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Criminals, bankruptcy, and cost of debt

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Abstract

We examine whether criminal records of CEOs and rank-and-file employees are associated with firms' likelihood of bankruptcy, and whether lenders adjust their required cost of debt accordingly. We use a nationwide sample of private firms and criminal registers covering all firm employees. We find that the likelihood of bankruptcy is positively associated with the CEO's criminal record and the proportion of employees with criminal records. We find some, though less robust, evidence that lenders price a firm's loan higher when the firm's CEO has a criminal record and when more of the employees have criminal records. The results suggest that the characteristics of firm employees represent a risk that, to some extent, is priced by lenders.

Keywords Bankruptcy prediction · Criminal records · Human capital · Cost of debt

JEL code $~G32 \cdot G33 \cdot G41 \cdot M12 \cdot M41 \cdot M54$

1 Introduction

Do characteristics of rank-and-file employees provide information about a firm's risk? If so, are these characteristics associated with the cost of debt? Mounting evidence shows that lenders assess the management of borrowing firms and that this assessment influences lending decisions (Grunert et al. 2005; De Franco et al. 2017; Donelson et al. 2017; Bui et al. 2018). This attention devoted by lenders to top managers makes sense, given the extensive research that examines how managers influence firm outcomes such as financial reporting (Davidson et al. 2015), performance (Bennedsen et al. 2020), and risk-taking (Kallunki and Pyykkö 2013). However, recent research moves beyond the characteristics of top managers to examine the association between the

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characteristics of rank-and-file employees and firm outcomes. Due to data availability constraints, this evidence is typically based on indirect proxies, such as educational level (Call et al. 2017), religiosity (McGuire et al. 2012; Dyreng et al. 2012), or attitudes about gambling (Christensen et al. 2018) among people near firms' headquarters, or it is limited to industries for which data are readily available (Amir et al. 2014a; Law and Mills 2019). In addition, practitioners have recently expressed interest in the value of information about a firm's employees (SEC 2017).

This paper examines whether traits of both CEOs and rank-and-file employees are associated with firm risk and the cost of debt. To measure these traits, we rely on proprietary access to comprehensive criminal registers from Denmark, which cover all criminal charges in the country, dating back to 1980, on top managers and rank-and-file employees in our sample firms. We access each employee's full criminal record, including convictions and investigations for crimes that led to case dismissals or acquittals. The records comprise felonies, misdemeanors, and legal infractions and thus cover both serious and petty crimes. We link individual employees and their criminal records to their employers and test how employee characteristics relate to firm outcomes in a much broader setting than has been used elsewhere.

The criminology literature predicts that crime is caused by a lack of self-control (Gottfredson and Hirschi 1990) or exposure to criminal peers (Akers 1973). Individuals lacking self-control are impulsive, risk-seeking, and shortsighted (Gottfredson and Hirschi 1990), characteristics that can lead to risk-taking.

Employees can influence firm risk in several ways. First, they can affect firm actions, such as investment decisions—a view supported in the literature. Graham et al. (2015) survey CEOs and CFOs and find that decisions about investments are commonly delegated to employees below the CEO or CFO level. In addition, research on employees in the financial industry finds that lending officers influence loan contracts (Campbell et al. 2019; Bushman et al. 2021) and that financial advisors with criminal records imperil their clients' well-being (Law and Mills 2019). Second, employees can influence firm decisions indirectly through their influence on coworkers (called peer effects). Peer effects are documented across many academic disciplines (e.g., Sunstein 2002). For example, Dimmock et al. (2018) show that fraud is contagious among coworkers in financial advisory firms. Finally, employees can provide internal governance (Dyck et al. 2010; Acharya et al. 2011; Li 2019) by disciplining (or not) managers from making risky decisions.

We estimate three bankruptcy prediction models one at a time, to empirically test whether the criminal records of CEOs and employees relate to firm risk. Specifically, we estimate the models of Altman (1968), Ohlson (1980), and Beaver et al. (2005), which we complement with additional control variables motivated by the literature. We include additional firm-specific controls, including the wealth of a firm's owners, earnings volatility, and employee counts. We further include personal controls for the CEOs and employees, such as their education, gender, and age. Finally, we add a variable for a CEO's criminal record (an indicator of whether the CEO has a record) and a variable for employees' criminal records (which measures the proportion of firm employees with criminal records). Incremental to all the control variables, we find that the criminal records of CEOs and employees help predict bankruptcies. We estimate that a CEO with a criminal record is associated with an increase in the likelihood of bankruptcy of 45–47 basis points or about 35%–36% of the unconditional mean. A one

standard deviation increase in the percentage of employees with criminal records is associated with an increase in the likelihood of bankruptcy of 31–34 basis points or about 20%–22% of the unconditional sample mean.

We then examine the out-of-sample prediction accuracy, as measured by the area under the curve (AUC) statistic. Our results are as follows: (1) In addition to all the variables described above, the criminal records of CEOs and employees significantly improve the prediction accuracy. (2) The personal control variables do not collectively improve the prediction accuracy. And (3) a specification that includes criminal records, model-specific accounting variables, and firm controls but excludes personal control variables leads to the highest prediction accuracy. The two variables of criminal records increase the AUC statistic by 22–45 basis points, depending on the specification. The economic magnitudes are meaningful, although the increase in the AUC statistic is modest compared to related research.

We find that our measure based on the percentage of *all* employees with criminal records outperforms alternative measures, such as those limited to employees with the highest salary (highest within-firm quartile) or to non-CEO top managers, in terms of the out-of-sample bankruptcy prediction accuracy. We view this finding as consistent with the predictions regarding peer effects, in which employees with decision-making authority are influenced by coworkers (e.g., Dimmock et al. 2018). Limiting our measure to those at the top of companies thus omits this information, leading to impaired predictions. Although we cannot directly observe the influence exerted by coworkers, we can observe whether they are associated with other decisions in terms of committing new crime. Consistent with peer influence, we find that people are more likely to commit new crime when they start working in a company that employs more criminals. This holds both for individuals with and without a record prior to the employment.

We then condition our analysis on different types of crime. First, we examine the nature of crime and find that the prediction accuracy is larger when we use only white-collar crime than when we use other crime. We infer that white-collar crime drives our results, although it strongly correlates with other types of crime.¹ Second, we condition by the severity of crime. We do not find that crime penalized by imprisonment (the most serious category considered) leads to better prediction accuracy than less serious crimes. Third, we condition by whether crime is disclosed on the certificate of criminal record at hiring.² We find that undisclosed crime predicts bankruptcies and leads to the largest prediction accuracy. Finally, we condition by recent versus nonrecent crimes. Both predict bankruptcy, although nonrecent crime does so more accurately. This is consistent with the notion that crime is an observable outcome of an inherent trait that persists throughout life, as proposed by Gottfredson and Hirschi (1990).

We conduct several exploratory analyses. (1) We find some evidence that our results are concentrated among small firms with weak governance and among firms managed

¹ The correlation coefficient is 0.57 between the percentage of employees with white-collar criminal records and the percentage with nonwhite-collar criminal records. Forty-four percent of the individuals in our sample with white-collar criminal records have committed other crimes.

² In Denmark, criminal records are not publicly available. The Danish police can issue a certificate of criminal record to an individual, who can then share it with employers (e.g., when applying for a job). Offenses of the Danish penal code and certain other offenses appear on the certificate of criminal record for two to five years, after which they are automatically spent (i.e., sealed). Spent crime still appears in our proprietary dataset.

by a CEO without a criminal record. (2) Current changes in the percentage of employees with criminal records positively predict future changes in measures of firm risk (investments, growth, and debt), suggesting that employees' criminal records convey information that later manifests in the accounting figures. (3) Criminal records of CEOs and employees are not associated with firm efficiency, on average. However, criminal records are positively associated with the likelihood of winning the "Gazelle Prize," which is awarded to fast-growing and successful firms, thus indicating more right-skewed extreme firm outcomes. And (4) criminal records of CEOs and employees, to some extent, predict bankruptcies over longer horizons.

We then examine whether these risk factors are associated with the cost of debt, which we measure as the interest rate. While the literature demonstrates that lenders view the characteristics of the management of the borrowing firm as an important factor in the lending decision (Grunert et al. 2005; De Franco et al. 2017; Donelson et al. 2017; Bui et al. 2018), we are not aware of any research on the interplay of lending decisions and the attributes of borrowers' employees. In our cross-sectional regressions, we find that firms pay higher interest rates when their CEOs have criminal records and when more of the employees have records. We then estimate panel models with firm fixed effects and find that the criminal records of CEOs are not significantly associated with interest rates, potentially due to the rarity of CEO turnovers in our sample. The criminal records of employees continue to be associated with firms' interest rates, although the results are sensitive to our control variables.

In summary, our results indicate that the criminal records of CEOs and employees help explain the likelihood of bankruptcy. Our interpretation is that these characteristics represent a source of information about firms' risk. Our estimations provide evidence that lenders charge more for debt when a borrower's CEO has a criminal record and when more of the borrower's employees have criminal records, although these results are not robust across different specifications.

We acknowledge that employees are not randomly assigned to firms (e.g., Van den Steen 2010), so we cannot rule out concerns about endogeneity.³ We conduct several tests to address these concerns. For example, we find that employees with undisclosed crime at hiring (no offenses appear on the certificate of criminal record) predict bankruptcies. That is, firms that unknowingly hire criminals are more likely to go bankrupt. This is consistent with matching of firms not conducting background checks (arguably a special type of firm) with record-holder employees not driving our results. Our results using propensity-score matching, a changes specification, and subsample estimations (where we condition on the CEOs having or not having a criminal record) help mitigate concerns about endogenous sorting driving our results.

We contribute to the literature in several ways. Our main contribution is to show that the traits of employees can be associated with firm outcomes. One strand of this literature approximates workforce characteristics using demographic variables of people living near firms' headquarters (McGuire et al. 2012; Dyreng et al. 2012; Call et al. 2017; Christensen et al. 2018; Beck et al. 2018). Our results provide direct evidence, using the traits of actual employees.

³ Related research, including the work of Kallunki and Pyykkö (2013) and Davidson (2015; 2019), recognizes limitations regarding endogeneity and sorting.

Another strand of this literature examines professional services companies for which researchers can obtain sufficiently detailed data and in which the outcomes of the single employee are traceable (Amir et al. 2014a; Law and Mills 2019; Griffin et al. 2019). This stream documents that the traits of each employee are associated with that employee's decisions. We make two contributions here. First, our results are consistent with the notion that employees influence their coworkers. Thus, they suggest how employees might affect firm outcomes. Second, we show that employee effects are not limited to professional services companies. For example, Law and Mills (2019) conclude that "financial advisors with pre-advisor criminal records … pose a greater risk to investors than those without" (p. 497). In this study, we show that the risk of criminal records permeates a large countrywide sample of firms in many different industries.

The remainder of this paper proceeds as follows. The next section discusses related research and develops testable hypotheses. Section 3 describes the sample and key measures. Section 4 outlines the research design and presents the results. Section 5 concludes and discusses possible limitations.

2 Related literature and hypothesis development

2.1 Overview of related literature

2.1.1 Individuals and corporate decisions

Since the formulation of the Upper Echelons Theory (Hambrick and Mason 1984), mounting evidence has emerged about the influence of top managers on corporate outcomes (e.g., review by Plöckinger et al. 2016). Several studies explore how manager characteristics are associated with risky corporate decisions. Researchers link these decisions to (1) observable off-the-job behavior, such as taking on leverage in personal real estate purchases (Cronqvist et al. 2012) and personal payment defaults (Kallunki and Pyykkö 2013); (2) experiences, such as military service (Benmelech and Frydman 2015) and exposure to natural disasters (Bonsall et al. 2017); (3) inherent characteristics, such as age (Li et al. 2017) or gender (Adhikari et al. 2019); and (4) proxies for psychological traits, such as overconfidence (Hirshleifer et al. 2012), risk-aversion (Graham et al. 2013), and sensation seeking (Cain and McKeon 2016; Sunder et al. 2017).

Recent research suggests that employee characteristics also can influence firm behavior. Due to data limitations, researchers use indirect geographic proxies, based on demographic variables of people surrounding a firm's headquarters, including their gambling attitudes (Christensen et al. 2018), educational levels (Call et al. 2017; Beck et al. 2018), or religiosity (McGuire et al. 2012; Dyreng et al. 2012). Other researchers limit their studies to professional services companies for which they can obtain sufficiently detailed data, such as firms in the financial (Law and Mills 2019; Griffin et al. 2019; Campbell et al. 2019; Bushman et al. 2021; Honigsberg and Jacob 2021) or auditing (Amir et al. 2014a) industries.

We identify several channels through which employees can influence a firm's risk. First, employees can directly influence corporate actions, such as investment decisions. Graham et al. (2015) survey CEOs and CFOs and find that investment decisions are

most commonly delegated to employees below the CEO or CFO level. Relatedly, McElheran (2014) provides establishment-level empirical evidence on delegation of information technology (IT) investment decision rights and finds that 62% of the sample establishments have decision rights over nonpersonal computer IT investments.⁴ Second, employees can contribute to internal governance, as suggested by Dyck et al. (2010), Acharya et al. (2011), and Li (2019). Third, employees with certain traits (e.g., criminals) can influence firm decisions through their influence on coworkers, a phenomenon termed "peer effects." Peer effects are documented across many academic disciplines (e.g., Sunstein 2002). Dimmock et al. (2018) show that financial advisors' propensity to commit financial misconduct increases with the proportion of new coworkers with a history of misconduct following mergers and acquisitions. Murphy (2019) exploits the random assignment of US soldiers to units and finds that those assigned to units with more criminal peers are more likely to misbehave. Based on the Cambridge-Somerville Youth Study, with random assignment within pairs matched prior to treatment, Dishion et al. (1999) show that boys sent to summer camp (part of the treatment) were more likely to commit crime and experience other adverse life outcomes (pre-mature death, alcoholism, or psychiatric impairment).

Beyond the potential to influence firm behavior, employees might endogenously sort themselves into certain firms and thereby reflect corporate culture (Van den Steen 2010). That is, employees may self-select into firms that fit their traits, and firm managers may hire people who share their own traits.

2.1.2 Criminal records

The criminology literature provides several theories of the causes of crime. Two theories have received considerable attention. First, Gottfredson and Hirschi's (1990) General Theory of Crime posits that a lack of self-control determines criminality, independent of the nature of the crime, and that crime provides easily accomplished and immediate gratification. The extent to which individuals lack self-control is determined in childhood and persists. Individuals lacking self-control are characterized as impulsive, risk-taking, and shortsighted. Second, Akers' (1973) social learning theory argues that individuals learn criminality the same way they learn other behaviors—from peers.

Although the two theories offer opposing predictions, the literature provides empirical support for both. Pratt and Cullen (2006) conduct a meta-analysis of 21 studies and 126 size effects and conclude that both sets of variables, one set from each theory, strongly predict crime.⁵ In sum, we conjecture that the presence of a criminal record an empirically observable outcome of a certain personal trait—affects decision-making either directly or through peer effects.

Several researchers within the finance and accounting literature associate criminal records with outcomes related to the firm or to individuals' actions within the firm. Top managers with criminal records are associated with corporate outcomes such as the

⁴ McElheran (2014) notes that such investments are economically large, ranging from US \$500,000 to above \$50 million.

⁵ Pratt and Cullen (2006) is listed among the most cited articles published in the influential *Criminology*, according to the journal's website.

propensity to commit fraud, financial reporting risk (Davidson et al. 2015), earnings volatility, goodwill impairments (Amir et al. 2014b), and insider trading (Kallunki et al. 2018; Davidson et al. 2020). On a more granular level, some researchers focus on professional services companies in which each employee's criminal record and production outcomes are traceable. For example, Amir et al. (2014a) find that audit partners with criminal records have riskier clients, and Law and Mills (2019) find that financial advisors with criminal records are more likely to receive future customer complaints along with other adverse outcomes. Honigsberg and Jacob (2021) show that financial advisors with adjudicated expungement requests (a process allowing brokers to remove financial misconduct from their public records) are more likely to misbehave in the future.

2.1.3 Bankruptcy prediction

Classic bankruptcy prediction models, such as Altman's (1968) Z-score and Ohlson's (1980) O-score, rely on accounting figures. Accounting-based econometric models are widely accepted due to their relatively high predictive power. However, researchers have complemented these models with factors based on stock return data⁶ (e.g., Shumway 2001; Chava and Jarrow 2004; Beaver et al. 2005) and macroeconomic information (Hillegeist et al. 2004) and find that doing so improves predictive accuracy. A limited amount of research investigates how observable manager effects may provide incremental information (e.g., Kallunki and Pyykkö 2013). However, to the best of our knowledge, no research examines the informational value of employee characteristics for bankruptcy prediction.

2.1.4 Cost of debt

Lenders rely on both hard and soft information when evaluating loan applicants or loan extensions (Liberti and Petersen 2019). Hard information, such as financial statement data, is undoubtedly important for lenders' credit assessments (Agarwal and Hauswald 2010; Donelson et al. 2017). Lenders also collect and use soft information in their assessments. Grunert et al. (2005) analyze internal credit files of four German banks and find that nonfinancial (soft) factors, incremental to financial (hard) factors, improve the accuracy of probability-of-default estimations. Interestingly, they find that a factor capturing the lending officer's subjective assessment of management quality significantly improves the prediction model. In a similar vein, the majority of the survey respondents of Donelson et al. (2017) indicate that, when they evaluate credit extensions, "character and reputation and experience of management" (Table 3, p. 2062) are among the most important factors, above "leverage and financial condition," "guarantees," and "liquidity."

Agarwal and Hauswald (2010) describe the decisions of a large US bank lending to small firms. They note that each branch has considerable autonomy in its decisions but "has to justify any deviation from bank-wide practices on the basis of predefined subjective criteria, such as impression of management quality" (p. 2763). This suggests

⁶ However, models based on stock return data are limited to public firms. This restricts other stakeholders, such as lenders, from applying stock market data to private firms.

that the quality of borrower firm management is an important component of the lending decision. The results of De Franco et al. (2017) and Bui et al. (2018) suggest that managers of higher ability obtain lower bank-loan prices.

2.2 Hypotheses

Based on the extensive body of research on top managers and corporate outcomes including studies that link criminal records to several corporate outcomes—we expect that firms whose CEOs have criminal records will have a higher likelihood of bankruptcy than other firms. We formally state this hypothesis as follows.

Hypothesis 1a: A firm has a higher likelihood of bankruptcy when the CEO has a criminal record.

We expect that the percentage of employees with criminal records is also associated with firm risks that reflect the likelihood of bankruptcy. As outlined in Section 2.1.1, employees can affect firm outcomes through their influence on corporate policies and investment decisions, their internal governance role, and their sway with coworkers. Alternatively, employee characteristics can explain firm outcomes through sorting mechanisms, whereby employees opt to work for firms that share their traits. We expect employees' criminal records to provide information about a firm's risk, independent of the channel. This leads to our next hypothesis.

Hypothesis 1b: A firm's likelihood of future bankruptcy increases with the proportion of employees with criminal records. The effect is incremental to that of the CEO's criminal records.

Lenders use evaluations of the management of a borrower firm in their credit assessments. We do not expect lenders to require the criminal records of borrower management.⁷ However, to the extent that the presence of a criminal record is an observable outcome of a certain personal trait, we expect that lenders can discover the type and traits of the borrower's CEO. Therefore we expect that lenders charge a higher price when a CEO has a criminal record. This leads to our next hypothesis.

Hypothesis 2a: A firm has a higher cost of debt when the CEO has a criminal record.

Lastly, we aim to explore the extent to which lenders adjust the cost of debt to the criminal records of the workforce of borrower firms. We are not aware of any studies assessing lenders' pricing of borrowers' workforce characteristics. We provide two sets of opposing arguments. The first set implies that lenders do not price the criminal records of a borrower's workforce. Firms are not required to disclose workforce information beyond the number of people employed. And under the current Danish regulation, lenders can only access criminal records if all employees consent to share

⁷ Our informal interviews with several large Danish banks suggest that banks do not routinely collect criminal records of managers or employees in borrower firms.

them with their employer's lender. Our interviews with banks indicate they do not collect this information. The second set of arguments implies that lenders do price the criminal records of a borrower's workforce. Lenders could indirectly learn about the records if they are reflected in firm behavior that lenders can observe. Our last hypothesis is therefore explorative, stated as follows.

Hypothesis 2b: *The cost of debt is increasing in the proportion of the borrower's workforce with criminal records.*

3 Sample construction, key variables, and descriptive statistics

3.1 Data sources and data description

Throughout our data sampling, we use unique firm identification numbers (CVR numbers) and unique personal identification numbers (CPR numbers) to merge datasets across sources. We use proprietary employment spells (employer-employee links) provided by Statistics Denmark to link the individuals, including their personal information, to the firms in which they work.

3.1.1 Firm-specific data

We gather financial statement data for all limited liability firms incorporated in Denmark for the period of 1998–2016 with total assets above DKK 1 million (EUR 0.13 million). We obtain data from Orbis, managed by Bureau Van Dijk, and complement that with data from Experian. The data include income statement items, balance sheet items, industry membership (NACE codes), full-time equivalent employee counts, and report publication dates. We hand-collect data on firm bankruptcies from Auktioner P/S, including firm identification numbers and filing dates.⁸ The bankruptcy data cover the period of 2004–2016.

3.1.2 Individual data and criminal records

We identify CEOs through firms' filings with the Danish Business Authority. Through Statistics Denmark, we obtain access to the Integrated Database for Labor Market Research (IDAN database), which keeps data on employment spells, including annual data on salary received from the firm as well as starting and ending dates of employment.⁹

Statistics Denmark further provides access to the Danish Criminal Registry (Kriminalstatistik Afgørelse), which covers all criminal decisions from 1980. The dataset provides information on (1) judicial decisions, including criminal convictions and investigations for crimes that led to dismissals and not guilty

⁸ The Danish Official Gazette (www.statstidende.dk) discloses all Danish bankruptcies. Auktioner P/S, through the website www.konkurser.dk, draws information from this information source.

⁹ For a further description of the IDAN database, see Timmermans (2010), Jinkins and Morin (2018), and Bennedsen et al. (2019).

verdicts, (2) penalties imposed on offenders, such as imprisonment, suspended sentences, and fines above DKK 1500 (EUR 200),¹⁰ and (3) the nature of the crime, based on seven-digit crime codes used by the Danish police. (The digit system has a tree structure, similar to industry classifications.) The offenses include felonies, misdemeanors, and legal infractions. The data thus cover serious crimes, such as sexual, violent, or drug-related offenses, and petty crimes, such as shoplifting. We use the crime codes to map the nature of crime reported in the Danish registers to the Federal Bureau of Investigation (FBI) definitions of general crime categories and white-collar crime, based on the conversion tables reported by Andersen et al. (2020), and present these mappings in Online Appendix C. We also use (4) the year of the criminal decision and (5) other information, such as length of incarceration.

Criminal records are not publicly available in Denmark. The Danish police can issue certificates of criminal records to individuals, who can then share them with employers (e.g., when applying for a job). The certificates include information on offenses of the Danish penal code and certain other offenses. Fines and suspended sentences appear on the certificates for two and three years following a conviction, respectively. Prison sentences appear for five years following release.¹¹ After this period, the crime is considered spent (comparable to sealing in the United States); that is, it is automatically removed from the certificate but appears in the police's databases and, thus, in our proprietary dataset.

We estimate that employers ask for criminal records of new employees in less than 63% of new employments.¹² To investigate whether banks request criminal records of borrowers, we called several of the largest Danish banks and asked about their practices. These conversations revealed that lenders do not routinely collect criminal records of managers or employees in borrower firms, although Danish legislation does not prevent this. The lenders do sometimes request the criminal record of the CEO of a potential borrower as part of the "Know Your Customer" procedure in cases where the lender suspects that the firm is seeking to become a customer for financial-crime purposes (such as money laundering and terror financing).

3.1.3 Sample selection

We keep firm-years for the period of 2003–2015 to allow for a year's lag between the last annual report and the bankruptcy filing. We merge the datasets and impose several screens. We exclude firm-year observations that do not cover 12 months,

¹⁰ This threshold was first set to DKK 1000 in May 1992 and later changed to DKK 1500 in July 2001.

¹¹ See https://politi.dk/straffeattest/afgoerelser-paa-din-straffeattest (in Danish).

¹² The average number of issued certificates of criminal records per new employment is 0.63 for the period of 2010–2015. We retrieve employee churn data from the Danish Agency for Labor Market and Recruitment and the number of issued certificates from the Danish police. Our estimate indicates how often employees ask for criminal records of their employees, although it is subject to limitations. The Danish police's estimates of issued certificates are crude, as about 57% (on average 257,000 annually between 2010 and 2015) of the certificates are rounded to the nearest 100,000. Certificates of criminal records are used for purposes other than hiring, which biases our estimate upward. For instance, employers can ask employees to submit certificates of criminal records on an ongoing basis, and authorities retrieve the certificate in the application process for Danish citizenship.

to make the observations comparable across firms and time. Consistent with the literature, we exclude certain industries (financial, utilities, and state-owned). To avoid double counting, we exclude subsidiaries for which the parent firm reports on a consolidated basis. We also impose several size thresholds. Based on the current auditing thresholds as outlined by Bernard et al. (2018), we keep firm-year observations with total assets of at least DKK 4 million (EUR 533,000) and at least 12 full-time equivalent employees. The minimum thresholds ensure that all of our sample firms are audited, prevent mom-and-pop stores from driving our results, and allow for variation in employee traits. We also impose an upper size threshold and keep only firms that conform to the small and medium-sized enterprises (SME) definition of the European Commission.¹³ Finally, we exclude observations for which data are missing in estimating the bankruptcy prediction models. Table 1 outlines the sample selection procedure. The final sample comprises 15,697 unique firms, 103,774 firm-years, 1,429,368 unique individuals, and 6,103,074 individual-firm-years.

3.2 Key variables

3.2.1 Criminal records of executives and employees

On the individual level, we set an indicator variable, *Record*, equal to one if an individual has a criminal record and zero otherwise.¹⁴ As is standard in the literature, we include both convictions and criminal charges that led to dismissals or acquittals in our measure of criminal records.¹⁵ We do not include traffic-related offenses for two reasons. First, this is consistent with the literature. (See Bennett (2018) and Breining et al. (2020) for examples with Danish data, and Kallunki et al. (2018) for an example with Swedish data.) Second, many individuals in our sample have traffic-related records: 70% (37%) of CEOs (employees).¹⁶ At the firm level, we define the variable *CEO_record* as an indicator variable that takes the value one if the CEO of the firm has a criminal record (if *Record* = 1).

¹³ The European Commission defines companies as SMEs if they have (1) less than 250 employees (full-time equivalents), and (2) either total assets below EUR 43 million (DKK 323 million) or revenues below EUR 50 million (DKK 375 million). The dataset excludes firms that do not meet these thresholds. We exclude the largest companies for two reasons. First, we observe a very low bankruptcy frequency of only 0.25% for firms larger than the SME definition. Second, the larger companies could differ fundamentally from SMEs in many other aspects, thus confounding our results. We do not analyze publicly listed firms separately, due to the small sample size. For the period of 2001–2015, we identify 236 unique nonfinancial firms listed on Danish stock exchanges; 18 of these firms went bankrupt.

¹⁴ In contrast to Kallunki et al. (2018) and Davidson et al. (2020) but consistent with Davidson et al. (2015), we define *Record* only if an individual has any *prior* convictions, to avoid look-ahead bias.

¹⁵ See Amir et al. (2014a, 2014b), Davidson et al. (2015; 2020), Kallunki et al. (2018), and Law and Mills (2019). Most of the criminal cases in our dataset lead to conviction. When we exclude dismissals and not-guilty verdicts, the means of *CEO_record* and *%EMPL_record* decrease by 8% (from 18.8% to 17.3%) and 8% (from 17.1% to 15.7%), respectively. We replicate all our analyses excluding these legal decisions and obtain essentially similar results as those reported in this paper. The inclusion of dismissals and not-guilty decisions on the criminal record leads to a significantly larger bankruptcy prediction accuracy (AUC). When these legal decisions are excluded, *%EMPL_record* is not significantly associated with the interest rate using the firm fixed effects specification reported in column 4 of Table 10.

¹⁶ In untabulated analyses where we add, to the bankruptcy prediction estimations (Eq. (1)), the variables *CEO_traffic* (one, if the CEO has a traffic-related offense) and *%EMPL_traffic* (the percentage of employees with traffic-related offenses), we find that both variables are statistically insignificant predictors of bankruptcy.

Table 1 Sample selection

Note	Screen	Sample size. Firm-year observations	Firm-year observations dropped	Decrease in sample size (%)
1	Firm-year observations with employer-employee links for the years 2003–2015	581,520		
	Keep financial reports with 12 months	569,678	11,842	2
2	Remove certain industries	502,919	66,759	12
3	Remove subsidiaries	495,384	7,535	1
	Remove listed firms	494,246	1,138	0
4	Keep firm-year observations with total assets above DKK 4 million (EUR 533,333)	274,691	219,555	44
5	Keep firm-year observations with at least 12 full-time equivalent employees	121,548	153,143	56
6	Remove firm-year observations that exceed the SME thresholds set by the European Commission	115,427	6,121	5
	Keep firm-year observations with variables available for the bankruptcy prediction estimation	103,774	11,653	10

This table shows the sample selection procedure. Notes: (1) The period 2003–2016 is the years for which bankruptcy data are available. To allow one year's lag between the fiscal year end and the bankruptcy filing, we restrict the period to 2003–2015. (2) Consistent with prior accounting and finance research, we exclude certain regulated industries (financials and utilities) and further exclude state-owned enterprises. (3) To avoid double counting we exclude subsidiaries for which the parent company reports on consolidated basis. (4) (5) We impose the minimum size requirements according to the current auditing thresholds in Denmark (Bernard et al. 2018). The minimum thresholds assure that all the sample firms undergo mandatory audit, that mom-and-pop stores do not drive our results, and that we have variation in the traits used to describe employees. (6) We impose a maximum threshold for two reasons: the bankruptcy rate for the excluded companies is very small (0.25%), and large corporations potentially differ significantly from SMEs on several aspects, which could influence our results

for the CEO) and zero otherwise. We define the variable $\% EMPL_record$ as the percentage of a firm's employees with criminal records.¹⁷ For each firm-year, we calculate the percentage of employees for whom *Record* = 1.

3.2.2 Bankruptcy variable and firm risk

We use the legal definition of bankruptcy to identify firms in financial distress, which is likely due to excessive risk-taking. Appiah et al. (2015) review the literature on corporate failure prediction and find that 84% of studies use the legal definition of bankruptcy to classify firms as failing or nonfailing. Hayden (2003) compares credit-scoring models with different default criteria (bankruptcy, restructuring, and delay-in-payment) and finds that models with bankruptcy as the dependent variable are as powerful in predicting credit losses as models with the alternative criteria as dependent variables, suggesting that the proxy for financial distress is of minor concern.

¹⁷ For this purpose, we count the number of individuals who have received salary from the firm during the year and use this measure (*Headcount*) as the deflator. Further, we have replicated the estimations outlined in Eqs. (1) and (2), weighting employee observations per firm-year by salary instead of using simple averages. Our qualitative conclusions remain unchanged.

Our data contain bankruptcy notice dates—the dates when a bankruptcy court has ruled that a company must undergo bankruptcy proceedings. Under Danish regulation, a bankruptcy filing leads to firm termination (i.e., liquidation), similar to a Chapter 7 filing in the United States. Following the bankruptcy notice, a trustee is appointed, and the firm's management loses control. The trustee sells off the assets and distributes the collected funds to creditors.¹⁸

We define an indicator variable, *Bankrupt*, which takes the value of one if the annual report is the last published report preceding the bankruptcy notice and zero otherwise.¹⁹ We use *Bankrupt* as the dependent variable in the bankruptcy prediction estimations.

3.2.3 Cost of debt

We use the interest rate to capture the cost of debt. We measure the interest rate as financial expenses divided by interest-bearing debt. We measure a firm's interest-bearing debt as total liabilities net of trade payables. We then define the variable *CoD* as financial expenses scaled by the average interest-bearing debt for year *t* and year *t*-1. Related research uses comparable approaches of dividing interest expenses with debt (e.g., Minnis 2011; Vander Bauwhede et al. 2015; Gassen and Fülbier 2015). However, while data on actual debt and interest expenses are very limited in our dataset, related studies use actual debt and interest expense data in their estimations.²⁰ We acknowledge that our approach could contain noise. To mitigate the effect of outliers, we follow Minnis (2011) and truncate the *CoD* measure at the 5th and 95th percentiles and truncate observations more than 10 percentage points over the interest rate of Danish government bonds for the year.²¹

3.3 Descriptive statistics

We present descriptive statistics in Table 2. Columns 1 and 2 describe the sample. Columns 3–5 condition the sample by *Bankrupt* and compare the samples. The average sample firm is relatively small, with total assets of about EUR 6 million, a headcount of about 58, which in full-time equivalent employees corresponds to about 37. On average across firm-years, 18.8% of the CEOs and 17.1% of the employees have criminal records. The percentage of CEOs with criminal records is slightly lower than that reported by Kallunki et al. (2018), likely because our study employs Danish data, whereas Kallunki et al. (2018) use Swedish data. The average interest rate in the sample is 4.0%, which conforms closely to the officially reported average interest rate charged to Danish SMEs for the period 2007–2015 (4.4%) (OECD 2017 Table 3.10).

¹⁸ See https://domstol.dk/alle-emner/konkurs-og-erhverv/selskab-konkurs/ (in Danish).

¹⁹ In our dataset, 78% and more than 99% of the bankruptcy notices are released within one and two years following the publication date of the report, respectively. Thirty-eight percent and more than 98% of the bankruptcy notices are released within one and two years following the fiscal year-end date, respectively.

²⁰ Seventy-six percent of the sample observations do not record interest expenses. Forty-four percent, 81%, and 31% of the sample observations are missing information on long-term debt, short-term part of mortgage, and short-term part of bank debt, respectively. The lack of data is likely due to low disclosure requirements for small firms, which are allowed to publish only aggregated accounts, such as financial expenses, without further specification.

²¹ Minnis (2011) uses the prime rate. We use the interest rate of government bonds in lieu of the prime rate because the prime rate is not available for Denmark.

We observe that 29.3% of bankrupt firms have a CEO with a criminal record, compared to 18.7% of nonbankrupt firms. In Fig. 1, we further depict how the number of CEO crimes relates to the bankruptcy rate and generally find that the bankruptcy rate increases by the number of CEO crimes. The univariate statistics provide initial evidence supporting H1a.

Bankrupt firms on average have more employees with criminal records. Specifically, 22.3% of employees in bankrupt firms have criminal records, versus 17.0% in nonbankrupt firms. In Fig. 2, we plot bankruptcy rates per criminal employee quintile (within-year quintiles based on $\% EMPL_record$), conditioned by the CEO's having a criminal record (i.e., by $CEO_record = 1$ and $CEO_record = 0$). We observe, in both subsamples, that the bankruptcy rate increases with the percentage of employees with criminal records. This suggests that employee characteristics are incremental to CEO characteristics in explaining bankruptcy rates, and provides initial evidence supporting H1b.

We tabulate a correlation matrix in Table 3. *Bankrupt* and *CoD* relate positively to both *CEO_record* and *%EMPL_record*. Table 4 provides information on the types of crime and the associated bankruptcy rate. Interestingly, the bankruptcy rate is higher across all CEO criminal record categories. Specifically, in column 2, we observe bankruptcy rates of 0.016–0.031, which are all higher than the unconditional mean bankruptcy rate at 0.013. We observe a similar pattern when we examine criminal records of employees in columns 4 and 5.

4 Empirical design and results

4.1 Bankruptcy prediction models

To test the relation between the likelihood of bankruptcy and the criminal records of the CEO and employees, we estimate Eq. (1) with a hazard estimation (Shumway 2001), which equals a logistic regression with adjusted standard errors. Specifically, chi-squared statistics are divided by the average number of years per firm to correct the standard logit estimates. We estimate the following model.

$$Bankrupt_{it} = \alpha_0 + \beta_1 CEO_record_{it} + \beta_2 \% EMPL_record_{it} + \beta_3 ACC_{it} + \beta_4 Firm \ variables_{it} + \beta_5 Person \ variables_{it} + \varepsilon_{it}$$
(1)

for firm *i* in year *t. Bankrupt, CEO_record*, and %*EMPL_record* are defined in Section 3.2 above. *ACC* represents accounting-based variables used to predict bankruptcy by Beaver et al. (2005) (henceforth, the BMR model), Altman (1968), and Ohlson (1980), respectively.²² *Firm variables* is additional firm-level control variables motivated by the literature. These include the relative wealth of a firm's owner(s) (Beaver et al. 2019), earnings volatility (Amir et al. 2014b), and the logarithm of employee counts. *Person variables* represents person-specific control variables for CEOs' and employees' other personal characteristics that the literature suggests are associated with firm outcomes. These include educational level (Call et al. 2017),

²² We use a private firm version of the Altman model (e.g., Altman and Saunders 1997; Kallunki and Pyykkö 2013).

	Entire san	nple	Condition	al on <i>Bankrup</i>	ot
N=	103,774		1 1349	0 102,425	
	Mean (1)	Median (2)	Mean (3)	Mean (4)	Diff. (5)
Dep. variables and variables of interest					
CEO_record	0.188	0.000	0.293	0.187	0.106***
%EMPL_record	0.171	0.143	0.223	0.170	0.053***
Bankrupt	0.013	0.000	1.000	0.000	n.a.
CoD	0.040	0.038	0.059	0.039	0.020***
BMR variables					
EBIT/TA	0.107	0.086	-0.056	0.109	-0.165***
TL/TA	0.666	0.679	0.945	0.662	0.283***
EBITDA/TL	0.281	0.199	0.004	0.284	-0.281***
Altman variables					
NWC/TA	0.179	0.175	0.129	0.179	-0.050***
RE/TA	0.197	0.182	-0.047	0.200	-0.247***
EBIT/TA	0.107	0.086	-0.056	0.109	-0.165***
BV/TL	0.786	0.473	0.126	0.794	-0.668***
GP/TA	0.833	0.684	0.745	0.835	-0.089***
Ohlson variables					
NWC/TA	0.179	0.175	0.129	0.179	-0.050***
TL/TA	0.666	0.679	0.945	0.662	0.283***
Log (TA)	9.975	9.823	9.686	9.978	-0.293***
CL/CA	0.898	0.786	1.330	0.892	0.438***
NI/TA	0.070	0.055	-0.085	0.072	-0.157***
EBITDA/TL	0.281	0.199	0.004	0.284	-0.281***
NITWO	0.211	0.000	0.698	0.204	0.493***
OENEG	0.044	0.000	0.301	0.041	0.260***
CHIN	0.012	0.037	-0.258	0.016	-0.274***
Other firm variables					
TA (EUR million)	5.671	2.473	3.866	5.695	-1.829***
Firm age	18.960	16.000	15.180	19.010	-3.830***
Headcount	57.606	36.000	53.170	57.664	-4.494**
Employees	36.791	24.000	31.690	36.858	-5.168***
EquityFirmOwner/TL	1.069	0.000	0.086	1.082	-0.996***
EquityPersOwner/TL	0.108	0.000	0.014	0.109	-0.095***
StdROA	0.083	0.055	0.128	0.082	0.046***

Table 2 Descriptive statistics

Table 2 (continued)

	Entire sam	ple	Conditiona	al on <i>Bankrup</i>	t
N=	103,774		1 1349	0 102,425	
	Mean (1)	Median (2)	Mean (3)	Mean (4)	Diff. (5)
Person-specific variables					
CEO_HighEduc	0.154	0.000	0.112	0.154	-0.042***
%EMPL_HighEduc	0.061	0.011	0.041	0.061	-0.020^{***}
CEO_Female	0.056	0.000	0.050	0.056	-0.005
%EMPL_Female	0.284	0.217	0.234	0.285	-0.051***
CEO_log(Age)	3.865	3.871	3.837	3.865	-0.028***
%EMPL_log(Age)	3.623	3.644	3.618	3.623	-0.005
CEO_Married	0.812	1.000	0.786	0.813	-0.027**
%EMPL_Married	0.456	0.467	0.429	0.456	-0.027***
CEO_CorrupIndex	-93.238	-93.787	-93.094	-93.240	0.146
%EMPL_CorrupIndex	-90.946	-92.626	-90.507	-90.952	0.445***

This table presents the summary statistics. Accounting ratios are winsorized at the lower and upper 1% level. All variables are defined in Appendix A. ***, **, and * represent significance levels at 0.01, 0.05, and 0.10, respectively (two tailed test)

gender (Adhikari et al. 2019), age (Belenzon et al. 2019), marital status (Roussanov and Savor 2014), and the corruption index at the country of ancestry (Liu 2016). We define all variables in Appendix A. We estimate Eq. (1) with three sets of *ACC* control variables (one model at a time) and further control for year and industry fixed effects.

The β_1 and β_2 slopes measure the extent to which CEOs' and employees' criminal records, respectively, provide information on a firm's likelihood of bankruptcy (beyond



Fig. 1 Bankruptcy frequency per number of CEO convictions. This figure depicts the bankruptcy frequency on the y-axis over the number of CEO convictions (*CEO_conv*) on the x-axis



Fig. 2 Bankruptcy frequency per quintile based on within-year $\% EMPL_record$. This figure depicts the bankruptcy frequency on the y-axis over the quintile based on within-year $\% EMPL_record$. To isolate the association between employees and bankruptcy frequency, we condition by the criminal record of the CEO (*CEO_record* = 1 and *CEO_record* = 0, respectively)

what is explained by accounting variables). They are the coefficients used to test H1a and H1b.

We estimate the models and present the results in Table 5. Consistent with our expectations (H1a and H1b), across three different estimation models, we find that the likelihood of bankruptcy increases with the CEO having a criminal record (*CEO_record*) and more of a firm's employees having criminal records (*%EMPL_record*). Although *CEO_record* and *%EMPL_record* are positively correlated (see Table 3), our estimations suggest that both variables predict bankruptcies incremental to each other. The economic significance is sizable. Using all the controls, the bankruptcy likelihood increases by 45–47 basis points (bps) when the CEO has a criminal record, or about 35%–36% of the unconditional sample mean. A one standard deviation (interquartile) change of *%EMPL_record* is associated with a change in the likelihood of bankruptcy of 26–28 (31–34) bps or about 20%–22% (24%–26%) of the unconditional sample mean.²³

The proportion of employees with bachelor's degrees or higher (%*EMPL_HighEduc*) is marginally significant in two of three estimations, but none of the other person-specific variables predict bankruptcies. The bankruptcy likelihood decreases with the wealth of the firm's owners (*EquityFirmOwner/TL* and *EquityPersOwner/TL*). The accounting variables generally relate to the likelihood of bankruptcy as expected, although some variables are not statistically significant, likely due to high correlations between the variables. (For example, the correlation between *EBIT/TA* and *EBITDA/TL* is 0.78.)

4.2 Out-of-sample tests

We then analyze the out-of-sample predictive ability of the bankruptcy likelihood scores based on five different specifications. First, we present the results using only the prediction model variables (*ACC*) and the extra firm variables (*Firm variables*) (Specification A). The literature documents that these variables predict bankruptcies. We then stepwise add

 $^{^{23}}$ The standard deviation of %*EMPL_record* is 0.1218, and the interquartile range equals 0.1474 (untabulated).

Pan	el A: Dependent variab	des, varis	ubles of i	interest,	and pre	ediction 1	nodel va	riables										
		1	7		4	S	9	٢	×	6	10	11	12	13	14	15	16	17
1	Bankrupt		0.10	0.03	0.05	-0.10	0.13	-0.12	-0.02	-0.12	-0.13	-0.02	-0.03	0.10	-0.12	0.14	0.14	-0.05
0	CoD	0.10		0.05	0.03	-0.17	0.30	-0.21	0.11	-0.19	-0.30	-0.17	0.00	0.22	-0.27	0.18	0.10	-0.03
Э	CEO_record	0.03	0.04		0.17	0.00	0.04	0.01	-0.06	-0.01	-0.04	0.01	-0.05	0.08	-0.01	0.00	0.01	0.00
4	%EMPL_record	0.05	0.03	0.20		0.00	0.08	0.02	-0.04	-0.02	-0.08	0.04	-0.07	0.11	-0.02	0.00	0.00	0.01
5	EBIT/TA	-0.11	-0.15	0.01	0.01		-0.36	0.86	-0.03	0.22	0.36	0.46	-0.10	-0.28	0.95	-0.58	-0.24	0.41
9	TL/TA	0.14	0.26	0.03	0.07	-0.37		-0.59	-0.07	-0.78	-1.00	-0.06	-0.07	0.68	-0.45	0.35	0.36	-0.06
2	EBITDA/TL	-0.09	-0.18	0.01	0.00	0.78	-0.57		-0.08	0.40	0.59	0.39	-0.09	-0.36	0.84	-0.54	-0.26	0.32
×	NWC/TA	-0.02	0.11	-0.05	-0.06	-0.03	-0.09	-0.05		0.12	0.07	-0.14	0.08	-0.32	-0.05	0.00	-0.06	-0.01
6	RE/TA	-0.12	-0.18	0.00	-0.02	0.25	-0.81	0.38	0.13		0.78	-0.03	0.13	-0.56	0.28	-0.32	-0.35	0.01
10	BV/TL	-0.07	-0.20	-0.03	-0.07	0.19	-0.81	0.52	0.04	0.63		0.06	0.07	-0.68	0.45	-0.35	-0.36	0.06
11	GP/TA	-0.02	-0.17	0.01	0.06	0.44	-0.01	0.30	-0.18	-0.05	-0.06		-0.51	-0.01	0.43	-0.21	-0.05	0.16
12	Log (TA)	-0.03	-0.01	-0.05	-0.11	-0.08	-0.10	-0.06	0.07	0.13	0.13	-0.44		-0.09	-0.07	0.00	-0.08	0.00
13	CL/CA	0.09	0.16	0.08	0.10	-0.25	0.51	-0.27	-0.34	-0.44	-0.39	-0.04	-0.03		-0.34	0.27	0.24	-0.06
14	NI/TA	-0.12	-0.21	0.00	0.00	0.96	-0.43	0.76	-0.03	0.31	0.24	0.41	-0.06	-0.29		-0.61	-0.26	0.43
15	OMLIN	0.14	0.18	0.00	0.00	-0.52	0.36	-0.42	0.00	-0.35	-0.18	-0.17	0.00	0.25	-0.54		0.34	-0.05
16	OENEG	0.14	0.10	0.01	0.00	-0.28	0.50	-0.21	-0.08	-0.49	-0.20	-0.02	-0.07	0.29	-0.33	0.34		-0.06
17	CHIN	-0.05	-0.04	0.00	0.01	0.36	-0.06	0.24	-0.01	0.02	0.02	0.15	0.00	-0.05	0.37	-0.06	-0.06	

1021

Table 3 (continued)

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Pan	el B: Dependent variabl	les, vari:	ables of	interest,	extra fir	m varia	bles, an	d persor	n-specific	: variab	les.								
		1	7	3	4	Ś	9	7	8	6	10	Π	12	13	14	15	16	17	18
1	Bankrupt		0.10	0.03	0.05	-0.08	-0.04	-0.02	0.06	0.00	-0.02	0.00	-0.03	-0.02	-0.01	0.00	-0.02	0.00	0.01
0	$C_{O}D$	0.10		0.05	0.03	-0.12	-0.04	-0.08	-0.07	-0.05	-0.13	-0.01	-0.01	-0.01	-0.05	0.00	-0.04	-0.01	0.00
б	CEO_record	0.03	0.04		0.17	0.02	0.03	-0.05	0.00	-0.06	-0.10	-0.03	-0.10	0.02	-0.07	-0.03	-0.12	0.00	0.00
4	%EMPL_record	0.05	0.03	0.20		0.05	0.03	0.03	0.02	-0.12	-0.31	-0.02	-0.54	0.01	0.09	-0.02	-0.11	-0.01	0.02
S	EquityFirmOwner/TL	-0.04	-0.12	-0.02	-0.03		0.11	0.00	0.01	-0.02	-0.05	0.00	-0.04	0.08	0.04	0.01	0.01	0.00	0.02
9	EquityPersOwner/TL	-0.03	-0.07	0.04	0.02	0.13		-0.10	-0.03	-0.04	-0.10	0.00	-0.08	0.24	0.00	0.02	-0.02	0.01	-0.05
٢	log (Employees)	-0.02	-0.08	-0.06	-0.02	0.02	-0.15		-0.02	0.07	0.18	0.00	0.02	0.02	0.06	0.03	0.06	0.01	0.22
×	StdROA	0.05	-0.04	0.00	-0.01	0.04	-0.02	-0.02		0.04	0.12	0.01	-0.01	-0.10	-0.06	-0.02	-0.06	0.00	0.06
6	CE0_HighEduc	-0.01	-0.08	-0.10	-0.19	0.02	-0.05	0.12	0.09		0.24	0.03	0.14	-0.05	0.03	0.02	0.04	0.03	0.06
10	%EMPL_HighEduc	-0.02	-0.16	-0.08	-0.29	0.00	-0.06	0.03	0.19	0.45		0.03	0.40	-0.03	0.05	0.00	0.10	0.02	0.19
11	CEO_Female	0.00	-0.03	-0.07	-0.05	0.01	0.00	-0.01	0.03	0.05	0.08		0.06	-0.01	0.00	0.00	-0.02	0.03	0.03
12	%EMPL_Female	-0.03	0.00	-0.09	-0.49	0.03	-0.02	0.03	0.03	0.20	0.26	0.18		-0.01	-0.10	0.00	-0.01	0.01	0.21
13	$CEO_log(Age)$	-0.02	-0.02	0.02	0.00	0.05	0.23	0.03	-0.11	-0.07	-0.04	-0.03	-0.01		0.21	0.10	0.13	0.00	-0.03
14	%EMPL_log(Age)	0.00	-0.05	-0.07	0.12	0.06	-0.01	0.05	-0.08	0.04	0.01	-0.03	-0.21	0.22		0.02	0.74	0.01	-0.09
15	CE0_Married	-0.01	0.00	-0.04	-0.05	0.01	0.00	0.04	-0.04	0.01	0.00	-0.05	-0.01	0.15	0.04		0.06	0.01	-0.02
16	%EMPL_Married	-0.02	-0.04	-0.12	-0.11	0.05	-0.03	0.04	-0.08	0.06	0.05	-0.05	-0.11	0.15	0.78	0.09		0.01	-0.13
17	CE0_CorrupIndex	0.00	0.01	0.02	0.00	0.00	0.02	0.00	0.01	0.01	0.02	0.01	0.04	-0.02	-0.04	-0.01	-0.02		0.03
18	%EMPL_CorrupIndex	0.01	0.02	0.05	0.01	0.02	0.01	0.08	0.05	0.02	0.01	0.06	0.15	-0.02	-0.11	-0.03	-0.09	0.29	
This	table presents pair-wise F	Pearson (below d	iagonal) a	and Spear	man (at	ove diag	tonal) co	rrelation	s. Correls	ations ab	ove 0.00	19 and be	low -0.(009 are s	ignificar	nt at the 0	05 level	(two-

tailed test). Accounting ratios are winsorized at the lower and upper 1% level. All variables are defined in Appendix A

Mean of variable:	CEOs		Employe	es	
	CEO _record	Bankrupt	%EMPL _record	Bankrupt	Bankrupt
Sample:		(CEO_record=1)		(% <i>EMPL_record></i> within year median)	(%EMPL-record> within year 80th percentile)
Offense	(1)	(2)	(3)	(4)	(5)
All	0.188	0.020	0.171	0.017	0.022
White-collar					
White-collar	0.111	0.022	0.057	0.017	0.021
Nonwhite-collar	0.102	0.020	0.139	0.017	0.022
White-collar types					
Fraud	0.054	0.025	0.039	0.017	0.022
Legal	0.007	0.028	0.007	0.015	0.018
Corporate	0.062	0.023	0.016	0.017	0.017
FBI NIBRS					
Person	0.029	0.022	0.042	0.016	0.021
Property	0.065	0.023	0.104	0.017	0.022
Society	0.022	0.021	0.037	0.016	0.021
Other	0.111	0.021	0.052	0.016	0.020
Seriousness					
Imprisonment	0.010	0.031	0.023	0.016	0.021
Suspended sentence	0.018	0.024	0.034	0.016	0.020
Other (e.g., fines)	0.160	0.019	0.114	0.017	0.020
On record					
Undisclosed	0.116	0.022	0.118	0.017	0.023
Disclosed	0.007	0.023	0.025	0.015	0.019
PostHire	0.065	0.016	0.028	0.014	0.014
Timing					
Before $t - 3$	0.176	0.020	0.151	0.017	0.021
After $t - 3$	0.023	0.030	0.037	0.016	0.021

Table 4 Criminal offense distribution and bankruptcy frequency by category of crime

This table shows the distribution of convictions per CEOs and employees. Column 1 shows the mean of firmyears in which a CEO has a criminal record pertaining to the respective crime category. Column 2 shows the mean bankruptcy frequency conditional on the CEO having a criminal record pertaining to the respective crime category. Column 3 shows the mean percentage of employees with criminal records pertaining to the respective crime category. Column 4 shows the mean bankruptcy frequency for firms with above within-year median *%EMPL_record* pertaining to the respective crime category. Column 5 shows the mean bankruptcy frequency for firms that belong to the highest within-year quintile of employees with criminal records (*%EMPL_record*) pertaining to the respective crime category

We map the crime codes used in the Danish Criminal Registers to the *White-collar* and *FBI NIBRS* categories reported in this table in Online Appendix C. The *Seriousness* variables denote the most serious penalty imposed on an individual. The *On record* categories denote whether a crime was disclosed on the certificate of criminal record at hiring (used by employers to screen criminal records). *PostHire* indicates that the first crime was committed after hiring. *Before* t - 3 (*After* t - 3) indicates that an individual had committed crime before (after) the end of year t - 3

Note that the total percentage of the *White-Collar*, *FBI NIBRS*, and *Timing* categories (for example, the sum of the percentages reported in column 1 regarding Person, Property, Society, and Other) differs from the percentage of record holders reported in Table 2, because one person can be involved in several criminal actions. For the *Seriousness* and *On Record* categories, the classifications are mutually exclusive

the personal variables (Specification B) and the criminal records of CEOs (Specification C) and employees (Specification D). For completeness, we also present our results excluding the personal variables (*Person variables*) (Specifications E and F). Within each specification, we compare the predictive accuracy of the estimations described above using area under the ROC curve (AUC) fit statistics. For each year *t*, we estimate the respective model and use the estimated coefficients to predict the out-of-sample bank-ruptcy likelihood for year t + 1.

We present the results in Table 6 with each of the three bankruptcy prediction models (BMR, Altman, and Ohlson). Compared to the specifications that include all the control variables (*ACC*, *Firm variables*, and *Person variables*), the AUC increases by 22–27 bps when we include the criminal records of CEOs and employees (Specification D versus B). The incremental AUC associated with employees' criminal records is only marginally statistically significant in the Beaver and Ohlson models and insignificant in the Altman model. Note that the AUC statistic decreases when we include personal variables (Specification B versus A), likely because of the inclusion of many low correlating estimators, leading to spurious patterns being picked up in the learning sample (Beaver et al. 2019). However, this decrease is only statistically significant in the Altman model.

When we exclude the personal variables from the estimation, the incremental AUC associated with CEOs' and employees' criminal records increases to 35–45 bps (Specification F versus A). With this specification, the criminal records of employees, incremental to the CEO's criminal record, significantly improve the out-of-sample prediction accuracy (Specification F versus E). In addition, we find that specifications that exclude the personal variables but include the criminal records of CEOs and employees (Specification F) outperform all other specifications.

The AUC improvements that we document are modest compared to related research. Gutierrez et al. (2020, Section 7.4) show an increase in the AUC of 110 bps by including the auditor's going-concern opinion, and Kallunki and Pyykkö (2013, Figs. 5 and 6) show increases in the AUC of 147–198 bps by including past payment defaults of the CEO and the firm's directors. However, even small increments matter to a firm's stakeholders. For example, Iyer et al. (2016) note that "a 0.01 (100 bps) improvement in the AUC is considered a noteworthy gain in the credit scoring industry" (p. 1565).

4.3 Types of employees

We then examine which employees' criminal records are associated with bankruptcy. First, we assess which *individuals* are associated with the bankruptcy likelihood. As reported in Online Appendix A, we find that only the criminal records of one person, the CEO, are significantly associated with the bankruptcy likelihood. The findings suggest that the CEO is unique, consistent with the conclusions of Bennedsen et al. (2020).

We then examine which *groups* of employees predict bankruptcies, besides the CEO. If, on the one hand, employee groups with the authority to make decisions do so based solely on their own traits (as measured by their criminal records), we would expect that only their

Table 5 Likelihood of bankruptcy estimation

	Dependent v Reported co N=103,774,	variable: <i>Banki</i> efficients: Mai π <i>Bankrupt</i> =	<i>rupt_t</i> ginal effects a 0.0130	t mean		
	BMR		Altman		Ohlson	
	(1)	(2)	(3)	(4)	(5)	(6)
Variables of interest						
CEO_record _t [H1a]	0.0049**	0.0047**	0.0046**	0.0045**	0.0047**	0.0045**
	(2.42)	(2.28)	(2.25)	(2.16)	(2.28)	(2.18)
% <i>EMPL_record</i> _t [H1b]	0.0287***	0.0230***	0.0267***	0.0210**	0.0270^{***}	0.0217***
	(3.87)	(2.75)	(3.58)	(2.50)	(3.63)	(2.58)
Personal variables						
$CEO_HighEduc_t$		-0.0003		-0.0003		-0.0004
		(-0.11)		(-0.08)		(-0.14)
%EMPL_HighEduc _t		-0.0244^{*}		-0.0223^{*}		-0.0205
		(-1.83)		(-1.66)		(-1.56)
CEO_Female_t		0.0017		0.0019		0.0016
		(0.41)		(0.47)		(0.39)
$%EMPL_Female_t$		-0.0017		-0.0018		-0.0025
		(-0.31)		(-0.31)		(-0.45)
$CEO_log(Age)_t$		0.0001		0.0007		-0.0012
		(0.03)		(0.13)		(-0.25)
$%EMPL_log(Age)_t$		0.0039		0.0067		0.0041
		(0.42)		(0.71)		(0.43)
$CEO_Married_t$		-0.0004		-0.0005		-0.0004
		(-0.17)		(-0.25)		(-0.18)
%EMPL_Married _t		-0.0104		-0.0104		-0.0096
		(-1.10)		(-1.10)		(-1.01)
CEO_CorrupIndex _t		0.0001		0.0001		0.0001
		(0.39)		(0.31)		(0.43)
%EMPL CorrupIndex _t		0.0001		0.0001		0.0001
		(0.50)		(0.57)		(0.43)
Extra firm variables						
EquityFirmOwner/TL _t	-0.0158***	-0.0160***	-0.0164***	-0.0166***	-0.0150***	-0.0151***
	(-4.35)	(-4.38)	(-4.32)	(-4.36)	(-4.22)	(-4.24)
EquityPersOwner/TL _t	-0.0146**	-0.0145**	-0.0137**	-0.0139**	-0.0145**	-0.0141**
	(-2.30)	(-2.29)	(-2.14)	(-2.16)	(-2.33)	(-2.25)
log (Employees) _t	-0.0028^{*}	-0.0028*	-0.0029^{*}	-0.0029*	-0.0017	-0.0021
· ·	(-1.88)	(-1.84)	(-1.92)	(-1.91)	(-0.86)	(-1.07)
StdROA _t	0.0008	0.0016	0.0061	0.0068	0.0048	0.0054
	(0.10)	(0.20)	(0.73)	(0.81)	(0.61)	(0.67)

1025	
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Table 5 (continued)

	Dependent v Reported co N = 103,774	variable: <i>Banki</i> efficients: Mai , π <i>Bankrupt</i> =	<i>rupt_t</i> rginal effects a = 0.0130	t mean		
	BMR		Altman		Ohlson	
	(1)	(2)	(3)	(4)	(5)	(6)
Prediction model va	riables					
$EBIT/TA_t$	0.0083	0.0079	-0.0343***	-0.0350^{***}		
	(0.86)	(0.81)	(-5.14)	(-5.22)		
TL/TA_t	0.0341***	0.0336***			0.0325***	0.0320***
	(7.45)	(7.28)			(4.76)	(4.67)
$EBITDA/TL_t$	-0.0429***	-0.0438***			-0.0248***	-0.0256***
	(-4.63)	(-4.68)			(-3.09)	(-3.16)
NWC/TA _t			-0.0019	-0.0018	-0.0023	-0.0023
			(-0.50)	(-0.47)	(-0.58)	(-0.56)
RE/TA_t			0.0019	0.0015		
			(0.29)	(0.24)		
BV/TL_t			-0.0280^{***}	-0.0274***		
			(-4.79)	(-4.68)		
GP/TA_t			-0.0000	-0.0002		
			(-0.02)	(-0.11)		
$Log (TA)_t$					-0.0011	-0.0006
					(-0.84)	(-0.44)
CL/CA_t					0.0006	0.0005
					(0.37)	(0.35)
NI/TA _t					0.0141	0.0132
					(1.38)	(1.28)
NITWO _t					0.0138***	0.0137***
					(5.57)	(5.53)
$OENEG_t$					-0.0032	-0.0031
					(-0.97)	(-0.94)
CHIN _t					-0.0032**	-0.0030**
-					(-2.20)	(-2.10)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R sq.	0.2255	0.2285	0.2200	0.2225	0.2431	0.2455
In-sample AUC	0.8890	0.8905	0.8849	0.8861	0.8948	0.8959

This table shows the results of estimating Eq. (1) and examines whether criminal records of CEOs and employees predict firm-level likelihood of bankruptcy. We estimate the regressions with a hazard estimation (Shumway 2001), which equals a logistic regression in which the chi-squared statistics are divided with the average number of years per firm. CEO_record indicates that the CEO has a prior criminal record. %EMPL_record is the percentage of employees with criminal records. Accounting ratios are winsorized at the lower and upper 1% level. All variables are defined in Appendix A. z statistics are in parentheses. ***, **, and * represent significance levels at 0.01, 0.05, and 0.10, respectively (two tailed test)

tests
of-sample
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Specification:	BMR						Altman						Ohlson					
	A (1)	B (2)	C (3)	D (4)	E (5)	F (6)	A (7)	B (8)	с (д	D (10)	E (11)	F (12)	A (13)	B (14)	C (15)	D (16)	E (17)	F (18)
Variables in estim	tion mode	, F																
ACC+ Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person		Yes	Yes	Yes				Yes	Yes	Yes				Yes	Yes	Yes		
CEO_record			Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes
%EMPL_record				Yes		Yes				Yes		Yes				Yes		Yes
Out-of-sample pred	liction acc	uracy																
AUC	0.8633	0.8614	0.8630	0.8641	0.8650	0.8678	0.8608	0.8571	0.8587	0.8593	0.8628	0.8643	0.8682	0.8667	0.8683	0.8691	0.8701	0.8722
Vs. Model A		B-A	C-A	D-A	E-A	F-A		B-A	C-A	D-A	E-A	F-A		B-A	C-A	D-A	E-A	F-A
Diff. (bps)		-19	-3	08	17	45		-37	-21	-15	20	35		-15	1	6	19	40
χ^2		1.55	0.03	0.21	5.98**	19.30***		6.44‡	1.82	0.92	8.55***	13.93***		1.28	0.01	0.43	9.88***	20.72***
Vs. Model B			C-B	D-B					C-B	D-B					C-B	D-B		
Diff. (bps)			16	27					16	22					16	24		
χ^2			7.25***	11.61***					8.44***	8.92***					9.51***	13.05***		
Vs. Model C				D-C						D-C						D-C		
Diff. (bps)				11						9						8		
χ^2				3.60^{*}						1.14						3.22*		

nued)	BMR
(conti	ation:
Table 6	Specific

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Specification:	BMR						Altmar						Ohlson					
	(1) A	B (2)	3 C	D (4)	E (2)	F (6)	A (j)	B (8)	Э (б	D (10)	E (11)	F (12)	A (13)	B (14)	C (15)	D (16)	E (17)	F (18)
Vs. Model E						F-E						F-E						F-E
Diff. (bps)						28						15						21
χ^2						9.89***						14.25***						9.00 ***
Vs. Model D						F-D						F-D						F-D
Diff. (bps)						37						50						31
χ^2						7.34***						13.68***						6.53**
This table show year $t + 1$. The tended variables the proportion of the proportion	s the ou able shc (BMR, f emplo	t-of-sam ws the A Altman, yees with	ple result Area Und and Ohls h crimina	s of rolling er the Cur son). <i>Firm</i> 1 records.	g estimatio ve (AUC) is the ext	on windov statistics ra <i>furm var</i> und * repre	/s. We es or the pr <i>iables</i> . P	stimate the edicted v erson is ufficance	ne models values. W the <i>perso</i> levels at	for year e present <i>n variable</i> 0.01, 0.05	<i>t</i> and use differences. CEO_	e the coeff es in basis <i>record</i> inc 0, respect	icient est s points fi licates a 1 ively (two	imates to or ease of record-ho o tailed te	predict t f interpre dder CEC est). The	he bankru tation. <i>AC</i>). <i>%EMP1</i> results of	ptcy likeli C is the p L-record i interest ar	hood for rediction measures e in bold

1029

records would predict bankruptcies. If, on the other hand, employee groups with decisionmaking authority are influenced by coworkers (i.e., are subject to peer effects), we would expect the criminal records of all employees to illuminate bankruptcy likelihood.

We use the salary received from the firm to identify employees with decisionmaking authority. For each firm-year, we sort the employees into quartiles based on their salaries and calculate the percentage with criminal records within each quartile. We then use the quartiles to re-estimate modified versions of Eq. (1) (one estimation for each quartile) and benchmark the prediction accuracies with our main model using all employees. To preserve space and avoid repetition, we report only the results using the Ohlson model and note, in the text below, cases where the results are sensitive to using the Altman and the Beaver models. We use the Ohlson model because it predicts the most accurately. Panel A of Table 7 shows the marginal effects at the mean for each quartile. The marginal effects of each quartile are statistically indifferent. Panel B shows the out-of-sample AUC. Our main model, based on all employees, outperforms each of the models based on salary quartiles.²⁴ We find similar results when we use job titles (non-CEO top managers) instead of salary to identify employees with decisionmaking authority (results reported in Online Appendix A). We interpret our results as consistent with the predictions regarding peer effects.

We cannot observe the corporate decisions by individuals in the company, as, for example, do Amir et al. (2014a) and Law and Mills (2019). However, we can observe individuals' new crime. To test whether the behavior of employees is consistent with the predicted peer effect, we identify a sample of job changers and examine whether their propensity to commit crime is associated with the percentage of employees with criminal records and the criminal record of the CEO at the new employer.²⁵ Table 8 shows that individuals are more likely to commit new crime when they start working in a company

²⁴ The differences are statistically significant at the 10% level. We find similar results for the BMR model. For the Altman model, we do not find any statistically significant differences for quarters four, three, and two.

²⁵ We estimate the propensity to commit new crime during the three years from year t + 1 to year t + 3 following a jobchange in year t as a function of CEO record and %EMPL record at the new employer in year t-1. The group and individual measures are for separate periods, to avoid the reflection problem described by Manski (1993). We additionally control for other personal characteristics that could influence the propensity to commit crime. There could be timing issues with respect to (1) the time between commitment of a crime and a judicial decision and (2) the time between treatment (job-change) and new crime. Regarding (1), we contacted the Danish attorney general and obtained data from the police's case management system for the period 2014-2020. The average period between when the police open a case to the legal decision is 15.2 months across all types of crime (excluding traffic-related cases). For fines determined in court, the average is lower, at 12.1 months. Extreme cases with long processing times potentially influence these averages. We also obtained data from the Danish courts on processing times. For the first half of 2020, the average period between the Prosecution Service's pressing charges and the legal decision was around 0.5 months for fines, 3.0 months for confession cases, and 5.1–5.4 months for cases with and without jury members, respectively. As our treatment (job-change) happens in year t and we then observe new crime (legal decisions) beginning in year t + t1, we naturally have a lag between treatment and outcome. Regarding (2), we follow Dimmock et al. (2018), who observe new misconduct in a three-year window beginning 100 days after the merger (the treatment). In their online appendix, they show that their conclusions are similar if they use windows of one, two, five, or 10 years. In untabulated tests, we find (1) consistent results when we use new crime in year t + 1, t + 2, or t + 3 as our dependent variable (one estimation at a time). These estimations help us avoid criminal cases that were committed during the prior employment, because we extend the period between treatment (new employment) and outcome (new crime). (2) Our results are also robust to using Year × Municipality fixed effects to control for differences in enforcement and other geographical differences across municipalities. (3) Our results hold for employees across the within-firm-year income distribution. Specifically, we condition by within-firm-year salary quartiles, based on salary received from the new employer in year t + 1.

Table 7 Types of employees



This table examines whether different types of employee groups predict bankruptcies. The results are based on estimating modified versions of Eq. (1) replacing *%EMPL_record* with the percentage of employees with criminal records for each within-firm-year salary quartile (denoted Q4, Q3, Q2, and Q1). Q4 (Q1) is the percentage of employees whose salary belongs to the highest (lowest) within-firm-year quartile with criminal records. Panel A graphs estimates for the marginal effects at the mean for the quartiles, as well as the 95% confidence intervals. Panel B graphs the out-of-sample AUC using the estimation all employees (bar 1) and the quartile in question (bars 2–5)

where more employees have criminal records, consistent with employees being subject to peer effects. This relation holds for both employees with and without prior records.

4.4 Types of crime

We condition our results by several types of crime. For each type of crime, we reestimate Eq. (1) including only the type of crime in question. As in Section 4.3 above, we report only the results using the Ohlson model and note, in the text below, cases where the results are sensitive to using the Altman and the Beaver models. We present all results in Table 9. We describe the different types of crime and the results regarding the out-of-sample AUC below. We do not find that any of the marginal effects of *CEO_record* and *%EMPL_record*, using only the type of crime in question, differ significantly from the marginal effects reported in our main analysis (Table 5). The marginal effects of some crime types are, however, insignificantly different from zero, possibly because these crimes are rare. For example, the marginal effects of both *CEO_record* and *%EMPL_record* for crimes against society are insignificant at the 10% level. Just 2.2% (3.7%) of CEOs (employees) have committed such crimes.

Nature of crime We conduct our tests regarding the nature of crime using two FBI classification systems: (1) white-collar crime and its subcategories (fraud, corporate, legal), and (2) National Incident-Based Reporting System (NIBRS) classifications, which separate offenses into crime against persons, property, society, and other. The AUC of our main analysis is statistically larger than the AUC of any crime category except white-collar crime and fraud (a subcategory to white-collar crime). That is, these categories predict firm bankruptcies as well as the use of all crimes does.

We analyze, in more detail, the out-of-sample AUC for white-collar crime and its subcategories. The AUC is statistically larger for white-collar crime than for nonwhite-collar crime (p value <0.01). Within the category of white-collar crime, we find that the AUC

of fraud is statistically higher than that of corporate and legal (p values in the range 0.02–0.03). Using the Altman model, the AUC of fraud is not larger than that of corporate white-collar crimes (p value = 0.12). The results provide some evidence that our main results are driven by employees whose criminal records pertain to white-collar crime, specifically fraud.

Nonwhite-collar crimes still predict bankruptcies, although less accurately. Moreover, the measures of the employees' criminal records pertaining to different types of crimes are highly correlated, which impedes disentangling the effects of each type of crime.²⁶

Severity of crime We condition individuals' criminal records by the most serious crime on the record and determine the severity based on whether the crime is penalized by imprisonment, suspended sentences, and other outcomes. Other outcomes include mainly fines but also diversion or deferred adjudications, military penalties, treatment sentences, and other penalties, as well as dismissals and acquittals. Our results show that the marginal effects of CEO record and %EMPL record using serious crimes are statistically indistinguishable from those reported in our main analysis (Table 5). This is consistent with the findings of Law and Mills (Law and Mills 2019, Table 6.1 of the online appendix), who also observe coefficients that are statistically indistinguishable across the seriousness of crimes. We do not find that serious crime leads to better prediction accuracy. In contrast, we find that nonserious crime (captured by the "other" category) leads to the largest prediction accuracy. This is expected, because 2.7% of white-collar crimes lead to imprisonment, compared to 8.0% of all crimes in our dataset. Nonserious crime hence overlaps with white-collar crime—the type of crime that leads to the largest prediction accuracy—as described above. In addition, the majority of the criminal records include only nonserious crime. On the firm-year level, 85% (67%) of the CEOs' (employees') records do not include prior prison time or suspended sentences.

Disclosed and undisclosed crime We condition by crime disclosed on the certificate of criminal records at hiring (individuals for which at least one offense appears on the certificate), undisclosed crime (individuals with prior criminal actions for which no offenses appear on the certificate), and crime committed following a hire (individuals who had not committed any crime before the employment). We find that undisclosed crime leads to the highest prediction accuracy, as measured by the out-of-sample AUC. This is consistent with the notion that crime is an observable outcome of a trait and persists throughout life, as proposed by Gottfredson and Hirschi (1990). Moreover, this is consistent with the fact that matching of firms not conducting background checks (arguably a special type of firm) with record-holder employees does not drive our results.

 $^{^{26}}$ For example, the correlation between the percentage of employees with a record of white-collar crime and nonwhite-collar crime is 0.57. Forty-four percent of the individuals in our sample with white-collar criminal records have committed other crimes.

	Reported coefficients: Marginal effects at mean Total sample: All new employments							
Sample:	Total sample	No prior record	With prior record	Total sample	No prior record	With prior record		
	(1)	(2)	(3)	(4)	(5)	(6)		
CEO_record _{arrival, t-1}	0.0033***	0.0033***	0.0040**	0.0015	0.0038***	-0.0038		
	(4.28)	(5.34)	(2.13)	(1.44)	(3.27)	(-1.35)		
%EMPL_record _{arrival, t-1}	0.1151***	0.0766***	0.1666***	0.0750***	0.0497***	0.1274***		
	(44.26)	(28.57)	(34.42)	(20.49)	(15.28)	(14.67)		
CEO_record _{departure, t-1}				0.0024**	0.0021**	0.0061		
				(2.46)	(2.34)	(1.44)		
%EMPL_record _{departure, t-1}				0.0825***	0.0450***	0.1017***		
				(13.15)	(7.32)	(9.85)		
log (Age) _t	-0.0703***	-0.0451***	-0.2701***	-0.0924***	-0.0527***	-0.2653***		
	(-19.23)	(-18.67)	(-27.61)	(-19.99)	(-15.82)	(-23.17)		
Female _t	-0.0767***	-0.0479***	-0.1034***	-0.0766***	-0.0437***	-0.1016***		
	(-35.37)	(-30.66)	(-33.24)	(-29.99)	(-25.55)	(-17.10)		
Married _t	-0.0224***	-0.0135***	-0.0334***	-0.0210***	-0.0119***	-0.0400***		
	(-23.37)	(-15.32)	(-20.39)	(-17.95)	(-10.44)	(-13.29)		
HighEduc _t	-0.0538***	-0.0328***	-0.0904***	-0.0413***	-0.0228***	-0.0626***		
	(-19.90)	(-21.68)	(-8.44)	(-7.57)	(-7.23)	(-3.05)		
CorruptionIndex _t	0.0301***	0.0204***	0.0431***	0.0254***	0.0157***	0.0406***		
	(14.92)	(12.45)	(13.49)	(9.95)	(7.93)	(7.71)		
$Personal Equity_t$	-0.2025***	-0.1255***	-0.2945***	-0.1715***	-0.0824***	-0.3101***		
	(-19.76)	(-15.56)	(-7.18)	(-13.59)	(-7.11)	(-8.10)		
$#Convictions_t$	0.0137***		0.0210***	0.0131***		0.0203***		
	(28.88)		(30.58)	(34.72)		(25.67)		
Year FE	Yes	Yes	Yes	Yes	Yes	Yes		
Ν	1,462,962	1,178,428	284,534	318,144	245,224	72,920		
Pseudo R sq.	0.1646	0.0959	0.1400	0.1819	0.1024	0.1416		
In-sample AUC	0.8033	0.7556	0.7596	0.8152	0.7635	0.7616		
π NewCrime t+1 to t+3	0.0656	0.0371	0.1836	0.0654	0.0337	0.1721		

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lable 8	New	employments	and	propensity	to	commit ne	w crime
				F - F			

Dependent variable: NowCrime

The table examines whether the new colleagues of individuals are associated with the propensity to commit new crime. The sample comprises job-changers. The estimations are carried out on the individual level. The dependent variable, NewCrime t+ 1 to t+ 3 indicates that an individual is subject to a criminal decision during the three-year period starting in year t + 1. CEO_record_{arrival} indicates that the CEO of the arrival company (the new employer) has a criminal record. %EMPL_record_{arrival} measures the proportion of employees with criminal records at the arrival company. CEO_record_departure indicates that the CEO of the departure company (the former employer) has a criminal record. %EMPL_record_departure measures the proportion of employees with criminal records at the departure company. Log (Age) is the logarithm of the age measured in years. Female indicates the gender. Married indicates marriage. HighEduc indicates a bachelor's degree or higher. CorruptionIndex is the average of Transparency International's corruption perception indexes for the years 1995–2018 multiplied by -1. PersonalEquity measures a person's equity in EUR million, calculated with data from the registers at Statistics Denmark. We note that private company stocks are not included in the personal equity calculation. This variable is winsorized at the 1st and 99th percentile. #Convictions denote the number of prior convictions. The results are based on logistic regressions with standard errors clustered by individual and year. Year fixed effects are estimated but for brevity not reported. The z-statistics are in parentheses. ***, **, and * represent significance levels at 0.01, 0.05, and 0.10, respectively (two tailed test)

Timing We partition on crime committed before the end of year t - 3 (indicates that an individual had a criminal record at the end of year t - 3) and crime committed after the end of year t - 3 (indicates that an individual had committed crime after the end of year t - 3 and before the end of year t). We find that both types of criminal records are significantly associated with bankruptcy (*%EMPL_record* is significantly different from zero). However, using criminal records before the end of year t - 3 leads to a significantly larger AUC. This is consistent with our results regarding disclosed and undisclosed crime; that is, crime is an observable outcome of a trait.



White collar

Nonwhite collar

All White collar White coll. type

Fraud

Legal

Corporate



This table examines whether different types of crime predict bankruptcies. The results are based on estimating modified versions of Eq. (1) using only the type of crime in question. Panels A and B graph estimates for the marginal effects at the mean as well as the 95% confidence intervals for CEOs and employees, respectively. Panel C graphs the out-of-sample AUC using the type of crime in question in the estimation

FBI NIBRS

Other

Other

Suspended sent.

Seriousness

Before t-3

Timing

Disclosed

On record at hire

4.4.1 Bankruptcy prediction: Additional tests

We perform several additional tests. In the following, we briefly describe each of these tests as well as the results. The Online Appendix B elaborates on results of these additional tests.

- *Subsample analyses.* We find some evidence that our results are concentrated among small firms with poor governance and among firms with a CEO with no criminal record.
- *Financial performance*. Using data envelopment analysis (Demerjian et al. 2012), we do not find that criminal records of CEOs and employees are associated with better firm efficiency on average (consistent with Law and Mills 2019). However, criminal records of CEOs and employees are positively associated with firms' likelihood of wining the "Gazelle Prize," which is awarded to successful, fast-growing firms. This indicates that criminal records correlate with extreme right-skewed outcomes (e.g., Levine and Rubinstein 2017).²⁷
- Changes in employees. Current changes in the percentage of employees with criminal records positively predict future changes in investments, growth, and debt. (We assess three periods: year t 1 to year t, t + 1, and t + 2.) An increase in these variables should indicate an increase in the firm risk, suggesting that the criminal records of employees convey information that manifests later in the accounting figures. In addition, we find some evidence that changes in the percentage of employees with criminal records predict bankruptcies, although this only holds for changes over three years (p value = 0.07).
- Long-term prediction. We explore the extent to which information about criminal records of CEOs and employees helps predict bankruptcies for longer horizons. Using all the control variables, we do not find that criminal records significantly predict bankruptcies at extended horizons. In specifications without the person-specific control variables, criminal records of employees predict bankruptcies when we extend the horizon by up to three years. (At horizons extended by two or more years, the results are only marginally significant at the 10% level.) Criminal records of CEOs marginally predict bankruptcies when we extend the prediction horizon by one year (marginally significant at the 10% level). They lose their predictive power for longer horizons.
- *Propensity-score matching*. Our main findings are robust to using propensityscore matching, where we match bankrupt firms with nonbankrupt firms using all the control variables. This suggests that bankrupt firms' being significantly different on these variables from nonbankrupt firms does not drive our results. We find similar results when we match on having a recordholder CEO or having a large proportion of employees with criminal records (above within-year median %*EMPL_record*).

²⁷ Other countries than Denmark award similar prices for high growth companies or study the "gazelles." See for instance González-Uribe and Reyes (2021), https://growingbusinessawards.co.uk/ (UK example), and https://www.ft.com/content/8b37a92b-15e6-4b9c-8427-315a8b5f4332 (European example).

Table 10 Cost of debt

	Dependent varia	able: CoD_{t+1}		
	Cross-sectional		Within-firm	
	(1)	(2)	(3)	(4)
Variables of interest				
CEO_record _t [H2a]	0.0018***	0.0014***	-0.0002	-0.0003
	(5.85)	(4.33)	(-0.57)	(-0.69)
%EMPL_record _t [H2b]	0.0011	0.0035**	0.0031	0.0039**
	(0.84)	(2.67)	(1.77)	(2.28)
Personal variables				
CEO_HighEduct		-0.0001		0.0005
		(-0.29)		(1.04)
%EMPL_HighEduct		-0.0118***		0.0048
		(-6.48)		(1.77)
CEO_Female_t		-0.0012^{*}		-0.0001
		(-1.96)		(-0.16)
$%EMPL_Female_t$		0.0056***		0.0032^{*}
		(7.28)		(1.86)
$CEO_log(Age)_t$		0.0013		0.0017**
		(1.74)		(2.40)
$%EMPL_log(Age)_t$		-0.0049***		-0.0046**
		(-3.85)		(-2.93)
$CEO_Married_t$		-0.0001		0.0001
		(-0.23)		(0.48)
%EMPL_Married _t		-0.0062***		0.0012
		(-5.11)		(0.87)
CEO_CorrupIndex _t		0.0000		0.0001
		(1.09)		(1.68)
%EMPL_CorrupIndex _t		0.0001*		0.0000
		(2.04)		(0.21)
Extra firm variables				
EquityFirmOwner/TL _t	-0.0005***	-0.0005***	-0.0001**	-0.0001^{**}
	(-9.21)	(-9.16)	(-2.68)	(-2.75)
EquityPersOwner/TL _t	-0.0027^{***}	-0.0028^{***}	-0.0009^{*}	-0.0011^{**}
	(-6.52)	(-6.90)	(-1.96)	(-2.42)
$log (Employees)_t$	-0.0045***	-0.0053***	-0.0014**	-0.0015***
	(-13.97)	(-16.62)	(-2.83)	(-3.07)
StdROA _t	-0.0026	-0.0021	0.0019	0.0020
	(-1.54)	(-1.26)	(0.97)	(0.99)
Ohlson model variables				
NWC/TA _t	0.0095***	0.0103***	0.0034***	0.0034***
	(10.65)	(11.62)	(4.49)	(4.48)

	Dependent variable: CoD_{t+1}						
	Cross-sectional		Within-firm				
	(1)	(2)	(3)	(4)			
TL/TA _t	0.0218***	0.0218***	0.0107***	0.0106***			
	(16.74)	(16.99)	(7.38)	(7.44)			
$Log (TA)_t$	0.0014***	0.0021***	0.0004	0.0004			
	(5.16)	(8.36)	(1.11)	(1.02)			
CL/CA_t	0.0022***	0.0019***	0.0016***	0.0016***			
	(6.88)	(6.08)	(5.08)	(5.11)			
NI/TA_t	-0.0181***	-0.0190***	-0.0079***	-0.0079***			
	(-11.85)	(-13.10)	(-6.35)	(-6.23)			
$EBITDA/TL_t$	0.0048^{***}	0.0048***	-0.0002	-0.0002			
	(8.21)	(8.45)	(-0.37)	(-0.35)			
NITWO _t	0.0032***	0.0033***	0.0017***	0.0017***			
	(9.59)	(9.66)	(7.00)	(7.04)			
$OENEG_t$	-0.0072^{***}	-0.0072^{***}	-0.0028^{***}	-0.0028^{***}			
	(-8.22)	(-8.40)	(-3.93)	(-3.97)			
CHIN _t	-0.0001	-0.0000	-0.0002	-0.0002			
	(-0.35)	(-0.05)	(-1.42)	(-1.41)			
Industry FE	Yes	Yes	No	No			
Year FE	Yes	Yes	Yes	Yes			
Firm FE	No	No	Yes	Yes			
Ν	85,757	85,757	85,757	85,757			
Adjusted R sq.	0.1809	0.1902	0.5546	0.5548			

Table 10 (continued)

This table shows the results of estimating Eq. (2) and examines whether criminal records of CEOs and employees are associated with the cost of debt. The dependent variable, *CoD*, is financial expenses scaled by average total liabilities net of trade payables. *CEO_record* indicates that the CEO has a prior criminal record. $%EMPL_record$ is the percentage of employees with criminal records. Columns 1 and 2 are cross-sectional regressions with industry and year fixed effects. Columns 3 and 4 are panel data regressions with firm and year fixed effects. Accounting ratios are winsorized at the lower and upper 1% level. All variables are defined in Appendix A. Standard errors are clustered by firm and year. *t* statistics are in parentheses. ***, **, and * represent significance levels at 0.01, 0.05, and 0.10, respectively (two tailed test)

4.5 Cost of debt

After having examined whether criminal records of CEOs and employees provide information about the likelihood of future bankruptcy, we turn to examine the consequences for the cost of debt.

We estimate Eq. (2) with OLS and cluster standard errors by firm and year (Gow et al. 2010) as follows.

$$CoD_{it+1} = \alpha_0 + \beta_1 CEO_record_{it} + \beta_2 \% EMPL_record_{it} + \beta_3 ACC_{it} + \beta_4 Firm variables_{it} + \beta_5 Person variables_{it} + \varepsilon_{it+1}$$
(2)

for firm *i* in year *t*. *CoD* is the cost of debt, which we measure with the interest rate. *CoD* is calculated as financial expenses scaled by average total liabilities net of trade payables (described further in Section 3.2.3). As with the bankruptcy prediction estimations, *ACC* represents accounting-based variables that are used to predict bankruptcy in the BMR, Altman, and Ohlson models. *Firm variables* are the extra firm controls, and *Person variables* are the controls for personal characteristics. That is, we apply the variables used to predict bankruptcy as control variables in our estimation of cost of debt, and thus estimate Eq. (2) with three sets of *ACC* control variables (one model at a time). We estimate the models with either of two fixed effects specifications: (1) year and industry fixed effects or (2) year and firm fixed effects (Gormley and Matsa 2014).

We present the estimation results in Table 10. For brevity and to avoid repetition, we report only the results using the Ohlson ACC variables and note, in the text, whether our results using the BMR or the Altman model provide different inferences. In our cross-sectional tests in columns 1 and 2, we find that firms whose CEOs have criminal records (CEO record) experience higher interest rates, consistent with H2a. Economically, these CEOs pay higher interest rates of 14-18 bps, corresponding to about 3.5%–4.5% of the unconditional sample mean. In the cross-sectional estimations, we find mixed evidence with respect to the criminal records of employees. In column 1, where we exclude the personal control variables, we do not find that the criminal records of employees are significantly associated with the interest rate. (Using the Altman model, this coefficient estimate is positive and marginally significant, p value = 0.065.) When we include these controls, however, we find that firms where more employees have criminal records pay a higher interest rate, which provides some evidence for H2b.28 The economic magnitude is small. A one standard deviation change in %EMPL record is associated with a higher interest rate of about 4 bps (1% of the unconditional sample mean).

Columns 3 and 4 estimate Eq. (2) with firm fixed effects to test whether the associations above are mainly driven by cross-sectional variation in *CEO_record* and *%EMPL_record*. In these estimations, *CEO_record* does not explain variation in the firm's interest rate. Few of the sample firms experience CEO turnover during our 13-year sample period, and even fewer change from a criminal CEO to a noncriminal CEO or vice versa. Only 1164 firms (7.4% of all sample firms or about 1.5% of the firm-year observations) experience the latter type of CEO turnover. We acknowledge that our tests have low power due to the rarity of CEO turnover. The results regarding *%EMPL_record* are comparable to the cross-sectional estimations.

Collectively, these findings provide some evidence that lenders require a higher cost of debt, in the form of higher interest rates, when lending to firms with a record-holder CEO and firms where a high proportion of employees have criminal records. Although

 $^{^{28}}$ In untabulated tests, we augment the estimations with the control variables used by Minnis (2011). In these cross-sectional estimations, the criminal records of employees are not significantly associated with the interest rate.

our estimate of cost of debt is a noisy proxy, we generally find that the control variables predictably relate to cost of debt.²⁹

5 Conclusion

This paper examines whether criminal records of CEOs and employees provide information about private firms' likelihood of bankruptcy and cost of debt. We conclude that firms whose CEOs have criminal records and firms where more employees have criminal records exhibit a higher likelihood of bankruptcy. We find some evidence that lenders price these criminal records, although these results are sensitive to different specifications. The likelihood of bankruptcy increases by 45–47 bps (35%–36% of sample mean) when a firm has a CEO with a criminal record and by 26–28 bps (20%–22% of sample mean) when the proportion of employees with criminal records increases by one standard deviation. Lenders require higher interest rates of 14–18 bps (3.5%–4.5% of sample mean) when firms have a criminal CEO and 4 bps (1% of sample mean) when the proportion of employees with criminal records increases by one standard deviation.

Our main contribution is to show that the traits of employees predict firm outcomes. First, we contribute to the literature by using a direct measure of employee traits instead of relying on measures based on the populations surrounding firms' headquarters.³⁰ Second, we contribute to the literature that examines effects of employees with criminal records by (1) providing evidence consistent with predictions of peer effects, hence illuminating one way employees can influence firm outcomes,³¹ and (2) showing that the risk of hiring employees with criminal records permeates a large countrywide sample of firms in many different industries and is not isolated to the financial industry.³² Finally, we contribute to the literature on top managers' criminal records and firm outcomes by documenting that criminal records of employees are associated with firm outcomes in a way that is incremental to the criminal records of CEOs and other top managers.³³

We caution the reader to interpret the results with care. First, our cost of debt estimations pertain to the interest rate only. We thus cannot rule out that criminal records of CEOs and employees are associated with nonprice loan terms, such as collateral, the use of covenants, and the length of the loan. Second, the criminal record data do not cover criminal actions before 1980, crimes committed outside Denmark, or crimes committed by a person without a Danish personal identification number (persons who were not born in and never resided in Denmark). However, these data limitations bias against our results because some individuals are classified as noncriminals when they may have

²⁹ Due to multicollinearity, some coefficients are either unexpectedly insignificant or with a wrong sign. For example, in contrast to expectations, *EBITDA/TL* is positively associated with *CoD*. This is due to multicollinearity between the independent variables. *EBITDA/TL* is highly correlated with *NI/TA* (correlation of 0.758, Pearson) and *NITWO* (-0.422, Pearson). When we re-estimate the regression without *NI/TA* and *NITWO*, the untabulated results show that the variable *EBITDA/TL* is (as expected) negatively related to *CoD*, and the relation is statistically significant with a *p* value <0.01.

³⁰ See McGuire et al. (2012), Dyreng et al. (2012), Call et al. (2017), Christensen et al. (2018), and Beck et al. (2018)

³¹ This complements the research by Amir et al. (2014a) and Law and Mills (2019).

³² See, e.g., Law and Mills (2019) and Honigsberg and Jacob (2021).

³³ See, e.g., Amir et al. (2014b), Davidson et al. (2015, 2020), and Kallunki et al. (2018).

criminal records not covered by our dataset. Finally, we cannot rule out concerns about endogeneity, although we conduct tests to address this concern. Despite these limitations, our results document that the criminal records of CEOs and employees provide information about a firm's risk.

Appendix A Variable definitions

Variable	Definition
Dependent variabl	es and variables of interest
Bankrupt	<i>Bankrupt</i> is an indicator variable that takes the value one for the last annual report published preceding a bankruptcy notice, and zero otherwise
CoD	Approximation for a firm's interest rate. Financial expenses to average debt net of trade payables. $CoD_t = \frac{Financial expenses_t}{(Debt_t+Debt_{t-1})}/2$ Where Debt=Total Liabilities - Trade Payables CoD is truncated at the 5th and 95th percentiles. CoD is truncated for observations larger than 10 percentage points above the interest rate of Danish government bonds.
CEO_record	<i>CEO_record</i> is an indicator variable that takes the value one if the CEO has a prior criminal record (traffic related offenses excluded), and zero otherwise.
CEO_conv	The CEO's number of prior convictions
%EMPL_record	%EMPL_record measures the percentage of the employees with criminal records.
Record	On the individual level, <i>Record</i> takes the value one if an individual has a prior criminal record and zero otherwise.
ACC variables. Ac BMR, Altman, and	counting variables used by the bankruptcy prediction models. <i>Ohlson</i> in brackets indicates that this prediction model uses the variable.
EBIT/TA (BMR, Altman)	Earnings Before Interest and Tax _t Total Assets _{t-1}

EBIT/TA (BMR, Altman)	$\frac{Earnings}{Total} \frac{Before Interest and Tax_t}{Total Assets_{t-1}}$
TL/TA (BMR, Ohlson)	<u>Total Liabilities</u> Total Assets,
EBITDA/TL (BMR, Ohlson)	Earnings Before Interest, Tax, Depreciation, and Amortization _t Total Liabilities _t
NWC/TA (Altman, Ohlson)	$\frac{NWC_t}{Total \ Assets_t}$
	Where
	NWC=Net Working Capital=WCA-WCL
	Where
	WCA=Working Capital Assets
	=Current Assets
	-cash and cash equivalents
	-properties held for sale
	-receivables from closely related parties
	WCL=Working Capital Liabilities
	=Current Liabilities
	-current part of mortgage
	-current part of bank debt

Variable	Definition
	-liabilities to closely related parties -dividends if included in current liabilities
RE/TA (Altman)	$\frac{Returned Eurings_{t}}{Total Assets_{t}}$
BV/TL (Altman)	$\frac{Book \ Value \ of \ Total \ Equity_t}{Total \ Liabilities_t}$
GP/TA (Altman)	Gross Profit _t Total Assets _{t-1}
	Due to disclosure exemption rules, private firms below specific size benchmarks are not required to publicly disclose revenues, and therefore we use gross profits instead of revenues.
Log (TA) (Ohlson)	Measure of size. The logarithm of total assets. Ohlson (1980) uses a related size variable, calculated as <i>log (total assets/price level index)</i> . In untabulated analyses, we substitute <i>Log (TA)</i> with Ohlson's size variable and find largely unchanged slope coefficient and standard errors.
CL/CA (Ohlson)	<u>Current Liabilities</u> _t Current Assets _t
NITWO (Ohlson)	<i>NITWO</i> is an indicator variable that takes the value one if the sum of current year's earnings and last year's earnings is below zero, and zero otherwise.
OENEG (Ohlson)	<i>OENEG</i> is an indicator variable that takes the value one if the equity is negative $(TL/TA > 1)$, and zero otherwise.
CHIN (Ohlson)	$\frac{\Delta Net \ Income_{t-1}}{ Net \ Income_{t-1} } + Net \ Income_t $
Firm variables	
EquityFirmOwner/TL	<i>EquityFirmOwner/TL</i> measures the equity of the parent company, for example a holding company, if the parent company does not report on consolidated basis (observations for these subsidiaries are not included in the sample), scaled by the firm's total liabilities.
EquityPersOwner/TL	<i>EquityPersOwner/TL</i> measures the equity of the personal owner scaled by the firm's total liabilities. We define an individual as a personal owner of the firm if (1) he/she owns 100% of the company, or (2) ownership data are not available, but he/she is the founder of the company. We get the data from the registers at Statistics Denmark and note that private company stocks are not included in the personal equity calculation.
Log (Employees)	Log (Employees) is the logarithm of the number of full-time equivalent employees.
StdROA	<i>StdROA</i> is the standard deviation of <i>ROA</i> , using the last five years' annual reports, requiring observations for at least two years.
Person variables	
CEO_HighEduc	<i>CEO_HighEduc</i> is an indicator variable that takes the value one if the CEO has a bachelor's degree or higher, and zero otherwise.
%EMPL_HighEduc	%EMPL_HighEduc measures the percentage of the employees with a bachelor's degree or higher.
CEO_Female	<i>CEO_Female</i> is an indicator variable that takes the value one if the CEO is a female, and zero otherwise.
%EMPL_Female	%EMPL_Female measures the percentage of female employees.
CEO_log(Age)	CEO_log(Age) is the logarithm of the CEO's age in years.
%EMPL_log(Age)	%EMPL_log(Age) measures the logarithm of the average age in years of the employees.
CEO_Married	<i>CEO_Married</i> is an indicator variable that takes the value one if the CEO is married, and zero otherwise.

Variable	Definition
%EMPL_Married	%EMPL_Married measures the percentage of the employees who are married.
CEO_CorrupIndex	 CEO_CorrupIndex is the CorruptionIndex of the CEO's country of ancestry. CorruptionIndex is the average of Transparency International's corruption perception indexes for the years 1995–2018 multiplied by -1. Our data on ancestry country cover only two generations back in time. That is, the individuals in our dataset are classified as foreigners only if they or their parents are immigrants.
%EMPL_CorrupIndex	 %EMPL_CorrupIndex measures the average CorruptionIndex of the employees. CorruptionIndex is the average of Transparency International's corruption perception indexes for the years 1995–2018 multiplied by -1. Our data on ancestry country cover only two generations back in time. That is, the individuals in our dataset are classified as foreigners only if they or their parents are immigrants.
Other variables	
TA	Total Assets.
Firm age	Firm age in years since incorporation.
Headcount	Headcount. <i>Headcount</i> measures the number of non-CEO persons who have received salary from the firm during the year. The measure does not distinguish between full time and part time workers.
Employees	Full-time equivalent employees.

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Code availability The Stata code of this study is written in a secured environment on the servers of Statistics Denmark. Upon request, the authors are willing to provide code to researchers with access to this data.

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Data availability This study employs proprietary registry data from Statistics Denmark on the firm and the personal level. The use of confidential data prohibits the authors from sharing the data and conveying microdata such as minimum and maximum values. Sharing restrictions are comparable to studies that use BITS data.

Declarations

Conflict of interest Morten has no conflicts of interest to disclose. Kasper has received funding from Nordea, a bank, for his PhD studies. Nordea has not influenced the contents of this paper.

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