

Grindaker, Morten; Kostøl, Andreas Ravndal; Roszbach, Kasper

Working Paper

Executive labor market frictions, corporate bankruptcy and CEO careers

Working Paper, No. 15/2021

Provided in Cooperation with:

Norges Bank, Oslo

Suggested Citation: Grindaker, Morten; Kostøl, Andreas Ravndal; Roszbach, Kasper (2021) : Executive labor market frictions, corporate bankruptcy and CEO careers, Working Paper, No. 15/2021, ISBN 978-82-8379-214-0, Norges Bank, Oslo, <https://hdl.handle.net/11250/2835492>

This Version is available at:

<https://hdl.handle.net/10419/264936>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



<https://creativecommons.org/licenses/by-nc-nd/4.0/deed.no>

WORKING PAPER

Executive Labor Market Frictions, Corporate Bankruptcy and CEO Careers

NORGES BANK
RESEARCH

15 | 2021

MORTEN GRINDAKER
ANDREAS R. KOSTØL
AND KASPER ROSZBACH



NORGES BANK

Working papers fra Norges Bank, fra 1992/1 til 2009/2 kan bestilles over e-post:
111facility@norges-bank.no

Fra 1999 og senere er publikasjonene tilgjengelige på www.norges-bank.no

Working papers inneholder forskningsarbeider og utredninger som vanligvis ikke har fått sin endelige form. Hensikten er blant annet at forfatteren kan motta kommentarer fra kolleger og andre interesserte. Synspunkter og konklusjoner i arbeidene står for forfatterens regning.

Working papers from Norges Bank, from 1992/1 to 2009/2 can be ordered by e-mail
FacilityServices@norges-bank.no

Working papers from 1999 onwards are available on www.norges-bank.no

Norges Bank's working papers present research projects and reports (not usually in their final form) and are intended inter alia to enable the author to benefit from the comments of colleagues and other interested parties. Views and conclusions expressed in working papers are the responsibility of the authors alone.

ISSN 1502-8190 (online)

ISBN 978-82-8379-214-0 (online)

Executive Labor Market Frictions, Corporate Bankruptcy and CEO Careers*

Morten Grindaker[†]

Andreas R. Kostøl[‡]

Kasper Roszbach[§]

This version: December 16, 2021

Abstract: CEOs of large firms filing for bankruptcy are more likely to exit the executive labor market after bankruptcy and experience substantial compensation losses (Eckbo et al., 2016). While the fear of reputational scarring can lead to lower risk-taking and manifest itself as lower rates of entrepreneurship and job growth, the mechanisms through which bankruptcy affects CEO careers are not well understood. In this paper, we examine the effect of "random bankruptcy" decisions on small and medium-sized business CEOs' careers. By random, we mean job separation for reasons unrelated to a firm or CEO quality but rather through a court's bankruptcy decision. We control for the unobserved ability of bankrupt and non-bankrupt CEOs by using randomly assigned judges' propensity to liquidate firms as an instrument. We then combine our sample of CEOs with administrative records containing granular information on income, wealth, new employers and job titles. Our results show that bankrupt CEOs find new employment quickly, but that a large share exits the executive labor force. On average, bankruptcy reduces CEOs' variable income components. While the net present value of CEOs' loss of future capital income equals more than 60 percent of annual pre-bankruptcy income, we observe no effect on wage income. We find that displaced CEOs are more likely to reallocate to new industries and new geographic areas, suggesting that managerial skills are portable. We explore how the income and employment effects of bankruptcy vary with industry conditions. Consistent with the executive labor market using bankruptcy as a noisy signal of managerial ability, we find the displacement effect is stronger when industry conditions are good. Our evidence is consistent with the presence of information frictions that could entail important social costs.

Keywords: Occupational Choice; Bankruptcy; CEO; Organizations; Executive Compensation.

JEL codes: G33, J24; K22, L29, M12

*This project received financial support from the President of ASU's Strategic Initiative Fund. This paper should not be reported as representing the views of Norges Bank. We would like to thank Domenico Ferraro, Jonas Hjort, Bart Hobijn, Aksel Mjøs, Espen Moen, Magne Mogstad, Gisle Natvik, Plamen Nenov, Dan Silverman and seminar participants at the Norwegian Business School for useful comments and suggestions. We are grateful to Leif Villars-Dahl, who helped in interpreting the legal framework, and Matthew Merkle for excellent research assistance. This paper should not be reported as representing the views of Norges Bank. The views expressed are those of the authors and do not necessarily reflect those of Norges Bank.

[†]Norwegian Business School; Norges Bank. E-mail: morten.grindaker@bi.no

[‡]Arizona State University, W.P. Carey School of Business; Norges Bank; 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 In this paper, we examine the effect of "random bankruptcy" decisions on the careers of chief operating officers (CEOs). By random, we mean separation from a business for reasons unrelated to the firm or CEO quality but rather forced separation through randomness in a court's bankruptcy decision. The executive labor market may nevertheless interpret bankruptcy as a signal of organizational failure or lack of managerial qualifications (e.g., [Gibbons and Katz, 1991](#)). Aside from informational asymmetries, CEO wages may fall if their remuneration reflects specific skills that are valued less by outside employers.² While empirical evidence on the importance of these channels is scarce, bankruptcy costs for managers have potentially important implications for aggregate employment and growth.³ A key concern among economists and policymakers is, for example, that CEOs may start fewer risky projects than what is socially desirable to avoid the risk of substantial personal costs (e.g., [Jensen and Meckling, 1976](#) and [Eckbo and Thorburn, 2003](#)).

Bridging the knowledge gap has proven difficult in part because corporate bankruptcy seldom occurs randomly. Instead, bankruptcy decisions are confounded by unobserved heterogeneity and selection of low-ability workers 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.⁴ Without addressing this selection problem, an observational analysis would be biased towards concluding that CEOs suffer financially from bankruptcy. What is more, examining CEOs' income and career trajectories requires large-scale firm-manager panel data to measure executive remuneration components and follow management 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 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 in Norway to district judges. Judges declare a firm bankrupt when they deem the company insolvent. While this condition applies equally to all firms, judges can differ in their interpretation of the insolvency condition. This scope for discretion is a source of significant variation in judges' stringency and the likelihood of opening bankruptcy proceedings. We use this exogenous variation in firms' likelihood of being declared bankrupt to study how corporate bankruptcy affects CEOs' careers.

¹See, 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.

²See, e.g., [Becker \(1962\)](#) and [Oi \(1962\)](#) for theoretical work on human capital. [Blanchard and Katz \(1992\)](#) discusses and documents 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 \(1999\)](#) reviews the literature on search frictions in general.

³[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.

⁴This 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. [Kroft et al. \(2013\)](#) find that employers use unemployment duration as a signal of worker ability, which is less informative in weak labor markets.

Our empirical strategy draws on a unique dataset we construct by combining six Norwegian administrative registers. These enable us to identify CEOs for all Norwegian 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. With these data, we can estimate the average effect of bankruptcy among CEOs who are separated from their firm because it was assigned to a stringent judge in a filed bankruptcy petition. To estimate the Local Average Treatment Effect (LATE), we run a two-stage least squares regression where the first stage equation instruments the indicator variable for firm bankruptcy. The second stage identifies the causal effect of bankruptcy resulting from the initial filing on a range of CEO outcomes.

Two broad conclusions emerge from our empirical analysis. First, we find that corporate bankruptcy has a long-lasting impact on CEOs' careers. CEOs whose firms are declared bankrupt are 25 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, the displacement effects of bankruptcy are significantly stronger when default rates in an industry are low. Dividing our sample 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 – suggesting that CEO wages are not tied to industry demand. This conclusion is supported by evidence of greater mobility between industries post-bankruptcy and by an increase in CEOs tendency to move to more productive firms with a higher-paid workforce.

Our interpretation of the displacement effect is simple. Suppose a manager has discretion over the firm and can affect the likelihood of defaulting on a loan. Avoiding bankruptcy thus signals to the executive market that the manager is of high ability. Conversely, the market infers that bankrupt managers are of lower quality and offers them less responsibility in the next job. The finding that occupational displacement is smaller when industry conditions are poor is consistent with a weaker informational signal and the seminal model of lemons by [Gibbons and Katz \(1991\)](#). In a related empirical study, [Kroft et al. \(2013\)](#) perform an experimental audit study and find that scarring effects from unemployment are weaker when aggregate unemployment is high. Consistent with these studies, we show the informational stigma of bankruptcy is less pronounced in weak labor markets but the difference much stronger when comparing the firms' overall industry performance.

Why does falling off the corporate job ladder not lead to persistent wage losses for the CEOs we study? 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 choosing or being recruited to more profitable firms suggests that CEOs have highly portable labor market skills (e.g., [Gathmann and Schonberg, 2010](#)) which mitigates the loss of wage income. Fourth, changes in the income of CEOs following a corporate bankruptcy are predominantly driven by changes in the variable income components. Taken together, the evidence indicates CEOs experience a reduction in remuneration when they fall off the corporate job 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. We complement [Eckbo et al. \(2016\)](#), who document the costs of bankruptcy for CEOs in 322 large US firms filing for Chapter 11 bankruptcy. They find that CEOs suffer a median remuneration loss of five times their annual pre-bankruptcy income over the remainder of their career. Two-thirds of those CEOs exit the executive labor market after bankruptcy and suffer substantial compensation losses. Although [Eckbo et al. \(2016\)](#) cannot fully control for endogenous selection out of the executive labor market, their observed income effect appears to be driven primarily by CEO and firm-specific effects. Our results are derived in a different context, for predominantly small and medium-sized firms and in a strict experimental setting in Norway, allowing us to overcome a fundamental econometric identification challenge. Our empirical analysis offers, to the best of our knowledge, the first experimental evidence of the effects of corporate bankruptcy on CEO careers. While our evidence is consistent with the presence of informational problems, our main contributions are to show, in an experimental setting, that younger CEOs essentially go through bankruptcy unscathed while older CEOs incur substantial costs, and that displacement effects are stronger when industry bankruptcy rates are low.

Our work contributes to the literature that studies the implications of frictions associated with corporate bankruptcy.⁵ [Shleifer and Vishny \(1992\)](#) demonstrate that when assets are specific to an industry, like oil rigs, and the market is thin, default costs are larger and credit supply lower. Consistent with these predictions, [Bernstein et al. \(2019a\)](#) find that straight bankruptcy (c.f. Chapter 7 in the US) often leads to inefficient allocation of assets, especially when search frictions are large and credit supply is low.⁶ Our evidence adds to this line of research by showing that bankruptcy costs for CEOs may be smaller when bankruptcy is more common. In a closely related paper, [Chang and Schoar \(2013\)](#) find that debtor-friendly judges cause lower firm survival rates, lower sales, and employment growth after Chapter 11 bankruptcy. Misaligned incentives between managers and owners and the extraction of private benefits by the managers of distressed firms are potential explanations for their findings. 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

⁵Our 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).

⁶[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.

bankruptcy shocks. Our evidence does not support the notion that CEOs have industry-specific skills and shows that CEOs are more likely to continue being CEO when industry demand is low. What is more, our findings show that CEO skills are highly transferable and employable in more productive firms in a range of different industries.

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. Our work adds to this literature by establishing that CEOs that experience bankruptcy move to larger and more profitable firms with higher-paid workers.

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 \(2006\)](#), [Aizer and Doyle Jr \(2015\)](#), [Mueller-Smith \(2015\)](#), [Di Tella and Scharrodsky \(2013\)](#) and [Dobbie et al. \(2018\)](#). Our results also relate to the empirical literature examining the costs and effects of corporate bankruptcy.⁷

Finally, our paper has implications for the design of debt contracts and policy. In an environment 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. Hence, by rolling over debt, the CEO may keep a marginal firm alive to avoid personal costs from stigma. Suppose CEOs then act on the signal strength by strategically delaying a bankruptcy filing to the time when personal bankruptcy costs are lowest. In that case, the mechanism we have identified would lower creditors' recovery rates due to thinner markets and can lead to financial vulnerabilities if industry debt is concentrated in a few banks. The existence of such strategic behavior would imply an important role for loan covenants to facilitate early liquidation (see, e.g., [Almeida et al. 2014](#) for a review).

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 the causal effects of bankruptcy on CEOs' careers 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.

⁷[White \(1989\)](#) reviews the efficiency of the bankruptcy process in the US. [Bris et al. \(2006\)](#) compare estate values of reorganized firms under Chapter 11 with those of firms liquidated under Chapter 7.

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.

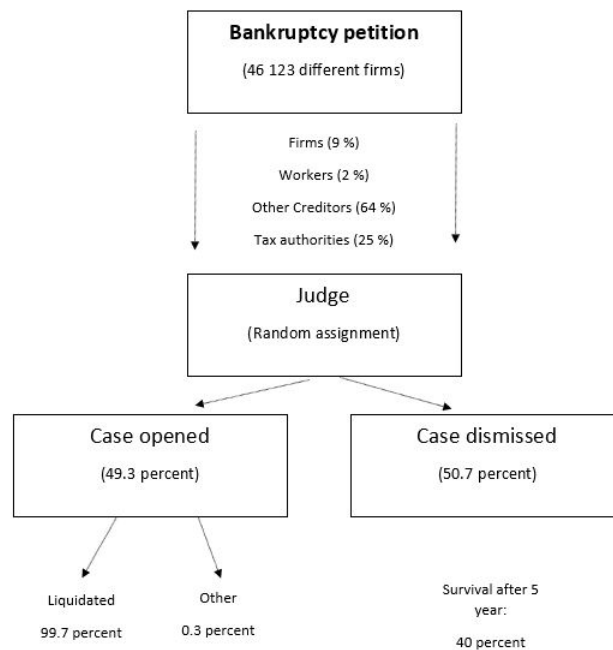
Regular judges are appointed permanently and covered by strong employment protection. Among them, the chief judge oversees the administration of the district court. Deputy judges are also law school graduates, but are appointed to a court for a maximum period of three years (five years in Oslo). We do 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. 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 Norwegian 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.

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 an administrator/trustee. The task of the administrator 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.

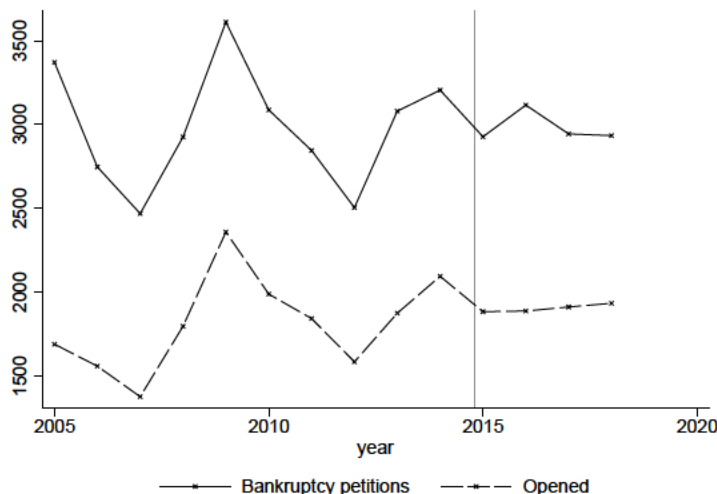
The administrator can sell the assets either individually or together, as a going concern, whichever gen-

erates 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 administrator also informs the court if there are reasons to impose a bankruptcy quarantine on board members or the CEO.⁸

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 1 cases.

Figure 2: Bankruptcy Petitions in Norway



Notes: Figure plots the evolution of the number of bankruptcy petitions and the number of straight bankruptcy openings (e.g., Chapter 7 in the US context) in Norway in the period 2004–2018. Dismissed cases do not meet the insolvency criteria. There are about 300,000 firms in Norway in 2018.

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

⁸Bankruptcy 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.

50 percent from 2007 to 2009. Chapter 7 bankruptcies in the US rose more sharply in the same period, 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.

Table 1: Summary Statistics for CEO Pay

	(1)	(2)	(3)	(4)	(5)
	Non-Distressed		Financially Distressed		Test of
	Mean	Std. Dev.	Mean	Std. Dev.	equal means
					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		

Standard deviations [in square brackets]

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.

2.4 CEO Recruitment and Compensation

CEO compensation consists of several fixed and variable components. 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. Table 1 displays summary statistics for the fixed wage, bonus and overtime pay parts of their compensation components. We measure each component in 2015 and define a firm as financially distressed if a bankruptcy petition was

filed in the period 2015-2018.

The first row of Table 1 shows that CEOs of firms that become distressed during the next three years are compensated 51 percent less than their average peers in non-distressed 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. 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. In such firms, CEOs receive about 35 percent of their total compensation as capital income, compared to four percent in distressed firms (column (3)) The t-statistics in column (5) make clear that the differences between CEOs in distressed and non-distressed firms are statistically significant for each compensation component.

Some of the CEOs own part of the firm they lead: in distressed firms the ownership rate is 12 percent, compared with six percent for their peers in non-distressed firms. Their ownership share is typically small though, but somewhat smaller in distressed firms (2.2 vs 2.8 percent). Ownership-based control and daily management in the average firm are thus clearly separated.

CEOs of healthy businesses manage more than three times as many employees as their distressed peers. Wage inequality in healthy businesses is also greater than in distressed firms, but only if bonus pay is included. CEOs of non-distressed firms receive 2.8 times the wage of an average employee, compared with 2.3 times in distressed firms. These figures are lower than those in the US ([Lazear et al., 2015](#)).

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 Centre (BRREG). The latter is a Norwegian 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 iden-

tifiers 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. SSB assigned each individual and each firm a unique, anonymized identification number to link its data to the other data sets. We refer to the Appendix for detailed information on each dataset. 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. 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 socio-economic characteristics of CEOs from longitudinal administrative registers provided by SSB. These administrative data sources cover every Norwegian resident from 1967 to 2018 and contain individual demographic information such as gender, age, residential municipality, and education. We supplement this with firms' income and balance sheet data .

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 wasn't met.

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 these data sources, including the assigned judges.

3.2 Sample Construction

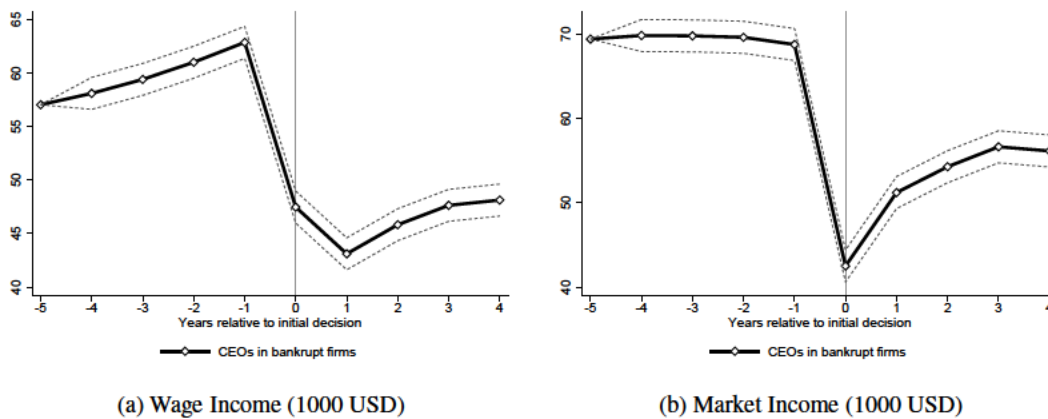
After completing the steps in Section 3.1 and dropping petitions filed after 2013, our sample of CEOs consists of individuals we can follow for at least four years before and after their employer's bankruptcy petition was registered by the district court. From this sample of bankruptcy petitions we exclude 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 exclude CEOs in companies without registered employees in the matched employee-employer register and limit the analysis to CEOs of limited companies (AS). Finally, for a very small number of firms and CEOs that go through more than one bankruptcy filing we use the first observed case.

3.3 Observational Challenges

We plot the evolution of wage income for bankrupt CEOs in Figure 3, showing a 10 percent wage growth over the five years leading up to the petition. Extrapolating the trend linearly four years out predicts that wage

income in absence of the bankruptcy event would equal about \$70,000. This would imply that bankruptcy causes a drop of more than \$20,000 in CEOs' wage income. However, wage income only accounts for a fraction of the total remuneration of the CEO. Including other sources of market income, such as capital and business income, changes the picture. The right panel of Figure 3 shows a flatter trend. In the last year before the petition, we observe a slight decline in market income, and the otherwise flat pattern indicates that other (declining) sources of income are offsetting the rise in wage income. The two trends show that estimating the effects of bankruptcy on earnings and market income is rather difficult using observational data alone.⁹ Using the overall population of CEOs (see Table 1) to filter out the pre-trends does not offer much help. Appendix Figure A.1 documents the trends in wage income among bankrupt CEOs, and the overall population of CEOs and shows they start to diverge long before the bankruptcy petition.

Figure 3: Income Trends for CEOs in Bankrupt Firms



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 individual characteristics such as age, gender, and education. 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.

Finally, we provide descriptive statistics for the CEOs of bankrupt and surviving firms in Appendix Table A.1. Panel A compares the background characteristics of the two groups while Panel B contrasts key, pre-determined, economic outcomes. Column (5) tests the equality of the two groups' means, and shows that CEOs of failing firms are on average less often foreign-born, more likely female and more likely to have completed high school. In the year before the bankruptcy event, CEOs of failing firms have slightly lower wage income, capital income, business income and total market income, i.e., all income components excluding transfers from the government, as well as less personal debt. This indicates that even the group of CEOs in distressed firms that survive may not be a suitable comparison group.

⁹Regressing wage income on a dummy for the period after bankruptcy would lead to a downward bias in the estimated effect of bankruptcy because of the apparent trend in wage income. Accounting for the trend of wage income requires parametric assumptions – potentially overfitting the data biasing the results.

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. Our empirical model exploits this exogenous variation to identify the causal effects of bankruptcy on CEOs. This enables us to avoid the above described risk of CEO selection into good and bad firms faced by observational studies.

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 daily 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).¹⁰

We closely follow the approach used by Bhuller et al. (2020). 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

¹⁰Certain severe crimes or cases involving juveniles are exempt from the randomness principle. These exemptions, however, do not apply to bankruptcy cases

Table 2: Testing for Random Assignment of Cases to Judges

Dependent Variable:	(1)	(2)	(3)	(4)	(5)
	Average	Firm Liquidated coef.	s.e.	Judge stringency 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)
F-statistic		13.8735		1.2002	
[p-value]		0.00		0.23	
Observations		7,586		7,586	

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

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.

conducted among the pool of *available* judges, i.e., 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 for a firm 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 2 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.

Reassuringly, we also clearly 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 certify 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. We find a weakly positive but statistically insignificant association between experience and stringency.

4.2 IV model

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

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

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. 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 causal effect of corporate bankruptcy on CEO careers – i.e., the difference in potential outcomes for a CEO who experiences bankruptcy or does not. The main challenge in estimating this parameter using OLS is that we can only observe a CEO in either a bankrupt or a surviving firm, and that bankruptcy $B_{i,t}$ is likely correlated with unobserved factors that also determine career out-

comes. 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 β_s from OLS regressions.

To deal with this problem, we take 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 causal 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. 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

$$B_{i,t} = \gamma Z_{j(i),t} + \alpha I_{i,t} \delta + e_{ii} \quad (2)$$

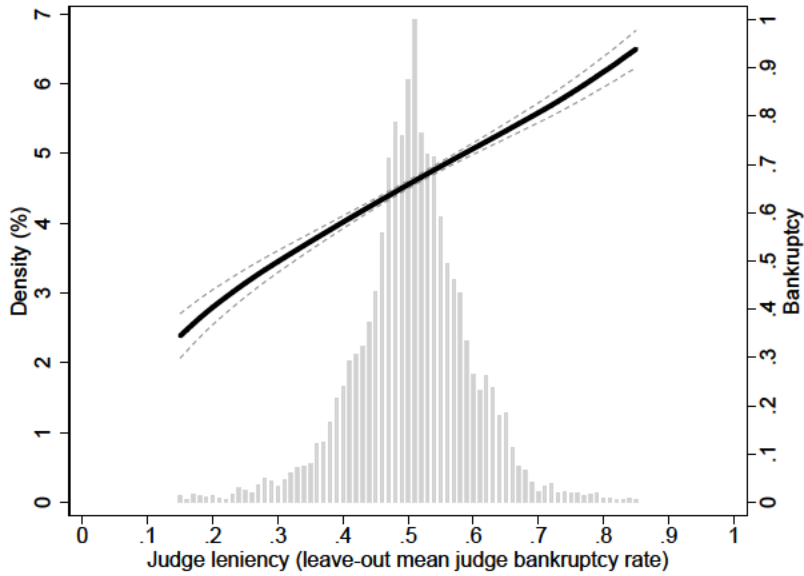
where, $Z_{j(i),t}$ is the stringency measure for judge j to which firm/CEO i ’s was assigned in bankruptcy court at time t . We will also decompose our estimates of β into the potential outcomes of treated and untreated compliers (see [Imbens and Rubin, 1997](#) and [Dahl et al., 2014](#) for continuous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

4.3 Relevance and Validity of the Instrument

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 the 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. Judges in the top 20 percent of the stringency distribution open bankruptcy proceedings in 90 percent or more of their cases compared with 40 percent or less by their colleagues in the bottom 20 percent. 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. The probability of a firm being declared bankrupt by a judge increases monotonically, almost linearly, in our stringency instrument. Table 3 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. 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.

Validity For our judge stringency instrument to be valid, it needs to satisfy three conditions: exogeneity, the exclusion restriction and monotonicity. For *exogeneity*, judge stringency must be uncorrelated with both potential CEO outcomes and pre-determined characteristics that may be relevant for whether bankruptcy proceedings are opened or not. In Table 2 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

Table 3: First Stage Regression

Control variables	Baseline	CEO controls	Firm controls	Judge controls
	(1)	(2)	(3)	(4)
First stage (γ)	0.808*** (0.080)	0.792*** (0.079)	0.800*** (0.079)	0.811*** (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

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

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.

(4) in Table 3 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 causal effects of being assigned to a judge who declares more firms bankrupt.

For the 2SLS estimates to also capture the causal effect of bankruptcy on CEO outcomes, judge stringency must also satisfy the *exclusion* restriction, i.e., strictness in bankruptcy filings is only allowed to influence future firm and CEO outcomes through the initial bankruptcy decision and not directly or through other channels. One concern could be that judges take multidimensional decisions, as in criminal cases. Decisions in corporate bankruptcy filings 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, generating an independent, operating in parallel with bankruptcy, effect on firm outcomes. We do not have data on judges' backgrounds and education to formally test this. Instead, we include the number of cases a judge has handled as an additional control in the next section. Reassuringly, this specification check shows that our estimates do not change appreciably, providing additional support for the exclusion restriction.

Under the assumption of constant treatment effects among marginal firms, no assumption of monotonicity is required to identify LATE. In such a situation, we would identify the population-wide average treatment effect. If the effects vary by unobserved characteristics of the bankruptcy case, 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 the monotonicity assumption. However, the monotonicity assumption has 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 judges who are stricter in one set of bankruptcy cases are also so for other groups of bankruptcy cases.

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 procedures consist of liquidation through a cash auction, we expect to see a termination of the firm following a corporate bankruptcy. Table 4 presents the reduced form estimates of our IV model for firm outcomes. All regressions in Table 4 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 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.12 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 effect shows, not surprisingly that bankruptcy closes all businesses. Two, three and four years after the court decision, the share of non-bankrupt complier firms that close business rise to .50, .58 and .60, which reduces the treatment effect to .39.

Panel B reports our evidence on the reduced form employment effects of bankruptcy. There is a significantly negative persistent effect on firm employment from the case year. 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.12 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, but have zero employees two years after being declared bankrupt.

5 Experimental Results

In this section, we present 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.

5.1.1 Executive Jobs and Regular Employment

Our first outcome 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 flexible estimation of the structural equation (1) using indirect least squares – where we estimate a local linear regression of the probability of continuing in a CEO role on judge stringency and fully interacted court and year dummies. To obtain the LATE estimate, we would 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

Table 4: **Reduced Form Estimates of Judge Stringency Firm Outcomes**

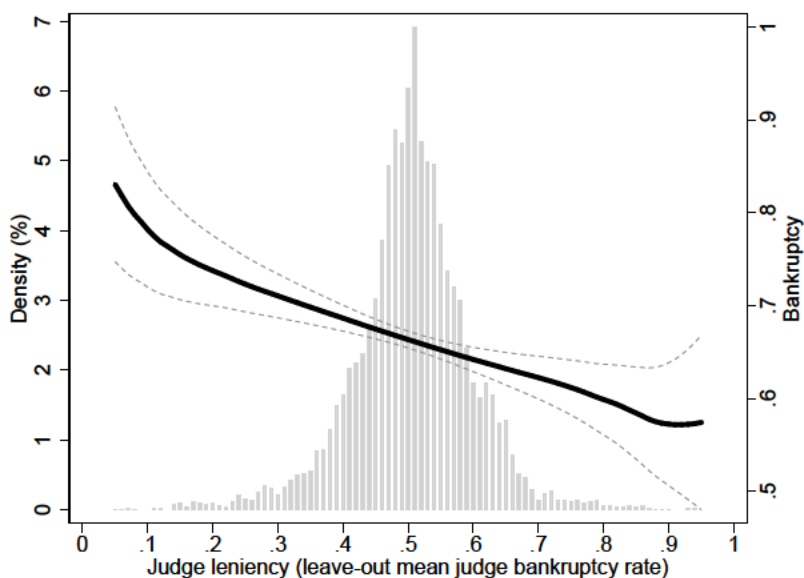
Panel A:	Dependent variable				
	y_{it} : Reporting No Sales				
<i>Years after decision (t):</i>	0	1	2	3	4
Judge stringency	0.048	0.585***	0.407***	0.334***	0.318***
(standard error)	(0.038)	(0.068)	(0.061)	(0.058)	(0.057)
Overall mean:	0.149	0.766	0.841	0.870	0.891
Panel B:	y_{it} : Firm Employment				
<i>Years after decision (t):</i>	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

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

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.

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.

Figure 5: **Effect of Judge stringency on CEO probability**



Notes: This figure displays the effect of judge stringency on bankruptcy, conditional on fully interacted year and district court dummies. 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. The lowest and top five percentiles are excluded.

Turning to the second stage equation, Panel A of Table 5 reports our 2SLS estimates of the impact of *bankruptcy* on holding a CEO position. We find that bankruptcy increases the likelihood of exiting the

executive labor market by 20 to 30 percentage points. The majority of CEOs do transition into new CEO positions, irrespective of whether their employers are declared bankrupt or not. Among marginal firms, between 94 and 83 percent of all CEOs occupy a CEO position one to four years after the bankruptcy filing. By contrast, [Eckbo et al. \(2016\)](#) document that 79 percent of CEOs of large firms shift into non-executive positions following Chapter 11 bankruptcy.

Bankruptcy thus has an important effect on CEO careers. To further explore whether displacement from the CEO position also affects the odds of regular employment, we construct an employment variable that equals one if a CEO is registered with an employment contract in the employer-employee register, and zero otherwise. Panel B of [Table 5](#) shows that bankruptcy lowers the likelihood of a CEO being employed, but only temporarily. The coefficient estimate is significant only in the year the petition is received and quickly levels off. The initial effect in the year of the bankruptcy petition likely results from uncertainty about whether the firm will be sold as a going concern and whether potential new owners would want the CEO to continue. It may also reflect that CEOs direct their search towards similar positions, that search is time-consuming, and that CEOs have the resources to search for a good match. Four years after the bankruptcy petition, however, the employment rate for CEOs who experience bankruptcy is almost equal to that of their peers. 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.¹¹

Lastly, we examine whether displacement from the top executive job implies falling off the corporate job ladder. 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 precede an outflow to lower-ranked jobs. In [Appendix Table A.3](#), we show that changes in the likelihood of being registered in a CEO occupation in [Table 5](#) essentially mirrors the changes 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 a significant minority of bankrupt CEOs are displaced from the job ladder and must climb back from non-management positions in new firms. Examining potential outcomes among managers of untreated marginal firms, we see that some CEOs in continuing firms end up in a non-management position too, but at a much lower rate of 7 to 16 percent. Hence, treated CEOs are more than twice as likely to take up non-management jobs. We also consider whether the relative decline in the probability of remaining in the executive workforce reflects a shift towards new business start-ups. For this purpose, we measure if a CEO is subsequently registered as the founder of a new business. [Appendix Table A.3](#) shows that bankrupted CEOs do not substitute more into (complementary) entrepreneurship than their non-bankrupt peers. The probability of being registered as the founder of a business is marginally smaller for the bankrupt CEOs than for the control group.

¹¹The 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 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.2](#) reports these alternative results.

Table 5: 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** (0.053)	-0.268*** (0.061)	-0.204*** (0.068)	-0.295*** (0.066)	-0.295*** (0.067)	-0.241*** (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 (standard error)	-0.127** (0.059)	-0.076 (0.064)	-0.082 (0.063)	0.005 (0.062)	-0.006 (0.066)	-0.057 (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

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

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.

5.1.2 Remuneration

Corporate bankruptcy can lead to potentially opposing effects 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). While our evidence on occupational displacement indicates this may be an important channel, the strength of the scarring effect will depend on the perceived noisiness of the signal. We will return to this aspect in the next section. On top of any informational asymmetries, a CEO's human capital could be tied to the firm or declining industry. Outside employers would value such skills less and therefore offer a lower wage (Becker, 1962). These mechanisms predict that CEOs who have gone through bankruptcy are more likely 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. While the effect of these mechanisms on remuneration will depend on how CEOs value the amenities, the different forces at play make the net effect an empirical question.

We examine the net effect of these mechanisms in Table 6. 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

Table 6: 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** (5.147)	-10.756* (5.638)	-4.598 (5.983)	-3.167 (5.881)	-6.913 (6.257)	-7.456 (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 (1.337)	-1.990 (1.215)	-3.256** (1.586)	-0.035 (1.491)	-3.256** (1.410)	-2.145** (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 (standard error)	-17.693*** (6.837)	-10.581 (6.538)	-11.204 (7.779)	-5.736 (7.467)	-13.798* (8.118)	-11.803* (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

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

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, 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.

statistically insignificant. Because executives typically receive other compensation on top of regular wages, we examine the role of high-powered incentives, such as equity holdings, by examining the effects of bankruptcy on stock income, i.e., dividend. Panel B presents the effect on stock income and shows that while the effects are initially noisy, the average effect on stock income over years 0 to 4 is about minus USD 2,100 among bankrupt CEOs.

Finally, we consider total market income, i.e., the sum of wage income, capital, stock gains, dividends and other business income. Panel C makes clear that once other variable compensation components are included, treated CEOs experience a greater income loss than their non-bankrupt peers. 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 effects on market income in the years after the bankruptcy case. In Appendix Table A.4, we 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. This shows that stock income explains the difference between the evolution of CEO wage income and total income. Panel C in Table 6 displays the negative effect on stock income: CEOs who experience bankruptcy on average have no stock income four years after the bankruptcy filing. This indicates that small business CEOs also rely on income components with high-powered incentives and that displacement following a bankruptcy is associated with the loss of this variable income component.

To facilitate a comparison of the career costs we find with 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 equal 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 the firm is bankruptcy petitioned. 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.¹² By contrast, Eckbo et al. (2016) find a 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 PV losses are comparable in relative terms despite the CEOs in our sample being on average 12 years younger and employed at small and medium-sized firms.

5.1.3 Robustness Checks

We perform a range of specification checks in Appendix Tables R.1 and R.2. First, we demonstrate that the main findings remain unchanged when we include CEO characteristics as controls.¹³ We then gauge the exclusion restriction in Panel C of R.1. Reassuringly, including judge experience as a proxy for procedural quality does not significantly change our results. We next allow for the possibility that the effects among managers of marginal firms differ but are weighted uniformly by the linear first and second stage. We examine this possibility by discretizing the instrument by each judge stringency quintile as a separate instrument. In Panel D of R.1, we show that the estimates of the probability of being CEO remain broadly unchanged, suggesting that effect heterogeneity is not strong in our sample.¹⁴ Finally, we 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. The stability of the treatment effects to variations in the instrument and to adding controls is reassuring.

5.1.4 Heterogeneity by CEO Age

The evidence we present complements the findings by Eckbo et al. (2016). The firms in our data are predominantly small and medium-sized enterprises (SME) with CEOs who are likely to be notably different from the managers of large listed corporations. The CEOs in our data are on average 43 years old, almost 10 years younger than those examined by Eckbo et al. (2016). In the early stages of their careers, people are likely to be less attached to the firm and more likely to switch to a new firm. By contrast, older CEOs are more likely to have accumulated human capital specific to the firm or have negotiated high wages due to loyal service to

¹²To convert the estimated remuneration changes in Table 6 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 .

¹³While 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.2 shows that the difference in pre-trends between bankrupt and non-bankrupt compliers is flat.

¹⁴The 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.

the firm. 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.

We examine the role the CEOs' age plays for the career effects of bankruptcy in Appendix Table A.5. We split our sample of CEOs at age 45, slightly above the mean of 43 years, and run separate second-stage regressions for each sub-sample. On average, we find that younger CEOs experience no loss of stock income, whereas older CEOs experience a loss of about USD 4,600. The results for wages 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 above age 44. Overall, these results suggest that the negative financial effects of bankruptcy stigma on CEOs are primarily borne by older CEOs.

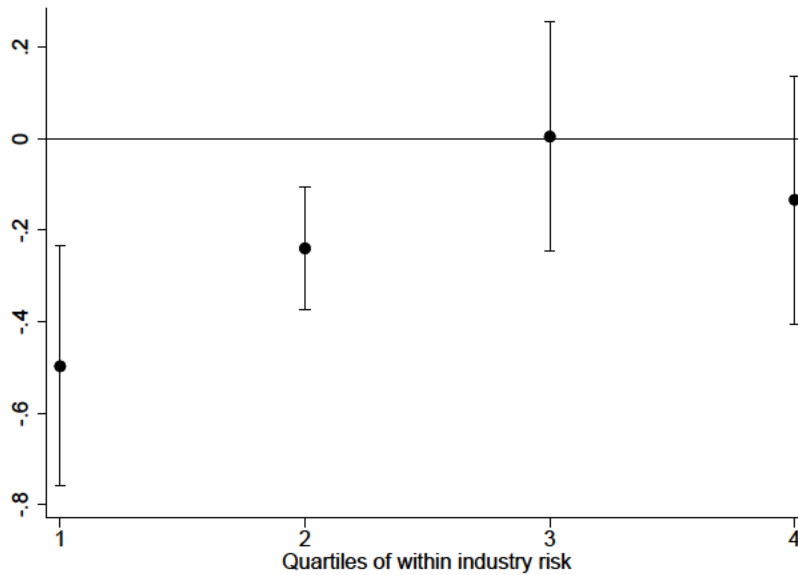
5.2 Informational Stigma and Industry Conditions

We next turn to the question of how the relationship between bankruptcy and career effects varies with industry conditions. We measure industry conditions as within-industry average bankruptcy rates, calculated by subtracting an industry's sample average bankruptcy rate to facilitate a comparison of the variation in industry conditions between industries with permanently different bankruptcy rates, e.g., construction vs. health services. We then split up our sample into four quartiles based on firms' de-measured bankruptcy risk. We plot the estimates of the average effects on the probability of being CEO for the four quartiles in Figure 6 with 95 percent confidence intervals, showing that the negative effect decays when bankruptcy rates in their industry go up. The pattern is consistent with the notion that the signal from bankruptcy is weaker when many firms face high default risk simultaneously.

Table 7 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 bootstrapping the standard error on their differences.¹⁵ Appendix Table A.11 confirms the significant differences in the probability of being a CEO by instead splitting the sample of CEOs at the median bankruptcy rate. By contrast, Appendix Table 7 shows that we do not detect significant differences in the effect of bankruptcy on wages for the four subsamples. Taken together, these findings indicate that the informational stigma is stronger when industries are performing relatively well, meaning that the stigma has little or no effect on CEOs' corporate careers when the aggregate conditions are poor. An implication of this result is that CEOs under certain conditions may have a personal interest in delaying default – a type of behavior that could, for example, reinforce the effects of boom-bust cycles on defaults and credit supply.

¹⁵Using 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.7).

Figure 6: 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. 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, age dummies, education level, gender. CEO probability is measured as the average probability from the case year to four years after.

Table 7: Effect of Bankruptcy on CEO Probability Industry Conditions

Years after decision (t): Average from 0-4				
Industry condition/Bankruptcy risk:	y_{it} : Being a CEO			
	1st Quartile (1)	2nd Quartile (2)	3rd Quartile (3)	4th Quartile (4)
Bankruptcy (β)	-0.500*** (0.144)	-0.233*** (0.074)	0.005 (0.137)	-0.141 (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

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

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. The i Industry is 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.

5.3 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. Moreover, task-specific human capital accounts for up to 52 percent of wage growth. Relatedly, [Nakamura et al. \(2021\)](#) find evidence that the gains of involuntary moving may be very large because of location-dependent mismatching. Their evidence suggests that careers may be tied to a location, either due to the concentration of industry or due to locational preferences.

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 sector after the filing. This increase in professionally more distant job switches supports the notion of [Gathmann and Schonberg \(2010\)](#) that CEO skills can be employed across sectors. Our second inquiry concerns the extent to which CEOs are tied to geographic locations. In Panel B of Table 8, we analyze the tendency of CEOs to move to a new commuting zone after going bankrupt due to a stringent judge. 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 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*** (0.060)	0.265*** (0.075)	0.238*** (0.079)	0.278*** (0.067)	0.260*** (0.064)	0.190*** (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 (0.043)	0.140** (0.057)	0.104* (0.054)	0.075 (0.055)	0.028 (0.062)	0.089** (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

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

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. The industry is 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.

To understand the demand for CEOs with bankruptcy experience, we take a closer look at the characteristics of new employers. For this purpose we concentrate on displaced CEOs who take up a new CEO job or a non-management position. Our inquiry starts with an examination of new firms' productivity, measured as the value-added per worker, i.e., firm revenue minus intermediaries. Panel A of Table 9 demonstrates that CEOs 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 a firm's overall attractiveness, reflecting wages and other amenities they offer, such as flexibility, office space, 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 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.¹⁶ Appendix Table A.9 and A.10 shows that new employers of CEOs' have higher value added, higher earnings, lower leverage and a better liquidity position than the employers of non-bankrupt peers.

Table 9: Productivity of CEOs' New Employer

Years after decision (t):	0	1	2	3	4	Average
Panel A:						
	y_{it} : Output per Worker (1000 USD)					
Bankruptcy (β)	-10.372 (9.639)	24.539*** (8.706)	17.339* (9.129)	11.025 (10.560)	22.479** (10.827)	13.002* (7.325)
Overall mean:	19.499	27.983	35.355	39.748	42.369	32.991
Untreated marginal firm:	22.506	20.483	32.615	31.173	28.985	27.150
Panel B:						
	y_{it} : Poaching Index					
Bankruptcy (β)	-0.045 (0.051)	0.110* (0.060)	0.069 (0.057)	0.081 (0.054)	0.096* (0.056)	0.027 (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:	7,586	7,586	7,586	7,586	7,586	7,586

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

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 intermediaries, 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 workers that are employed in a new firm 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.

An alternative way to benchmark the relative position of the new employers in terms of the average wages they offer and their size can be obtained by sorting firms along these dimensions relative to the full population

¹⁶The 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.

of firms. These rankings can act as a proxy for firm quality and productivity (e.g., [Herkenhoff et al., 2016](#)). Consistent with the evidence on productivity, Table 10, panel A 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 size distribution than the employers of CEOs of non-bankrupt, marginal firms. To assess the net effect of occupational displacement, industry changes, and moves to larger, more productive, and desirable employers on relative wages, we next inspect the relative income position of CEOs. This measure ranks a bankrupt CEO relative to employees in the new firm. Our 2SLS estimates show that bankrupt CEOs move to new jobs located approximately 10-15 percent lower in the intra-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, when CEOs move to higher-paying firms, they partly offset some of this downward pressure on their total remuneration.

Table 10: The Relative Wage and Size at New Employer Firms 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 (0.042)	0.147*** (0.055)	0.096** (0.045)	0.072* (0.042)	0.069* (0.040)	0.053 (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 (0.047)	0.247*** (0.051)	0.136** (0.055)	0.129*** (0.048)	0.137** (0.054)	0.098** (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 (standard error)	-0.166*** (0.061)	-0.158** (0.070)	-0.138** (0.062)	-0.174*** (0.055)	-0.089 (0.061)	-0.132*** (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

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

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. 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.

6 Discussion

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 (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. This suggests the potential for economically significant negative effects of corporate bankruptcy through CEO behavior is substantial.

Our analysis attempts to fill a gap in the literature that studies CEOs' careers and compensation by identifying the causal effects of corporate bankruptcy on the income and career of Norwegian CEOs. To this end, we exploit that the Norwegian judicial system's requirement that bankruptcy petitions are randomly assigned to judges who have different degrees of strictness in their approval of bankruptcy filings. This generates exogenous variation in firms' likelihood of being declared bankrupt that we exploit to study how corporate bankruptcy affects CEO's income and career. We benefit from a rich register data set with all Norwegian CEOs that enables us to track their careers and remuneration.

Our paper offers, to the best of our knowledge, the first experimental evidence on the consequences of straight bankruptcy (c.f. US Chapter 7) on CEOs' careers. We establish three new empirical facts about corporate bankruptcy and document the evolution of CEO careers. First, although a substantial share of bankrupt CEOs are displaced from the top executive position, we find that CEOs who experience a 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 the displacement effects of bankruptcy are significantly stronger when default rates in an industry are low. When sorting CEOs 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.

Third, when we investigate the heterogeneity in the age of CEOs, we find evidence that the negative effects on income are 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 bankruptcy. We argue that this is likely driven by the accumulation of firm- or industry-specific human capital over the course of a CEO's career. Evidence of a substantial increase in the mobility of CEOs across industries following the bankruptcy and of CEOs moving to more productive firms with a higher-paid workforce lend support to the notion that CEO skills are highly portable, in particular for younger managers.

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, which have substantially higher bankruptcy rates than large listed corporations. This may weaken the negative signal that potentially arises after bankruptcy and the adverse effects it has on CEO remuneration. 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.

Our paper has some implications for the design of debt contracts and policy. In an environment 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 or avoid default. On the one hand, by rolling over debt, a CEO may keep his firm alive to avoid the personal costs from bankruptcy stigma. Suppose CEOs act on the signal strength strategically, for example by delaying a bankruptcy filing to the time when personal bankruptcy costs are lowest. In that case, the informational mechanism we identify implies that creditors would take larger losses because the market for liquidated assets is thinner at the time of bankruptcy. The existence of such strategic behavior could hint at a role for loan covenants to facilitate early liquidation. On the other hand, our evidence also points to a potential mechanism in which CEOs can use their discretion over the firm to start less risky projects to avoid the risk of personal costs in the event of default. While examining such behavior empirically is outside the scope of this paper, we believe it would be an interesting avenue for future research.

References

- Aghion, P. and Howitt, P. (1992). A model of growth through creative destruction. *Econometrica*, 60:323–351.
- Aizer, A. and Doyle Jr, J. J. (2015). Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. *The Quarterly Journal of Economics*, 130(2):759–803.
- Almeida, H., Campello, M., Cunha, I., and Weisbach, M. S. (2014). Corporate liquidity management: A conceptual framework and survey. *Annu. Rev. Financ. Econ.*, 6(1):135–162.
- Antill, S. (2020). Do the right firms survive bankruptcy? Available at SSRN 3383227.
- Araujo, A., Ferreira, R., Lagaras, S., Moraes, F., Ponticelli, J., and Tsoutsoura, M. (2021). The labor effects of pro-labor bias in bankruptcy. Technical report, National Bureau of Economic Research.
- Bagger, J. and Lentz, R. (2019). An empirical model of wage dispersion with sorting. *The Review of Economic Studies*, 86(1):153–190.
- Bandiera, O., Lemos, R., Prat, A., and Sadun, R. (2017). Managing the Family Firm: Evidence from CEOs at Work. *The Review of Financial Studies*, 31(5):1605–1653.
- Barth, E., Moene, K. O., and Willumsen, F. (2014). The scandinavian model—an interpretation. *Journal of Public Economics*, 117:60–72.
- Becker, G. S. (1962). Investment in human capital: A theoretical analysis. *The journal of political economy*, pages 9–49.
- Bernile, G., Bhagwat, V., and Rau, P. R. (2017). What doesn't kill you will only make you more risk-loving: Early-life disasters and ceo behavior. *The Journal of Finance*, 72(1):167–206.
- Bernstein, S., Colonnelli, E., Giroud, X., and Iverson, B. (2019a). Bankruptcy spillovers. *Journal of Financial Economics*, 133(3):608–633.
- Bernstein, S., Colonnelli, E., and Iverson, B. (2019b). Asset allocation in bankruptcy. *The Journal of Finance*, 74(1):5–53.
- Bertrand, M. (2009). Ceos. *Annu. Rev. Econ.*, 1(1):121–150.
- Bertrand, M. and Schoar, A. (2003). Managing with style: The effect of managers on firm policies. *The Quarterly Journal of Economics*, 118(4):1169–1208.
- Bhuller, M., Dahl, G. B., Løken, K. V., and Mogstad, M. (2020). Incarceration, recidivism, and employment. *Journal of Political Economy*, 128(4):1269–1324.
- Blanchard, O. J. and Katz, L. F. (1992). Regional evolutions. *Brookings papers on economic activity*, 1992(1):1–75.

- Bohn, A. (2000). *Domstolsloven, Kommentarutgave [Law of Courts, Annotated Edition]*, volume In Norwegian. Oslo: Universitetsopplaget.
- Bonfim, D. and Nogueira, G. (2021). Corporate Reorganization as Labor Insurance in Bankruptcy. *NYU Stern School of Business Working Paper*.
- Bris, A., Welch, I., and Zhu, N. (2006). The costs of bankruptcy: Chapter 7 liquidation versus chapter 11 reorganization. *The journal of finance*, 61(3):1253–1303.
- Chang, T. and Schoar, A. (2013). Judge specific differences in chapter 11 and firm outcomes. *Unpublished working paper, National Bureau of Economic Research Cambridge*.
- Custodio, C., Ferreira, M. A., and Matos, P. (2013). Generalists versus specialists: Lifetime work experience and chief executive officer pay. *Journal of Financial Economics*, 108(2):471–492.
- Custodio, C. and Metzger, D. (2013). How do ceos matter? the effect of industry expertise on acquisition returns. *The Review of Financial Studies*, 26(8):2008–2047.
- Custodio, C. and Metzger, D. (2014). Financial expert ceos: Ceos work experience and firms s financial policies. *Journal of Financial Economics*, 114(1):125–154.
- Dahl, G., Kostøl, A., and Mogstad, M. (2014). Family welfare cultures. *The Quarterly Journal of Economics*, 129(4):1711–1752.
- Di Tella, R. and Schargrodsy, E. (2013). Criminal recidivism after prison and electronic monitoring. *Journal of political Economy*, 121(1):28–73.
- Dobbie, W., Goldin, J., and Yang, C. S. (2018). The effects of pretrial detention on conviction, future crime, and employment: Evidence from randomly assigned judges. *American Economic Review*, 108(2):201–40.
- Eckbo, B. E. and Thorburn, K. S. (2003). Control benefits and ceo discipline in automatic bankruptcy auctions. *Journal of Financial Economics*, 69(1):227–258.
- Eckbo, B. E., Thorburn, K. S., and Wang, W. (2016). How costly is corporate bankruptcy for the ceo? *Journal of Financial Economics*, 121(1):210–229.
- Edmans, A., Gosling, T., and Jenter, D. (2021). Ceo compensation: Evidence from the field. Technical report, Working Paper. London Business School.
- Fama, E. F. and Jensen, M. C. (1983). Separation of ownership and control. *The journal of law and Economics*, 26(2):301–325.
- Gathmann, C. and Schonberg, U. (2010). How general is human capital? a task-based approach. *Journal of Labor Economics*, 28(1).
- Gennaioli, N., Porta, R. L., de Silanes, F. L., and Shleifer, A. (2013). Human Capital and Regional Development. *The Quarterly Journal of Economics*, 128(1):105–164.

- Gibbons, R. and Katz, L. F. (1991). Layoffs and lemons. *Journal of labor Economics*, 9(4):351–380.
- Graham, J. R., Kim, H., Li, S., and Qiu, J. (2019). Employee costs of corporate bankruptcy. Technical report, National Bureau of Economic Research.
- Herkenhoff, K., Phillips, G., and Cohen-Cole, E. (2016). How credit constraints impact job finding rates, sorting & aggregate output. Technical report, National Bureau of Economic Research.
- Hurst, E. and Pugsley, B. W. (2011). What do small businesses do? *Brookings Papers on Economic Activity*, pages 73–143.
- Imbens, G. W. and Rubin, D. B. (1997). Estimating outcome distributions for compliers in instrumental variables models. *The Review of Economic Studies*, 64(4):555–574.
- Jensen, M. C. and Meckling, W. H. (1976). Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of financial economics*, 3(4):305–360.
- Kling, J. R. (2006). Incarceration length, employment, and earnings. *The American Economic Review*, 96(3):863–876.
- Konkursradet (2012). Insolvency in norway. Retrieved on 18 May 2021 at.
- Kostol, A. and Mogstad, M. (2014). How financial incentives induce disability insurance recipients to return to work. *American Economic Review*, 104(2):624–55.
- Kroft, K., Lange, F., and Notowidigdo, M. J. (2013). Duration dependence and labor market conditions: Evidence from a field experiment. *The Quarterly Journal of Economics*, 128(3):1123–1167.
- Lazear, E. P. and Rosen, S. (1981). Rank-order tournaments as optimum labor contracts. *Journal of Political Economy*, 89(5):841–864.
- Lazear, E. P., Shaw, K. L., and Stanton, C. T. (2015). The value of bosses. *Journal of Labor Economics*, 33(4):823–861.
- Malmendier, U. and Tate, G. (2015). Behavioral ceos: The role of managerial overconfidence. *Journal of Economic Perspectives*, 29(4):37–60.
- Ministry of Justice and Public Security (1984). Bankruptcy act.
- Ministry of Labour and Social Affairs (2004). Lov om arbeidsmarkedstjenester.
- Ministry of Labour and Social Affairs (2005). Working environment act.
- Mortensen, D. T. and Pissarides, C. A. (1999). New developments in models of search in the labor market. *Handbook of labor economics*, 3:2567–2627.
- Mueller-Smith, M. (2015). The criminal and labor market impacts of incarceration. *Unpublished Working Paper*, 18.

- Nakamura, E., Sigurdsson, J., and Steinsson, J. (2021). The Gift of Moving: Intergenerational Consequences of a Mobility Shock. *Review of Economic Studies*.
- Neal, D. (1999). The complexity of job mobility among young men. *Journal of Labor Economics*, 17(2):237–261.
- OECD (2020). Oecd trade union data. Retrieved on 31 August 2020 at.
- Oi, W. Y. (1962). Labor as a quasi-fixed factor. *Journal of political economy*, 70(6):538–555.
- PwC Strategy& (2018). Ceo success study 2018.
- Ridder, G. and Van den Berg, G. J. (2003). Measuring labor market frictions: a cross-country comparison. *Journal of the European Economic Association*, 1(1):224–244.
- Shleifer, A. and Vishny, R. W. (1992). Liquidation values and debt capacity: A market equilibrium approach. *The journal of finance*, 47(4):1343–1366.
- Thesmar, D. and Thoenig, M. (2000). Creative destruction and firm organization choice. *The quarterly journal of economics*, 115(4):1201–1237.
- White, M. J. (1989). The corporate bankruptcy decision. *Journal of Economic Perspectives*, 3(2):129–151.

A Appendix: Additional Tables

A.1 Robustness

Table R.1: Robustness of probability of being CEO

Years after decision (t):	0	1	2	3	4	Average
Panel A.						
	Baseline Estimate					
Bankruptcy (β)	-0.134** (0.053)	-0.268*** (0.061)	-0.204*** (0.068)	-0.295*** (0.066)	-0.295*** (0.067)	-0.266*** (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** (0.052)	-0.243*** (0.061)	-0.175*** (0.068)	-0.269*** (0.067)	-0.274*** (0.068)	-0.243*** (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*** (0.050)	-0.251*** (0.059)	-0.178*** (0.067)	-0.275*** (0.067)	-0.277*** (0.068)	-0.248*** (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*** (0.056)	-0.270*** (0.067)	-0.173** (0.072)	-0.268*** (0.073)	-0.293*** (0.078)	-0.252*** (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** (0.056)	-0.239*** (0.063)	-0.174** (0.071)	-0.240*** (0.070)	-0.241*** (0.073)	-0.227*** (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

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

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. 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.

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** (5.147)	-10.756* (5.638)	-4.598 (5.983)	-3.167 (5.881)	-6.913 (6.257)	-4.069 (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* (4.983)	-8.302 (5.458)	-2.066 (5.677)	-0.684 (5.544)	-5.250 (5.789)	-4.069 (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** (4.991)	-9.981* (5.300)	-3.076 (5.612)	-1.694 (5.601)	-6.102 (5.807)	-5.206 (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 (5.338)	-3.608 (6.539)	5.372 (6.131)	4.864 (6.349)	-2.163 (6.143)	1.138 (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 (4.977)	-7.232 (5.560)	-1.623 (5.915)	0.453 (5.682)	-3.929 (5.913)	-3.066 (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

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

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. 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.

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

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

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

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).

A.2 Additional results

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

	Non-Bankrupt Firms		Bankrupt Firms		Test of
	Mean	Std. Dev.	Mean	Std. Dev.	equal means 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		

Standard deviations [in square brackets]

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.

Table A.2: 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** (0.059)	-0.076 (0.064)	-0.082 (0.063)	0.005 (0.062)	-0.006 (0.066)	-0.057 (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 (0.042)	-0.127** (0.058)	-0.101* (0.059)	-0.045 (0.060)	-0.016 (0.060)	-0.065 (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

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

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.

Table A.3: Changes in Management Positions Caused by Bankruptcy

Years after decision (t):	0	1	2	3	4	Average
Panel A.						
	y_{it} : Probability of being an entrepreneur					
Bankruptcy (β)	-0.034 (0.070)	-0.066 (0.069)	-0.102 (0.074)	-0.134* (0.073)	-0.120* (0.073)	-0.081 (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
Panel B.						
	y_{it} : Prob (No management)					
Bankruptcy (β)	0.147*** (0.048)	0.270*** (0.059)	0.216*** (0.065)	0.293*** (0.060)	0.316*** (0.062)	0.249*** (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 C.						
	y_{it} : Prob (Lower management)					
Bankruptcy (β)	0.001 (0.028)	0.015 (0.027)	0.010 (0.030)	0.032 (0.036)	0.005 (0.035)	0.013 (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 D.						
	y_{it} : Prob (Top management)					
Bankruptcy (β)	-0.192*** (0.056)	-0.296*** (0.062)	-0.229*** (0.066)	-0.311*** (0.064)	-0.322*** (0.066)	-0.270*** (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
Number of observations:						

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

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.

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** (5.147)	-10.756* (5.638)	-4.598 (5.983)	-3.167 (5.881)	-6.913 (6.257)	-7.456 (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*** (3.350)	-1.836 (2.715)	-6.172* (3.325)	-0.399 (3.236)	-4.398 (3.016)	-4.837** (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 (1.337)	-1.990 (1.215)	-3.256** (1.586)	-0.035 (1.491)	-3.256** (1.410)	-2.145** (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 (2.426)	1.583 (2.863)	-1.033 (2.761)	-3.780 (2.700)	-2.339 (2.843)	-0.741 (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*** (6.837)	-10.581 (6.538)	-11.204 (7.779)	-5.736 (7.467)	-13.798* (8.118)	-11.803* (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

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

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.

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	
	Below 45	Above 44	Below 45	Above 44	Below 45	Above 44
Sample	(1)	(2)	(3)	(4)	(5)	(6)
Bankruptcy (β)	-3.089 (7.369)	-11.547 (7.492)	-0.388 (0.974)	-4.602** (1.952)	-0.189*** (0.073)	-0.308*** (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

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

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.

Table A.6: Effect of Bankruptcy on Wage Income by Bankruptcy Risk

Years after decision (t): Average from 0-4				
Industry condition/Bankruptcy risk:	y_{it} : Wage Income (1000 USD)			
	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
	(1)	(2)	(3)	(4)
Bankruptcy (β)	-7.551 (9.916)	-8.312 (8.218)	-5.348 (12.697)	-10.477 (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

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

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. The industry is classified by its two-digit NACE code and commuting zones are measured according to firm location using 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. 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.

Table A.7: Probability of Being 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*** (0.080)	0.502*** (0.094)	0.364*** (0.013)
Number of replications	200	200	200
Number of observations:	1,815	1,893	1,773

***p<.01, **p<.05, *p<.10. Standard errors (in parentheses) are bootstrapped using 200 repetitions.

Note: This table reports the difference in the effect of bankruptcy on the probability of 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 and commuting zones are measured according to firm location using 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.

Table A.8: Effects of Bankruptcy on CEO Probability by Local Unemployment

Years after decision (t):	0	1	2	3	4	Average
Panel A.						
	y_{it} : CEO Probability (below median unemployment)					
Bankruptcy (β)	-0.216*** (0.068)	-0.294*** (0.078)	-0.264*** (0.078)	-0.289*** (0.079)	-0.309*** (0.076)	-0.276*** (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} : CEO Probability (above or equal median unemployment)					
Bankruptcy (β)	-0.010 (0.089)	-0.234** (0.109)	-0.095 (0.125)	-0.293*** (0.113)	-0.273** (0.121)	-0.181** (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

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

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. We can only observe the industry and commuting zone of CEOs that are registered as employed. The estimation sample is limited to this sample.

Table A.9: Value Added CEOs' New Employer

Years after decision (t):	0	1	2	3	4	Average
Panel A.						
	y_{it} : Value Added (1000 USD)					
Bankruptcy (β)	-0.904 (19.130)	42.396 (34.638)	92.555** (39.555)	106.771*** (41.003)	80.611** (41.020)	64.286** (26.824)
Overall mean:	19.041	47.759	61.002	67.850	73.077	53.746
Untreated marginal firm:	5.011	6.564	26.661	24.993	46.150	21.868
Number of observations:	7,586	7,586	7,586	7,586	7,586	7,586

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

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. 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.

Table A.10: Earnings, Leverage and Liquidity of CEOs' New Employers

Years after decision (t):	0	1	2	3	4	Average
Panel A.						
	y_{it} : Log EBIT					
Bankruptcy (β)	2.766*** (1.025)	2.229*** (0.831)	2.066*** (0.716)	2.236*** (0.738)	0.961 (0.725)	1.224*** (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 (0.589)	-0.854* (0.516)	-0.474 (0.367)	-0.740* (0.385)	-0.017 (0.304)	-0.409* (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 (0.445)	0.693** (0.273)	0.329* (0.192)	0.346* (0.189)	0.140 (0.195)	0.410** (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:	7,586	7,586	7,586	7,586	7,586	7,586

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

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. 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.

Table A.11: Effect of Bankruptcy on CEO Probability, by Bankruptcy risk

Years after decision (<i>t</i>):	0	1	2	3	4	Average
Panel A.						
	<i>y_{it}</i> : CEO Probability (below median risk)					
Bankruptcy (β)	-0.248*** (0.073)	-0.414*** (0.083)	-0.326*** (0.080)	-0.409*** (0.086)	-0.359*** (0.082)	-0.353*** (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.						
	<i>y_{it}</i> : CEO Probability (above or equal median risk)					
Bankruptcy (β)	0.025 (0.081)	-0.061 (0.111)	-0.058 (0.125)	-0.169 (0.122)	-0.189 (0.119)	-0.100 (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

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

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. We can only observe the industry and commuting zone of CEOs that are registered as employed. The estimation sample is limited to this sample.

Table A.12: Effects of Bankruptcy on Firm Outcomes

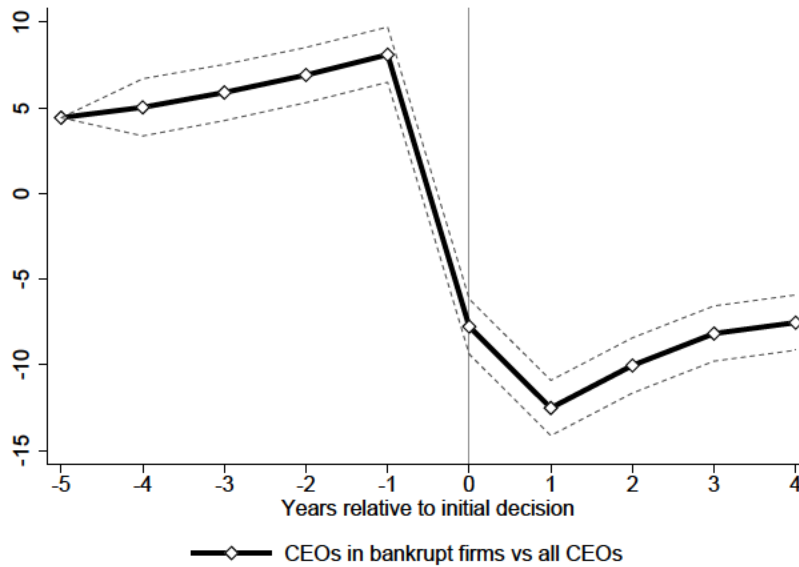
		Dependent variable				
		<i>y_{it}</i> : Reporting No Sales				
Years after decision (<i>t</i>):		0	1	2	3	4
Bankruptcy (β)		0.059 (0.046)	0.724*** (0.038)	0.503*** (0.046)	0.413*** (0.047)	0.394*** (0.048)
Overall mean:		0.149	0.766	0.841	0.870	0.891
Untreated marginal firm:		0.048	0.256	0.497	0.584	0.603
		<i>y_{it}</i> : Firm Employment				
Years after decision (<i>t</i>):		0	1	2	3	4
Bankruptcy (β)		-3.129** (1.347)	-3.749*** (0.915)	-2.962*** (0.892)	-2.814*** (1.090)	-2.916*** (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

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

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.

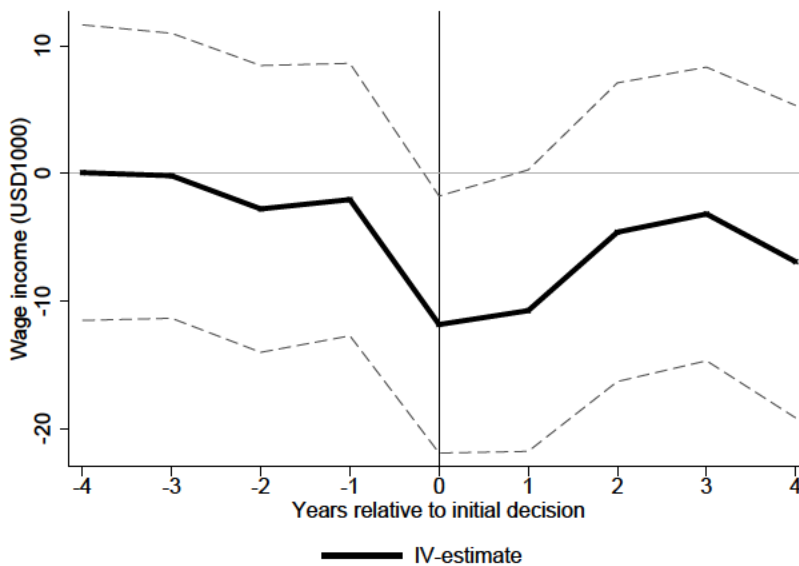
B Appendix: Additional Figures

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



Notes: This figure displays the difference in wage income between CEOs in bankrupt firms relative to all registered CEO controlling for age fixed effects, case year fixed effects, gender and education. Wage income is winsorized at the 99th percentile. The sample of CEOs in bankrupt firms includes all identified CEOs of bankrupt 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.2: 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, 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 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 Appendix: Data Sources

C.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.

C.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 balance sheet information the year prior to the bankruptcy.

Individual CEO characteristics

We use information from the Norwegian Tax Authority on manager remuneration (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.