21	[0.41]	-1.55
	B. Pre-determined Outcomes of CEO (1,000 USD)				
Wage income	70.76	[64.08]	63.50	[40.26]	-5.24
Capital income	5.80	[53.72]	3.43	[39.10]	-1.98
Business income	3.07	[18.40]	2.11	[15.52]	-2.28
Dividend	3.58	[31.77]	2.36	[23.38]	-1.72
Total market income	79.64	[88.66]	69.04	[59.76]	-5.46
Bank deposits	15.87	[61.59]	17.45	[85.01]	0.93
Personal debt	329.54	[402.28]	250.81	[321.14]	-8.61
Total processing time	29.70	[44.95]	349.61	[294.40]	75.42
Number of observations	2,567		5,038		

Notes: This table reports descriptive statistics for our sample of CEOs in financial distress. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. Pre-determined variables and outcomes are measured the year before the case was received in the bankruptcy court. Income and balance sheet information is reported by third-party to tax authorities. Market income includes wage, dividend business and capital income. Nominal values are divided by 1,000 and deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. Standard deviations [in square brackets]

Table A.2: **Effects of Bankruptcy on Firm Outcomes**

	Dependent variable				
Panel A:	y_{it} : **Reporting No Sales**				
Years after decision (t):	0	1	2	3	4
Bankruptcy (β)	0.059	0.724***	0.503***	0.413***	0.394***
	(0.046)	(0.038)	(0.046)	(0.047)	(0.048)
Overall mean:	0.149	0.766	0.841	0.870	0.891
Untreated marginal firm:	0.048	0.256	0.497	0.584	0.603
Panel B:	y_{it} : **Firm Employment**				
Years after decision (t):	0	1	2	3	4
Bankruptcy (β)	-3.129**	-3.749***	-2.962***	-2.814***	-2.916***
	(1.347)	(0.915)	(0.892)	(1.090)	(0.975)
Overall mean:	4.602	2.080	1.438	1.314	1.147
Untreated marginal firm:	7.700	4.107	3.145	3.020	2.986
Number of observations:	7,586	7,586	7,586	7,586	7,586

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions control for fully interacted year and court dummies. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.3: **Effects of Bankruptcy on CEO Careers, registered employment**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : Registered as employed					
Bankruptcy (β)	-0.127**	-0.076	-0.082	0.005	-0.006	-0.057
	(0.059)	(0.064)	(0.063)	(0.062)	(0.066)	(0.045)
Overall mean:	0.741	0.641	0.671	0.685	0.689	0.685
Untreated marginal firm:	0.852	0.727	0.731	0.701	0.716	0.745
	y_{it} :Employment (Wages > Substantial Gainful Activity)					
Bankruptcy (β)	-0.037	-0.127**	-0.101*	-0.045	-0.016	-0.065
	(0.042)	(0.058)	(0.059)	(0.060)	(0.060)	(0.042)
Overall mean:	0.867	0.765	0.748	0.750	0.755	0.777
Untreated marginal firm:	0.917	0.876	0.803	0.783	0.818	0.839
Number of observations:	7,586	7,586	7,586	7,586	7,586	7,586

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.4: **Effects of Bankruptcy on CEO Remuneration**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : Wage Income (USD 1,000)					
Bankruptcy (β)	-11.847**	-10.756*	-4.598	-3.167	-6.913	-7.456
	(5.147)	(5.638)	(5.983)	(5.881)	(6.257)	(5.092)
Overall mean:	51.459	46.750	47.926	48.858	49.377	48.874
Untreated marginal firm:	63.225	57.766	53.654	52.008	56.380	56.608
Panel B.	y_{it} : Capital Income (USD 1,000)					
Bankruptcy (β)	-11.379***	-1.836	-6.172*	-0.399	-4.398	-4.837**
	(3.350)	(2.715)	(3.325)	(3.236)	(3.016)	(2.120)
Overall mean:	-4.876	1.717	2.721	3.414	3.594	1.314
Untreated marginal firm:	4.962	3.471	7.039	3.482	6.080	5.006
Panel C.	y_{it} : Stock Income (USD 1,000)					
Bankruptcy (β)	-2.191	-1.990	-3.256**	-0.035	-3.256**	-2.145**
	(1.337)	(1.215)	(1.586)	(1.491)	(1.410)	(1.058)
Overall mean:	1.098	1.116	1.273	1.529	1.759	1.355
Untreated marginal firm:	3.171	1.956	3.506	2.336	3.783	2.951
Panel D.	y_{it} : Business Income (USD 1,000)					
Bankruptcy (β)	1.865	1.583	-1.033	-3.780	-2.339	-0.741
	(2.426)	(2.863)	(2.761)	(2.700)	(2.843)	(2.109)
Overall mean:	4.199	5.865	5.801	5.425	5.306	5.319
Untreated marginal firm:	2.618	3.936	4.799	5.601	6.765	4.744
Panel D.	y_{it} : Total Market Income (USD 1,000)					
Bankruptcy (β)	-17.693***	-10.581	-11.204	-5.736	-13.798*	-11.803*
	(6.837)	(6.538)	(7.779)	(7.467)	(8.118)	(6.070)
Overall mean:	50.366	54.793	57.112	58.492	59.010	55.955
Untreated marginal firm:	69.121	65.841	66.371	60.705	70.156	66.439
Number of observations:	7,586	7,586	7,586	7,586	7,586	7,586

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions control for fully interacted year and court dummies. Market income includes wage, dividend and business income. Variables are winsorized at the 99th percentile levels, divided by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.5: **Heterogeneity by CEO Age**

Years after decision (t): Average from 0-4						
Panel	**A. Wage Income**		**B Stock Income**		**C. CEO Position**	
Sample	Below 45	Above 44	Below 45	Above 44	Below 45	Above 44
	(1)	(2)	(3)	(4)	(5)	(6)
Bankruptcy (β)	-3.089	-11.547	-0.388	-4.602**	-0.189***	-0.308***
	(7.369)	(7.492)	(0.974)	(1.952)	(0.073)	(0.078)
Overall mean:	50.776	46.431	1.008	1.801	0.689	0.737
Untreated marginal firm:	54.923	56.955	1.984	4.067	0.862	0.941
Number of observations:	4,231	3,257	4,231	3,257	4,231	3,257

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions control for fully interacted year and court dummies. Market income includes wage, dividend and business income. Variables are winsorized at the 99th percentile levels, divided by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.6: **Value Added CEOs' Employer**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : Value Added (1000 USD)					
Bankruptcy (β)	-0.016	66.317	147.624***	142.864***	131.421**	72.761**
	(23.461)	(48.200)	(54.378)	(56.298)	(55.707)	(30.563)
Overall mean:	25.656	74.514	90.869	99.043	73.077	65.719
Untreated marginal firm:	5.792	10.414	37.990	40.937	46.150	24.906
Number of observations:	5,602	4,833	5,071	5,179	5,202	5,202

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions control for fully interacted year and court dummies.Value added is measured as revenue minus intermediates and is winsorized at the 99th percentile level. The outcome variable is only measured for CEOs that are registered as employed and estimation is done for this subsample.Nominal values are divided by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.7: **Earnings, Leverage and Liquidity of CEOs' Employers**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			y_{it} : Log EBIT			
Bankruptcy (β)	2.766***	2.229***	2.066***	2.236***	0.961	1.224***
	(1.025)	(0.831)	(0.716)	(0.738)	(0.725)	(0.452)
Overall mean:	6.808	7.387	7.458	7.562	7.586	7.196
Untreated marginal firm:	5.838	5.606	6.571	6.197	6.655	6.410
Panel B.			y_{it} : Log Debt to Equity Ratio			
Bankruptcy (β)	0.213	-0.854*	-0.474	-0.740*	-0.017	-0.409*
	(0.589)	(0.516)	(0.367)	(0.385)	(0.304)	(0.220)
Overall mean:	1.399	1.161	1.167	1.053	1.043	1.183
Untreated marginal firm:	1.377	1.687	1.490	1.118	0.984	1.435
Panel C.			y_{it} : Log Current Ratio			
Bankruptcy (β)	0.308	0.693**	0.329*	0.346*	0.140	0.410**
	(0.445)	(0.273)	(0.192)	(0.189)	(0.195)	(0.174)
Overall mean:	-0.234	0.018	0.064	0.103	0.147	0.025
Untreated marginal firm:	-0.524	-0.278	-0.087	-0.076	0.081	-0.207
Number of observations:	5,602	4,833	5,071	5,179	5,202	5,202

Note: This table reports the first and second stage coefficients of equations 1 and 2. The sample includes all identified CEOs of bankrupt petitioned firms from 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions control for fully interacted year and court dummies. Market income includes wage, dividend and business income. The outcome variables are only measured for CEOs that are registered as employed and estimation is done for this subsample. Variables are winsorized at the 99th percentile level, divided by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.8: **The Relative Wage and Size and CEO's Wage Ranking**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : Average firm wage percentile rank					
Bankruptcy (β)	-0.028	0.147***	0.096**	0.072*	0.069*	0.053
	(0.042)	(0.055)	(0.045)	(0.042)	(0.040)	(0.036)
Overall mean:	0.451	0.551	0.570	0.578	0.579	0.517
Untreated marginal firm:	0.492	0.498	0.526	0.553	0.540	0.497
Panel B.	y_{it} : Firm size (employees) percentile rank					
Bankruptcy (β)	0.016	0.247***	0.136**	0.129***	0.137**	0.098**
	(0.047)	(0.051)	(0.055)	(0.048)	(0.054)	(0.044)
Overall mean:	0.591	0.709	0.724	0.734	0.737	0.657
Untreated marginal firm:	0.566	0.547	0.633	0.649	0.628	0.584
Panel C.	y_{it} : Within firm wage percentile rank					
Bankruptcy	-0.166***	-0.158**	-0.138**	-0.174***	-0.089	-0.132***
(standard error)	(0.061)	(0.070)	(0.062)	(0.055)	(0.061)	(0.040)
Overall mean:	0.602	0.588	0.600	0.617	0.622	0.616
Untreated marginal firm:	0.697	0.687	0.726	0.764	0.713	0.689
Number of Observations:	5,602	4,833	5,071	5,179	5,202	5,602

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions control for fully interacted year and court dummies.The firm percentile rank is estimated on the population of firms each year. Average wage is the average wage for the employees in the firm in a given year. Firm size is measured by the total number of employees each year and comprises all main work relationships on the first of January each year. The within-firm wage percentile rank is measured as the CEO's wage rank within the firm s(he) is employed for a given year. The outcome variables are only measured for CEOs that are registered as employed and estimation is done for this subsample. Due to different number of singleton groups for the estimated average effect and the year specific effects, the estimated average effect deviates somewhat from the average of the coefficients in columns 0-4. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.9: **Effect of Bankruptcy on Holding the CEO Position, By Bankruptcy Risk**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : Holding the CEO Position (below median risk)					
Bankruptcy (β)	-0.248***	-0.414***	-0.326***	-0.409***	-0.359***	-0.353***
	(0.073)	(0.083)	(0.080)	(0.086)	(0.082)	(0.069)
Overall mean:	0.864	0.729	0.688	0.659	0.639	0.718
Untreated marginal firm:	0.986	0.965	0.834	0.847	0.804	0.889
Mean bankruptcy risk	0.072	0.072	0.072	0.072	0.072	0.072
Number of observations:	3,745	3,745	3,745	3,745	3,745	3,745
Panel B.	y_{it} : Holding the CEO Position (above or equal median risk)					
Bankruptcy (β)	0.025	-0.061	-0.058	-0.169	-0.189	-0.100
	(0.081)	(0.111)	(0.125)	(0.122)	(0.119)	(0.091)
Overall mean:	0.871	0.711	0.660	0.636	0.622	0.703
Untreated marginal firm:	0.991	0.914	0.899	0.893	0.851	0.915
Mean bankruptcy risk	0.04	0.04	0.04	0.04	0.04	0.04
Number of observations:	3,727	3,727	3,727	3,727	3,727	3,727

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument by industry bankruptcy risk. We measure industry bankruptcy risk as the variation within each industry to facilitate comparison s across industries with different permanent bankruptcy rates. Industries are classified by its two-digit NACE code. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that managed a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded. Regressions controls for fully interacted year and court dummies, age dummies, education level, gender and an indicator for whether the manager is foreign-born. CEO position is measured as the average years held from the case year to four years after. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.10: **Effect of Bankruptcy on Wage Income by Bankruptcy Risk**

Years after decision (t): Average from 0-4				
	y_{it} **: Wage Income (1000 USD)**			
Industry condition/Bankruptcy risk:	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
	(1)	(2)	(3)	(4)
Bankruptcy (β)	-7.551	-8.312	-5.348	-10.477
	(9.916)	(8.218)	(12.697)	(10.985)
Overall mean:	48.278	49.106	49.690	48.592
Untreated marginal firm:	52.783	58.022	47.147	67.462
Number of observations:	1,908	1,815	1,839	1,773

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. by quartiles of industry bankruptcy risk. We measure industry bankruptcy risk as the variation within each industry to facilitate comparison s across industries with different permanent bankruptcy rates. Industries are classified by its two-digit NACE code. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that managed a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies, age dummies, education level, gender and an indicator for whether the manager is foreign-born. CEO position is measured as the average years held from the case year to four years after. Wage income is winsorized at the 99th percentile level, divided by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.11: **Being the CEO: Differences in Estimates by Quartiles of Industry Condition**

	y_{it} : Being a CEO		
	Q2-Q1	Q3-tQ1	Q4-Q1
	(1)	(2)	(3)
Difference in effect of bankruptcy (β)	0.258***	0.502***	0.364***
	(0.080)	(0.094)	(0.013)
Number of replications	200	200	200
Number of observations:	1,815	1,893	1,773

Note: This table reports the difference in the effect of bankruptcy on an indicator being a CEO using the second stage coefficients of equation 1 with judge stringency as an instrument. We run a separate regression for each quartile of within-industry bankruptcy risk and bootstrap this estimation with 200 replications. The reported standard errors are the bootstrapped standard errors from this procedure. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. The industry is classified by its two-digit NACE code. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are bootstrapped using 200 repetitions.

Table A.12: **Effects of Bankruptcy on CEO Position by Local Unemployment**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : Holding CEO Position (below median unemployment)					
Bankruptcy (β)	-0.216***	-0.294***	-0.264***	-0.289***	-0.309***	-0.276***
	(0.068)	(0.078)	(0.078)	(0.079)	(0.076)	(0.063)
Overall mean:	0.861	0.708	0.662	0.638	0.622	0.701
Untreated marginal firm:	0.986	0.907	0.850	0.840	0.820	0.885
Mean unemployment rate	0.045	0.045	0.045	0.045	0.045	0.045
Number of observations:	3,501	3,501	3,501	3,501	3,501	3,501
Panel B.	y_{it} : Holding CEO Position (above or equal median unemployment)					
Bankruptcy (β)	-0.010	-0.234**	-0.095	-0.293***	-0.273**	-0.181**
	(0.089)	(0.109)	(0.125)	(0.113)	(0.121)	(0.090)
Overall mean:	0.874	0.731	0.685	0.655	0.638	0.719
Untreated marginal firm:	1.008	1.001	0.877	0.897	0.858	0.931
Mean unemployment rate	0.031	0.031	0.031	0.031	0.031	0.031
Number of observations:	4,002	4,002	4,002	4,002	4,002	4,002

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Unemployment is measured as the local annual unemployment rate at the commuting zone level. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.13: **Effect of Bankruptcy on CEO Careers (excluding firms that were liquidated later from control group)**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			y_{it} **: Being a CEO**			
Bankruptcy (β)	-0.105	-0.245***	-0.225**	-0.398***	-0.434***	-0.280***
	(0.078)	(0.092)	(0.100)	(0.098)	(0.097)	(0.077)
Overall mean:	0.847	0.695	0.654	0.630	0.617	0.691
Untreated marginal firm:	0.972	0.952	0.928	0.964	0.952	0.956
Panel B.			y_{it} **: Regular Employment**			
Bankruptcy	-0.002	-0.047	-0.043	0.020	0.096	0.005
(standard error)	(0.066)	(0.078)	(0.080)	(0.082)	(0.085)	(0.058)
Overall mean:	0.862	0.762	0.753	0.759	0.763	0.780
Untreated marginal firm:	0.900	0.830	0.769	0.763	0.784	0.809
Number of observations:	6,370	6,370	6,370	6,370	6,370	6,370

Note: This table reports the second stage coefficients of 2SLS equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms from 2005-2013 that manage a limited company with registered employees. We exclude non-treated firms that later on, within four years from the initial bankruptcy filing, experience a bankruptcy . Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.14: **Effects of Bankruptcy on CEO Remuneration (excluding firms that were liquidated later from the control group)**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			y_{it} **: Wage Income (USD 1,000)**			
Bankruptcy (β)	-9.476	-12.180	-5.924	-1.676	-3.411	-6.534
	(7.380)	(7.793)	(8.580)	(8.577)	(9.081)	(7.353)
Overall mean:	50.630	46.611	48.387	49.650	50.083	49.072
Untreated marginal firm:	64.815	62.586	58.612	54.528	56.861	59.477
Panel B.			y_{it} **: Stock Income (USD 1,000)**			
Bankruptcy (β)	-2.632	-3.569*	-5.691**	-1.166	-5.524**	-3.717**
	(2.141)	(2.006)	(2.613)	(2.218)	(2.230)	(1.712)
Overall mean:	1.106	1.196	1.335	1.653	1.862	1.430
Untreated marginal firm:	3.816	3.052	5.367	4.060	5.760	4.410
Panel C.			y_{it} **: Total Market Income (USD 1,000)**			
Bankruptcy	-18.848*	-15.263	-18.629	-10.270	-17.372	-16.076*
(standard error)	(9.972)	(9.290)	(11.766)	(11.324)	(11.811)	(9.098)
Overall mean:	48.723	55.268	57.714	59.438	59.538	56.136
Untreated marginal firm:	69.149	65.973	66.351	60.753	70.272	66.505
Number of observations:	6,370	6,370	6,370	6,370	6,370	6,370

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms from 2005-2013 that manage a limited company with registered employees. We exclude untreated firms that later on, within four years from the initial bankruptcy filing, experience a bankruptcy. Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. Market income includes wage, dividend and business income. Variables are winsorized at the 99th percentile levels, divided by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

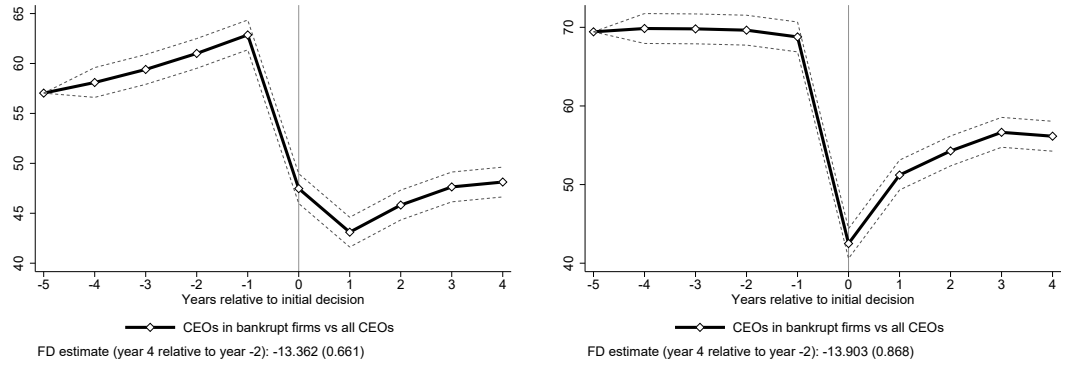
Table A.15: **Effect of Bankruptcy on Holding the CEO Position , by Tenure**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.	y_{it} : Holding the CEO Position (below median tenure)					
Bankruptcy (β)	-0.028	-0.189**	-0.073	-0.194*	-0.224**	-0.149*
	(0.076)	(0.092)	(0.114)	(0.113)	(0.113)	(0.078)
Overall mean:	0.878	0.707	0.660	0.629	0.609	0.700
Untreated marginal firm:	0.985	0.962	0.837	0.839	0.808	0.894
Number of observations:	4,098	4,098	4,098	4,098	4,098	4,098
Panel B.	y_{it} : Holding the CEO Position (above or equal median tenure)					
Bankruptcy (β)	-0.203***	-0.303***	-0.257***	-0.343***	-0.307***	-0.285***
	(0.074)	(0.086)	(0.090)	(0.088)	(0.090)	(0.076)
Overall mean:	0.856	0.735	0.692	0.670	0.657	0.723
Untreated marginal firm:	0.989	0.908	0.862	0.887	0.844	0.900
Number of observations:	3,411	3,411	3,411	3,411	3,411	3,411

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Median tenure pre-bankruptcy is three years. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

B Appendix: Additional Figures

Figure A.1: **Income Trends for CEOs in Bankrupt Firms**

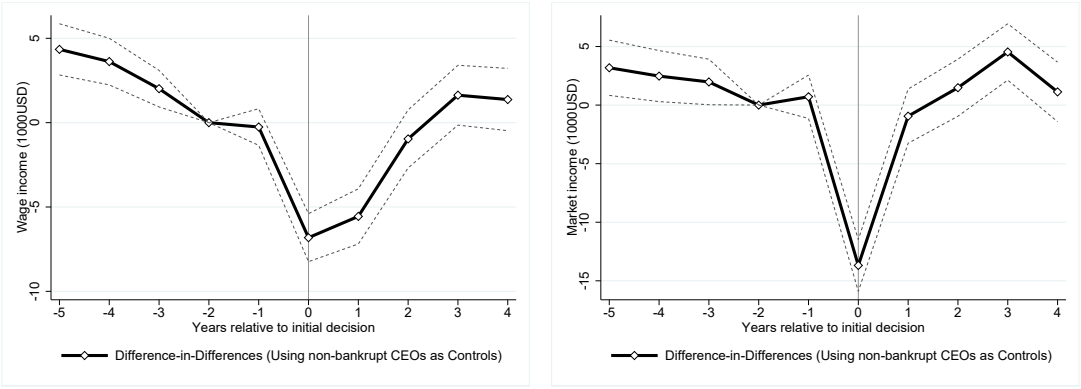


(a) Wage Income (1000 USD)

(b) Market Income (1000 USD)

Notes: The left panel plots the difference in wage income between CEOs in bankrupt firms relative to all registered CEO controlling for age fixed effects and case-year fixed effects. The right panel plots difference in market income between CEOs in bankrupt firms relative to all registered CEO controlling for age fixed effects and case-year fixed effects. Time is normalized to zero at the year the bankruptcy petition was filed. The sample includes all identified CEOs of liquidated firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. Wage income is winsorized at the 99th percentile. Market income includes capital income, business income and wage income and is winsorized at the 99th percentile. Both measures are deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9.

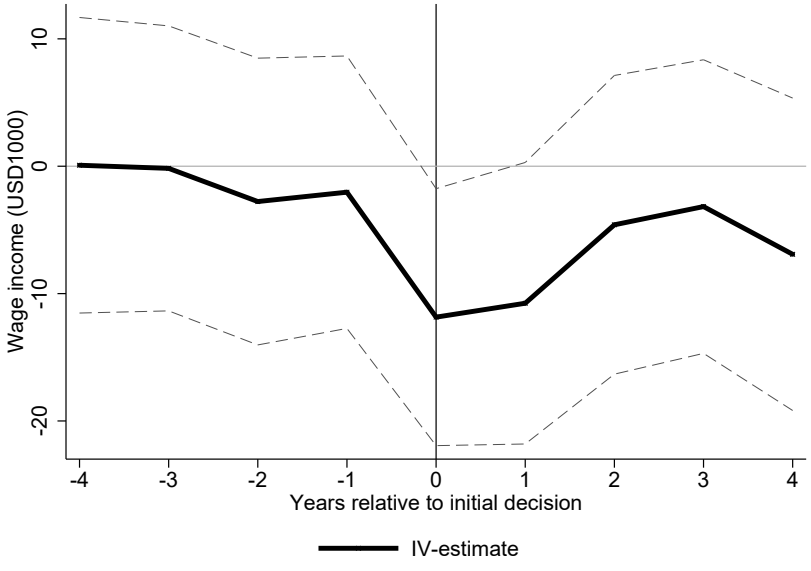
Figure A.2: **Conditional difference: Bankrupt vs non-bankrupt CEOs**



(a) Wage Income (1000 USD) (b) Market Income (1000 USD)

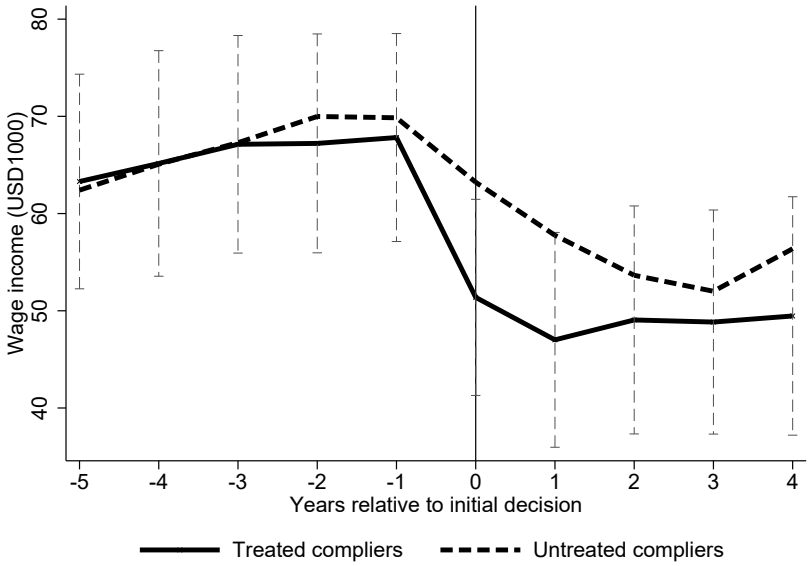
Notes: The left panel plots the difference between bankrupt and non-bankrupt CEOs' wage income and the right panel plots the difference in market income five years prior to the case is received by the bankruptcy court to four years after, conditional on case year fixed effects and age fixed effects. Time is normalized to zero at the year the bankruptcy petition was filed. The sample includes all identified CEOs of liquidated firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded. Wage income is winsorized at the 99th percentile. Market income includes capital income, business income and wage income and is winsorized at the 99th percentile. Both measures are deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9.95% confidence intervals are represented by dashed lines.

Figure A.3: **IV-estimate Wage Income**



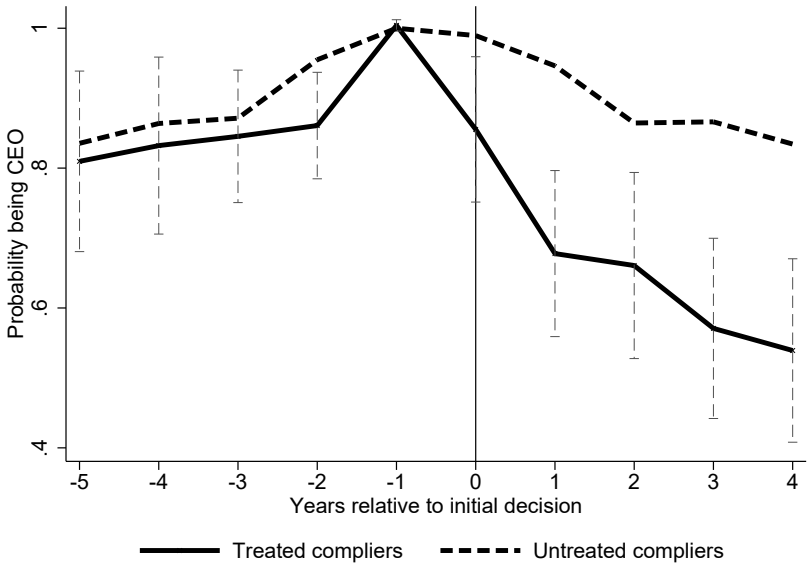
Notes: This figure displays IV-estimate of CEOs' wage income using our measure of judge stringency as instrument. Wage income is winsorized at the 99th percentile, dvidid by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded.

Figure A.4: **Potential outcomes Wage Income**



Notes: This figure displays estimated complier mean and estimated non-treated complier mean of CEOs' wage income using our measure of judge stringency as instrument. Wage income is winsorized at the 99th percentile, dvidid by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded.

Figure A.5: **Potential outcomes: Being a CEO**



Notes: This figure displays estimated complier mean and estimated non-treated complier mean of an indicator for being a CEO.The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handle less than 20 cases are excluded.

C Continuing Firms

A phenomenon that could potentially puncture the interpretation of our results is the fact that some bankrupt firms may re-appear in the data with a new identity if the firm was either sold as a going concern with the same CEO or because the CEO starts up as a restructured entity but with the original workforce. To explore the empirical importance of such "implicit reorganizations" of bankrupt firms, we follow the approach of Hethey-Maier and Schmieder (2013) who identify implicitly reorganized firms using information from matched employer-employee data.

The underlying idea in Hethey-Maier and Schmieder (2013) is that if the workers of a new firm mainly consist of workers that also worked in a bankrupt firm, then this indicates that a bankrupt firm has been reestablished after bankruptcy. Because implicit reorganizations only have the potential to influence our results if the CEO continues in her job, we extend the approach of Hethey-Maier and Schmieder (2013) by also requiring that the new firm includes the bankrupt firm's CEO. Specifically, we use the share of workers in a new firm previously employed by a bankrupt firm. To identify continuing firms, we proceed in two steps. First, we identify all workers of petitioned firms in the year prior to the petition. In the second step, we identify the new firms that employ these workers are employed one year after the court's bankruptcy decision. In each of these new firms, we calculate the share of workers that was previously employed in a petitioned firm and identify if the firm also has the same CEO. Finally, we identify which new firms have more than 50 percent and 80 percent, respectively, coming from a single bankrupt firm.

Panel A in Table A.16 reports the summary statistics for our sample of bankruptcy petitions. Of the set of firms that go bankrupt, we identify 531 "new" firms that employees at least half of the employees that previously worked together, constituting about 11 percent of the number of bankrupt firms in our sample. However, only 111 firms also have the same CEO – about 2 percent of the number of bankrupt firms – indicating that implicit reorganization in bankruptcy is not very prevalent in the Norwegian setting. As a comparison, Panel B displays the same statistics for the population of petitioned non-bankrupt firms. About 27 of these firms continue to employ at least half of their employees, indicating that periods of financial distress lead firms to reduce the number of employees. We also see that the majority of the continuing firms keep their CEOs.

Table A.16: **Continuing Firms**

	(1)	(2)	(3)	(4)
	A. Bankrupt		**B. Non-Bankrupt**	
	Number of firms	Share	Number of firms	Share
Sample	5,029	1	2,565	1
Continuing Firms ($>50\%$)	531	0.11	705	0.27
New CEO	420	0.08	153	0.06
Same CEO	111	0.02	552	0.22
Continuing Firms ($>80\%$)	343	0.07	315	0.12
New CEO	295	0.06	81	0.03
Same CEO	48	0.01	234	0.09

Notes: This table reports the number of firms that employs more than 50 percent and more than 80 percent of the workers that previously worked together at a petitioned firm the year prior to petition. These "new" firms are identified among the set of firm in which workers of petitioned firms are employed at one-year after the case-year. The sample of petitioned firms includes bankrupt petitioned firms with a unique identify CEO from 2005-2013 that manage a limited company with registered employees. Cases assigned to judges who handles less than 20 cases are excluded.

In tables A.17 and A.18**,** we show that how our main results change if we exclude continuing firms from our sample. This exclusion does not significantly affected our main results. If anything, the effects on total market income are slightly stronger, reflecting lower income losses among bankrupt CEOs who are able to reestablish their firm.

Table A.17: **Main Effects of Bankruptcy on CEO Careers (excluding cont. establishments)**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			y_{it} **: Being a CEO**			
Bankruptcy (β)	-0.139***	-0.266***	-0.199***	-0.283***	-0.281***	-0.235***
	(0.053)	(0.060)	(0.069)	(0.065)	(0.066)	(0.052)
Overall mean:	0.866	0.716	0.670	0.644	0.627	0.707
Untreated marginal firm:	0.989	0.945	0.863	0.865	0.832	0.902
Panel B.			y_{it} **: Regular Employment**			
Bankruptcy	-0.134**	-0.078	-0.093	-0.003	-0.017	-0.065
(standard error)	(0.059)	(0.064)	(0.064)	(0.063)	(0.066)	(0.045)
Overall mean:	0.740	0.636	0.668	0.683	0.688	0.683
Untreated marginal firm:	0.851	0.728	0.732	0.700	0.715	0.745
Number of Observations:	7,473	7,473	7,473	7,473	7,473	7,473

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms from 2005-2013 that manage a limited company with registered employees. We exclude 111 bankrupt firms that we identify as reestablished after bankruptcy using the method explained in Section C from the sample. Cases assigned to judges who handles less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

Table A.18: **Effects of Bankruptcy on CEO Remuneration (excluding cont. establishments)**

Years after decision (t):	**0**	**1**	**2**	**3**	**4**	**Average**
Panel A.			y_{it} **: Wage Income (USD 1,000)**			
Bankruptcy (β)	-12.211**	-11.145*	-5.406	-3.949	-8.052	-8.153
	(5.176)	(5.681)	(6.041)	(5.922)	(6.344)	(5.151)
Overall mean:	51.249	46.464	47.723	48.743	49.291	48.694
Untreated marginal firm:	63.203	57.757	53.666	52.043	56.378	56.614
Panel B.			y_{it} **: Stock Income (USD 1,000)**			
Bankruptcy (β)	-1.972	-1.908	-3.091**	-0.131	-3.360**	-2.092**
	(1.299)	(1.179)	(1.538)	(1.471)	(1.414)	(1.037)
Overall mean:	1.073	1.077	1.243	1.497	1.707	1.319
Untreated marginal firm:	3.098	1.979	3.471	2.336	3.802	2.938
Panel C.			y_{it} **: Total Market Income (USD 1,000)**			
Bankruptcy	-17.156**	-11.453*	-12.038	-6.503	-15.689*	-12.568**
(standard error)	(6.787)	(6.579)	(7.800)	(7.477)	(8.269)	(6.100)
Overall mean:	50.167	54.426	56.916	58.371	58.835	55.743
Untreated marginal firm:	69.149	65.973	66.351	60.753	70.272	66.505
Number of observations:	7,473	7,473	7,473	7,473	7,473	7,473

Note: This table reports the second stage coefficients of equation 1 using judge stringency as an instrument. The sample includes all identified CEOs of bankrupt petitioned firms in the period 2005-2013 that manage a limited company with registered employees. We exclude 111 bankrupt firms that we identify as reestablished after bankruptcy using the method explained in Section C from the sample. Cases assigned to judges who handle less than 20 cases are excluded. There are 845 unique judges. Regressions controls for fully interacted year and court dummies. Market income includes wage, dividend and business income. Variables are winsorized at the 99th percentile levels, divided by 1,000, deflated to 2015 using the average wage growth (base amounts) and converted to USD. The average exchange rate in 2020 was about NOK/USD = 9. ***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are clustered at the judge level.

D Bankruptcy Stigma and the Reallocation of Managerial Talent

The effect of "random" bankruptcy on the probability that a CEO exits the executive labor market suggests that bankruptcy can limit the available pool of managerial talent in the economy through stigma effects. In this section, we quantify this effect with the help of a simple accounting framework.

Let C_t denote the stock of CEOs in the economy at time t. This stock represents the pool of managerial talent in the economy that are potentially available for firms in production. Each year, there is a probability δ that a CEO will be displaced from a firm through bankruptcy. A fraction f of these CEOs reenters the executive labor market the next period, while $(1-f)$ remains outside. We denote the number of other entries by C_t^N and the number of other exits by C_t^E. Other entries capture all other individuals that apply for a CEO position and are available for the hiring firms. This measure includes applicants without previous CEO experience and applicants who have been displaced from or left their former CEO position for reasons other than bankruptcy. Other exits capture individuals who have been displaced from and left their former CEO position for reasons other than bankruptcy and do not reenter the market. The stock of CEOs at time t is then determined by the following equation:

$$\underset{}{C_t} = \underset{\{\text{Remaining}\}}{(1-\delta)C_{t-1}} + \underset{\{\text{Reentry }\}}{\delta f C_{t-1}} - \underset{\{\text{Other exits}\}}{C_t^E} + \underset{\{\text{Other entries}\}}{C_t^N}$$

We assume that there is a constant flow of entry into the CEO market given by $C^N$and that other exits are given by $C_t^E = \lambda C_{t-1}$, where λ denotes a constant share of CEOs that separates for other reasons. The steady state level of CEOs can then be written as:

$$C_s = \frac{C^N}{\lambda + \delta(1-f)}$$

The analysis in section 5.3 indicates that stigma effects are an important mechanism in explaining why bankruptcy leads CEOs to exit the executive market. We capture this in our framework by assuming that the share of bankrupt CEOs that reenters the market f is given by a matching probability $f = A(s)p(\theta)$ that depends on the ratio of vacancies to applicants (the tightness θ) and a matching efficiency $A(s)$ that is decreasing function of the bankruptcy stigma s in the economy. If firms view bankruptcy as a strong signal of managerial inability, fewer CEOs are re-hired for any given level of tightness. We can think of this as the result of a more thorough and time consuming vetting process of managers that previously led a firm that was liquidated through bankruptcy. Formally we can write, $f = f(s)$, such that $f(s') < f(s)$ for any $s' > s$.

It follows that if stigma is higher, the steady state level of CEOs falls. This follows mechanically as the reentry share f falls if bankruptcy stigma is higher. That is, $f = f(s)$, such that $f(s') < f(s)$ for any $s' > s$. We can write the change in the steady state level from a shift from s to $s' > s$ as:

$$\frac{\Delta C_s}{C_s} = \frac{f(s) - f(s')}{(\lambda/\delta + 1 - f')}$$

This measures the percentage change in the stock of available CEOs for a shift in the reentry rate after bankruptcy from $f(s)$ to $f(s')$. To quantify this effect, we use the estimated exit rate after bankruptcy when within-industry bankruptcy rates are low as measure of $(1 - f(s'))$ and the estimated exit rate when within-industry bankruptcy rates are high as a measure of $(1 - f(s))$. See section 5.3 for discussion and Table A.9 for the estimated effects. [1] We measure δ as the share of all CEOs in a given year that experiences bankruptcy. Using the information on the number of CEOs in total and petitioned firms from Table 1, and the average bankruptcy rate of 0.6 for petitioned firms, we get a measure of $\delta = 0.017$. Finally, we use the information on CEO-survival rates from Table 2 and subtract the share that exits due to bankruptcy to measure $\lambda = 1 - s - \delta = 0.04$.

$$\frac{\Delta C_s}{C_s} = \frac{f(s) - f(s')}{(\lambda/\delta + 1 - f')} \approx \frac{0.94 - 0.59}{(0.04/0.017 + 0.414)} \approx 0.13$$

From this quantification we find that our estimates are consistent with a 13 percent reduction in the steady-state level of the number of available CEOs. Finally, we can use the equation for the stock of CEOs to quantify the mechanical effect on the number of CEOs and firm matches. Assume that a new firm (start-

[1] Our estimate of $(1 - f(s))$ is 0.061 our estimate of $(1 - f(s'))$ is 0.414, which is the probability that a CEO displaced through bankruptcy does not reenter within the first year after the bankruptcy case. Thus, we measure $f(s)$ as 0.94 and $f(s')$ as 0.59 where $f(s) - f(s') = 0.26$

up) only can operate if it is matched by a CEO. All available CEOs are assumed to be searching on and off the job and represent the available applicant pool of managerial talent. Matching is represented by a constant returns to scale matching function $m = Ax(v,c)$, where v is the number of vacant CEO positions and c denotes the pool of available CEO applicants. Let c^s denote the share of CEO applicants that were displaced through bankruptcy and c^n all other applicants. Matching efficiency A is assumed to differ among "normal" applicants, c^n, and applicants displaced through bankruptcy c^s. In particular, for the latter group, matching efficiency depends on the degree of bankruptcy stigma s. The total number of matches can be written as the sum of matches from the pool of normal applicants c^n and from the pool of applicants with bankruptcy experience c^s:

$$M = A_n m(v,c^n) + A(s)m(v,c^s)$$

Now, we can analyze how a change in bankruptcy stigma s affects the total number of matches in the economy:

$$\frac{dM}{ds} = \frac{dA}{ds}m(v,c^s) = \frac{dA}{ds}p(\theta)c^s = \frac{df}{ds}c^s$$

The above equation shows that we can write the change in the number of matches as the product of the effect on the reentry probability of bankruptcy displaced CEOs df/ds and the number of CEO applicants that experienced bankruptcy. We quantify df/ds as we did above, where $df/ds = f(s') - f(s)$ is informed by the IV-estimates in Table A.9. Our estimates are consistent with a reduction in the annual reentry probability of bankruptcy displaced CEOs by 0.35 percentage points, which in our setting translates into approximately 220 fewer matches each year due to bankruptcy stigma. Note that this derivation ignores any changes in firms' vacancy posting from changes in matching efficency and assumes that firm's cannot direct their search towards specific submarkets of CEOs. However, the reduced form evidence on the effect on the reentry probability of CEOs is independent of the particular model structure.

We can also express our calculation in terms of the probability that a new firm is able to find a CEO, or the hiring probability. Denote $q(\theta)$ the probability that a firm meets a CEO. With a constant returns to scale matching function, this is given by $q(\theta) = p(\theta)\theta$. The change in this probability from a change in bankruptcy stigma is given by:

$$\frac{dq(\theta)}{ds} = \frac{dA}{ds}\frac{m(v,c^s)}{v} = \frac{df}{ds}\frac{c^s}{v}$$

Using data on the average annual number of CEO vacancies from official vacancy data and an online job portal (Bhuller et al., 2021), we find a reduction in the hiring probability of 6 p.p.

E Appendix: Data Sources

E.1 Data Sources

State Register of Employers and Employees. Workers' earnings and employment histories, and transitions between jobs and occupations come from the Norwegian matched employer employee register owned and managed by NAV and provided by Statistics Norway. This data set covers virtually all employment contracts from 1995 to 2014. Every worker-level contract is reported by the employer to the authorities at the end of the year, and includes information on the dates of alterations to the contract, and the corresponding wage, industry and occupational codes, geographic location and tenure at the establishment. From this source, we construct time series of monthly earnings for each worker, and the transitions between establishments and occupations.

BRREG. The data from the Bronnoysund Register Center (BRREG) provides us with information on corporate roles, including manager, board members, and the chairman of the board. For each year, a person is registered with one or more roles in a firm. The data set also includes information on firm ownership, dividend payouts, number of stocks, and historical value of the stock. Ownership is registered to persons and legal entities such as firms and holding companies.

Judicial System/DA. This data set provides us with information on cases related to bankruptcy. It provides us with the legal paragraphs, an anonymized identifier for the assigned judge, dates on which the case was received and when it was decided. It includes some information on creditors and some information on the court's proceedings. Using bankruptcy cases that can be linked to firm identifiers, we identified the manager via the corporate role register from BRREG. We created our leave-out mean judge instrument based on the full set of cases in which a judge opened bankruptcy proceedings.

Administrative Population Registers. To capture complete information on workers' geographic location, education, annual earnings, and household income, social security data is merged with longitudinal administrative registers provided by Statistics Norway and covering every Norwegian resident from 1967 to 2014. These administrative data sources contain individual demographic information (including sex, age, zip codes, and years of education) and, since 1993, all sources of annual income, including earnings, self employment income, capital income, and cash transfers.

E.2 Baseline sample

Data from the Court Administration

We use data from the Norwegian Court Administration on bankruptcy petitions in the period 2005-2018. The dataset contains information on case outcomes, anonymized judge identifiers, case processing time and district court and case dates. We keep cases that are decided within that court (not appealed) and with a clear outcome. We use this data (102 961 cases) to estimate the leave-one-out judge stringency rate based on this data. We then keep corporate bankruptcy cases (cases with a registered firm). This dataset comprises of 70

681 cases for 46 123 unique firms. The mean bankruptcy rate is 0.5 with a standard deviation of 0.5. The mean stringency rate is 0.5 with a standard deviation of 0.13.

Identify managers from the Norwegian Register of Business Enterprises

We keep the first registered bankruptcy petition for each firm in our dataset. We combine the information on these bankruptcy petitioned firms with information on registered CEOs at the case year from the combined Central Coordinating Register for Legal Entities and the Norwegian Register of Business Enterprises (RBE). The register gives information on all individuals with a role as accountant, business manager, CEO, contact person, and people who have signature authority or power of attorney in a registered business in Norway. The registers are continuously updated and checked against the population register every week. We only keep CEOs that are connected to one bankruptcy petitioned firm in the case year and keep the first case for these CEOs. We are left with a sample of 31 469 CEOs.

Information on firms from the Norwegian Register of Business Enterprises

We then combine the dataset with information on number of employees and status for the company from the RBE with information on firm revenue and balance sheet. 29 518 of the firms are registered as limited companies (AS) operating the year prior to the bankruptcy. Of these firms, only 10 244 are registered with detailed balance sheet information the year prior to the bankruptcy.

Individual CEO characteristics

We use information from the Norwegian Tax Authority on manager renumeration (wages, capital income, business income) and balance sheet information (debt, bank-deposits etc.) in addition to information on personal characteristics such as age, education level, sex and marital status from the population register. Of managers we identify, 542 are not registered in the population register and 993 are not registered in the tax register.

Employment from the employer-employee register:

Finally, we used employer-employee matched data to identify the workers of the bankruptcy petitioned firm one year prior to the opening of the bankruptcy proceedings. We keep the last main work relationship for each worker in the sample. The main work relationship is the relationship that pays the highest wage. Of the 31 469 CEOs we identify, only 13 574 are CEO in a firm with registered employees.

We restrict the sample to CEOs in limited companies with registered employees. This sample consists of 13 274 unique CEOs. Of these CEOs, we can only follow 8 203 of them four years after the case-year and only 7 605 when we restrict the number of cases a judge needs to 20. In the estimation, we drop singleton groups, i.e., groups with only one observation within our fixed effects. There are 19 singleton groups. This leaves us with a final estimation sample of 7 586 CEOs.