ESSAYS ON CORPORATE DISTRESS AND BANKRUPTCY

by

Ibrahim Pelja

Dissertation submitted to the

Department of Accounting, Auditing and Law

NHH Norwegian School of Economics

in partial fulfillment of the requirements for the Ph.D. degree

May 2022

ACKNOWLEDGEMENTS

First and foremost, I want to thank my supervisor, Finn Kinserdal, for all his help throughout my PhD. Your feedback, advice, guidance, and our many discussions have been invaluable. Special thanks for helping me keep an eye on the goal line over the last four years while enabling my many side-projects – I know it cannot have been easy. I also want to thank my co-supervisors Frøystein Gjesdal and Maria Correia for the time spent reading and commenting on my work and for your insightful guidance. It has undoubtably made me a better researcher. To my coauthor Anna Gold, thank you for collaborating with me on our project, your experience has been irreplaceable.

The papers in this thesis have benefited greatly from the comments received across various conferences and seminar. Special thanks to Thomas Plenborg for his valuable feedback at my midway evaluation; Aasmund Eilifsen for his constructive comments during the PhD day; Zhifang Zhang and her insightful discussion at the Financial Accounting and Auditing Workshop; and Liwei Zhu, my excellent discussant at the Nordic Accounting Conference. I also appreciate the many comments I received during the doctoral colloquium at the Nordic Accounting Conference in 2018; the parallel sessions at FIBE 2020 and the Nordic Accounting Conference in 2021; and across the many brown bag seminars at NHH.

To my esteemed colleagues, many thanks for making work a fantastic place to be. Particular thanks go out to the other PhD students that went through much of this journey with me, Andreas, Dan-Richard, David, Hussnain, Joel, Kasper, Kyrre, Mikael, Saad, and Shrey, you made the last four years both enjoyable and enlightening. Thank you Katarina Kaarbøe for being a supporting PhD coordinator that is always looking out for the wellbeing of the PhD students. My sincerest gratitude also goes out to the administration at the department, particularly Maren Dale-Raknes and Ingrid Cecilia Sæthre, you more than made up for my inherent lack of organization. Special thanks to Aksel Mjøs and SNF, the data provided by you has been the basis for the third paper in this thesis as well as multiple other projects.

To my parents, refugees that left their old life behind and started over in a new country to give their children the best life possible, I owe everything. I would *literally* not be here without your sacrifice. Thanks to my little sister, the first student I ever taught. To my friends, thank you for the joy you bring. Finally, I want to give my most heartfelt thanks to Kristine, who will likely be my wife by the time I defend this thesis. You have been a constant source of happiness throughout my PhD, always listening to my problems, ideas, and ramblings. Thank you for being there for me, I am happy I could share this experience with you.

CONTENT

INTRODUCTION

In this PhD dissertation, I explore topics relating to corporate distress and bankruptcy. Importantly, this includes the prediction and consequences of distress and bankruptcy. The thesis consist of three individual research papers: one literature review of distress prediction studies and two archival studies; one paper on the long-term predictability of auditors' going concern opinions (GCOs) and one paper on how disclosure of bankruptcy experience affects cost of debt. In the following, I provide a general introduction to corporate distress and bankruptcy before briefly summarizing the studies included in this thesis and the methodology.

1. Corporate distress and motivation for my research

Although the concept of distress is purposefully broad and vague, it generally relates to a company's (in)ability to meet ones obligations when due (Beaver, Correia and, McNichols 2011). Distress is often used as a catchall term to refer to various levels of financial struggle, all of which have different characteristics, such as omittance or reduction in dividend, default, bankruptcy filing, and liquidation (Lau 1987). However, some studies also use the term distress to refer to lower levels of distress (e.g. Tinoco and Wilson 2013). Generally, distressed firms are those whose financial wellbeing has deteriorated significantly, and the sources of distress are many and varied, from poor product innovation to global pandemics.

The definition of distress leaves us with two important questions: to whom are the obligations owed and what constitutes an obligation? Investors may feel entitled to positive returns and dividends, but failure to deliver on these expectations has little to no real consequences. However, companies have legally binding obligations to various stakeholders. Banks are entitled to interest and principal, customers expect to receive the product they have paid for, and employees depend on their salaries being paid in full and on time. Accordingly, one typically refers to legally binding obligations to stakeholders that can, broadly speaking, be considered “creditors”. However, distress is perhaps of particular interest to professional creditors, such as banks and bondholders, that place great importance on predicting distress (Beaver et al. 2011). As such, much of the literature on corporate distress has focused on professional creditors – and to some extent investors – because these are both impacted by and have the ability to evaluate distress. Nevertheless, it should be emphasized that distress is of interest to many stakeholders, including employees, the government, suppliers, and customers.

Upon failing to meet one’s obligations, the best-case scenario is that the issue is resolved behind closed doors without major consequences to the company.[1] Worst-case: the company defaults on its obligations, files for bankruptcy, and is liquidated. The aftermath of the latter typically involves substantial capital losses for creditors and owners and loss of jobs for employees. Understanding distress can thus be valuable, particularly if it enables stakeholders to either turn the tide or minimize losses. Tangentially, the primary focus is not only to predict distress, but rather the expected losses under different degrees of distress (Ohlson 1980). On the other hand, Miller and Modigliani (1958) and Stiglitz (1969) posit that the cost of distress is zero in perfect and rational markets with no friction or bankruptcy costs. Empirically, this is likely not the case (Beaver et al. 2011), but the argument still offers some insight. Bankruptcy – a consequence of severe distress – is the process by which inefficient coordination of human and physical capital is reorganized. The capital is made available to new owners or firms that can allocate it in a more efficient manner. Accordingly, distress and bankruptcy are natural and necessary aspects of an efficient market, and are as such not entirely negative, even if individual stakeholders may suffer some consequences from them.

[1] Examples of such resolutions are postponement of payments, debt restructuring or debt relief.

Much of the literature has focused on predicting distress, particularly bankruptcy, starting with the seminal studies of Beaver (1966) and Altman (1968) and continuing to today. Numerous models have been constructed over the decades, utilizing a plethora of predictors (Bellovary, Giacomino, and Akers 2007; Kumar and Ravi 2007) and prediction techniques (Kumar and Ravi 2007). However, other aspects of distress have also been explored, such as the costs of distress events (Bris, Welch, and Zhu 2006; Davydenko and Franks 2008; Franks and Torous 1994; Thorburn 2000), the consequences to the people involved (Eckbo, Thorburn, and Wang 2016; Gilson 1989), and changes in behavior upon experiencing distress (Ahmed, Christensen, Olson, and Yust 2018; Dittmar and Duchin 2016; Gopalan, Gormley, and Kalda 2021; Guo et al. 2022). Distress is consequently a mature and broadly researched topic.

Given the vast literature on distress prediction, summarized in my first paper, it is evident that it is a saturated topic. However, as suggest above, there are other interesting directions beyond the prediction of distress using conventional financial ratios. I therefore explore two adjacent avenues of research. The first is the use of information in the audit opinions, particularly how it relates to bankruptcy. This is not a new topic (see Geiger, Gold, and Wallage 2021), but my research provides some new insights and challenges some of the established ideas in the literature. The second avenue is the importance of personal characteristics of management for firms, with a focus on bankruptcy experience. Approaching distress and bankruptcy from this perspective is relatively recent (e.g., Dittmar and Duchin 2016; Gopalan et al. 2021) and my research subsequently contributes to a new and growing literature.

2. Summary of my research

The first study of the thesis is a solo-authored literature review of distress prediction studies. Developing a distress prediction model roughly involves three steps. First, the concept of distress has to be operationalized in order to accurately classify firms as distressed and non-distressed. Second, a method for estimating the level of distress and categorizing firms has to be selected. Third, a set of predictors associated with distress have to be picked. I structure the review to mimic this process and focus on the definitions, methods, and predictors applied across 117 studies published between 1966 and 2017. The study combines quantitative data and qualitative discussion to establish common approaches to distress prediction as well as potential issues that need to be considered.

Bankruptcy is the most commonly used definition of distress across the whole sample, although a few studies also use default or financial indicators to identify distressed firms. The latter two are also used to identify firms in the ‘grey area’ between bankrupt and healthy. These definitions reflect different levels of distress, and one expects financial indicators and default to precede bankruptcy. Estimation methods have varied significantly over time and two particular branches have emerged: a financial branch and computer science branch. The financial branch has predominantly used discriminant analysis, logistic regression, and hazard models, although discriminant analysis is not commonly used today. The computer science branch has utilized numerous intelligent methods, neural networks being the most common. Finally, hundreds of predictors have been tried and included across numerous models. The most frequently used are conventional financial ratios, although market and qualitative variables are also utilized. Interestingly, many conventional ratios underperform in studies that use feature selection approaches, suggesting that there is a selection bias and that frequency does not necessarily reflect the predictive ability of these predictors.

These findings synthesize the vast literature on distress prediction and provide a more complete overview, focusing on all three important aspects of designing a distress prediction model. The findings provide important information to researchers and practitioners that want to develop and utilize distress prediction models. Importantly, the findings also highlight that the approach to predicting bankruptcy has to some extent been unsystematic, with studies using different definitions interchangeably and trivial predictor selection approaches. Lastly, the findings also shed some light on unexplored avenues for research, such as the use of non-financial predictors.

The second study is an archival study of auditors' GCOs, coauthored with Prof. Anna Gold. Auditors are asked to assess an entity's ability to continue as a going concern for a reasonable period of time, not exceeding one year beyond the audit date (PCAOB AS 2415). However, there have been discussions about extending this look-forward period beyond twelve months (FASB 2013; 2014; IAASB 2020; SAG, 2011, 2012). In this study, we explore whether currently issued GCOs are accurate beyond the applied horizon of twelve months. Additionally, we investigate whether previously issued opinions still retain some incremental information value upon the issuance of a new GCO. The study is based on a dataset of opinions issued to non-financial U.S. between 2000 and 2018 and utilized logistic-regression to estimate how GCOs relate to the firm filing for bankruptcy.

We find that auditors' GCOs are accurate for at least two years after their issuance. Old opinions also provide incremental information even when more recent opinions are available. As such, this study provides evidence that auditors going concern assessments are accurate for periods exceeding twelve months. This has the important policy implication that extending the time horizon is viable, as opinions issued under the current institutional setting have long-term predictive ability despite auditors not being asked to consider a longer look-forward period. Our findings also have important practical implications, as we highlight that different combinations of successive opinions reflect different levels of bankruptcy risk. Only considering the latest GCO thus results in a loss of information. Ironically, the findings of this study also suggest that a formal change to the look-forward period may not be needed, as currently issued opinions already are accurate beyond twelve months. However, the accuracy of GCOs could potentially improve if auditors were tasked with considering a longer look-forward periods.

Finally, the third study is a solo-authored archival study on how the CEO's bankruptcy record affects the cost of debt. CEOs play an important role in setting firm policy and firm performance (Bertrand and Schoar 2003). Their personal traits and experiences are thus of interest to creditors. Bankruptcy experience could be one personal characteristic that is of interest if it conveys information about personal traits or preferences, e.g., low managerial ability or appetite for risk. In this study, I explore whether creditors respond to firms employing CEOs with a bankruptcy record by raising interest rates. Importantly, I utilize a unique setting to explore the role of public disclosure of bankruptcy records through a publicly available bankruptcy register. The expectations are that public disclosure reduces information collection costs and ensures that creditors use decision-relevant information. Consequently employing a CEO with a public bankruptcy record should on average result in a stronger creditor reaction, compared to employing a CEO with a non-public record. The study utilizes a generalized staggered difference-in-difference approach to compare firms that employ (i) a CEO without bankruptcy experience, (ii) a CEO with a non-public bankruptcy record, and (iii) a CEO with a public bankruptcy record.

The findings show that creditors respond to firms that employ a CEO with bankruptcy history by increasing cost of debt. However, the effect is only consistently significant when the bankruptcy record is publicly available. Additionally, the results suggest that firms led by CEOs with bankruptcy experience have higher accounts payable and are more likely to raise capital through equity, consistent with what one expects when the cost of debt is higher. Furthermore, these firms do in fact have a higher risk of bankruptcy, suggesting that creditors may be justified in increasing interest rates. These findings suggests that public disclosure of bankruptcy experience provides creditors with decision-relevant information and that creditors cannot or do not gather the same information privately. This is particularly useful with regard to private and small firms, where the cost of information collection is relatively higher. The policy implications from this study is that public bankruptcy registers could serve a valuable role in disseminating decision-relevant information concerning important individuals within firms.

As discussed earlier and identified in the literature review, distress and bankruptcy prediction is a mature research topic. Consequently, the second and third paper focus on more time relevant and unexplored issues surrounding distress and bankruptcy. The second paper is timely given the recent discussions surrounding expanding the look-forward period for auditors. Meanwhile, the third paper contributes to a more recent and growing literature on the effects of bankruptcy. Overall, I expect that there is room for further research on the topic of distress and bankruptcy – especially with the increasing access to new and exciting data – but that this research should focus on the effects of bankruptcies and use of non-financial information, rather than developing prediction models using conventional financial information.

3. A note on methodology

I want to make two notes on the methodology used in this thesis. The first note relates to the use of measurements or proxies, either when constructs are unobservable or when data is unavailable. The second note relates to the use of empirical methods and their limitations with regard to causal inference.

The first and second study in this thesis both suffer from a similar issue: distress and "substantial doubt" about an entity's ability to continue as a "going concern" are both unobservable constructs. Because of this, researchers and practitioners rely on alternative measurements, typically "bankruptcy" in both cases (Beaver et al. 2011: Geiger et al. 2021). My third study also suffers from a similar issue, as lack of data requires the construction of proxy measures. The conclusions formed in these studies depend on the validity of these measurements. Following the test of validity proposed by Borsboom, Mellembergh and van Heerden (2004), these measurements are seemingly appropriate as both distress and interest costs (a) exist and (b) variation in both constructs likely causally produce variation in the measured outcomes. As such, the measurements likely reflect the underlying construct. However, one still has to be conscious of the limitations of using proxies and the conclusions drawn across all three studies therefore depend on the assumption that these measurements appropriately reflect the underlying construct.

Both the second and third study are based on empirical models where the independent variable has not been exogenously manipulated. This limits our ability to make causal claims regarding the relationship between the variables of interest and the dependent variables. This is not uncommon in social sciences, but it is unfortunate as causal claims are important for society (Antonakis, Bendahan, Jacquart, and Lalive 2010). Although it is unlikely that we can ever remove all doubts concerning endogeneity, both studies utilize several methods to strengthen our belief in the causality of our findings, consistent with the suggestions of Antonakis et al. (2010).

References

Ahmed, A. S., Christensen, B. E., Olson, A. J., & Yust, C. G. (2019). Déjà Vu: The Effect of Executives and Directors with Prior Banking Crisis Experience on Bank Outcomes around the Global Financial Crisis. Contemporary Accounting Research, 36(2), 958-998.

Altman, E. I. (1968). Financial Ratios, Discriminant Analysis and the Prediction of Corporate Bankruptcy. The Journal of Finance, 23(4), 589-609.

Antonakis, J., Bendahan, S., Jacquart, P., & Lalive, R. (2010). On making causal claims: A review and recommendations. The Leadership Quarterly, 21, 1086-1120.

Beaver, W. H. (1966). Financial Ratios as Predictors of Failure. Journal of Accounting Research, 4, 71-111.

Beaver, W. H., Correia, M., & McNichols, M. F. (2011). Financial Statement Analysis and the Prediction of Financial Distress. Foundations and Trends® in Accounting, 5(2), 99-173.

Bellovary, J. L., Giacomino, D. E., & Akers, M. D. (2007). A Review of Bankruptcy Prediction Studies: 1930 to Present. Journal of Financial Education, 33, 1-42.

Bertrand, M., & Schoar, A. (2003). Managing with Style: The Effect of Managers on Firm Policies. The Quarterly Journal of Economics, 118(4), 1169-1208.

Borsboom, D., Mellenbergh, G. J., & van Heerden, J. (2004). The Concept of Validity. Psychological Review, 111(4), 1061-1071.

Bris, A., Welch, I., & Zhu, N. (2006). The Costs of Bankruptcy: Chapter 7 Liquidation versus Chapter 11 Reorganization. Journal of Finance, 61(3), 1253-1303.

Davydenko, S., & Franks, J. (2008). Do Bankruptcy Codes Matter? A study of Defaults in France, Germany, and the U.K. Journal of Finance, 63(2), 565-608.

Dittmar, A., & Duchin, R. (2016). Looking in the Rearview Mirror: The Effect of Managers' Professional Experience on Corporate Financial Policy. The Review of Financial Studies, 29(3), 565-602.

FASB. (2013). Proposed Accounting Standards Update - Presentation of Financial Statements (Topic 205): Disclosure of Uncertainties about an Entity's Going Concern Presumption. FASB in Focus.

FASB. (2014). Going concern. FASB Board Meeting.

Franks, J. R., & Torous, W. N. (1994). A comparison of financial recontracting in distressed exchanges and chapter 11 reorganizations. Journal of Financial Economics, 35(3), 349-370.

Geiger, M. A., Gold, A., & Wallage, P. (2021). Auditor Going Concern Reporting: A Review of Global Research and Future Research Opportunities (1 ed.). Routledge.

Gilson, S. C. (1989). Management turnover and financial distress. Journal of Financial Economics, 25(2), 241-262.

Gopalan, R., Gormley, T. A., & Kalda, A. (2021). It’s not so bad: Director bankruptcy experience and corporate risk-taking. Journal of Financial Economics, 142(1), 261-292.

Guo, F., Lisic, L. L., Pittman, J., Seidel, T. A., Zhou, M., & Zhou, Y. (2022). Fool Me Once, Shame on You; Fool Me Twice, Shame on Me: The Long-Term Impact of Arthur Andersen's Demise on Partners’ Audit Quality. Contemporary Accounting Research, Accepted manuscript.

IAASB. (2020). Fraud and going concern in an audit of financial statements: Exploring the differences between public perceptions about the role of the auditor and the auditor's responsibilities in a financial statement audit. Discussion paper.

Kumar, P. R., & Ravi, V. (2007). Bankruptcy prediction in banks and firms via statistical and intelligent techniques – A review. European Journal of Operational Research, 180(1), 1-28.

Lau, A. J. (1987). A Five-State Financial Distress Prediction Model. Journal of Accounting Research, 25(1), 127-138.

Miller, M., & Modigliani, F. (1958). The Cost of Capital, Corporation Finance, and the Theory of Investment. American Economics Review, 48, 261-297.

Ohlson, J. A. (1980). Financial Ratios and the Probabilistic Prediction of Bankruptcy. Journal of Accounting Research, 18(1), 109-131.

PCAOB Standing Advisory Group. (2011). Evaluating Whether There is Substantial Doubt About an Entity's Ability to Continue As a Going Concern. Standing Advisory Group Meeting.

PCAOB Standing Advisory Group. (2012). Going concern. Standing Advisory Group Meeting.

Stiglitz, J. (1969). A Reexamination of the Modigliani-Miller Theorem. American Economics Review, 59, 784-793.

Thorburn, K. S. (2000). Bankruptcy auctions: costs, debt recovery, and firm survival. Journal of Financial Economics, 58(3), 337-368.

Tinoco, M. H., & Wilson, N. (2013). Financial Distress and bankruptcy prediction among listed companies using accounting, market and macroeconomic variables. International Review of Financial Analysis, 30, 394-419.

DESIGNING DISTRESS PREDICTION MODELS

A REVIEW OF THE DEFINITIONS, METHODS, AND PREDICTORS UTILIZED FOR DISTRESS PREDICTION

Ibrahim Pelja*

ABSTRACT: This paper reviews the definitions, methods, and predictors applied in distress prediction. The findings show that bankruptcy is the most common definition of distress and further scrutiny of the operational definitions shows that they relate to varying levels of distress. Hence, the choice of definition reflects the level of distress the model aims to predict. The most common estimation methods are hazard models and neural networks, although the latter are limited to studies in the fields of computer science and information systems. The most commonly used predictors are conventional ratios. Some predictors show strong and robust levels of inclusion and significance across studies. However, many popular predictors show low levels of inclusion and significance, especially when the sample is limited to studies that use feature-selection approaches. These findings demonstrate that few predictors are consistently selected.

KEYWORDS: distress, failure, bankruptcy, default, prediction

* Ibrahim Pelja is a PhD candidate at the Norwegian School of Economics and can be reached at: ibrahim.pelja@nhh.no.

1. Introduction

Identifying corporate financial distress is an important but difficult exercise. The consequences of distress are often substantial and wide reaching, and may include unemployment, lost tax revenue, and a reduction or complete loss of value for the claims of owners and creditors. Therefore, a great deal of effort is dedicated to diagnosing the financial health of firms. Creditors construct models to estimate the expected loss of their loan portfolios, investors consider the downside risks of their investments and may even engage in short selling, and auditors are tasked with assessing whether there is substantial doubt surrounding an entity's ability to continue as a going concern (PCAOB AS 2415). Yet, borrowers still default on their obligations to banks, investors fail to predict large financial crises, and auditors fail to raise substantial doubt in 40 percent of bankruptcy filings (Carson et al. 2013). Researchers have also devoted substantial effort to predicting distress. In fact, since the publication of Beaver's (1966) seminal paper, hundreds of studies have covered the topic. However, despite the many important contributions, researchers have failed to invent the perfect "mousetrap", even if that is unreasonable to expect. Ultimately, predicting distress can be viewed as an empirical exercise in which one has to take the aim and context of the prediction into consideration.[1]

This paper reviews the distress prediction literature and covers 117 studies from different fields (i.e., accounting, finance, economics, operational research, computer science, and information management) published between 1966 and 2017. The review is divided into three parts. I first review how the literature has defined "distress", and then examine the development of the methods and models used to estimate the distress risk. Finally, I cover the predictors (variables) that have been utilized across the numerous predictions models. This structure mimics the process through which distress prediction models are constructed. In that process,

[1] For example, see Altman's Z-score, which has been modified numerous times to accommodate different firms and contexts, such as private firms, non-manufacturers, and emerging markets (Altman, 2018).

the first step is typically to operationalize distress and then choose which method to apply in order to estimate the distress risk and select the predictors to include.[2] For all three aspects (i.e., definition, method, and predictors), an overview based on the reviewed literature is provided as is quantitative data on how often different definitions, methods, and predictors have been applied.

Bankruptcy is the element most often used to differentiate between distressed and healthy firms, followed by default and financial indicators. These definitions reflect different levels of distress, with bankruptcy reflecting the most severe level of distress, followed by default and financial indicators. In addition, there is a temporal relationship among the different groups of definitions, with bankruptcy often following a default, which in turn follows deterioration in financial indicators. In terms of estimation methods, there is a strong time trend across the sample. Simple statistical methods, such as discriminant analysis (DA)[3] or logit models, were often used in earlier studies, while hazard models and complex intelligent methods dominate the research today. The latter are typically used in the fields of computer science and information systems. Finally, a plethora of predictors have been tested and utilized in distress prediction models. The most widely used are the current ratio, the EBIT to total assets ratio, the return on assets, the leverage ratio, and the working capital to total assets ratio. However, many of these common predictors are not always selected when other variables are also considered. This is most pronounced when inclusion is based on a feature-selection approach. When considering the relative level of inclusion (rather than absolute), leverage, the market value of equity to total debt, excess return, relative index weight, and the standard deviation of return are the predictors used most often in distress models.

[2] Although I describe the process as sequential, the different decisions might be made in parallel. In fact, this may be preferable.

[3] Here, DA is used as an umbrella term for all types of discriminant analysis, and refers to linear discriminant analysis, multivariate discriminant analysis (MDA), and quadratic discriminant analysis (QDA).

The approach outlined above results in a structured review of the distress prediction literature, which offers future researchers and practitioners a frame of reference for designing distress models. Both researchers and practitioners occasionally adopt a trivial approach when designing distress models, as they appear to pick distress definitions based on data availability or select predictors with little forethought and based on popularity. The quality and usefulness of models may come into question if the approach to their design is not carefully considered. As such, the findings in this paper can guide researchers and practitioners in their construction of distress models and, help improve the quality of prediction models. In addition, this review uncovers several interesting avenues for future research, especially research on the potential importance of different operational definitions of distress and the relevance of qualitative predictors.

2. Methodology and included papers

This study utilizes a systematic review method to answer three research questions:

1. How has "distress" been defined and operationalized in the distress prediction literature and which definitions have been the most widely used?
2. What methods have been used to estimate distress risk and which methods have been the most common?
3. What predictors have been used in distress prediction models and which predictors have been used most often?

These three questions follow the three important aspects of developing a distress prediction model: defining distress (the dependent variable), choosing an estimation method, and selecting the predictors (the independent variables). By reviewing these aspects, this study provides an overview of the different approaches to predicting distress. Furthermore, by quantifying the frequency of definitions, methods, and predictors, it provides insights into what is the most "common" approach.

The search strategy focuses on literature found in four databases: Scopus, Semantic Scholar, Mendeley, and ScienceOpen. When possible, the search is limited to peer-reviewed articles and conference papers in the areas of social science and computer science.[4] This is consistent with accepted practice for systematic reviews and ensures that only papers of sufficient quality are included. As studies tend to use different terms as synonyms for "distress", I use multiple search terms to identify potential studies: "distress", "default", "bankruptcy", "insolvency", and "failure", all of which are combined with the terms "prediction", "forecasting", or "model". Finally, all of the identified studies are used as seed papers to perform a backwards search of the articles cited in them.

Inclusion criteria for this review are chosen to ensure that the studies considered are relevant to the review's topic. Many studies may fall under the broad term "distress prediction" or similar terms. As such, overly strict or weak inclusion criteria could result in a biased review, thereby hurting the ability to draw inferences and failing to reflect reality. I set the following criteria for a study to be considered relevant and included in the review:

1. The purpose of the study is, at least to some extent, the development of a model for distress prediction.
2. The paper could not solely concern distress in financial institutions.
3. The paper had to cover at least one of the following aspects:
    a. The definition of distress applied in the study,
    b. The method used to estimate distress, or
    c. The predictors included in the final model.

[4] For most fields, the search is limited to articles published in journals. However, some conference proceedings are included because they are important in the field of computer science.

The first inclusion criteria ensures that the review is limited to studies that consciously develop a model for the purpose of measuring distress. This includes studies that specifically aim to develop a bankruptcy model (e.g., Altman, 1968; Ohlson, 1980). However, it also ensures that other studies are included even if their main purpose is not to construct a bankruptcy model. For example, Zmijewski (1984) examines estimation biases in distress models, but because the paper explicitly develops a bankruptcy model, it is included in the review. Likewise, Beaver, McNichols, and Rhie (2005) explore whether financial statements have become less informative, but they do so by testing the ability of financial ratios to predict bankruptcy. The review also includes several studies designed to test whether certain methods are suitable for distress prediction without necessarily trying to construct an ultimate model (e.g., Min and Lee, 2005; Min, Lee, and Han, 2006; Shumway, 2001). In contrast, studies that simply utilize existing distress prediction models are excluded, such as studies that rely on existing models to control for distress as well as studies that re-test existing models in different settings (e.g., Begley, Ming, and Watts, 1996; Grice and Ingram, 2001; Moyer, 1977). Their exclusion is preferable, as they do not explicitly construct a distress model and their inclusion would likely bias the findings towards the most widely recognized models, such as Altman's Z-score or Ohlson's O-score. Following the second criteria, studies that focus solely on financial institutions are excluded, as such institutions are significantly different from non-financial firms (Beaver et al., 2011). Finally, the third criteria ensures that only studies that provide sufficient information given the focus of this review are included.

This process yields 117 studies. A complete list of the included papers is provided in Appendix I. The majority of the studies are within the fields of accounting, finance, and information management. Among the 117 studies, 102 provide adequate information on the definition of distress that is applied, 107 adequately explain the method used in the estimation of distress risk, and 104 list the predictors included in the model.

3. Definitions of distress

3.1. Operationalizing distress

The concept of "financial distress" is both broad and vague, but it generally relates to a firm's ability to meet its obligations in a timely manner (Beaver et al. 2011). As distress is a concept and cannot be measured directly, studies operationalize it using different events or requirements. Table 1 summarizes the various definitions that are used to separate between distressed and non-distressed firms. The definitions are grouped into four main categories: bankruptcy, default, financial, and other. These categories are then broken down into the more specific indicators of distress that are used in the studies. Finally, it should be noted that even when studies apply seemingly identical definitions, differences might still exist. Such differences could be due to changes over time, such as the U.S. replacing the U.S. Bankruptcy Act with the Bankruptcy Code in 1978, or to cross-sectional differences in, for instance bankruptcy regulations or accounting and auditing standards. Moreover, some studies use slight variations of the definitions. For example, McKee (1995; 2000) categorizes firms in which a "substantial subsidiary" filed for bankruptcy as distressed.

As seen in Table 1, bankruptcy definitions are, by far, the element most often used to differentiate between distressed and non-distressed firms in the literature (e.g., Altman, 1968; Deakin, 1972; Altman, Haldeman, and Narayanan, 1977; Ohlson, 1980; Zmijewski, 1984; Dimitras, Slowinski, Susmaga, and Zopounidis, 1999; McKee and Lensberg, 2002; Chava and Jarrow, 2004; Beaver et al., 2012; Bauer and Agarwal, 2014). This is likely due to the relevant information being publicly available and possible to date accurately (Beaver et al. 2011).

The second most common type of definitions relates to default. As defaults involve non-fulfilment of an obligation, they are seemingly appropriate proxies for distress. However, they are not as popular as bankruptcy definitions. As suggested by Beaver et al. (2011), this might

Table 1: Operational definitions of distress across 102 studies

Definition	Indicator of distress	Num.
Bannkrupcy (1)	(a) Filed for bankruptcy/appointment of receiver or liquidator	49
	(b) Went bankrupt (liquidation, administration, receivership, reorganized)	34
	(c) No bankruptcy indicator specified	24
Default (2)	(a) Debt default	6
	(b) Debt renegotiation/restructuring	4
	(c) Failure to meet listing requirements/delisting	8
	(d) Technical default	1
	(e) Nonpayment of preferred stock dividend	1
	(f) Overdrawn bank account	1
Financial (3)	(a) Non-profit making	4
	(b) Dividend omission or reduction	3
	(c) EBITDA < Financial expenses	2
	(d) Decrease in market value	2
	(e) Current ratio less than one	1
Other (4)	(a) Auditor report	5
	(b) Signal from regulator	2
	(c) Governmental support	2
	(d) Junk bond	1
	(e) Capital raising	1
	(f) Forced merger	1
	(g) Loan classifications	1

The table shows the number of papers that apply the different definitions of failure and their respective indicators. Note that a paper can apply more than one definition and indicator. Bankruptcy (1) refers to definitions related to the bankruptcy process. These include: (1a) filing for bankruptcy, (1b) going through a bankruptcy process, and (1c) firms were referred to as "bankrupt" but no clear indicator was specified. Default (2) refers to definitions related to types of default. These include: (2a) the firm defaulted on a debt obligation, (2b) the firm renegotiated/restructured debt, (2c) the firm failed to meet listing requirements or was delisted, (2d) the firm experienced a technical default (e.g., a covenant breech), (2e) the firm did not pay dividends on preferred stock, and (2f) the firm had an overdrawn bank account. Financial (3) refers to definitions that use financial indicators of distress. These include: (3a) the firm experienced losses, (3b) the firm substantially reduced or omitted dividends, (3c) the firm's earnings before interest, taxes, depreciation, and amortization (EBITDA) was less than financial expenses, (3d) the firm's market value decreased, and (3e) the firm's current ratio was less than one. Other (4) refers to definitions that do not clearly fit into common categories. These include: (4a) an auditor's report showed substantial doubt about the firm's ability to continue as a going-concern, (4b) a regulatory organ signaled that the firm was distressed/struggling, (4c) the firm received substantial governmental support, (4d) the firm's bonds were classified as junk bonds, (4e) the firm raised capital to meet future obligations, (4f) the firm participated in a forced merger, and (4g) the firm had a poor loan classification in a bank database. Relevant excerpts from the papers included in this table are presented in Appendix II.

be because the relevant data are not as publicly available or harder to date. Studies using default definitions include Beaver (1966), Lau (1987), Kahya and Theodossiou (1999), Jones and Hensher (2004), and Duffie, Saita, and Wang (2007). Default definitions are to some extent more in line with the theoretical definition of distress. However, they are less frequently used due to the lack of relevant data.

Some studies also utilize financial indicators of distress that do not relate to particular events. Examples of such indicators include reporting losses (e.g., Peel and Peel, 1987) or having EBITDA less than financial expenses (e.g., Tinoco and Wilson, 2013). These definitions are sometimes used to categorize firms into the "gray area" between healthy and severely distressed (e.g., Lau, 1987).

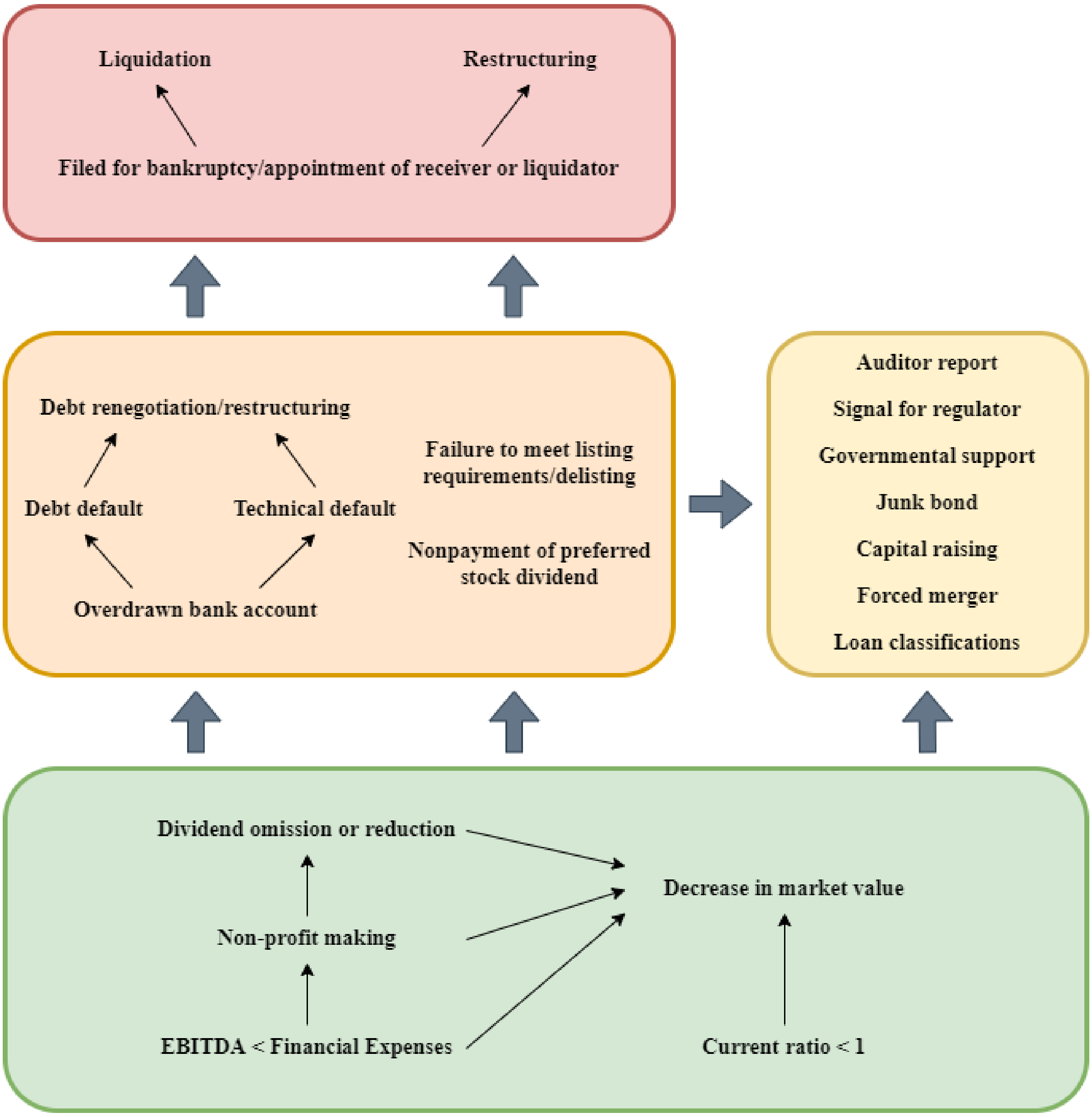
Lastly, some studies use definitions of distress that are not easily categorized. Studies that apply these alternative definitions include Lacher et al. (1995), Jones and Hensher (2004), and Hua, Wang, Xu, Zhang, and Liang (2007). These definitions are also sometimes used to categorize firms into the "gray area".

3.2. Structuring the definitions of distress

In reality, distress is continuous, although many studies use simple dummy indicators (e.g. bankrupt and nonbankrupt) for practical reasons and many of the different definitions represent varying levels of distress. It is useful to contextualize how the different definitions relate to each other in order to develop a stronger understanding of the level of distress they reflect. Figure 1 illustrates the relationships among the different definitions and indicators. Naturally, this is simply a stylized illustration – the relationships among the different definitions and indicators may be more complex in reality.

The weakest signals of distress are typically identified using financial indicators, such as those listed in Table 1. Changes in financial figures and ratios are typically the first sign that a firm is struggling, and they usually precede other events, such as defaults or bankruptcies. However, these indicators are not always indicative of distress. For example, many growth firms will likely fail to report a profit for many years without necessarily being distressed. As such, even though deterioration in financial figures can serve as an early sign of distress, such indicators may be less suited for properly distinguishing between distressed and healthy firms.

Figure 1: Relationships among distress indicators



This figure illustrates the relationship between different (groups of) distress indicators utilized in the distress prediction literature. Thin arrows indicate a potential relationship between the indicators, while thick arrows indicate a relationship between the groups. The distress indicators in the red box are bankruptcy indicators, as seen in Group 1 in Table 1. The indicators in the orange box are default indicators, as seen in Group 2 in Table 1. The indicators in the green box are financial indicators, as seen in Group 3 in Table 1. Finally, the indicators in the yellow box are various other indicators, as seen in Group 4 in Table 1. The level of distress represented by the groups of indicators increases from the bottom to top, but this is not necessarily the case within the groups.

When a firm's financial performance remains poor over a prolonged period of time, we might expect it to eventually default on its obligations. Default events typically follow deterioration in financial figures and signal higher levels of distress. Recovery rates for default firms vary from 49 percent to 92 percent, depending on the country, procedure, and other factors. Therefore, the risk of loss is significant (Davydenko and Franks, 2008; Grunert and Weber, 2009). However, different types of default might indicate varying levels of distress. Non-payment of interest and debt renegotiations are arguably signals of severe financial distress. In contrast, technical defaults might be less severe. When utilizing default as the definition of distress, "healthy" firms are those that have not yet defaulted. As such, one should keep in mind that some "healthy" firms might be distressed but have yet to default.

Poor financial performance can also lead to other events that could signal severe levels of distress, such as an auditor raising substantial doubt about an entity's ability to continue as a going concern or a poor credit rating. Such events can also be consequences of the firm defaulting on its obligations. Naturally, the level of distress that is reflected in these definitions varies depending on the specific indicator, but most of these indicators suggest a relatively high level of distress. As is the case with definitions of default, using any of these definitions to identify distress may result in some firms being wrongfully classified as "healthy".

Finally, definitions of distress based on bankruptcy represent the most conservative definition of failure and are a clear indicator of severe distress. A filing for or declaration of bankruptcy typically follows prolonged periods of underperformance, and such firms have often defaulted on one or more obligations. Studies find that the mean and median recovery rates for creditors vary from 25 percent to 39 percent depending on the procedure and debt class, and most stakeholders are expected to realize a loss in these cases (Frank and Torous, 1994; Tashjian, Lease, and McConnell, 1996; Thorburn, 2000). However, as bankruptcy regulations differ among countries and across time periods, the extent to which bankruptcy proceedings are an indicator of a substantial risk of an ultimate loss might differ. Although bankruptcy definitions clearly identify distressed firms, they also run the risk of classifying some severely distressed firms as "healthy". This can have negative consequences for the accuracy of the prediction model, as identified by Gilbert, Menon, and Schwartz (1990).

In summary, the operational definitions applied in the literature serve as proxies for varying levels of distress. The suitability of the different definitions depends on the purpose and context of the prediction model. For stakeholders with high priority claims or substantial collateral, it might be appropriate to use bankruptcy definitions when constructing the model. Other stakeholders, such as unsecured creditors, may want to utilize definitions that are timelier, such as those focused on default. Finally, investors or similar stakeholders with weak claims might be best served by utilizing the most aggressive definitions of distress. As a final note, this discussion also highlights the need for caution when using the different terms as synonyms and shows that comparing models constructed using different definitions of distress can be problematic.

4. Methods and models

Many suitable estimation methods are available for predicting distress. They can be separated into two broad categories: statistical techniques and intelligent techniques (Kumar and Ravi, 2007). Statistical techniques include methods with which many researchers are familiar, such as DA, OLS regression, logit and probit regression, and hazard models. Intelligent methods comprise complex machine-learning methods, including neural networks (NN), decision trees, case-based reasoning (CBR), evolutionary approaches, rough sets, soft computing, and support vector machines (SVM). With regard to the latter, Kumar and Ravi (2007) highlight that researchers have employed "almost all" intelligent techniques to predict distress.

This section builds on Kumar and Ravi (2007) and provides an overview of the different methods used across the literature and the frequency of the respective methods. These findings are summarized in Table 2, which separates the frequency of the different methods across decades (Panel A) and the various fields (Panel B). Overall, the findings show that there seem to be two branches within the literature. The first, categorized as the accounting, finance, and economics branch, utilizes mostly statistical techniques, with some studies relying on intelligent methods. The second branch, which focuses on computer science and information systems, utilizes the more complex intelligent methods. As such, the literature has generally moved towards more complex methods, with the literature apparently splitting into two different branches in the 1990s. In the following section, I go into more detail about the use of statistical and intelligent techniques.

Table 2: Methods used for distress prediction across 107 studies

Panel A: by year	**1960s**	**1970s**	**1980s**	**1990s**	**2000s**	**2010s**
Statistical methods						
Discriminant analysis	1	12	11	4	1	0
OLS regression	0	0	1	0	0	0
Logit/probit regression	0	0	7	3	3	0
Hazard model	0	0	0	0	6	3
Intelligent methods						
Neural networks	0	0	0	16	8	1
Decision trees	0	0	2	0	1	0
Case-based reasoning	0	0	0	3	4	0
Evolutionary algorithms	0	0	0	2	5	0
Rough sets	0	0	0	2	2	0
Support vector machines	0	0	0	0	5	0
Fuzzy logic	0	0	0	1	1	2
Soft computing*	0	0	0	2	9	2

Panel B: by field	**Accounting/finance/economics**	**Computer science**	**Information systems**
Statistical methods			
Discriminant analysis	25	1	2
OLS regression	1	0	0
Logit/probit regression	9	1	2
Hazard model	8	0	0
Intelligent methods			
Neural networks	3	12	7
Decision trees	2	1	0
Case-based reasoning	1	2	4
Evolutionary algorithms	2	2	3
Rough sets	1	1	0
Support vector machines	0	0	5
Fuzzy logic	0	4	0
Soft computing*	0	6	6

The table shows the number of papers that utilize the different methods for estimating distress risk. Panel A summarizes the use across different decades. Panel B summarizes the use across the three main fields in which research on distress prediction is published.
* “Soft computing” refers to hybrid models that combine two or more different methods. In cases where a paper utilizes soft computing, the different methods used are also counted within their respective categories.

4.1. Statistical techniques

After Beaver's (1966) univariate study, the distress prediction literature quickly moved towards a multivariate approach. The first notable multivariate method applied for failure prediction was DA. The most notable paper using DA is the Z-score model developed by Altman (1968). As seen in Table 2, this method was the most popular approach to predicting bankruptcy from the 1960s through the 1980s, but it is rarely used today. Moreover, it has been almost exclusively used in the fields of accounting, finance, and economics. When used in the computer science and information systems fields, it is applied alongside other methods in a hybrid approach. One of the benefits of DA is that it is a relatively intuitive and simple method to apply. However, as the approach depends on assumptions that are frequently violated, it has been criticized (e.g. Joy and Tollefson, 1975). In addition, Alaka et al. (2018) find that the average accuracy of DA models is the lowest of the methods they compare in their study.

Given the criticisms of DA, new methods have been applied to predict failure. Methods that became popular during the 1980s and 1990s include logistic regression and probit regression. Similar to DA, these methods have mostly been confined to the fields of accounting, finance, and economics, and have only been utilized in other fields in hybrid models. The two most notable logit and probit models were developed by Ohlson (1980) and Zmijewski (1984), respectively. Both favored alternative approaches to DA due to the statistical issues associated with the method. These issues are less prevalent for logistic regression and probit regression. Logistic regression also seemingly outperforms DA across the literature (Alaka et al. 2018). Consequently, logit and probit models offer greater predictive ability than DA, even though they remain simple and intuitive.

More recently, Shumway (2001) applied a hazard model for the purpose of predicting failure. Previous statistical models could only incorporate firm characteristics from one time period, typically the prior year. However, a hazard model can use all of the available information. This approach has been relatively popular in the past two decades, albeit only within the accounting, finance, and economics literature. Other papers applying hazard models include Chava and Jarrow (2004), Xu and Zhang (2009), and Bauer and Agarwal (2014). Hazard models are arguably more appropriate for distress prediction than the single-period models utilized in previous research and are expected to perform better than other statistical techniques (Shumway, 2001).

4.2. Intelligent methods

Tree-based models were the first intelligent methods applied for failure prediction in the sample (e.g., Marais, Patel, and Wolfson, 1984; Frydman, Altman, and Kao, 1985). One benefit of tree-based models is that they provide intuitive "if-then" rules and are suited for classification (Kumar and Ravi, 2007). Despite representing the first foray into the use of intelligent techniques, tree-based models have rarely been used as the primary model for failure prediction. Alaka et al. (2018) find that decision trees, on average, are as accurate as logistic regression.

The most commonly used intelligent method is NN (e.g., Coats and Fant, 1993; Serrano-Cinca, 1996; Anandarajan, Lee, and Anandarajan, 2001), which has been used regularly since the 1990s and is also popular in hybrid models. Table 2 shows that NNs have been used across different fields, but most often in the fields of computer science and information systems. One of the benefits of NN and other intelligent techniques is that they do not depend on several statistical assumptions. As such, they are arguably more suitable when the data fails to conform to the statistical requirements of conventional statistical methods. However, one of the drawbacks of NN is that they require large training samples and cycles (Kumar and Ravi, 2007). Alaka et al. (2018) find that NNs are the most accurate tool for distress prediction across the studies they considered.

Several other intelligent techniques have also been used for distress prediction, starting in the 1990s. These include CBR (e.g., Jo, Han, and Lee, 1997; Bryant, 1997), evolutionary algorithms (e.g., Back, Laitinen, and Sere, 1996; Shin and Lee, 2002; Ahn and Kim, 2009), rough sets (e.g., Dimitras, Slowinski, Susmaga, and Zopounidis, 1999; McKee, 2000), fuzzy techniques (e.g., Spanos et al., 1999; Bian and Mazlack, 2003), and SVM (e.g., Min and Lee, 2005; Shin, Lee, and Kim, 2005; Li and Sun, 2009). These approaches have been used sparingly and mostly in the fields of computer science and information systems.

Lastly, there has been an increase in the use of hybrid models or "soft computing" since the mid-1990s (e.g., Jo and Han, 1996; McKee and Lensberg, 2002; Chen et al., 2011). These techniques involve developing methods that incorporate two or more intelligent and statistical techniques. The use of these models is limited to the fields of computer science and information systems. Kumar and Ravi (2007) suggest that these types of methods outperform individual methods. Hybrid models have been widely used in the past ten years. Notably, this approach amplifies the benefits of different techniques while reducing the disadvantages, but it requires a large amount of data (Kumar and Ravi, 2007).

5. Predictors of distress

5.1. Predictor selection

Distress prediction models typically rely on a set of predictors, usually financial ratios. The choice of predictors affects the model's predictive ability (Laitinen, 1991). Researchers and practitioners should thus select suitable predictors. However, there is no consensus on which variables should be included to predict distress (Tian, Yu, and Guo, 2015). Furthermore, formal guiding theory on which and how many variables to include, or what weights to assign them is lacking (Beaver et al., 2011). Several conceptual models that view the firm as a reserve of liquid assets that change with certain probabilities (Beaver 1966; Wilcox, 1971; Blum, 1974) could be used. However, these models are highly stylized and do not explain which predictors should be used as proxies for these constructs. Therefore, studies have varied in their approach to predictor selection.

Many studies start with an initial sample of predictors, which are typically chosen because of their popularity and inclusion in previous studies (e.g., Beaver, 1966; Norton and Smith, 1979; Frydman, Altman, and Kao, 1985; Lacher et al., 1995; McKee and Lensberg, 2002; Jones and Hensher, 2004; Beaver et al., 2012; Tian et al., 2015). Some studies also include predictors based on professional judgement, their perceived relevance, or theoretical grounding (e.g., Dambolena and Khoury, 1980; Izan, 1984; Becchetti and Sierra, 2003). Naturally, all of these studies are limited by data availability. On average, the initial sample across the studies consists of more than twenty variables. However, these variables do not make up the final set of predictors used to predict distress. Altogether, around 400 distinct predictors are tested across the reviewed studies.[5]

Studies that include an initial sample of predictors typically select the final predictors based on attempts to maximize the predictive ability of the model or statistical tests, such as univariate t-tests, a stepwise-approach, or more complicated feature-selection methods (e.g., Altman, 1968; Norton and Smith, 1979; Betts and Belhoul, 1987; Koh and Killough, 1990; Jones and Hensher, 2004; Min and Lee, 2005; Xu and Zhang, 2009; Tian et al., 2015). However, several studies forgo the initial sample. These studies typically select the final predictors based on their

[5] Accurately separating predictors can be difficult. Some predictors can be viewed as variants of practically identical predictors. A simple example is "current assets to current liabilities" and "current liabilities to current assets". Another less straightforward example is the fact that "liabilities to total assets" (L/TA) can be categorized together with "book value of equity to total assets" (BVE/TA) because:

$$\frac{L}{TA} = \frac{(TA - BVE)}{TA} = \left(1 - \frac{BVE}{TA}\right)$$

In addition, some studies use terms that could refer to the same figure but may be technically different, such as "debt" and "liability". Distinctions are thus made using professional judgment, but it is difficult to ensure perfect distinction between the predictors.

popularity in previous studies (e.g., Deakin, 1972; Castagna and Matolcsy, 1981; Lacher et al., 1995; Chava and Jarrow, 2004; Beaver et al., 2012) and/or professional judgement (e.g., Zmijewski, 1984; Lau, 1987; Laitinen, 1991; Shumway, 2001; Pindado, Rodrigues, and de la Torre, 2008).

In the following sections, I review the different predictors that have been deemed relevant for predicting distress and, thereby, provide insights into which predictors are best suited for this task. Inclusion in models constructed on different samples and using different methods suggest a robust predictive ability. The same is true for significance in different samples. However, the selection process varies across the studies, which could induce certain biases. Selection based on previous studies creates a bias towards conventional predictors. Similarly, professional discretion could be biased towards familiar predictors. On the other hand, statistical processes might be less biased, although this only applies to the final selection. Therefore, although frequent inclusion and significance across studies could be an indicator of predictive ability, it is impossible to conclude definitively that this is due to predictive ability alone. In total, 350 distinct predictors show significant differences between distressed and non-distressed firms or are included in a prediction model. These predictors are classified into three categories: accounting predictors, market predictors, and non-financial predictors.

5.2. Accounting predictors

Predictors constructed using accounting figures are, by far, the most commonly used for distress prediction. This is not surprising, as accounting figures have been shown to be relevant for predicting distress and are readily available (Beaver et al. 2011). These predictors have been used since the earliest studies. Table 3 lists the number of studies in which the individual predictors are included (*Included*), the percentage of studies that include the predictor in the final model after being included in the initial sample of predictors (*%Included*), the change in *%Included* when limiting the studies to those that used a feature-selection approach (*Δ%Included*), the number of studies that show univariate significant differences between the distressed and non-distressed groups for that predictor (*Significant*), and the percentage of studies that show univariate significant differences between the two groups (*%Significant*).

Table 3: Common accounting predictors across 104 distress studies

Predictor	Included	%Included	Δ%Included	Significant	%Significant
CA/CL	35	73%	-14pp	16	94%
EBIT/TA	30	83%	-12pp	13	93%
NI/TA	28	70%	-13pp	15	100%
L/TA	27	84%	-1pp	17	94%
WC/TA	27	64%	-22pp	12	71%
RE/TA	22	76%	-20pp	9	100%
QA/CL	21	66%	-16pp	6	67%
S/TA	21	55%	-9pp	10	59%
D/TA	16	67%	-9pp	8	100%
C/CL	14	66%	-10pp	12	92%
CA/TA	13	52%	-10pp	3	43%
NI/BVE	12	67%	-7pp	4	80%
SIZE(TA)	11	79%	-12pp	4	67%
CF/D	11	61%	-11pp	8	100%
S/WC	11	52%	-5pp	5	83%
S/INV	11	52%	-8pp	2	33%
C/TA	10	48%	-17pp	5	71%
BVE/D	9	75%	-2pp	4	80%
CL/TA	8	73%	-10pp	5	100%
EBITDA/TA	8	67%	0	4	80%
S/CA	8	47%	-16pp	3	50%

The table lists the most commonly used accounting predictors in distress studies by the number of times they are used in a prediction model (*Included*), the percentage of times the predictors are included across all studies that have them in their initial sample (*%Included*), the change in *%Included* when limiting the sample to studies that utilize a feature-selection approach for variable selection (Δ*%Included*), the number of times they are found to significantly differentiate between distressed and non-distressed firms (*Significant*), and the percentage of times the predictors are found to be significant across all studies that test their significance (*%Significant*). They are defined as follows: *CA* (current assets), *CL* (current liabilities), *EBIT* (earnings before interest and tax), *TA* (total assets), *NI* (net income), *L* (total liabilities), *WC* (working capital), *RE* (retained earnings), *QA* (quick assets), *S* (sales), *D* (total debt), *C* (cash and cash equivalent), *BVE* (book value of equity), *SIZE* (raw or log transformed variable for size), *CF* (cash flow), *INV* (inventory), and *EBITDA* (earnings before interest, taxes, depreciation, and amortization).

As shown in Table 3, the most commonly used predictors are the conventional ratios covered in most textbooks – that is, the current ratio, EBIT to total assets, return on assets, leverage, and working capital to total assets. Not surprisingly, the predictors utilized in Altman's Z-score models are among the most frequently used for distress prediction. Most of these predictors also exhibit relatively high levels of inclusion, with around 70 percent of them being included in the final model after initially being considered for selection. Using this metric, the highest levels of inclusion are for leverage and EBIT to total assets. However, the level of inclusion falls drastically for most predictors after limiting the sample to those studies that utilize a feature-selection approach to select the final predictors. In that case, the most utilized predictors are leverage, book value of equity to debt, and EBIT to total assets. Looking at the results of univariate significance tests across the studies, most predictors show significant differences across the distressed and non-distressed groups.

5.3. Market predictors

There are some concerns that accounting data is ill suited for predicting distress. For instance, Hillegeist et al. (2004) question the effectiveness of accounting data in predicting failure and express their preference for market information, as it is inherently forward looking and incorporates accounting information, making it more suitable for predicting future events. Beaver et al. (2005) find that financial statements have become less informative when testing their ability to predict bankruptcy. This is somewhat remedied by including market predictors, although accounting predictors still provide incremental information (Beaver et al. 2012). Similarly, Tian et al. (2015) find an increased predictive ability for some ratios when replacing accounting values with market values. Consequently, some researchers include predictors constructed using market data. The first example of this was Altman's original Z-score (1968), which included the market value of equity. Subsequently, many other studies have incorporated market information, typically along with accounting predictors. Table 4 lists the most commonly used market predictors utilized across the studies included in this review.

Table 4: Common market predictors across 104 distress studies

Predictor	Included	%Included	Δ%Included	Significant	%Significant
MVE/D	15	94%	-14pp	5	83%
EXRET	8	100%	0pp	4	100%
SIZE(wINDEX)	7	100%	0pp	4	100%
SD(RET)	6	100%	0pp	2	100%
MVE/L	3	43%	-10pp	3	100%
RET	2	66%	-66pp	1	100%

The table lists the most commonly used market predictors in distress studies by the number of times they are selected for a prediction model (*Included*), the percentage of times the predictors are included across all of the studies that have them in their initial sample (*%Included*), the change in *%Included* when limiting the sample to studies that utilize a feature-selection approach for variable selection (*Δ%Included*), the number of times they are found to significantly differentiate between distressed and non-distressed firms (*Significant*), and the percentage of times the predictors are found to be significant across all studies that test their significance (*%Significant*). They are defined as follows: *MVE* (market value of equity), *EXRET* (excess return, various horizons), *SIZE* (firm market capitalization over index capitalization), and *RET* (raw return, various horizons), *L* (total liabilities).

The most commonly used market predictor is market value of equity to debt, which is another measure of leverage. This is consistent with the finding that leverage predictors are the most commonly used accounting figures. Some studies also utilize different measures of stock returns, often excess returns, but also the standard deviation of returns or simply raw returns. Finally, a few studies include a market-based size predictor, which measures the firm's market capitalization to the total index capitalization. The level of inclusion for the four most common market predictors is generally high and these predictors show significant differences in univariate tests. Even when limiting the sample to studies that utilize feature-selection methods, the most common market predictors show high levels of inclusion – higher than the general level for accounting predictors.

5.4. Non-financial predictors

Most predictors are based on financial data. However, distress prediction models can also include predictors constructed using non-financial data. These predictors can have significant predictive power even after including common financial predictors (Becchetti and Sierra, 2003). However, non-financial predictors are not frequently utilized in the literature, likely due to the perceived lack of data availability. Given their infrequent usage, this section does not provide a table of the most commonly used qualitative predictors. Rather, in the following, I discuss some of the qualitative predictors that have been used across various studies.

The most common qualitative predictors across the reviewed studies are various industry indicators for either a specific industry (e.g., Wilson, Chong, Peel, and Kolmogorov, 1995) or multiple industries/sectors (e.g., Alfaro, García, Gámez, and Elizondo, 2008; Jones and Hensher, 2004). Chava and Jarrow (2004) stress the importance of including industry effects in distress prediction models, as industry groupings effect both the intercepts and slope coefficients. Alfaro et al. (2008) include the firm's legal structure (i.e., public, limited, and other corporations) in their analysis. Wilson et al. (1995) introduce different predictors that reflect the ownership composition, including the percent of shares owned by directors and ownership concentration. Lennox (1999) examines some macroeconomic indicators, including the number of firms that entered bankruptcy that year and the results of an industrial trends survey. Park and Han (2002) add several qualitative measures to their model, such as growth potential, profit perspective, market niche, industry position, personnel and hiring policy, quality of innovation, competitive advantage, quality of management, and working conditions. Similarly, Becchetti and Sierra (2003) find many qualitative predictors (e.g., customer concentration, subcontracting status, and presence of competitors in the same region) that have significant predictive power.

6. Summary and suggestions for future research

Developers of distress prediction models face several choices when constructing their models. Among those are selecting the definition of distress (i.e., the dependent variable), the method used to estimate risk, and the predictors to include in the model (i.e., the independent variables). This review considers how more than 100 studies have approached these decisions and summarizes the decisions made across the studies.

When defining distress, the vast majority of studies utilize bankruptcy as an indicator of distress, categorizing firms that do not file for bankruptcy as “healthy”. Although some studies utilize different definitions (e.g., default or financial), these definitions are rarely used and often only to identify “gray area” firms (i.e., those that are somewhere between bankrupt and healthy). This type of definition ensures that the bar for classification as “distressed” is set rather high, as bankruptcy is an indicator of severe levels of distress relative to the various default and financial definitions. As such, the most commonly used definition is not suitable for constructing early warning models. Ultimately, users of distress prediction models should consider the priority of their claims when deciding which definition to apply. For highly prioritized claimants, using bankruptcy may be appropriate, while for others it might be more insightful to use more aggressive definitions of distress, such as default or omittance of dividends.

With respect to the methods used to estimate distress risk, two branches of the literature should be considered. The first consists of the fields of accounting, finance, and economics. Here, the most commonly used method has changed over time, with DA being the most common method from the 1960s through the 1980s, while logit and hazard models have become more common in recent decades. Today, few studies utilize DA for distress prediction. In contrast, the second branch, which consists of studies in the fields of computer science and information management, has primarily utilized complex intelligent methods, the most common being neural networks. This branch emerged during the 1990s and has been the driving force behind the significant development and testing of various techniques for distress prediction, some of which have also been utilized in other fields. Generally, intelligent methods are considered superior to statistical technique, although there is a trade-off between accuracy and simplicity.

Finally, conventional financial ratios make up the majority of the predictors utilized across the distress models, with accounting predictors being the most common by far. Not surprisingly, the variables used by Altman (1968) and Ohlson (1980) are among the most widely utilized and their models are considered the most influential within the literature. The most popular accounting predictors have inclusion rates of around 60 percent to 80 percent, and univariate tests generally find significant differences between the distressed and non-distressed groups for these predictors. However, the rate of inclusion is lower when limiting the sample to studies that use a feature-selection approach. In contrast, the market predictors, although less popular than the accounting predictors, exhibit high levels of inclusion regardless of whether a feature-selection approach is utilized. Finally, some non-financial predictors have also been utilized but not frequently enough to allow for a more comprehensive analysis. These findings suggest that many predictors may be suitable for distress prediction, but their relevance is likely contingent on the context, as evidenced by the variations in inclusion rates.

The findings of this review highlight several potential avenues for future research. First, more research is needed on whether the choice of distress definition has a substantial impact on distress models. One key aspect in this regard is prediction accuracy – that is, whether models are inherently more accurate when asked to predict bankruptcy, default, or other indicators of distress. Another is whether the distress definition influences predictor selection, as certain levels of distress may be reflected differently in a firm’s financials. In addition, more research is needed on how contextual differences affect distress models. Such contextual differences may include differences in bankruptcy regulations and practices, the level of competition, and firm composition within countries or industries. Finally, there is a need for additional research on whether and which qualitative predictors are suitable for predicting distress. Although various studies have shown that non-financial predictors can provide incremental predictive power, additional research might be needed, as there could be significant untapped potential in qualitative predictors.

References

Ahn, H., & Kim, K.-j. (2009). Bankruptcy prediction modeling with hybrid case-based reasoning and genetic algorithms approach. Applied Soft Computing, 9(2), 599-607.

Alaka, H., Oyedele, L., Owolabi, H., Kumar, V., Ajayi, S., Akinade, O., & Bilal, M. (2018). Systematic review of bankruptcy prediction models: Towards a framework for tool selection. Expert Systems with Application, 94, 164-184.

Alfaro, E., García, N., Gámez, M., & Elizondo, D. (2008). Bankruptcy Forecasting: An empirical comparison of AdaBoost and neural networks. Decision Support Systems, 45(1), 110-122.

Altman, E. I. (1968). Financial Ratios, Discriminant Analysis and the Prediction of Corporate Bankruptcy. The Journal of Finance, 23(4), 589-609.

Altman, E. I. (2018). A Fifty-Year Retrospective on Credit Risk Models, the Altman Z-Score Family of Models and their Applications to Financial Markets and Managerial Strategies. Journal of Credit Risk, 14(4), 1-34.

Altman, E. I., Haldeman, R. G., & Narayanan, P. (1977). ZETA Analysis: A new model to identify bankruptcy risk of corporations. Journal of Banking and Finance, 1(1), 29-54.

Anandarajan, M., Lee, P., & Anandarajan, A. (2001). Bankruptcy prediction of financially stressed firms: an examination of the predictive accuracy of artificial neural networks. Intelligent Systems in Accounting, Finance and Management, 10(2), 69-81.

Back, B., Laitinen, T., & Sere, K. (1996). Neural networks and genetic algorithms for bankruptcy predictions. Expert Systems with Applications, 11(4), 407-413.

Bauer, J., & Agarwal, V. (2014). Are hazard models superior to traditional bankruptcy prediction approaches? A comprehensive test. Journal of Banking and Finance, 40, 432-442.

Beaver, W. H. (1966). Financial Ratios as Predictors of Failure. Journal of Accounting Research, 4, 71-111.

Beaver, W. H., Correia, M., & McNichols, M. F. (2011). Financial Statement analysis and the Prediction of Financial Distress. Foundations and Trends in Accounting, 5(2), 99-173.

Beaver, W. H., Correia, M., & McNichols, M. F. (2012). Do differences in financial reporting attributes impair the predictive ability of financial ratios for bankruptcy? Review of Accounting Studies, 17, 969-1010.

Beaver, W. H., McNichols, M. F., & Rhie, J.-W. (2005). Have Financial Statements Become Less Informative? Evidence from the Ability of Financial Ratios to Predict Bankruptcy. Review of Accounting Studies, 10, 93-122.

Becchetti, L., & Sierra, J. (2003). Bankruptcy risk and productive efficiency in manufacturing firms. Journal of Banking and Finance, 27(11), 2099-2120.

Begley, J., Ming, J., & Watts, S. (1996). Bankruptcy classification errors in the 1980s: An empirical analysis of Altman's and Ohlson's models. Review of Accounting Studies, 1(4), 267-284.

Betts, J., & Belhoul, D. (1987). The Effectiveness of Incorporating Stability Measures in Company Failure Models. Journal of Business Finance and Accounting, 14(3), 323-334.

Bian, H., & Mazlack, L. (2003). Fuzzy-rough nearest-neighbor classification approach. International Conference of the North American Fuzzy Information Processing Society, (pp. 500-505).

Blum, M. (1974). Failing Company Discriminant Analysis. Journal of Accounting Research, 12(1), 1-25.

Bryant, S. M. (1997). A case-based reasoning approach to bankruptcy prediction modeling. Intelligent Systems in Accounting, Finance and Management, 6(3), 195-214.

Carson, E., Fargher, N. L., Geiger, M. A., Lennox, C. S., Raghunandan, K., & Willekens, M. (2013). Audit Reporting for Going-Concern Uncertainty: A Research Synthesis. Auditing: A Journal of Practice & Theory, 32(1), 353-384.

Castagna, A. D., & Matolcsy, Z. P. (1981). The Prediction of Corporate Failure: Testing the Australian Experience. Australian Journal of Management, 23-50.

Chava, S., & Jarrow, R. A. (2004). Bankruptcy Prediction with Industry Effects. Review of Finance, 8(4), 537-569.

Coats, P. K., & Fant, L. F. (1993). Recognizing Financial Distress Patterns Using a Neural Network Tool. Financial Management, 22(3), 142-155.

Dambolena, I. G., & Khoury, S. J. (1980). Ratio Stability and Corporate Failure. The Journal of Finance, 35(4), 1017-1026.

Davydenko, S., & Franks, J. (2008). Do Bankruptcy Codes Matter? A Study of Defaults in France, Germany, and the U.K. Journal of Finance, 63(2), 565-608.

Deakin, E. B. (1972). A Discriminant Analysis of Predictors of Business Failure. Journal of Accounting Research, 10(1), 167-179.

Dimitras, A. I., Slowinski, R., Susmaga, R., & Zopounidis, C. (1999). Business failure prediction using rough sets. European Journal of Operational Research, 114, 263-280.

Duffie, D., Saita, L., & Wang, K. (2007). Multi-period corporate default prediction with stochastic covariates. Journal of Financial Economics, 83(3), 635-665.

Franks, J. R., & Torous, W. N. (1994). A comparison of financial recontracting in distressed exchanges and chapter 11 reorganizations. Journal of Financial Economics, 35(3), 349-370.

Frydman, H., Altman, E. I., & Kao, D.-L. (1985). Introducing Recursive Partitioning for Financial Classification: The Case of Financial Distress. The Journal of Finance, 40(1), 269-291.

Gilbert, L. R., Menon, K., & Schwartz, K. B. (1990). Predicting Bankruptcy for Firms in Financial Distress. Journal of Business Finance & Accounting, 17(1), 161-171.

Grice, J. S., & Ingram, R. W. (2001). Tests of the generalizability of Altman's bankruptcy prediction model. Journal of Business Research, 54(1), 53-61.

Grunert, J., & Weber, M. (2009). Recovery rates of commercial lending: Empirical evidence for German companies. Journal of Banking and Finance, 33(3), 505-513.

Hillegeist, S. A., Keating, E. K., Cram, D. P., & Lundstedt, K. G. (2004). Assessing the Probability of Bankruptcy. Review of Accounting Studies, 9(1), 5-34.

Hua, Z., Wang, Y., Xu, X., Zhang, B., & Liang, L. (2007). Predicting corporate financial distress based on integration of support vector machine and logistic regression. Expert Systems with Applications, 33(2), 434-440.

Izan, H. Y. (1984). Corporate distress in Australia. Journal of Banking & Finance, 8(2), 303-320.

Jo, H., Han, I., & Lee, H. (1997). Bankruptcy Prediction Using Case-Based Reasoning, Neural Networks, and Discriminant Analysis. Expert Systems with Applications, 13(2), 97-108.

Jones, S., & Hensher, D. A. (2004). Predicting Firm Financial Distress: A Mixed Logit Model. The Accounting Review, 79(4), 1011-1038.

Joy, O. M., & Tollefson, J. O. (1975). On the Financial Applications of Discriminant Analysis. The Journal of Financial and Quantitative Analysis, 10(5), 723-739.

Kahya, E., & Theodossiou, P. (1999). Predicting Corporate Financial Distress: A Time-Series CUSUM Methodology. Review of Quantitative Finance and Accounting, 13(4), 323-345.

Koh, H. C., & Killough, L. N. (1990). The Use of Multiple Discriminant Analysis in the Assessment of the Going-Concern Status of an Audit Client. Journal of Business Finance & Accounting, 17(2), 179-192.

Kumar, P. R., & Ravi, V. (2007). Bankruptcy prediction in banks and firms via statistical and intelligent techniques - A review. European Journal of Operational Research, 180(1), 1-28.

Lacher, R. C., Coats, P. K., Sharma, S. C., & Fant, L. F. (1995). A neural network for classifying the financial health of a firm. European Journal of Operational Research, 85(1), 53-65.

Laitinen, E. K. (1991). Financial Ratios and Different Failure Processes. Journal of Business Finance & Accounting, 18(5), 649-673.

Lau, A. H.-L. (1987). A Five-State Financial Distress Prediction Model. Journal of Accounting Research, 25(1), 127-138.

Lennox, C. (1999). Identifying failing companies: a re-evaluation of the logit, probit and DA approaches. Journal of Economics and Business, 51(4), 347-364.

Li, H., & Sun, J. (2009). Predicting business failure using multiple case-based reasoning combined with support vector machine. Expert Systems with Applications, 36(6), 10085-10096.

Marais, M. L., Patell, J. M., & Wolfson, M. A. (1984). The Experimental Design of Classification Models: An Application of Recursive Partitioning and Bootstrapping to Commercial Bank Loan Classifications. Journal of Accounting Research, 22, 87-114.

McKee, T. E. (1995). Predicting bankruptcy via induction. Journal of Information Technology, 10(1), 26-36.

McKee, T. E. (2000). Developing a bankruptcy prediction model via rough sets theory. Intelligent Systems in Accounting, Finance and Management, 9(3), 159-173.

McKee, T. E., & Lensberg, T. (2002). Genetic programming and rough sets: A hybrid approach to bankruptcy classification. European Journal of Operational Research, 138, 436-451.

Min, J. H., & Lee, Y.-C. (2005). Bankruptcy prediction using support vector machine with optimal choice of kernel function parameters. Expert Systems with Applications, 28(4), 603-614.

Min, S.-H., Lee, J., & Han, I. (2006). Hybrid genetic algorithms and support vector machines for bankruptcy prediction. Expert Systems with Applications, 31(3), 652-660.

Moyer, R. C. (1977). Forecasting financial failure: a re-examination. Financial Management, 6(1), 11-17.

Norton, C. L., & Smith, R. E. (1979). A Comparison of General Price Level and Historical Cost Financial Statements in the Prediction of Bankruptcy. The Accounting Review, 54(1), 72-87.

Ohlson, J. A. (1980). Financial Ratios and the Probabilistic Prediction of Bankruptcy. Journal of Accounting Research, 18(1), 109-131.

Park, C.-S., & Han, I. (2002). A case-based reasoning with the feature weights derived by analytic hierarchy process for bankruptcy prediction. Expert Systems with Applications, 23(3), 255-264.

Peel, M. J., & Peel, D. A. (1987). Some Further Empirical Evidence on Predicting Private Company Failure. Accounting and Business Research, 18(69), 57-66.

Pindado, J., Rodrigues, L., & de la Torre, C. (2008). Estimating financial distress likelihood. Journal of Business Research, 61(9), 995-1003.

Serrano-Cinca, C. (1996). Self organizing neural networks for financial diagnosis. Decision Support Systems, 227-238.

Shin, K.-S., & Lee, Y.-J. (2002). A genetic algorithm application in bankruptcy prediction modeling. Expert Systems with Applications, 23(3), 321-328.

Shin, K.-S., Lee, T. S., & Kim, H.-j. (2005). An application of support vector machines in bankruptcy prediction model. Expert Systems with Applications, 28(1), 127-135.

Shumway, T. (2001). Forecasting Bankruptcy More Accurately: A Simple Hazard Model. The Journal of Business, 74(1), 101-124.

Spanos, M., Dounias, G., Matsatsinis, N., & Zopouinidis, C. (1999). A fuzzy knowledge-based decision aiding method for assessment of financial risk: the case of corporate bankruptcy prediction. European Symposium on Intelligent Techniques, (pp. 14-21).

Taffler, R. J. (1984). Empirical Models for the Monitoring of UK Corporations. Journal of Banking and Finance, 8, 199-227.

Tashjian, E., Lease, R. C., & McConnell, J. J. (1996). An empirical analysis of prepackaged bankruptcies. Journal of Financial economics, 40(1), 135-162.

Thorburn, K. (2000). Bankruptcy auctions: costs, debt recovery, and firm survival. Journal of Financial Economics, 58(3), 337-368.

Tian, S., Yu, Y., & Guo, H. (2015). Variable selection and corporate bankruptcy forecasts. Journal of Banking & Finance, 52, 89-100.

Tinoco, M. H., & Wilson, N. (2013). Financial distress and bankruptcy prediction among listed companies using. International Review of Financial Analysis, 30, 394-419.

Wilcox, J. W. (1971). A Simple Theory of Financial Ratios as Predictors of Failure. Journal of Accounting Research, 9(2), 389-395.

Xu, M., & Zhang, C. (2009). Bankruptcy prediction: the case of Japanese listed companies. Review of Accounting Studies, 14, 534-558.

Zmijewski, M. E. (1984). Methodological Issues Related to the Estimation of Financial Distress Prediction Models. Journal of Accounting Research, 22, 59-82.

Appendix

Appendix I: List of papers included in the review

Paper	Def.	Met.	Pred.
Aharony et al. (1980)	Yes	Yes	-
Ahn and Kim (2009)	Yes	Yes	Yes
Alfaro et al. (2008)	Yes	Yes	Yes
Altman (1968)	Yes	Yes	Yes
Altman et al. (1977)	Yes	Yes	Yes
Anandarajan et al. (2001)	Yes	Yes	Yes
Atiya (2001)	Yes	Yes	Yes
Aziz and Lawson (1989)	Yes	Yes	Yes
Back et al. (1996)	Yes	Yes	Yes
Baek and Cho (2003)	Yes	Yes	Yes
Barboza et al. (2017)	Yes	-	Yes
Bauer and Agarwal (2014)	Yes	Yes	Yes
Beaver (1966)	Yes	-	Yes
Beaver et al. (2005)	Yes	Yes	Yes
Beaver et al. (2012)	Yes	Yes	Yes
Becchetti and Sierra (2003)	Yes	Yes	Yes
Betts and Belhoul (1982)[T]	-	Yes	Yes
Betts and Belhoul (1983)[T]	Yes	Yes	Yes
Betts and Belhoul (1987)	Yes	Yes	Yes
Bian and Mazlack (2003)	-	Yes	Yes
Bilderbeek (1977)[A]	Yes	Yes	Yes
Blum (1974)	Yes	Yes	Yes
Bryant (1997)	Yes	Yes	Yes
Cadden (1991)	-	Yes	Yes
Casey and Bartczak (1984)	Yes	-	Yes
Castagna and Matolcsy (1981)	Yes	Yes	Yes
Charitou et al. (2013)	Yes	-	Yes
Chava and Jarrow (2004)	Yes	Yes	Yes
Chaudhuri and De (2011)	Yes	Yes	Yes
Chen and Du (2009)	Yes	Yes	Yes
Chen et al. (2011)	Yes	Yes	Yes
Cheng et al. (2010)	Yes	Yes	Yes
Cielen et al. (2004)	Yes	Yes	Yes
Coats and Fant (1993)	Yes	Yes	Yes
Dambolena and Khoury (1980)	-	Yes	Yes
Datastream (1980)[T]	Yes	Yes	Yes
Deakin (1972)	Yes	Yes	Yes
Dimitras et al. (1999)	Yes	Yes	Yes
Duffie et al. (2007)	Yes	-	No
Edmister (1972)	Yes	Yes	Yes
El Hennawy and Morris (1983)	Yes	Yes	Yes
Fletcher and Goss (1993)	Yes	Yes	Yes
Frydman et al. (1985)	Yes	Yes	Yes
Gentry et al. (1985)	Yes	Yes	Yes
Gilbert et al. (1990)	Yes	Yes	Yes
Gombola et al. (1987)	Yes	-	Yes
Hillegeist et al. (2004)	Yes	Yes	-
Hua et al. (2007)	Yes	Yes	Yes
Izan (1984)	Yes	Yes	Yes
Jardin (2015)	Yes	-	Yes
Jardin and Severin (2011)	Yes	Yes	Yes
Jo and Han (1996)	Yes	Yes	-
Jo et al. (1997)	Yes	Yes	-
Jones (2017)	Yes	-	-
Jones and Hensher (2004)	Yes	Yes	Yes
Kahya and Theodossiou (1999)	Yes	Yes	Yes
Kaski et al. (2001)	Yes	Yes	No
Ketz (1978)	Yes	Yes	Yes
Kim and Kang (2010)	Yes	-	Yes
Kiviluoto (1998)	Yes	Yes	Yes
Ko (1982)[A]	Yes	Yes	Yes
Koh and Killough (1990)	-	Yes	Yes
Kwak et al. (2012)	Yes	Yes	Yes
Lacher et al. (1995)	Yes	Yes	Yes
Laitinen (1991)	Yes	Yes	Yes
Lau (1987)	Yes	Yes	Yes
Lee et al. (1996)	Yes	Yes	Yes
Lee et al. (2005)	Yes	Yes	Yes
Lennox (1999)	Yes	Yes	Yes
Leshno and Spector (1996)	Yes	Yes	-
Levitan and Knoblett (1985)	Yes	Yes	Yes
Li and Sun (2009)	Yes	Yes	Yes
Libby (1975)	Yes	Yes	Yes
Lin and McClean (2001)	Yes	Yes	-
Lis (1972)[T]	Yes	Yes	Yes
Lyandres and Zhdanov (2013)	Yes	-	Yes
Marais (1979)[T]	No	Yes	Yes
Marais et al. (1984)	Yes	Yes	No
McKee (2000)	Yes	Yes	Yes
McKee (2005)	Yes	-	Yes
McKee and Lensberg (2002)	Yes	Yes	Yes
Michael et al. (1999)	Yes	Yes	Yes
Min and Lee (2005)	Yes	Yes	Yes
Min et al. (2006)	Yes	Yes	Yes
Nam et al. (2008)	Yes	Yes	Yes
Nanda and Pendharkar (2001)	Yes	Yes	Yes
Norton and Smith (1979)	Yes	Yes	Yes
Ohlson (1980)	Yes	Yes	Yes
Park and Han (2002)	Yes	Yes	Yes
Peel and Peel (1987)	Yes	Yes	Yes
Pindado et al. (2008)	Yes	-	Yes
Pompe and Feelders (1997)	Yes	Yes	Yes
Raghupathi et al. (1991)	Yes	Yes	Yes
Reisz and Perlich (2007)	Yes	-	-
Ryu and Yue (2005)	Yes	Yes	Yes
Serrano-Cinca (1996)	Yes	Yes	Yes
Shin and Lee (2002)	Yes	Yes	Yes
Shin et al. (2005)	Yes	Yes	Yes
Shumway (2001)	Yes	Yes	Yes
Sun (2007)	Yes	Yes	Yes
Taffler (1976)[T]	Yes	Yes	Yes
Taffler and Tisshaw (1977)[T]	Yes	Yes	Yes

Appendix I: List of papers included in the review (cont.)

Paper	Def.	Met.	Pred.	Paper	Def.	Met.	Pred.
Takahashi et al. (1984)	Yes	Yes	Yes	Wilson et al. (1995)	Yes	Yes	Yes
Theodossiou (1993)	Yes	Yes	Yes	Xu and Zhang (2009)	Yes	Yes	Yes
Tian et al. (2015)	Yes	-	Yes	Yu et al. (2014)	Yes	-	Yes
Tinoco and Wilson (2013)	Yes	-	Yes	Zavgren (1985)	Yes	Yes	Yes
Tisshaw (1976)[T]	-	Yes	Yes	Zhang et al. (1999)	Yes	-	Yes
Tsukuda and Baba (1994)	Yes	Yes	Yes	Zimmer (1980)	Yes	Yes	Yes
Wilcox (1973)	Yes	Yes	No	Zmijewski (1984)	Yes	Yes	Yes
Wilson and Sharda (1994)	Yes	-	Yes				

[T] Indicates that information was gathered from Taffler (1984).
[A] Indicates that information was gathered from Altman (1984).

GOING BEYOND TWELVE MONTHS

ARE AUDITORS' GOING CONCERN ASSESSMENTS ACCURATE BEYOND THE APPLIED TIME HORIZON?

Anna Gold and Ibrahim Pelja*

ABSTRACT: Standard-setters are considering extending the time horizon for auditors' going concern opinions (GCOs) beyond twelve-months. We explore whether GCOs accurately predict bankruptcy beyond this horizon, consider the consequences of recurring GCOs, and examine the circumstances under which long-term accuracy is greatest. We show that auditors' GCOs are accurate two years after their issuance. Furthermore, we demonstrate that "old" opinions provide important information even considering more recent opinions. Recurring GCOs suggest a higher likelihood of bankruptcy. Similarly, the risk of bankruptcy is significantly higher if the previous opinion raised doubt, even if the most recent opinion did not. Finally, we show that long-term accuracy is greater if GCOs cite liquidity, profitability, or default issues, and Big4 auditors are generally more accurate. The findings have policy implications, as they contribute to the debate concerning whether auditors should consider a longer period by showing that auditors are capable of doing so even in today's institutional setting. However, a change may have limited value, as opinions issued today already have long-term predictive power.

KEYWORDS: going concern opinion, bankruptcy, long-term prediction

---

* Anna Gold is a Professor at the Vrije Universiteit Amsterdam and adjunct Professor at the Norwegian School of Economics. Ibrahim Pelja is a PhD candidate at the Norwegian School of Economics. Ibrahim Pelja is the corresponding author and can be reached at: ibrahim.pelja@nhh.no.

1. Introduction

We explore whether auditors' going concern (GC) assessments are accurate beyond the currently applied time horizon of 12 months. This an important topic for two reasons. First, whether auditors' GC assessments should consider a look-forward period beyond 12 months has been the subject of multiple discussions in the U.S. and internationally.[1] For instance, the extension of the look-forward period has been a topic of discussion in PCAOB Standing Advisory Group meetings. The argument for extending this period is that doing so would enable auditors to consider more events and conditions, which could increase the usefulness of their evaluations (SAG 2011, 2012). In addition to raising this issue during its meetings, the PCAOB Investor Advisory Group conducted an investor survey that included questions on the topic. This showed that 27.5 percent of respondents felt that the look-forward period should be one to three years, while 60 percent believed it should be one year but also consider foreseeable events (IAG 2012). Moreover, the FASB has proposed and discussed extending the look-forward period to 24 months after the financial statement date (FASB 2013, 2014). Outside the U.S., the IAASB has issued a call for perspectives on whether entities should be required to assess substantial doubt beyond 12 months (IAASB 2020).

The question of whether GC assessments are accurate beyond the currently applied time horizon contains some inherent tension. On one hand, they may be accurate beyond 12 months if the issues that are raised fail to be resolved in a timely manner or if auditors do not have a strictly defined period in mind when making the assessment. Conversely, the assessments may not apply beyond the applied horizon if auditors are reluctant to communicate GC issues that are only relevant beyond 12 months (Guiral, Ruiz, and Rodgers 2011) or if they simply lack the ability to make long-term predictions (IAASB 2020). All of these scenarios are plausible. By

[1] The "look-forward period" is the period of time over which the entity's ability to continue as a going-concern is assessed.

exploring the accuracy of opinions issued under the current institutional setting, we can provide insights relevant for the aforementioned debate and highlight important policy implications. If auditors' opinions are accurate beyond 12 months, then they can readily make more long-term assessments, which would suggest that a standard change might not be necessary. In contrast, if auditors' opinions are not accurate beyond 12 months, we would have evidence indicating that a change might be necessary.

The second key issue relates to the fact that "old" GC assessments (i.e., those issued more than 12 months ago) may provide incremental improvements to the accuracy of new assessments. For example, consecutive going concern opinions (GCOs) could indicate a greater risk of bankruptcy compared to first-time GCOs.[2] Similarly, an entity that has not recently received a GCO may still have a significant likelihood of failure if it has previously received a GCO. Alternatively, consecutive GCOs may only provide incremental predictive power if the basis for their issuance differs, such that the most recent GCO is not simply a copy of the former. Similarly, as the most recent assessment is based on newer information, a clean bill of health may negate the risk that was reflected in a previous GCO.

We utilize data on auditors' GCOs for U.S. firms between 2000 and 2019 to explore how accurate they are beyond the currently applied horizon. Consistent with prior research, accuracy is determined by whether the conclusions drawn in an auditor's assessment are significantly associated with the client filing for bankruptcy within the period in question i.e., the opposite of a type II error (Carson et al. 2013; Geiger, Gold, and Wallage 2021). To assess the long-term accuracy of GCOs, we estimate a model in which the dependent variable indicates whether the client filed for bankruptcy within subsequent look-forward periods. By using look-forward periods beyond the currently applied horizon, we can effectively test whether opinions

[2] We follow the established convention in the academic literature and use "GCO" to indicate a going concern assessment that communicates substantial doubt regarding the entity's ability to continue as a going concern.

accurately predict bankruptcy beyond one year. In addition, we explore whether "old" GC assessments provide incremental predictive power to a model that includes the current assessment. We do so by decomposing the successive opinions based on whether the conclusions changed between two opinions and regress this on the client filing for bankruptcy.

The results of our analyses show that auditors' GC assessments accurately predict bankruptcies that occur within three years of their issuance. As expected, the predictive value of the GCO is (relatively) higher when using a look-forward period of 12 months. Furthermore, we show that the conclusions from the previously issued GCO provide incremental information to the most recently issued GCO. We also find that recurring GCOs suggest a higher likelihood of bankruptcy and that a firm has a significant risk of bankruptcy if the previously issued opinion raised substantial doubt, even when the most recent opinion did not raise substantial doubt. These findings are relatively robust to propensity-score matching and the use of a more sophisticated bankruptcy model.

Moreover, we perform several additional cross-sectional analyses to further explore these findings. These analyses include testing the role of the specific GC issues raised in the auditor's opinion (e.g., liquidity, profitability), auditor conservatism, and auditor size. We find that references to liquidity, profitability, and default issues in the auditor's opinion consistently show predictive power across different look-forward periods. In contrast, solvency and business-model issues do not show consistent predictive ability. With regard to auditor characteristics, we do not find that auditor conservatism explains variations in GCO accuracy. However, we do find that Big4 auditors are more accurate across all look-forward periods.

Our findings provide insights into the longer term accuracy of auditors' GCOs. First, we find robust evidence that auditors can make accurate assessments about a client's financial health beyond 12 months. Furthermore, we show that previously issued opinions provide some important information and context beyond the most recent opinion. As such, auditors' opinions appear to be relevant beyond 12 months, even if a new assessment is issued in the meantime. Our findings also have policy implications. They suggest that extending the applied horizon could be a viable option and would not be limited by auditors' abilities to make assessments relevant to the longer term. However, these findings could also be used to argue that no change is needed, as auditors' opinions are already accurate beyond 12 months. In addition, our analysis shows that opinions citing liquidity, profitability, or default issues are associated with the greatest risk of bankruptcy across all look-forward periods. Finally, our findings have some academic implications, as we highlight how different combinations of successive opinions reflect varying levels of bankruptcy risk. As such, limiting a sample to first-time or non-recurring GCOs could result in a loss of valuable information.

2. Background and hypothesis development

2.1. Background and literature review

Financial statement users, standard-setters, and regulators assign considerable importance to management's and auditors' determinations of whether an entity will continue as a "going concern." Under existing financial reporting standards, management is tasked with evaluating "whether there are conditions and events […] that raise substantial doubt about an entity's ability to continue as a going concern within one year after the date that the financial statements are issued" (FASB Subtopic 205-40). Such events and conditions ordinarily relate to the entity's ability to meet its obligations as they fall due and should be evaluated accordingly. The evaluation should be based on both qualitative and quantitative information concerning conditions and events that are reasonably knowable at the time the financial statements are issued. Initially, the assessment should not incorporate any mitigating effects from unimplemented plans. However, management should also evaluate whether its plans will alleviate substantial doubt about the entity's ability to continue as a GC. In the event that substantial doubt is raised but alleviated by management's plans, the entity should disclose the following information in the footnotes: (1) the conditions and events that raised substantial doubt, (2) management's evaluation of those conditions and events in relation to the entity's ability to continue as a going concern, and (3) how management's plans will alleviate the substantial doubt (FASB Subtopic 205-40). Should the substantial doubt not be alleviated, the entity must disclose the aforementioned information and include a statement indicating that there is substantial doubt about the entity's ability to continue as a GC for a period of one year.

Auditors are also responsible for evaluating whether there is substantial doubt regarding an entity's ability to continue as a GC for "a reasonable period of time," which is "not to exceed one year beyond the date of the financial statements being audited" (PCAOB AS 2415). Similar to management, auditors should base their evaluations on relevant conditions and events, and obtain information about management's plans and assess the likely effectiveness of those plans. The auditor is responsible for obtaining sufficient appropriate audit evidence and to conclude on management's use of the going concern assumption in the preparation of the financial statements (PCAOB AS 2415). If the auditor concludes that there is substantial doubt, they should: (1) consider the adequacy of disclosure concerning the entity's inability to continue as a going concern and if adequate (2) include an explanatory paragraph that reflects their conclusion (PCAOB AS 2415).

An auditor might issue a GCO for a number of reasons. The determinants of GCOs can be roughly divided into four categories: client characteristics, auditor characteristics, auditor-client relationship characteristics, and environmental factors (Carson et al. 2013). Client characteristics are arguably the most important factor when issuing an opinion (Geiger et al. 2021). Several studies find that measures of financial distress relate to the issuance of GCOs (see Carson et al. 2013), and a survey by LaSalle and Anandarajan (1996) confirms that audit partners consider various news and characteristics when making GC assessments. Furthermore, other factors may ultimately play a role in whether a GCO is issued. These include the auditor's experience (Lehman and Norman 2006), auditor size (Berglund, Eshleman, and Guo 2018), the threat of auditor switching (Lennox 2000), the audit firm's tenure (Geiger and Raghunandan 2002), and litigation risk (Geiger, Raghunandan, and Rama 2006). However, we expect the potential for financial distress to be the dominant determinant.

Naturally, attention is paid to whether these GCOs are ultimately "accurate."[3] A GCO's accuracy is typically defined by whether the client files for bankruptcy within the look-forward period (Carson et al. 2013; Geiger et al. 2021).[4] Carson et al. (2013) find that 40 to 50 percent of all companies filing for bankruptcy in the U.S. between 2000 and 2010 had not received a GCO in the year prior to filing. The academic literature labels this type of classification error as a Type II error. These errors are typically considered costly for the audit firm in question. There are multiple explanations for why Type II error rates are relatively high, including a reduced threat of litigation (Geiger and Raghunandan 2001), a fear of auditor switching (Lennox 2000), or a lack of auditor expertise (Arnold, Collier, Leech, and Sutton 2001).[5] The error rate also depends on various auditor, client, and environmental attributes, such as BigN (Myers, Schmidt and Wilkins 2014), the client's business strategy (Chen, Eshleman, and Soileau 2017), events like the 2008-2009 global financial crisis (Carson, Fargher, and Zhang 2019; Geiger, Raghunandan, and Riccardi 2014), and awareness of SEC enforcement (DeFond, Francis, and Hallman 2018). Despite the seemingly high Type II error rates, several studies find

[3] Interest in this topic is not limited to researchers. This is a topic of public interest and articles documenting the issuance of a GCO – or lack therefore – are frequently published in the media (e.g., Financial Times 2019; Wall Street Journal 2021).
[4] Notably, auditing standards do not clearly specify how the opposite of a "going concern" should be defined. As such, the standards leave room for different interpretations of how to categorize these firms. Although bankruptcy is the most commonly applied definition, a "non-going concern" could also be categorized in terms of outcomes such as takeovers, mergers, or delistings (Nogler, 1995).
[5] In addition, some business failures are due to fraud (e.g., Enron). Misclassification of these cases is not strictly due to an inappropriate going concern assessment.

a significant positive correlation between GCOs and bankruptcy. In fact, GCOs may even provide incremental information to bankruptcy models (Sun 2007).

In addition to not issuing a GCO prior to a client filing for bankruptcy, an auditor can commit another type of error. Carson et al. (2013) find that 15.7 percent of all surviving firms in his sample received a GCO. Such a classification error is labeled a Type I error. These errors are typically considered less costly than a Type II error but can still have negative consequences for the client and the auditor. However, denoting these misclassifications as "errors" may be misleading. First, auditors are not tasked with predicting bankruptcy specifically and a distressed firm has numerous alternatives to bankruptcy (Nogler, 1995). Second, "substantial doubt" does not necessarily entail a greater probability of bankruptcy than survival. As such, these Type I errors should not be considered 'errors' to the same extent as Type II errors.

As auditors' GCOs communicate important information about the financial health of an entity, we expect the consequences of GCOs to be substantial. An extensive stream of research considers whether financial markets react to GCOs (see Carson et al. 2013; Geiger et al. 2021). However, the results of these studies are mixed, with the biggest challenge being the need to isolate the impact of the GCO from concurrent disclosure (Myers, Shipman, Swanquist, and Whited 2018). For lenders, GCOs signal a substantial risk of loss and are consequently associated with loan spreads, size, and maturity, required collateral, and the use of covenants (Chen, He, Ma, and Stice 2016). Another frequently mentioned consequence is the "self-fulfilling prophecy," which suggests that an entity receiving a GCO is more likely to fail because the GCO creates negative consequences. However, Gerakos, Hahn, Kovrijnykh, and Zhou (2016) find that the increase in probability of bankruptcy is negligible. Nevertheless, GCOs do have some negative consequences for the client, such as a higher cost of capital (Amin, Krishnan, and Yang 2014; Chen et al. 2016) and credit-rating downgrades (Feldman and Read 2013). Finally, GCOs have consequences for the auditors, including an increased risk of auditor switching (Lennox 2000; Vanstraelen 2003), a decreased litigation risk for auditors that issue GCOs (Kaplan and Williams 2013), and more favorable judgments among investors (Wright and Wright 2014).

2.2. Hypothesis development

Despite the extensive research on auditors' GCOs, no study to date has explored the accuracy of GCOs beyond the currently applied one-year time horizon. In some ways, this is not surprising, as auditors are currently not asked to consider a time period beyond 12 months. As such, there are no institutional settings in which auditors make long-term evaluations of an entity's ability to continue as a GC. Furthermore, as GCOs are issued on an annual basis, another opinion should be issued by the time the initial look-forward period ends. Nevertheless, this topic warrants some exploration for two reasons. First, whether auditors should be asked to consider a longer look-forward period has been the topic of multiple discussions (IAG 2012; IAASB 2020; FASB 2013, 2014; SAG 2011, 2012). Second, stakeholders are unlikely to disregard previously issued GCOs. The extant literature acknowledges this, with studies that measure market responses to GCOs often limiting their analyses to first-time or non-recurring GCOs (e.g., Menon and Williams, 2010; Myers et al. 2018).

In light of these considerations, we present the first hypothesis we aim to answer in this study:

> **H1:** *An auditor's reporting of going concern problems is accurate beyond the auditor's currently applied time horizon.*

We also explore how different combinations of successive opinions reflect varying degrees of distress. Successive opinions may provide incremental information about the risk of failure for two reasons. First, recurring GCOs suggest that either the issue at hand was not resolved in the preceding 12 months or a new issue has materialized. Both possibilities imply that the problems facing the company are substantial. Second, the risk of failure, as communicated by a previous opinion, may still be significant even if the firm subsequently receives an opinion that does not raise substantial doubt. We therefore present the following two hypotheses:

> **H2:** *Recurring GCOs reflect an increased risk of bankruptcy.*
>
> **H3:** *The risk of bankruptcy is significant and positive when the previous opinion raised substantial doubt, even if the most recent opinion did not do so.*

Note that we do not formulate any hypotheses for cases in which a GCO follows an opinion that does not communicate substantial doubt or two consecutive opinions that do not raise substantial doubt. The reason for this is that these specifications are typically the ones used in prior research, and as such the risk of bankruptcy for these combinations of successive opinions has already been established.

We also explore several factors that might explain why and when GCOs are more accurate beyond the applied period. First, we consider whether GCOs are more accurate when using longer look-forward periods if they raise specific issues, similar to Desai, Desai, Kim, and Raghunandan (2020). Specifically, we expect liquidity issues to be less relevant when using longer look-forward periods, while solvency and business-model issues should be more relevant when using longer horizons. This is because liquidity is inherently a short-term issue, whereas capital structure and business-model viability are long-term issues. Next, we investigate whether auditor conservatism partly explains the variation in accuracy beyond the applied horizon. This could be the case if conservative auditors are more motivated to issue GCOs based on issues that may lead to bankruptcy further down the road. Finally, we explore whether opinions issued by Big4 auditors are more or less accurate beyond the applied time horizon. The extant literature finds that Big4 auditors are more inclined to raise substantial doubt (Berglund et al. 2018). Therefore, we might expect these auditors to be more likely to raise substantial doubt due to issues that are relevant beyond 12 months.

3. Research design

3.1. Logit model

We utilize the following general logit model to answer the research questions raised in this study:

$$BANK_{it} = \alpha + \lambda GC_{it} + \gamma X_{it} + \delta_t + \delta_j$$

Consistent with previous studies, we use bankruptcy to assess the accuracy of GCOs (Carson et al. 2013; Geiger et al. 2021). Accurate GCOs are those that issue a clean opinion prior to the entity remaining in business or communicate substantial doubt prior to the client filing for bankruptcy. The dependent variable, *BANK*, is an indicator variable equal to 1 if firm *i* files for bankruptcy within a specified period following the issuance of a GCO at time *t*, and 0 otherwise. We utilize the following time periods depending on the purpose of the analysis: (i) 0 to 12 months, (ii) 0 to 24 months, (iii) 0 to 36 months, (iv) 12 to 24 months, and (v) 24 to 36 months. The different time periods enable us to assess whether GCOs are relevant for different look-forward periods. We include both Chapter 7 and Chapter 11 filings, and we only consider first-time bankruptcies, in line with prior studies.[6]

The variable(s) of interest are represented by the vector GC. This includes the conventional *GCO* variable, which is equal to 1 if the auditor of firm *i* communicates substantial doubt at time *t*, and 0 otherwise. We also utilize three indicator variables that identify whether recurring

[6] Although our sample covers the period from 2000 through 2021, our bankruptcy data spans back to 1988. A bankruptcy is thus defined as a first-time bankruptcy if it is the first time the firm files for bankruptcy across the full period spanning back to 1988.

GCOs were issued (*recGCO*), a GCO was issued following a non-GCO (*newGCO*), or a non-GCO was issued following a GCO (*newnoGCO*). For the additional analyses, we decompose GCOs by whether they raise a specific GC issue related to profitability (*GCO_PRF*), liquidity (*GCO_LIQ*), solvency (*GCO_SOL*), the business model (*GCO_BUS*), default (*GCO_DFLT*), or miscellaneous (*GCO_MISC*).[7] Substantial doubt is identified by the first occurrence of substantial doubt being communicated following the fiscal year-end and no later than six months after the fiscal year-end. Observations in which the issuance of a GCO precedes the fiscal year-end are omitted. Following Desai et al. (2020), we also remove observations in which the opinion specifically mentions filing for bankruptcy as an issue.

The vector X represents different control variables commonly used in the literature (e.g., Berglund et al. 2018; Desai et al. 2020; Geiger and Rama, 2006). These include whether the auditor is a Big4 auditor (*BIG4*), the natural log of total assets (*SIZE*), Altman's Z-score (*ZSCORE*) as modified by Hillegeist, Keating, Cram, and Lundstedt (2004), and whether the firm is listed on a large stock exchange (*EXCH*). In addition, as a robustness test, we construct a more sophisticated model using several additional control variables, including leverage (*LEV*), working capital to total assets (*WCTA*), the current ratio (*CURR*), a dummy for negative equity (*NEG_EQ*), the return on assets (*ROA*), operating cash flow to liabilities (*OCFL*), consecutive losses (*LOSS2*), change in net income (*CHNG_NI*), cash to total assets (*CASH*), book-to-market ratio (*MB*), and the change in the stock price (*CHNG_P*). Finally, we follow prior literature and include both year fixed effects, δ_t, and industry fixed effects, δ_j. All variable definitions are provided in Appendix I.

[7] See Appendix II for an explanation of how issues are classified and the frequency with which they occur in our sample.

3.2. Sample and descriptive statistics

The study uses data from 2000 to 2021, and includes a total of 10,712 unique non-financial U.S. firms and a total of 85,162 firm-year observations. Data on auditors' GCOs is obtained from Audit Analytics. We identify a unique GCO as the first filing subsequent to the fiscal year-end that communicates substantial doubt over the ensuing period of up to six months. For non-GCO opinions, we limit the sample to one filing per firm per year, and we prioritize 10K filings, followed by 10K405, 10KSB, 10KSB40, 20F, and 40F filings. We exclude all firms for which SIC codes are missing across all firm-years and financial firms (SIC codes 60-69). The sample is also limited to U.S. firms. Data on bankruptcy filings is obtained from Audit Analytics, and we exclude all firm-year observations following the first bankruptcy filing. Following Desai et al. (2020), we also remove GCOs that cite "filing for bankruptcy" as a reason for issuing the GCO. Finally, financial data is obtained by merging the Audit Analytics data with Compustat data. A breakdown of our sampling procedures is provided in Table 1. As robustness analyses, we also construct two matched samples in which we match bankrupt/GCO firms with non-bankrupt/non-GCO firms.

Table 1: Sample construction process

Description	**ΔFirm-year**	**Firm-year**	**Firm**
Unique going-concern opinions from Audit Analytics (2000-2019)		264,506	53,123
(Missing SIC code)	(66,129)	198,377	29,840
(Financial firms, SIC codes 60-69)	(46,514)	151,863	21,932
(Foreign firms)	(26,597)	125,266	17,688
(Observations following first bankruptcy filing)	(2,751)	122,515	17,589
(Bankruptcy filing highlighted in GCO)	(165)	122,350	17,560
(Merge with Compustat)	(37,188)	85,162	10,712
(Missing data for control variables)	(18,500)	66,662	8,848
Initial sample		**66,662**	**8,848**
Bankrupt		*762***	*762*
GCO		*9,788*	*2,980*

The table describes how we construct our initial sample. The data used in this study is unique GCOs collected from Audit Analytics spanning the years 2000 to 2019. We drop firms for which no industry can be identified using SIC codes, and we follow the common practice of excluding financial firms. The sample is limited to U.S. firms. We limit our sample to first-time bankruptcy filers and exclude all observations following the first bankruptcy filing. Finally, the data is merged with Compustat using firm CIK codes.

* By construct, no firm can have multiple firm-years where BANK = 1. However, we also define a firm as bankrupt if it files for bankruptcy within two or three years. In these cases, the numbers of bankrupt firm-year observations are 1,650 and 2,424, respectively.

Table 2 presents descriptive statistics for the variables used in the study. We generally find that for those observations in which the client is categorized as healthy (i.e., did not receive a GCO or file for bankruptcy), most variables indicate lower levels of distress. Interestingly, the average value for most variables in the GCO group suggest higher levels of distress when compared to the bankrupt group. Altogether, the descriptive statistics indicate that there are large differences between healthy firms and those that receive a GCO or file for bankruptcy.

Table 2: Descriptive statistics

	Healthy	**GCO**	**Bankrupt**
BANK	-	4.6%	-
GCO	-	-	60.4%
recGCO	-	78.0%	26.9%
newGCO	-	22.0%	34.6%
newnoGCO	-	-	1.54%
GCO_PRF	-	91.4%	51.3%
GCO_LIQ	-	57.1%	37.7%
GCO_SOL	-	20.0%	15.6%
GCO_BUS	-	28.0%	7.74%
GCO_DFLT	-	10.2%	20.2%
GCO_MISC	-	1.56%	1.44%
BIG4	68.8%	15.5%	51.0%
SIZE	5.70	1.51	4.45
	(2.21)	(1.76)	(2.21)
ZSCORE	-5.59	-6.88	-4.81
	(1.98)	(3.11)	(1.35)
EXCH	47.7%	12.0%	5.77%
LEV	0.52	1.49	1.12
	(0.32)	(0.98)	(0.66)
WCTA	184.1	-2.91	19.1
	(301.5)	(26.83)	(128.7)
CURR	2.85	1.13	1.21
	(2.17)	(1.80)	(1.27)
NEG_EQ	4.9%	57.1%	43.0%
ROA	-0.07	-1.40	-0.67
	(0.33)	(1.07)	(0.81)
OCFL	0.02	-0.64	-0.30
	(0.63)	(0.80)	(0.62)
LOSS2	34.4%	78.6%	70.6%
CHNG_NI	0.02	-0.05	-0.15
	(0.56)	(0.51)	(0.56)
CASH	0.21	0.24	0.14
	(0.23)	(0.28)	(0.20)
MB	2.85	0.66	0.78
	(3.43)	(5.71)	(3.65)
CHNG_P	1.14	0.99	0.65
	(0.60)	(0.86)	(0.63)

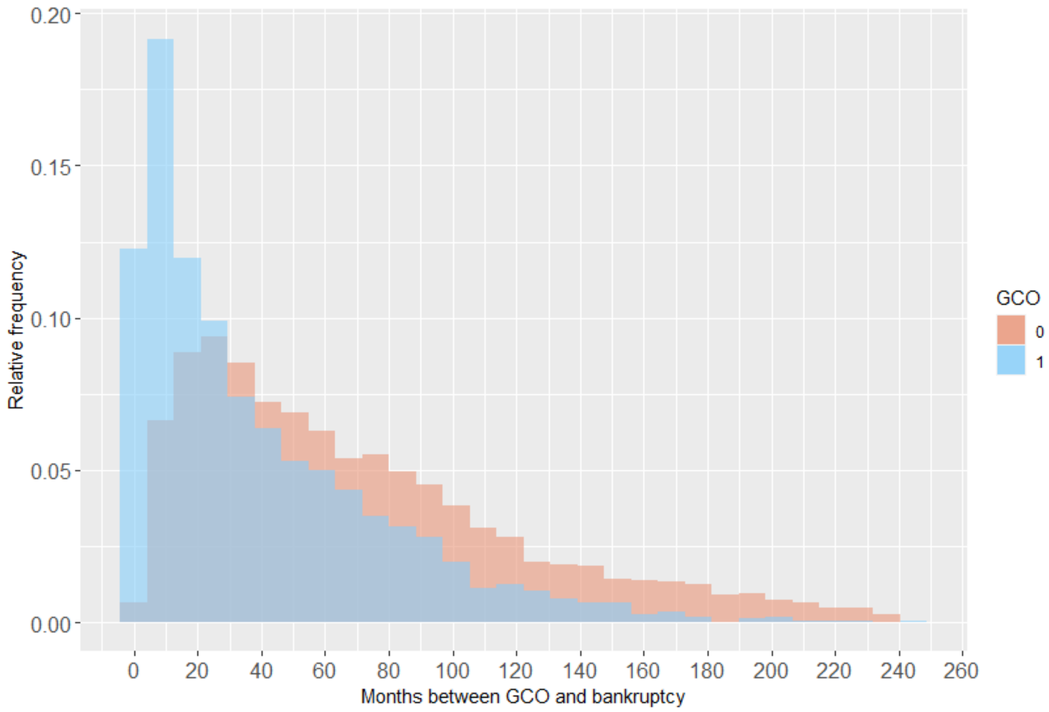
The table shows descriptive statistics for all variables applied in the study. A healthy firm is defined as a firm that did not receive a GCO or file for bankruptcy in the given year (regardless of whether it ultimately received a GCO or filed for bankruptcy). A GCO firm is a firm that received a GCO that year. Similarly, a bankrupt firm is a firm that files for bankruptcy in the given year. The values in the table are mean values and standard deviations are provided in parentheses for continuous variables.

4. Findings

4.1. Main analysis

We start by plotting the number of months that pass before a firm files for bankruptcy following the issuance of each GCO and exclude firms that do not file for bankruptcy within our sample period. The distribution is shown in Figure 1, and the data is split between GCOs that communicate substantial doubt (GCO = 1, blue) and those that do not (GCO = 0, red). Subsequent to a GCO, the mean (median) time until bankruptcy is 41 (29) months, with the distribution being strongly right-skewed. In contrast, the mean (median) time to bankruptcy in the absence of a GCO is 70 (58) months, also with a right-skewed distribution. Consistent with our expectations, we find that firms file for bankruptcy faster following a GCO. However, these descriptive statistics illustrate that most bankruptcy filings do not occur immediately following the issuance of a GCO.

Figure 1: Distribution of months between GCO and bankruptcy



The figure plots the distribution of months between the issuance of the auditor's opinion and the client's subsequent bankruptcy filing. The number of months is plotted on the horizontal axis, while the relative frequency is plotted on the vertical axis. We separate between those opinions that raise substantial doubt (GC = 1, blue) and those that do not raise substantial doubt (GC = 0, red). The sample is limited to firms that ultimately file for bankruptcy.

To answer our first research question, we estimate our logit model regressing auditors' GCOs on bankruptcy using different look-forward periods. For look-forward periods that start 12 (24) months after the issuance of a GCO, we omit all bankruptcies occurring in the first 12 (24) months. The remaining GCOs are thus those that would conventionally be categorized as type I errors. The regression results are presented in Table 3. We find that auditors' GCOs are significantly and positively associated with filing for bankruptcy across all look-forward periods utilized in the analysis. Furthermore, the coefficients for GCOs decrease as we extend the look-forward period. All other variables are as expected.[8]

Table 3: Accuracy of GCOs across six look-forward periods

	Look-forward period (months):					
	0-12	**0-24**	**12-24**	**0-36**	**12-36**	**24-36**
DV: BANK	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
GCO	3.19***	2.12***	1.13***	1.60***	0.79***	0.36***
	(0.13)	(0.09)	(0.12)	(0.07)	(0.09)	(0.13)
BIG4	-0.05	-0.23***	-0.35***	-0.28***	-0.35***	-0.34***
	(0.12)	(0.08)	(0.10)	(0.06)	(0.07)	(0.10)
EXCH	-2.36***	-2.48***	-2.52***	-2.49***	-2.48***	-2.42***
	(0.17)	(0.11)	(0.14)	(0.09)	(0.10)	(0.15)
SIZE	0.33***	0.23***	0.15***	0.18***	0.11***	0.07***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
ZSCORE	0.38***	0.31***	0.25***	0.27***	0.23***	0.20***
	(0.06)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)
_CONS	-5.23***	-4.30***	-16.3***	-2.76***	-2.71***	-2.53***
	(0.99)	(0.96)	(0.42)	(0.61)	(0.75)	(0.76)
YEAR FE	YES	YES	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES	YES	YES
No. observations	66,662	66,662	65,900	66,662	65,900	65,012
Pseudo R^2	0.24	0.19	0.13	0.17	0.13	0.12

The table shows the results of regressing auditors' GCOs on the client filing for bankruptcy. The dependent variable, BANK, is equal to 1 if the firm files for bankruptcy within the respective look-forward period, and 0 otherwise. The look-forward periods are 0-12 months, 0-24 months, 12-24 months, 0-36 months, 12-36 months, and 24-36 months. The variable of interest, GCO, is equal to 1 if the auditor raised substantial doubts in the GCO, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, and EXCH. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

Overall, the results show that auditors' GCOs are accurate beyond the currently applied time horizon of 12 months. This suggests that they are able to make predictions that are relevant

[8] Control variables are suppressed in subsequent regression outputs but remain consistent with expectations across all additional regressions.

for the longer term, as even assessments made under the current institutional setting have some long-term predictive ability. Consequently, doubts regarding auditors' abilities to evaluate the GC assumption beyond 12 months are partially alleviated by these findings. However, the findings also suggest that a change in auditing practices may not be needed, as opinions are already accurate beyond 12 months. Another interesting insight from this analysis is that GCOs that would be classified as type I errors within the one-year horizon still accurately predict future bankruptcy. This adds weight to the argument that these "misclassifications" should not be considered "errors."

We subsequently investigate whether "old" GCOs provide incremental information to the model when including the most recently issued opinion (RQ2). This is of interest because a new GCO is typically issued before the end of the previous GCO's look-forward period. As such, there are likely to be few instances in which a GCO older than 12 months exists without a more recent assessment also being available. We test the incremental contribution of previously issued GCOs by decomposing the issuance of two successive GCOs using the three indicator variables: *recGCO*, *newGCO*, and *newnoGCO*.[9] Consequently, the baseline is opinions that do not raise substantial doubt in the current or previous opinion. The results are presented in Table 4, and we only use the currently applied look-forward period of 12 months. We see that *newGCO* and *recGCO* are positively and significantly associated with bankruptcy across all models, as expected. Meanwhile, the *newnoGCO* variable is insignificant by itself but positively associated with bankruptcy when included in the full model.

[9] We do not simply include a variable for the lagged GCOs because there is very high correlation between the conclusions across successive GCOs. In our sample, we find that more than 80 percent of all opinions that raised substantial doubt were preceded by an opinion that shared the same conclusion. For opinions that did not raise substantial doubt, the corresponding figure is more than 90 percent. Therefore, the inclusion of a lagged GCO would result in multicollinearity. We test this, and the untabulated results show evidence of sign changes and coefficient strengthening, resulting in inference being practically meaningless.

Table 4: Decomposition of successive opinions

DV: BANK	**(1)**	**(2)**	**(3)**	**(4)**
newGCO	2.54***			3.66***
	(0.11)			(0.15)
recGCO		1.06***		2.76***
		(0.15)		(0.19)
newnoGCO			-0.52	1.08***
			(0.34)	(0.36)
CONTROLS	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES
No. observations	59,633	59,633	59,633	59,633
Adjusted R^2	0.20	0.13	0.13	0.25

The table shows the results of regressing the changes in conclusion across two successive auditor GCOs on the client filing for bankruptcy. The dependent variable, BANK, is equal to 1 if the firm files for bankruptcy in the twelve-month period following the issuance of the latest GCO, and 0 otherwise. The variables of interest are: (i) newGCO, (ii) recGCO, and (iii) newnoGCO. They are equal to 1 if the auditor: (i) issued a GCO following a non-GCO, (ii) issued a GCO following a GCO, or (iii) issued a non-GCO following a GCO, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, and EXCH. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

As expected, we find that newly issued GCOs are associated with an increased risk of bankruptcy. As these opinions are utilized in many studies, these findings are not surprising. However, we also find that recurring GCOs for a single entity reflect a higher risk of bankruptcy. This shows that recurring opinions offer incremental value even if their conclusions are the same. This increased risk of bankruptcy is thus not incorporated when studies limit their samples to first-time or non-recurring GCOs, as is common in prior research. Furthermore, we find that receiving a clean bill of health following a GCO is not the same as receiving a recurring non-GCO. This suggests that only considering the most recent opinion could result in some information being lost. Effectively, failing to account for these differences could result in GCOs not reflecting the true difference in bankruptcy risk. Altogether, these findings highlight that recently issued opinions should be viewed in the context of previously issued opinions and that those “old” opinions still contain some informational content. In untabulated results, we also find that this consideration should not be implemented by simply including the lagged opinion, as doing so could lead to spurious results due to multicollinearity.

4.2. Additional analyses

Going concern issues

Auditors may communicate different issues that give rise to substantial doubt surrounding the entity's ability to continue as a GC. Desai et al. (2020) show that different issues reflect the risk of bankruptcy to varying degrees. We argue that some issues could be highly time-sensitive, while others might be less urgent (e.g., short-term liquidity versus the long-term viability of the business model). We explore whether different issues are more or less important for long-term accuracy. We do so by decomposing the GCO based on the GC issues that are raised and regressing those issues on bankruptcy. Our findings are presented in Table 5. The results show that most issues are accurate beyond the currently applied time horizon, at least to some extent.

Table 5: Accuracy of different going-concern issues across six look-forward periods

	Look-forward period (months):					
	0-12	**0-24**	**12-24**	**0-36**	**12-36**	**24-36**
DV: BANK	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
GCO_PRF	2.04***	1.41***	0.86***	1.02***	0.53***	0.12
	(0.19)	(0.12)	(0.15)	(0.10)	(0.11)	(0.16)
GCO_LIQ	0.42***	0.29***	0.13	0.30***	0.22**	0.33**
	(0.14)	(0.10)	(0.14)	(0.08)	(0.11)	(0.16)
GCO_SOL	0.27**	0.24***	0.21	0.22**	0.20*	0.19
	(0.13)	(0.10)	(0.15)	(0.09)	(0.12)	(0.18)
GCO_BUS	0.06	-0.11	-0.24	-0.18*	-0.27**	-0.28
	(0.14)	(0.11)	(0.16)	(0.10)	(0.13)	(0.20)
GCO_DFLT	1.14***	1.05***	0.64***	0.98***	0.57***	0.35
	(0.14)	(0.11)	(0.17)	(0.10)	(0.14)	(0.23)
GCO_MISC	0.06	0.25	0.44	0.24	0.35	0.15
	(0.37)	(0.29)	(0.41)	(0.26)	(0.34)	(0.53)
CONTROLS	YES	YES	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES	YES	YES
No. observations	66,662	66,662	65,900	66,662	65,900	65,012
Pseudo R^2	0.23	0.19	0.13	0.17	0.13	0.12

The table shows the results of regressing auditors' GCOs on the client filing for bankruptcy. The dependent variable, BANK, is equal to 1 if the firm files for bankruptcy within the respective look-forward period, and 0 otherwise. The look-forward periods are 0-12 months, 0-24 months, 12-24 months, 0-36 months, 12-36 months, and 24-36 months. Variables of interest are GC_LIQ, GC_PRF, GC_SOL, GC_BUS, GC_DFLT, and GC_MISC, which are equal to 1 if the auditor cited issues relating to liquidity, profitability, solvency, the business model, default, or others, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, and EXCH. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

We find that liquidity issues are consistently positively and significantly associated with an increased likelihood of bankruptcy. This suggests that even though liquidity may only be a short-term problem, liquidity issues could be indicative of more persistent underlying problems. The correlation matrix in Appendix III shows that liquidity and profitability are highly correlated, which supports the idea that liquidity issues simply reflect the firm's inability to generate sufficient cash. As such, profitability issues are consistently associated with an increased risk of bankruptcy. In contrast, we find that solvency- and business-related issues are only significantly associated with the risk of bankruptcy across a few look-forward periods. We expect these issues to have a more long-term nature, and the results suggest that their inclusion in the CGO does not accurately explain bankruptcy risk, especially when compared to references to liquidity and profitability.[10] Finally, default issues are consistently significant across all but one look-forward period. Default is a strong indicator of future bankruptcy and its predictive power beyond the 12-month horizon is not surprising. Finally, given the high correlation between *GCO_PRF* and *GCO_LIQ*, one might worry that the model suffers from multicollinearity issues. We therefore redo the previous analysis, combining the two groups of issues. The untabulated results are qualitatively similar to those presented in Table 5, with the exception that *GCO_BUS* was significantly and negatively associated with bankruptcy across all but the first and last look-forward periods.

Auditor conservatism

As the currently applied look-forward period is 12 months, auditors may be reluctant to issue a GCO if the short-term risk of bankruptcy is low, regardless of the long-term risk. We therefore investigate whether accuracy using longer look-forward periods increases when auditor conservatism is higher. For each opinion, auditor conservatism is measured at the audit-office

[10] The descriptive statistics show that these issues are much less common than liquidity and profitability. As such, we cannot rule out the possibility that the insignificant results found in this study are a power issue.

level and is calculated by taking the type I error rate for the given office across the whole sample, excluding any opinions relating to the firm in question.[11] Audit offices that have issued less than 100 opinions across our sample are excluded to ensure that the estimate is based on a sufficiently large number of opinions across multiple clients. We split our sample into those opinions issued by conservative and non-conservative audit offices, and re-estimate our model across the different look-forward periods. The findings are summarized in Table 6. For all but one model specification, auditors' GCOs are significantly associated with a higher likelihood of bankruptcy, but we find no significant differences between the conservative and non-conservative sample. These findings suggest that more conservative auditors are not more accurate at predicting bankruptcy beyond 12 months. However, as conservatism is defined by the frequency of type I errors, the results might be affected by these auditors being less skilled at assessing the GC risk.

Big4 versus non-Big4

Previous studies have shown that GCO errors vary across auditor size (Geiger et al. 2021). Berglund et al. (2018) find that BigN auditors are more likely to issue GCOs, and that they have lower type I errors and similar type II errors to non-BigN auditors. We therefore explore whether GCOs issued by Big4 auditors are more accurate across longer look-forward periods. To do so, we split our sample into those opinions issued by Big4 and non-Big4 auditors, and re-estimate our model across different look-forward periods. The results are summarized in Table 7. We find that auditors' GCOs are significantly associated with a higher likelihood of bankruptcy across all regressions and that Big4 auditors are better at predicting bankruptcy beyond 12 months.

[11] The audit office on an engagement is identified through the combination of the audit firm and the auditor's city, as stated in Audit Analytics. Some cities may have more than one office for a given audit firm (e.g., New York City). In these cases, the multiple offices located in one city are effectively classified as one office.

Table 6: Variation in accuracy due to auditor conservatism

	GCO coefficient		No. observations		Pseudo R^2	
Look-forward period:	**CONS = 0**	**CONS = 1**	**CONS = 0**	**CONS = 1**	**CONS = 0**	**CONS =1**
0-12	3.62***	3.27***	20,128	21,343	0.33	0.29
	(0.26)	(0.24)				
Difference		-0.35				
0-24	2.56***	2.32***	20,128	21,343	0.26	0.22
	(0.20)	(0.16)				
Difference		-0.24				
12-24	1.24***	1.36***	19,929	21,117	0.20	0.16
	(0.27)	(0.21)				
Difference		0.12				
0-36	2.09***	1.79***	20,128	21,343	0.24	0.20
	(0.17)	(0.13)				
Difference		-0.30				
12-36	0.86***	0.98***	19,929	21,117	0.20	0.16
	(0.24)	(0.16)				
Difference		0.12				
24-36	0.15	0.50**	19,700	20,860	0.18	0.14
	(0.39)	(0.24)				
Difference		0.35				

The table summarizes the results of regressing auditors' GCOs on the client filing for bankruptcy across two subsamples. The first subsample (CONS = 0) includes opinions issued by auditors with below-median conservatism. The second subsample (CONS = 1) includes opinions issued by auditors with above-median conservatism. The table only provides some of the output from the regressions: coefficients, standard errors, number of observations, pseudo R^2, and the difference between coefficients (including the significance level). The dependent variable in the regressions is BANK, which is equal to 1 if the firm files for bankruptcy within the respective look-forward period, and 0 otherwise. The look-forward periods are 0-12 months, 0-24 months, 12-24 months, 0-36 months, 12-36 months, and 24-36 months. The variable of interest, GCO, is equal to 1 if the auditor raised substantial doubts in the GCO, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, and EXCH. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

The results in Table 7 suggest that Big4 auditors are better at predicting bankruptcy across all look-forward periods. This could be because Big4 auditors are better at performing GC assessments in general, as shown in previous studies (Geiger et al. 2021). However, it could also reflect a lack of economic dependence on a particular client, which might make Big4 auditors more willing to raise substantial doubt even if the risk of immediate bankruptcy is lower. The reputational costs for Big4 auditors could also be greater, as investors, creditors, and the public might expect auditors to raise substantial doubt earlier than 12 months. Finally, there could be differences in client characteristics among Big4 and non-Big4 auditors that make long-term predictions relatively easier for the former.

Table 7: Variation in accuracy due to auditor size

	GCO coefficient		No. observations		Pseudo R^2	
Look-forward period:	**BIG4 = 0**	**BIG4 = 1**	**BIG4 = 0**	**BIG4 = 1**	**BIG4 = 0**	**BIG4 = 1**
0-12	2.67***	3.47***	26,044	40,618	0.20	0.32
	(0.16)	(0.17)				
Difference		0.80***				
0-24	1.84***	2.55***	26,044	40,618	0.17	0.25
	(0.11)	(0.12)				
Difference		0.71***				
12-24	1.17***	1.42***	25,671	40,229	0.12	0.17
	(0.14)	(0.17)				
Difference		0.25				
0-36	1.36***	2.18***	26,044	40,618	0.15	0.23
	(0.09)	(0.10)				
Difference		0.82***				
12-36	0.78***	1.22***	25,671	40,229	0.12	0.17
	(0.11)	(0.13)				
Difference		0.44**				
24-36	0.32**	0.85***	25,223	39,789	0.11	0.15
	(0.15)	(0.20)				
Difference		0.53**				

The table summarizes the results of regressing auditors' GCOs on the client filing for bankruptcy across two subsamples. The first subsample (BIG4 = 0) includes opinions issued by non-Big4 auditors. The second subsample (BIG4 = 1) includes opinions issued by Big4 auditors. The table only provides some of the output from the regressions: coefficients, standard errors, number of observations, pseudo R^2, and the difference between coefficients (including significance level). The dependent variable in the regressions is BANK, which is equal to 1 if the firm files for bankruptcy within the respective look-forward period, and 0 otherwise. The look-forward periods are 0-12 months, 0-24 months, 12-24 months, 0-36 months, 12-36 months, and 24-36 months. The variable of interest, GCO, is equal to 1 if the auditor raised substantial doubts in the GCO, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, and EXCH. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

5. Robustness tests

5.1. Matching

We replicate our two main analyses using two matched samples. First, we match firms that ultimately file for bankruptcy with those that never file for bankruptcy within our sample. Second, firms that ultimately receive at least one GCO that raises substantial doubt are matched with firms that never receive an opinion that raises substantial doubt. In both cases, the groups are matched on the same control variables that we apply in the regressions (i.e., *SIZE*, *ZSCORE*, *EXCH*, and *BIG4*) using nearest-neighbor propensity score matching, and all firms are matched exactly by year and industry. The matching is done on the year prior to bankruptcy/the first

GCO when possible. The results of these matched regressions are presented in Tables 8 and 9. Our findings remain qualitatively unchanged for both analyses across both matching procedures.

Table 8: Accuracy of GCOs across six look-forward periods (matched sample)

	Look-forward period (months):					
DV: BANK	**0-12**	**0-24**	**12-24**	**0-36**	**12-36**	**24-36**
Matching: BANK	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
GCO	2.92***	2.11***	1.01***	1.64***	0.67***	0.21
	(0.13)	(0.10)	(0.14)	(0.08)	(0.11)	(0.17)
CONTROLS	YES	YES	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES	YES	YES
No. observations	10,740	10,740	10,002	10,740	10,002	9,399
Pseudo R^2	0.20	0.13	0.05	0.09	0.04	0.03
	Look-forward period (months):					
DV: BANK	**0-12**	**0-24**	**12-24**	**0-36**	**12-36**	**24-36**
Matching: GCO	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
GCO	3.53***	2.54***	1.69***	2.05***	1.34***	0.89***
	(0.14)	(0.09)	(0.11)	(0.07)	(0.08)	(0.12)
CONTROLS	YES	YES	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES	YES	YES
No. observations	35,584	35,584	35,074	35,584	35,074	34,520
Pseudo R^2	0.31	0.22	0.13	0.18	0.12	0.10

The table shows the results of regressing auditors' GCOs on the client filing for bankruptcy on a matched sample. The firms are matched using propensity-score matching, and we match bankrupt (GCO) and non-bankrupt (non-GCO) firms using the control variables otherwise utilized in the study. The dependent variable, BANK, is equal to 1 if the firm files for bankruptcy within the respective look-forward period, and 0 otherwise. The look-forward periods are 0-12 months, 0-24 months, 12-24 months, 0-36 months, 12-36 months, and 24-36 months. The variable of interest, GCO, is equal to 1 if the auditor raised substantial doubts in the GCO, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, and EXCH. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

5.2. Sophisticated bankruptcy model

The purpose of our study is to explore whether auditors' GCOs are accurate beyond the currently applied time horizon. However, the practical relevance of an auditor's opinion is limited if the conclusion does not provide some incremental value beyond what is explained by conventional bankruptcy models. As such, we also investigate whether auditors' GCOs are

Table 9: Decomposition of successive opinions (matched sample)

DV: BANK **Matching: BANK**	**(1)**	**(2)**	**(3)**	**(4)**
newGCO	2.47***			3.40***
	(0.12)			(0.15)
recGCO		1.09***		2.57***
		(0.15)		(0.19)
newnoGCO			-0.72*	0.77**
			(0.37)	(0.38)
CONTROLS	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES
No. observations	9,374	9,374	9,374	9,374
Adjusted R^2	0.15	0.07	0.06	0.21

DV: BANK **Matching: GCO**	**(1)**	**(2)**	**(3)**	**(4)**
newGCO	2.75***			3.79***
	(0.12)			(0.16)
recGCO		1.75***		3.18***
		(0.15)		(0.20)
newnoGCO			-0.70	0.86**
			(0.45)	(0.47)
CONTROLS	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES
No. observations	31,061	31,061	31,061	31,061
Adjusted R^2	0.22	0.14	0.10	0.30

The table shows the results of regressing the changes in conclusion across two successive auditor GCOs on the client filing for bankruptcy on a matched sample. The firms are matched using propensity-score matching, and we match bankrupt (GCO) and non-bankrupt (non-GCO) firms using the control variables otherwise utilized in the study. The dependent variable, BANK, is equal to 1 if the firm files for bankruptcy in the twelve-month period following the issuance of the latest GCO, and 0 otherwise. The variables of interest are: (i) newGCO, (ii) recGCO, and (iii) newnoGCO. They are equal to 1 if the auditor: (i) issued a GCO following a non-GCO, (ii) issued a GCO following a GCO, or (iii) issued a non-GCO following a GCO, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, and EXCH. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

significantly able to predict bankruptcy across various look-forward periods when included in a more sophisticated bankruptcy model: Ohlson (1980). We replicate our main analyses, and present the results in Tables 10 and 11. The findings are qualitatively similar, but statistically weaker than those of the simpler model. Auditors' GCOs provide incremental predictive power up to two years following their issuance. As an additional test, we also perform a stepwise logistic regression using the Akaike information criterion to test whether this approach results in a model that includes the auditor's GCO.[12] The variables in question are the same as those included in the sophisticated model. We find that GCO is selected when using the following look-forward periods: 0-12, 0-24, 12-24, and 0-36 months. This provides additional evidence that auditors' GCOs provide incremental information when assessing the risk of bankruptcy for longer look-forward periods.

6. Conclusion

The findings in this paper demonstrate that auditors' GC assessments are accurate beyond the currently applied time horizon of 12 months. Their GCOs are significantly associated with an increased risk of bankruptcy for at least two years following their issuance. These results are robust to a set of control variables commonly applied in prior research as well as matching procedures. In addition, the coefficient has some incremental prediction value in a more sophisticated model. Furthermore, our results show that previously issued GCOs can provide incremental information. Recurring GCOs suggest an even higher risk of bankruptcy, while receiving a non-GCO subsequent to a GCO does not seem to negate the risk of bankruptcy associated with the initial opinion. These findings suggest that auditors can make GC assessments beyond 12 months. Proposed changes to auditors' responsibilities can thus be seen as a viable option. Conversely, one may argue that a change in auditing practices may not be needed because the opinions are already accurate beyond 12 months. However, one unresolved question is whether those opinions would be more accurate if the look-forward period was extended.

[12] The Akaike information criterion (AIC) is a method used to compare different models and determine which model best fits the data. The formula for AIC is given by:

$$AIC = 2K - 2\ln(L)$$

where K is the number of independent variables and L is the log-likelihood estimate. The model that best fits the data is the one with the lowest AIC value. As such, this method penalizes models with more parameters (higher K) and rewards more accurate models (higher L).

Table 10: Accuracy of GCOs across six look-forward periods using a sophisticated bankruptcy model

	Look-forward period (months):					
	0-12	**0-24**	**12-24**	**0-36**	**12-36**	**24-36**
DV: BANK	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
GCO	2.15***	1.15***	0.24*	0.83***	0.13	-0.05
	(0.15)	(0.11)	(0.14)	(0.09)	(0.11)	(0.17)
BIG4	-0.28*	-0.35***	-0.36***	-0.38***	-0.34***	-0.37***
	(0.14)	(0.09)	(0.11)	(0.07)	(0.08)	(0.12)
EXCH	-1.93***	-2.10***	-2.15***	-2.17***	-2.20***	-2.19***
	(0.17)	(0.12)	(0.15)	(0.09)	(0.11)	(0.16)
SIZE	0.61***	0.49***	0.38***	0.43***	0.34***	0.27***
	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
ZSCORE	0.26***	0.29***	0.29***	0.24***	0.22***	0.15***
	(0.06)	(0.04)	(0.05)	(0.03)	(0.03)	(0.04)
LEV	0.25*	0.26***	0.34***	0.36***	0.49***	0.69***
	(0.13)	(0.09)	(0.13)	(0.08)	(0.10)	(0.15)
WCTA	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CURR	-0.06	-0.04	-0.03	-0.02	-0.01	-0.01
	(0.05)	(0.03)	(0.03)	(0.02)	(0.02)	(0.04)
NEG_EQ	1.01***	0.87***	0.60***	0.63***	0.30*	0.18
	(0.21)	(0.14)	(0.19)	(0.13)	(0.15)	(0.23)
ROA	-0.02	0.00	-0.02	0.07	0.01	0.22**
	(0.10)	(0.07)	(0.09)	(0.06)	(0.08)	(0.10)
OCFL	-0.77***	-0.82***	-0.84***	-0.79***	-0.79***	-0.71***
	(0.12)	(0.08)	(0.10)	(0.06)	(0.07)	(0.10)
LOSS2	1.85***	1.69***	1.55***	1.56***	1.44***	1.29***
	(0.19)	(0.11)	(0.13)	(0.08)	(0.09)	(0.12)
CHNG_NI	-0.09	-0.21***	-0.26***	-0.21***	-0.24***	-0.19***
	(0.08)	(0.05)	(0.07)	(0.05)	(0.05)	(0.07)
CASH	-0.20	-0.39*	-0.56**	-0.55***	-0.70***	-0.83***
	(0.29)	(0.20)	(0.27)	(0.17)	(0.21)	(0.30)
MB	0.04**	0.03**	0.01	0.02*	0.00	-0.01
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
CHNG_P	-0.74***	-0.38***	-0.16**	-0.25***	-0.09*	-0.01
	(0.13)	(0.06)	(0.07)	(0.05)	(0.05)	(0.07)
_CONS	-22.2***	-18.9***	-19.0	-5.10***	-4.50***	-4.40***
	(1.10)	(0.06)	(596.1)	(0.78)	(0.78)	(0.81)
YEAR FE	YES	YES	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES	YES	YES
No. observations	57,371	57,371	56,798	57,371	56,798	56,093
Pseudo R^2	0.33	0.28	0.21	0.26	0.21	0.18

The table shows the results of regressing auditors' GCOs on the client filing for bankruptcy when using a sophisticated bankruptcy model. The dependent variable, BANK, is equal to 1 if the firm files for bankruptcy within the respective look-forward period, and 0 otherwise. The look-forward periods are 0-12 months, 0-24 months, 12-24 months, 0-36 months, 12-36 months, and 24-36 months. The variable of interest, GCO, is equal to 1 if the auditor raised substantial doubts in the GCO, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, EXCH, LEV, WCTA, CURR, NEG_EQ, ROA, OCFL, LOSS2, CHNG_NI, CASH, MB, and CHNG_P. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

Table 11: Decomposition of successive opinions using a sophisticated bankruptcy model

DV: BANK	(1)	(2)	(3)	(4)
newGCO	1.68***			2.42***
	(0.12)			(0.16)
recGCO		0.20		1.64***
		(0.17)		(0.20)
newnoGCO			-0.50	0.51
			(0.37)	(0.39)
CONTROLS	YES	YES	YES	YES
YEAR FE	YES	YES	YES	YES
INDUSTRY FE	YES	YES	YES	YES
No. observations	57,371	57,371	57,371	57,371
Adjusted R^2	0.32	0.29	0.29	0.33

The table shows the results of regressing the changes in conclusion across two successive auditor GCOs on the client filing for bankruptcy when using a sophisticated bankruptcy model. The dependent variable, BANK, is equal to 1 if the firm files for bankruptcy in the twelve-month period following the issuance of the latest GCO, and 0 otherwise. The variables of interest are: (i) newGCO, (ii) recGCO, and (iii) newnoGCO. They are equal to 1 if the auditor: (i) issued a GCO following a non-GCO, (ii) issued a GCO following a GCO, or (iii) issued a non-GCO following a GCO, and 0 otherwise. The control variables are BIG4, SIZE, ZSCORE, EXCH, LEV, WCTA, CURR, NEG_EQ, ROA, OCFL, LOSS2, CHNG_NI, CASH, MB, and CHNG_P. We include calendar-year fixed effects and two-digit SIC code industry fixed effects. Robust standard errors are presented in parentheses, and we cluster on firm level. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

In addition, we find that GCOs that highlight liquidity, profitability, and default issues are more accurate beyond the applied horizon. Opinions that point to solvency and business-model issues are only significantly associated with bankruptcy for some look-forward periods. These findings stand in contrast to our ex-ante expectations, as we expect issues concerning the long-term viability of the firm to be more important when using longer look-forward periods. In particular, the weak significance of solvency issues stand in contrast to the importance of leverage in the more sophisticated bankruptcy model, which is one of the few variables that remain significant using the longest look-forward periods. One possible explanation is that these issues are not frequently cited in GCOs, perhaps because they are only important for long-term assessments. Future research could therefore further examine whether auditors might consider and collect audit evidence on different issues if asked to consider a longer look-forward period.

There is also some variation in accuracy across large and small audit firms, as GCOs issued by Big4 firms are more accurate beyond the applied horizon. This is consistent with previous studies, which find that Big4 auditors are more likely to raise substantial doubt and are more accurate (Berglund et al. 2018; Myers et al. 2014). However, we do not find any differences between conservative and less conservative auditors in our study. An interesting avenue for future researchers could be other auditor characteristics that are associated with better long-term assessments.

References

Amin, K, J Krishnan, and J S Yang. 2014. "Going Concern Opinion and Cost of Equity." Auditing: A Journal of Practice & Theory 33 (4): 1-39.

Arnold, V, P A Collier, S A Leech, and S G Sutton. 2001. "The impact of political pressure on novice decision makers: are auditors qualified to make going concern judgements?" Critical Perspectives on Accounting 12 (3): 323-338.

Berglund, N R, J D Eshleman, and P Guo. 2018. "Auditor Size and Going Concern Reporting." Auditing: A Journal of Practice & Theory 37 (2): 1-25.

Carson, E, N Fargher, and Y Zhang. 2019. "Explaining auditors' propensity to issue going-concern opinions in Australia after the global financial crisis." Accounting & Finance 59 (4): 2415-2453.

Carson, Elizabeth, Neil L Fargher, Marshall A Geiger, Clive S Lennox, K Raghunandan, and Marleen Willekens. 2013. "Audit Reporting for Going-Concern Uncertainty: A Research Synthesis." Auditing: A Journal of Practice & Theory 32 (1): 353-384.

Chava, Sudheer, and Robert A Jarrow. 2004. "Bankruptcy Prediction with Industry Effects." Review of Finance 8 (4): 537-569.

Chen, P F, S He, Z Ma, and D Stice. 2016. "The information role of audit opinions in debt contracting." Journal of Accounting and Economics 61 (1): 121-144.

Chen, Y, J D Eshleman, and J S Soileau. 2017. "Business Strategy and Auditor Reporting." Auditing: A Journal of Practice & Theory 36 (2): 63-86.

DeFond, M L, J R Francis, and N J Hallman. 2018. "Awareness of SEC Enforcement and Auditor Reporting Decisions." Contemporary Accounting Research 35 (1): 277-313.

Desai, Vikram, Renu Desai, Joung W Kim, and Kannan Raghunandan. 2020. "Are going-concern issues disclosed in audit reports associated with subsequent bankruptcy? Evidence from the United States." International Journal of Auditing 24 (1): 1-14.

Dodd, P, N Dopuch, R Holthausen, and R Leftwich. 1984. "Qualified audit opinions and stock prices. Information content, announcement dates, and concurrent disclosures." Journal of Accounting and Economics 6 (1): 3-38.

Elliott, J. 1982. "Subject to audit opinions and abnormal security returns: Outcomes and ambiguities." Journal of Accounting Research 20 (2): 617-638.

FASB. 2014. "Going concern." FASB Board Meeting.

FASB. 2013. "Proposed Accounting Standards Update - Presentation of Financial Statements (Topic 205): Disclosure of Uncertainties about an Entity's Going Concern Presumption." FASB In Focus.

Feldmann, D, and W J Read. 2013. "Going-concern audit opinions for bankrupt companies – impact of credit rating." Managerial Auditing Journal 28 (4): 345-363.

Financial Times. 2019. Auditors told to be tougher on ‘going concern’ checks. https://www.ft.com/content/c17ae992-3e91-11e9-9bee-efab61506f44.

Geiger, M A, A Gold, and P Wallage. 2021. Auditor Going Concern Reporting: A Review of Global Research and Future Research Opportunities. Routledge.

Geiger, M A, and D V Rama. 2006. "Audit Firm Size and Going-Concern Reporting Accuracy." Accounting Horizons 20 (1): 1-17.

Geiger, M A, and K Raghunandan. 2002. "Auditor Tenure and Audit Reporting Failures." Auditing: A Journal of Practice & Theory 21 (1): 67-78.

Geiger, M A, and K Raghunandan. 2001. "Bankruptcies, Audit Reports, and the Reform Act." Auditing: A Journal of Practice & Theory 20 (1): 187-195.

Geiger, M A, K Raghunandan, and D V Rama. 2006. "Auditor decision-making in different litigation environments: The Private Securities Litigation Reform Act, audit reports and audit firm size." Journal of Accounting and Public Policy 25 (3): 332-353.

Geiger, M A, K Raghunandan, and W Riccardi. 2014. "The Global Financial Crisis: U.S. Bankruptcies and Going-Concern Audit Opinions." Accounting Horizons 28 (1): 59-75.

Gerakos, J, P R Hahn, A Kovrijnykh, and F Zhou. 2016. "Prediction versus Inducement and the Informational Efficiency of Going Concern Opinions." Working paper.

Guiral, A, E Ruiz, and W Rodgers. 2011. "To what extent are auditors' attitudes toward the evidence influenced by the self-fulfilling prophecy?" Auditing: A Journal of Practice & Theory 30 (1): 173-190.

Hillegeist, Stephen A, Elizabeth K Keating, Donald P Cram, and Kyle G Lundstedt. 2004. "Assessing the Probability of Bankruptcy." Review of Accounting Studies 9 (1): 5-34.

IAASB. 2020. "Fraud and going concern in an audit of financial statements: Exploring the differences between public perceptions about the role of the auditor and the auditor's responsibilities in a financial statement audit."

Kaplan, S E, and D D Williams. 2013. "Do Going Concern Audit Reports Protect Auditors from Litigation? A Simultaneous Equations Approach." The Accounting Review 88 (1): 199-232.

LaSalle, R E, and A Anandarajan. 1996. "Auditors' views on the type of audit report issued to entities with going concern uncertainties." Accounting Horizons 10 (2): 51-72.

Lehman, C M, and C S Norman. 2006. "The Effects of Experience on Complex Problem Representation and Judgment In Auditing: An Experimental Investigation." Behavioral Research in Accounting 18 (1): 65-83.

Lennox, C. 2000. "Do companies successfully engage in opinion-shopping? Evidence from the UK." Journal of Accounting and Economics 29 (3): 321-337.

Menon, Krishnagopal, and David D Williams. 2010. "Investor Reaction to Going Concern Audit Reports." The Accounting Review 85 (6): 2075-2105.

Myers, L A, J Schmidt, and M Wilkins. 2014. "An investigation of recent changes in going concern reporting decisions among Big N and non-Big N auditors." Review of Quantitative Finance and Accounting 43 (1): 155-172.

Myers, Linda A, Jonathan E Shipman, Quinn T Swanquist, and Robert L Whited. 2018. "Measuring the market response to going concern modifications: the importance of disclosure timing." Review of Accounting Studies 23: 1512-1542.

Nogler, G E. 1995. "The resolution of auditor going concern opinions." Auditing: A Journal of Practice & Theory 14 (2): 54-73.

PCAOB Investor Advisory Group. 2012. "Presentation of the PCAOB's Investor Advisory Group Working Group on Going Concern." Investor Advisory Group Meeting.

PCAOB Standing Advisory Group. 2011. "Evaluating Whether There is Substantial Doubt About an Entity's Ability to Continue As a Going Concern." Standing Advisory Group Meeting.

PCAOB Standing Advisory Group. 2012. "Going Concern." Standing Advisory Group Meeting.

Sun, L. 2007. "A re-evaulation of auditors' opinions versus statistical models in bankruptcy prediction." Review of Quantitative Finance and Accounting 28: 55-78.

Vanstraelen, A. 2003. "Going-Concern Opinions, Auditor Switching, and the Self-Fulfilling Prophecy Effect Examined in the Regulatory Context of Belgium." Journal of Accounting, Auditing & Finance 18 (2): 231-254.

Wall Street Journal. 2021. China Evergrande Auditor Gave Clean Bill of Health Despite Debt. https://www.wsj.com/articles/china-evergrande-never-got-auditor-warning-despite-big-debt-load-11632475800.

Wright, A M, and S Wright. 2014. "Modification of the Audit Report: Mitigating Investor Attribution by Disclosing the Auditor's Judgment Process." Behavioral Research in Accounting 26 (2): 35-50.

Appendix

Appendix I: Variable definitions

Variable		Definition
BANK	=	1 if the client files for bankruptcy within the look-forward period of the latest going-concern opinion, 0 otherwise.
GCO	=	1 if client receives a going-concern opinion that raises substantial doubt about the ability to continue as a going concern, 0 otherwise.
recGCO	=	1 if the client receives two going-concern opinions in a row that raise substantial doubt, 0 otherwise.
newGCO	=	1 if the client receives a going-concern opinion that raises substantial doubt subsequent to an opinion that did not raise substantial doubt, 0 otherwise.
newnoGCO	=	1 if the client receives a going-concern opinion that does not raise substantial doubt subsequent to an opinion that did raise substantial doubt, 0 otherwise.
GCO_LIQ	=	1 if the auditor raised a liquidity related issue when communicating substantial doubt, 0 otherwise.
GCO_PRF	=	1 if the auditor raised a profitability related issue when communicating substantial doubt, 0 otherwise.
GCO_SOL	=	1 if the auditor raised a solvency related issue when communicating substantial doubt, 0 otherwise.
GCO_BUS	=	1 if the auditor raised an issue related to the business model when communicating substantial doubt, 0 otherwise.
GCO_DFLT	=	1 if the auditor raised default as an issue when communicating substantial doubt, 0 otherwise.
GCO_MISC	=	1 if the auditor raised an issue that does not fit in the aforementioned categories when communicating substantial doubt, 0 otherwise.
BIG4	=	1 if the auditor is Deloitte, EY, KPMG, or PwC, 0 otherwise.
SIZE	=	Natural log of total assets.
ZSCORE	=	Altman's Z-score as modified by Hillegeist et al. (2004).
EXCH	=	1 if the client is listed NASDAQ, NYSE, or NYSE American (prev. NYSE MKT).
LEV	=	Total liabilities over total assets.
WCTA	=	Current assets minus current liabilities (working capital) over total assets.
CURR	=	Current assets over current liabilities.
NEG_EQ	=	1 if the client has total liabilities greater than total assets, 0 otherwise.
ROA	=	Income before extraordinary items over total assets.
OCFL	=	Cash flow from operations over total liabilities.
LOSS2	=	1 if the client has reported a loss over the two last fiscal years, 0 otherwise.
CHNG_NI	=	Net income minus lagged net income over the absolute value of net income plus lagged net income.
CASH	=	Cash and cash equivalents over total assets.
MB	=	Market value of equity over book value of equity.
CHNG_P	=	Stock price at the issuance of the most recent opinion over the stock price at the issuance of the previous opinion.

Appendix II: Classification of going-concern issues listed in Audit Analytics

Going concern issue (AA key)	Frequency
Liquidity	
Working capital/current ratio deficit/inadequacy (8)	
Need for additional financing for funding obligations and/or servicing debt (12)	
Need for additional financing to sustain operations (14)	
Insufficient/limited cash, capital or liquidity concerns (19)	
No dividends (29)	
Credit line reduced, unavailable or due (36)	
Profitability	
Net losses since inception (2)	
Net/operating loss (including recurring losses) (9)	
Accumulated/retained earnings deficit (10)	
Negative cash flow from operations (17)	
Initial loss (18)	
Profitability concerns (27)	
Gross margin – negative (49)	
Solvency	
Assets – inadequate, limited, immaterial or impaired (13)	
Stockholder equity or partner capital – deficiency or decrease (15)	
Liabilities exceed assets (35)	
Refinancing contingencies (40)	
Debt is substantial (41)	
Restructuring contingencies (44)	
Credit quality deterioration (46)	
Regulatory capital – decline or deficiency (financial firms only) (47)	
Business model	
Development stage (6)	
Not commenced, limited or no operations (7)	
Absence of significant revenues (11)	
Need for additional financing for growth or to meet business objectives (16)	
Changed industry or business (20)	
Seeking or needs to combine with existing company (21)	
Discontinued/disposal of operations (22)	
Liquidation of assets (25)	
Competitor threat (26)	
Decline in revenue (30)	
Product demand or pricing – decline or limited (31)	
Subsidiary – spin off (45)	
Exploration/pre-exploration stage (oil and gas firms only) (50)	
Recoverability of (natural) resources – uncertain (oil and gas firms only) (52)	
No marketable product(s) (53)	
Vendor-supplier disputes or disruptions (55)	
Default	
Notes payable/debt – default, due, delinquency (28)	
Debt covenants/agreements uncertain or not in compliance (33)	
Miscellaneous	
No explanation (1)	
Compensation deferred (38)	
Regulatory settlements, obligations and contingencies (39)	
Litigation contingencies (42)	
Derivatives – obligations, losses (43)	
Benefit plan, pensions, etc. – Obligations (48)	
Act of God (extreme weather, war, illness, even death etc.) (65)	

Appendix III: Correlation between groups of going-concern issues

	GCO_PRF	GCO_LIQ	GCO_SOL	GCO_BUS	GCO_DFLT	GCO_MISC
GCO_PRF		0.67	0.39	0.39	0.21	0.08
GCO_LIQ	0.67		0.32	0.32	0.25	0.09
GCO_SOL	0.39	0.32		0.14	0.20	0.09
GCO_BUS	0.39	0.32	0.14		0.04	0.04
GCO_DFLT	0.21	0.25	0.20	0.04		0.10
GCO_MISC	0.08	0.09	0.09	0.04	0.10	

PUBLIC DISCLOSURE OF CEO'S PRIOR BANKRUPTCY EXPERIENCE AND THE COST OF DEBT

Ibrahim Pelja*

ABSTRACT: Firms that employ a CEO with bankruptcy experience may have different risk profiles than comparable firms whose CEOs do not have such experience. Creditors can respond to this by increasing the cost of debt. However, this requires creditors to collect information about the CEOs bankruptcy history. This study utilizes a unique setting to explore whether cost of debt increases when firms employ a CEO with bankruptcy experience and whether public disclosure of such information increases the effect on cost of debt. The findings show robust evidence that bankruptcy experience that is publicly available is associated with an increase in cost of debt. In contrast, employing a CEO with a non-public bankruptcy record is not associated with a change in cost of debt. Additional analyses also show that firms run by an individual with bankruptcy experience have a higher risk of filing for bankruptcy in the future, and that they have higher accounts payable and raise more capital through equity.

KEYWORDS: CEO experience, bankruptcy, cost of debt

* Ibrahim Pelja is a PhD candidate at the Norwegian School of Economics and can be reached at: ibrahim.pelja@nhh.no.

1. Introduction

Chief Executive Officers (CEOs) have considerable influence over firms' policies and performance (Bennedsen, Pérez-González, and Wolfenzon 2020; Bertrand and Schoar 2003; Cain and McKeon 2016; Malmendier and Tate 2005), but their decisions may be influenced by personal characteristics. Several studies explore whether CEO characteristics explain variations in firm outcomes, and evidence suggests that factors such as overconfidence (Malmendier and Tate 2005; 2008), risk preferences (Kallunki and Pyykkö 2013), military experience (Malmendier, Tate, and Yan 2011), living through a recession (Malmendier et al. 2011; Schoar and Zuo, 2017), and bankruptcy experience (Dittmar and Duchin 2016) influence the firm's policies and performance. Consequently, stakeholders should care about CEO characteristics, and several studies suggest that investors (Jian and Lee 2011; Malmendier and Tate 2008; Schoar and Zuo 2016) and creditors (Hsu, Lee, and Liu 2022; Regenburg and Seitz 2021) consider CEO-specific information when making investing and lending decisions.

To incorporate CEO characteristics into their decision making, stakeholders must obtain relevant information. Some information is easily identifiable (e.g., age, gender, and nationality), whereas other information is less readily available (e.g., education and work experience), and some information may even be unobservable (e.g., preferences, confidence, and ability). Information collection comes at a cost, which varies with stakeholder and information characteristics and with available technologies (Blankespoor, deHaan, and Marinovic 2020). Consequently, stakeholders are unlikely to gather information on CEO characteristics when the benefits do not outweigh the costs. However, when information is made more readily available, costs are lower (Blankespoor et al. 2020), which could result in better decision making. Consistent with this, extant literature on information disclosure suggests that greater disclosure generally improves market quality (Goldstein and Yang 2017).

This study explores whether the public disclosure of a CEO's prior involvement with a bankrupt firm informs creditors' decision making. Norway offers a unique setting for exploring the effects of bankruptcy-history disclosure because the Norwegian government maintains a public bankruptcy register. This register lists the CEO and directors of every bankrupt firm, and the records are publicly available for up to five years. The register reduces information-collection costs, which in turn ensures that creditors have access to relevant information concerning CEOs' recent bankruptcy experience. Information on bankruptcy history beyond the register's timespan is more costly to collect, which could restrict creditors from obtaining this information.

A CEO's bankruptcy record could be relevant to creditors for several reasons. First, prior involvement with a bankrupt firm could reflect certain CEO characteristics – it could suggest a greater appetite for risk (Kallunki and Pyykkö 2013) or reflect poor managerial ability because management is often substantially responsible for bankruptcies (Ooghe and De Prijcker 2008). Alternatively, employing a CEO with bankruptcy experience could reflect the fact that the previously bankrupt firm and the current firm have similar characteristics. For example, hiring a CEO with bankruptcy experience could suggest that the current firm is more risk-taking or that it is underperforming and is less attractive to other managers. In both cases, creditors may consider employing a CEO with bankruptcy experience to be a signal of increased risk, and may act accordingly.

In contrast, CEOs could learn from their prior experience with bankruptcy, and it could affect their decision making in ways that decrease risk. For example, Dittmar and Duchin (2016) finds that CEOs become less risk seeking after experiencing a bankruptcy. Similarly, Ahmed, Christensen, Olson, and Yust (2018) shows that executives learn from their distress experience. However, Gopalan, Gormley, and Kalda (2021) finds that some directors become more risk seeking following a bankruptcy, suggesting that this experience can have different effects on different people depending on the context, e.g. whether the bankruptcy was "swift and painless" (Gopalan et al. 2021). Overall, creditors could view bankruptcy experience as either positive or negative.

In this paper, I examine whether a CEO's former bankruptcy experience impacts the cost of debt following the appointment of a new CEO and whether the visibility of this bankruptcy experience matters. To isolate the effect of public bankruptcy disclosure, I use a difference-in-differences (DID) approach and compare firms that hire a new CEO without bankruptcy experience with firms that hire a new CEO with *non-public* bankruptcy experience, as well as with firms that hire a new CEO with *public* bankruptcy experience.[1] I attribute the difference in the cost of debt for a public versus non-public CEO bankruptcy record to the public bankruptcy disclosure. In contrast, I attribute the difference in the cost of debt for a non-public CEO bankruptcy record versus a non-bankrupt record to the effect of bankruptcy experience in general. Because firms that hire individuals with bankruptcy history may differ significantly from other firms, I perform my analyses using samples of firms matched on several covariates in the year prior to the new CEO's employment.

I find that the cost of debt generally increases when a firm employs a CEO with bankruptcy experience and the effect is significantly stronger when the bankruptcy record is public. In contrast, the effect of a non-public bankruptcy record is significant only in some specifications. The effect of a public bankruptcy record on the cost of debt is robust to matching and to the inclusion of control variables, as well as robustness tests focusing on a narrower event window around the hiring of new CEOs, an alternative control sample comprised of firms whose CEOs experience concurrent bankruptcies at another firm rather than new hires, a within-CEO DID specification, and control variables that ensure that the findings are not due to old bankruptcies

[1] Note that "public bankruptcy experience" refers to bankruptcy experience that was publicly available in the bankruptcy register when the CEO was hired, and "non-public bankruptcy experience" refers to bankruptcy experience that was not publicly available in the bankruptcy register when the CEO was hired.

being perceived as less relevant than recent bankruptcies. Finally, the difference in the effect of a public versus non-public bankruptcy record is significant across all specifications.

Overall, my findings are consistent with creditors using the bankruptcy register as a convenient source of information about the CEO's bankruptcy history, and with their failing to obtain similar information from sources other than the register. These findings imply that the public disclosure of bankruptcy records reduces information collection costs and provides creditors with relevant information that they subsequently act upon. Additional analyses also show that firms whose CEOs have a bankruptcy record utilize different types of financing (specifically, equity and accounts payable rather than loans) and that the risk of bankruptcy is higher for firms that employ CEOs with bankruptcy records, regardless of whether the record is public.

This paper contributes to three strands of literature. First, it contributes to the disclosure literature by illustrating how improved disclosure of the CEO's bankruptcy history can aid creditors in their decision making. Second, it contributes to the cost of debt literature by providing evidence that creditors respond to a CEO's bankruptcy record by increasing interest rates. Third, it contributes to the growing literature on the effects of corporate bankruptcy by showing that a CEO's bankruptcy experience can have an adverse effect on future employers by increasing their cost of debt.

2. Background and development of hypotheses

2.1. Personal characteristics and experience

Standard agency models recognize that managers can use their discretion to influence firm policies and outcomes (Bertrand and Schoar 2003). There are two potential channels through which managers and heterogeneity in firm policies may be related. First, an individual manager can impose their distinct “style” on the firm. Second, the firm may choose to hire a manager with a style that aligns with the firm’s strategy. In both cases, management’s characteristics can be informative because they may reflect the direction that management will take the firm or the direction in which the firm wants to go. Such characteristics include various personal traits and experiences, with the latter reflecting certain characteristics and/or influencing new characteristics.

Numerous studies examine the relation between management characteristics and firm outcomes or decisions. These include studies of management overconfidence and firm investment decisions (Malmendier and Tate 2005), management marital status and firm risk (Roussanov and Savor 2014), management retirement preferences and firm merger activity (Jenter and Lewellen 2015), management personal risk preferences and firm bankruptcy risk (Kallunki and Pyykkö 2013), management experience with early-life disasters and firm risk-taking (Bernile, Bhagwat, and Rau 2017), management military experience and firm leverage (Malmendier et al. 2011), and management experience with recession and conservative firm practices (Schoar and Zuo 2017). As such, the evidence indicating that management’s personal characteristics affect firm policies and outcomes is vast. These findings are consistent with established psychology literature, which highlights that individuals’ decisions are influenced by their personal biases, experiences, and preferences (Hertwig, Barron, Weber, and Erev 2004; Hertwig and Erev 2009; Hilbert 2012).

Recent studies explore how executives’ distress experience affects risk-taking (Ahmed et al. 2018; Dittmar and Duchin 2016; Gopalan et al. 2021), with bankruptcies being the rarest and most extreme type of distress experience. Bankruptcy experience could indicate that the individual has certain traits, such as low managerial ability or an appetite for risk. Some evidence supports this because low managerial ability is associated with the receipt of a going-concern opinion (Krishnan and Wang 2015) and because personal risk preferences are associated with the risk of bankruptcy (Kallunki and Pyykkö 2013). In addition, simply experiencing a bankruptcy could alter an individual's behavior. Dittmar and Duchin (2016) finds that CEOs become more risk averse after experiencing a bankruptcy. This could be due to the "hot stove" effect (Denrell and March 2001) or could reflect the desire to reduce the risk of being fired because CEO job prospects worsen following bankruptcy (Eckbo, Thorburn, and Wang 2016). In contrast, Gopalan et al. (2021) finds that directors become more risk-seeking following a bankruptcy. This could be because they reassess the true cost of bankruptcy (Gopalan et al. 2021), try to recoup lost earnings associated with the bankruptcy experience (Eckbo, Thorburn and Wang 2016), or perceive the likelihood of experiencing another bankruptcy as lower (i.e., "the gambler's fallacy").

In summary, firms managed by individuals with bankruptcy experience may have different risk profiles than those managed by individuals without such experience. This could be because bankruptcy experience reflects certain personal preferences or because individuals change their preferences upon experiencing a bankruptcy. The direction of the relation between bankruptcy experience and firm risk is therefore an empirical question. Personal characteristics that lead individuals to experience a corporate bankruptcy are likely associated with increased risk-taking but a change in preferences upon experiencing a bankruptcy could either decrease or increase future risk-taking.

2.2. Cost of debt

Creditors respond to riskier borrowers (i.e., those with a higher risk of default) by increasing the cost of debt. In order to assess the likelihood of default, creditors utilize both financial and non-financial information (Bhimani, Gulamhussen, and Lopes 2013; Grunert et al. 2005). They consider various financial aspects (Donelson, Jennings, and McInnis 2017), market position

(Grunert et al. 2005), competition (Valta 2012), earnings performance (Jiang 2008), and disclosure quality (Sengupta 1998), as well as many other factors.

One non-financial factor that may play a role in credit decisions is management. Donelson et al. (2017) find that the "character and reputation and experience of management" are important factors in evaluating whether to give credit, more so than several financial factors. However, they do not find that managerial aspects are important when setting the terms of the credit agreement, including interest. However, other studies have shown that managerial ability and practices are associated with the cost of debt (De Franco, Hope, and Lu 2017; Rahaman and Zaman 2013). Building on this, Bui et al. (2018) provide evidence that lenders are able to distinguish between luck and managerial ability when pricing debt. Recently, Regenburg and Seitz (2021) show that creditors consider criminal history, as firms led by CEOs with criminal records have higher costs of debt. These findings suggest that creditors incorporate information about management into their decision-making processes. However, seemingly no study has investigated how bankruptcy experience among CEOs affects the cost of debt. Based on the discussion in Section 2.1, this type of information should be of interest to creditors, as it could convey important information about the characteristics of the CEO and, potentially, the firm.

2.3. Hypotheses

Based on the extant literature, creditors likely view firms that employ a CEO with bankruptcy experience as riskier than comparable firms whose CEO does not have bankruptcy experience. Consequently, creditors may charge higher interest rates for the former. This is the initial assumption of the paper – that creditors do, in fact, respond to the CEO's bankruptcy history. Building on this, the first hypothesis is as follows:

H1: Firms that employ a CEO with bankruptcy experience have a higher cost of debt than other comparable firms.

Subsequently, the second hypothesis considers whether public disclosure of bankruptcy history is associated with a stronger response. This would be the case if information collection costs are greater than the expected return of said information (Blankespoor et al. 2020), but the public register reduces this cost making the return competitive. Creditors would likely collect information on the CEO's bankruptcy record in many cases, but there are undoubtedly some instances where collections costs are too high or the information is simply unavailable. Information asymmetry between small and medium-sized enterprises and banks is, for example, well-established and a major obstacle to accessing credit (Petersen and Rajan 1994), due to the opaqueness of the information available (Berger and Frame 2007). As such, the expectations are that, on average, creditors have more complete information about public bankruptcy records than non-public records, and the subsequent creditor response is, on average, stronger. The second hypothesis is as follows:

H2: Firms that employ a CEO with a public bankruptcy record have a higher cost of debt than firms whose CEOs have non-public bankruptcy records.

Additionally, because creditors might restrict financing when they observe risky management characteristics (Donelson et al. 2017), it is interesting to explore whether firms that employ a CEO with a bankruptcy record use different sources of financing. Finally, it is relevant to consider whether firms that employ a CEO with bankruptcy experience do in fact have a higher risk of bankruptcy. If this is the case, creditors are seemingly justified if they increase the cost of debt.

3. Research design and data

3.1. Identification strategy

This study utilizes the following generalized staggered DID model to determine whether the (type of) bankruptcy record of a CEO affects the firm's cost of debt:

$$INTEREST_{it} = \beta_0 + \beta_1(POST_{it} \times P\_BANKEXP_{it}) + \beta_2(POST_{it} \times NP\_BANKEXP_{it}) + \gamma X_{it} + \delta_t + \delta_i$$

Here, *INTEREST* represents the creditor response, which is cost of debt for firm *i* at time *t*. Consistent with prior literature (e.g., Minnis 2011; Regenburg and Seitz 2021; Vander Bauwhede, De Meyere, and Van Cauwenberge 2015), the cost of debt (*INTEREST*) is measured by dividing interest expenses by the average interest-bearing debt for year *t* and *t-1*, and is truncated at the 5th and 95th percentiles and at 15 basis points over the NIBOR3M rate.

The variables of interest identify firms that employ a CEO with a (non-)public bankruptcy record. *P_BANKEXP* is set equal to 1 if firm *i* at time *t* employs a CEO with public bankruptcy experience, and 0 otherwise. Likewise, *NP_BANKEXP* is set equal to 1 if firm *i* at time *t* employs a CEO with a non-public bankruptcy record, and 0 otherwise. Consequently, if both variables are equal to 0, the firm does not employ a CEO with bankruptcy experience of any kind. Both variables are interacted with *POST*, which signals whether this is before (1) or after (0) the firm hired a CEO with bankruptcy experience. The difference between the coefficients for *P_BANKEXP* and *NP_BANKEXP* measures the effect of bankruptcy history being public on the dependent variable, while the coefficient for *NP_BANKEXP* measures the effect of bankruptcy experience on the dependent variable.

The DID model also includes a vector of control variables (*X*): *SIZE*, *EBITDA*, *CASH*, *LEV*, *NI*, *CFO*, *GROWTH*, *TANGIBILITY*, *DIVIDEND*, *ZSCORE*, *BOARDSIZE*, *HHI*, *CEOAGE*, and *CEOEXP* (see Appendix I for variable definitions). Finally, year (δ_t) and firm

(δ_i) fixed effects are included, as both year and firm characteristics could affect our dependent variables as well as the likelihood of the firm having a CEO with bankruptcy experience. Note that because the treatment is staggered, the year fixed effect indicators do not subsume the *POST* effect. However, the *POST* effect is highly correlated with the two interaction terms and is consequently omitted.

As some level of self-selection or matching could occur between CEOs and firms, all analyses are performed on matched samples.[2] First, the sample is limited to firms that hire a new CEO within the sample period. This is important because there could be fundamental differences between firms that hire a new CEO and those that do not. Next, firms with CEOs who experience a concurrent bankruptcy with another firm are also initially eliminated.[3] Firms that eventually hire a new CEO with bankruptcy experience are subsequently matched with firms that eventually hire a new CEO without bankruptcy experience. Consistent with prior studies (e.g., Ahmed et al. 2018; Gopalan et al. 2021), firms are matched on year and industry as well as a set of covariates, including the dependent variables *INTEREST*, *LEV*, *SIZE*, *EBITDA*, *ZSCORE*, and *BOARDSIZE*. In line with Gopalan et al. (2021), Mahalanobis distance matching (MDM) is utilized for the matching procedure and the firms are matched on the year prior to hiring the new CEO.[4] This matching procedure should provide some assurance that the findings are not driven by matching or confounding factors, and that the estimated effect is that of hiring a CEO with bankruptcy experience.

---

[2] This concern is seemingly warranted, as untabulated results suggest that, on average, CEOs with bankruptcy experience work for riskier/more distressed firms.

[3] Concurrent bankruptcies typically occur when the CEO is also the director of the board at another firm that files for bankruptcy, although there are instances in which one individual holds multiple CEO positions. These concurrent bankruptcies are included in a robustness test later in the study.

[4] The reason for using MDM rather than propensity score matching (PSM) is that although both approaches yield balanced samples, MDM ensures that pairs have close covariate values, while PSM does not (King and Nielsen 2019). This is beneficial, as it ensures that firms led by CEOs with public and non-public records are matched with similar firms, effectively resulting in two matched control samples. The benefit of this is observable when splitting the sample into public and non-public bankruptcy experience (see Figure 1).

To ensure that the findings are robust, some alternative approaches are explored. First, the regression analysis is limited to a narrower event window to alleviate concerns that differences are due to firm effects. The analysis is limited to two (three) years before and after the hiring event. Second, following Gopalan et al. (2021), the analyses are performed on a matched sample of firms in which the CEO experiences a concurrent bankruptcy and comparable firms in which the CEO does not experience a concurrent bankruptcy. This approach should alleviate some concerns that the findings are driven by firm-CEO matching, as the CEOs were hired before they had any bankruptcy experience. The original matched sample and the new matched sample are also combined, and the model is estimated on a sample in which the treatment is either due to a new hire or a concurrent bankruptcy. Third, a within-CEO DID regression is estimated using the same model specification, but firm fixed effects are replaced with CEO fixed effects. This should again alleviate some concerns as to whether the effect is due to firm-specific characteristics. Fourth, two sets of variables are included to ensure that the effect is not due to creditors considering bankruptcies that happened far in the past as less relevant. The first is *TIME_BANK*, which is a discrete variable indicating the number of years since the CEO of firm *i* experienced their previous bankruptcy at time *t*. The second is *TIME_BANK_N*, which is a set of indicator variables equal to 1 if the previous bankruptcy for the CEO of firm *i* at time *t* was *N* years ago, and 0 otherwise.

Finally, two additional analyses further explore how employing a CEO with bankruptcy experience affects the firm. First, sources of funding are considered to evaluate whether firms whose CEO has a bankruptcy record utilize other means of financing. Specifically, the level of financial debt (*FINDEBT*), accounts payable (*AP*) and issuance of new equity (*NEWEQUITY*) is regressed on the original DID model (see Appendix I for variable definitions). *FINDEBT* represents financing from more sophisticated creditors, *AP* represents financing from suppliers (i.e., non-professional creditors), and *NEWEQUITY* represents financing from shareholders.[5] Second, the relationship between prior bankruptcy experience and the risk of future bankruptcies is explored. The analysis also utilizes a DID specification, but the control variables are replaced with those in Ohlson's (1980) bankruptcy model: *SIZE*, *LEV*, *WC*, *CR*, *NEGEQ*, *NI*, *CFOL*, *LOSS2*, and *gNI* (see Appendix I for variable definitions). Additionally, some of the variables from the original model are retained, specifically: *BOARDSIZE*, *HHI*, *CEOAGE*, and *CEOEXP*. Lastly, *INTEREST* is included as a control variable to control for the increased bankruptcy risk coming from higher cost of debt. A positive relationship between the bankruptcy experience indicators and bankruptcy would suggest that creditors are justified in increasing interest rates and/or withholding funding.

3.2. Data sources and samples

The data for this study is provided by the Brønnøysund Register Centre (BRREG), a Norwegian government body to which all Norwegian firms report. BRREG supplies subject-specific yearly datafiles containing both private and public firms' (i) financial figures, (ii) descriptive information, or (iii) CEO and board information. These are combined using unique firm-year identifiers into one large panel-data set, which contains firm-level data on all Norwegian firms from 1996 through 2018. In addition, BRREG supplies case-level data on all bankruptcy filings in Norway from 1999 through 2020. This data is aggregated at the firm and CEO levels, and subsequently merged with the large panel-data set using unique firm-year or CEO-year identifiers. The CEO-specific information is aligned with the respective firm's fiscal year. The

[5] The analysis utilizes whether new equity is issued rather than the level of equity, as equity is a noisy measure and an increase/decrease in equity could be the result of the firm's performance.

initial merged dataset consist of 594,639 unique firms and includes a total of 4,909,729 firm-year observations spanning from 1996 to 2018.

Table 1 describes the procedure used to construct the different samples utilized in the study. First, financial firms and firms that never have total assets greater than or equal to NOK 1,000,000 (approximately USD 120,000) during our sample period are excluded. Firm-year observations prior to 2004 are excluded from the analyses because bankruptcy information is only available from 1999, making it impossible to verify whether an individual is included in the register due to a bankruptcy that occurred before 1999.[6] However, firm-year observations prior to 2004 are still used to compute various variables, help handle missing values, and for matching purposes. Next, firm-year observations with no identified CEO are omitted. Finally, singleton observations are omitted. This procedure yields an initial sample of 213,424 unique firms and a total of 1,923,059 firm-year observations spanning from 2004 to 2018.

Table 1: Sample construction process

Description	ΔFirm-year	ΔFirm	Firm-year	Firm
Matched firm-year data from BRREG (1996-2018)			**4,909,729**	**594,639**
(Financial sector)	(1,003,827)	(140,014)		
(Total assets < NOK 1,000,000)	(741,481)	(152,736)		
(Year > 2003)	(815,356)	(32,231)		
(No CEO)	(411,991)	(42,219)		
(Singleton observations)	(14,015)	(14,015)		
Initial sample			**1,923,059**	**213,424**
(Matching procedure – new hires)	(1,395,697)	(170,998)		
(Missing data – control variables)*	(74,038)	(660)		
Sample: new hires			**453,324**	**41,766**
(Matching procedure – concurrent)	(1,725,979)	(196,334)		
(Missing data – control variables)*	(35,182)	(734)		
Sample: concurrent			**161,898**	**16,356**

The table describes how the initial sample and the two main subsamples are constructed. The data used in this study consists of firm-year observations provided by BRREG. To construct the initial sample, five exclusion criteria are imposed. First, financial sector firms are excluded. Second, firms that never report assets greater than MNOK 1 over the sample period are excluded. Third, observations prior to 2004 are excluded due to data limitations. Fourth, observations in which the firm has no current CEO are excluded. Fifth, singleton observations are excluded. Two subsamples are consequently constructed using the matching procedures described in Section 3.1. Note that some variables only have data from 2007 and beyond, in which case those observations are excluded.
* Regressions that do not include the control variables are estimated using the data from 2004 and beyond.

[6] This also means that we do not have complete information about whether an individual has ever experienced a bankruptcy. Any individual who experience a bankruptcy prior to 1999 is coded as non-bankrupt unless they experience another bankruptcy later. In practice, this means that some CEOs are wrongly classified as non-bankrupt. However, we expect this to negatively affect the likelihood of finding significant differences and any significant results should thus be viewed as robust to this.

The initial sample is then used as a basis to construct the two matched samples, as explained in Section 3.1. The matching procedure naturally results in a large reduction in the number of firm and firm-year observations, as hiring a CEO with bankruptcy experience and experiencing a concurrent bankruptcy are somewhat uncommon occurrences. As some of the variables only have information from 2007 and beyond, the regressions that include the control variables are performed on a sample of firms starting from 2007. The final matched sample for new hires consists of 41,766 firms and 453,324 firm-year observations. The final matched sample for concurrent bankruptcies consists of 16,356 firms and a total of 161,898 firm-year observations.

4. Empirical results

4.1. Descriptive statistics

Descriptive statistics are presented in Table 2. All continuous variables are winsorized at 0.01 and 0.99 (with the exception of *INTEREST*, as explained in Section 3.1) and standard deviations are omitted for indictor variables. The descriptive statistics show that there are significant differences between firms that employ a CEO with bankruptcy experience (3) and those that do not (4), as the former typically have higher interest rates, more debt, lower profitability, and greater distress risk. These differences could be due to the impact of having a CEO with bankruptcy experience, or due to selection or matching between CEOs and firms. This highlights the importance of matching the two groups.

Figure 1 depicts *INTEREST* three years before and after the appointment of a new CEO with bankruptcy experience (triangle – blue line) and without bankruptcy experience (circle – red line). It differentiates between any type of bankruptcy experience (left column), non-public bankruptcy experience (middle column), and public bankruptcy experience (right column). The

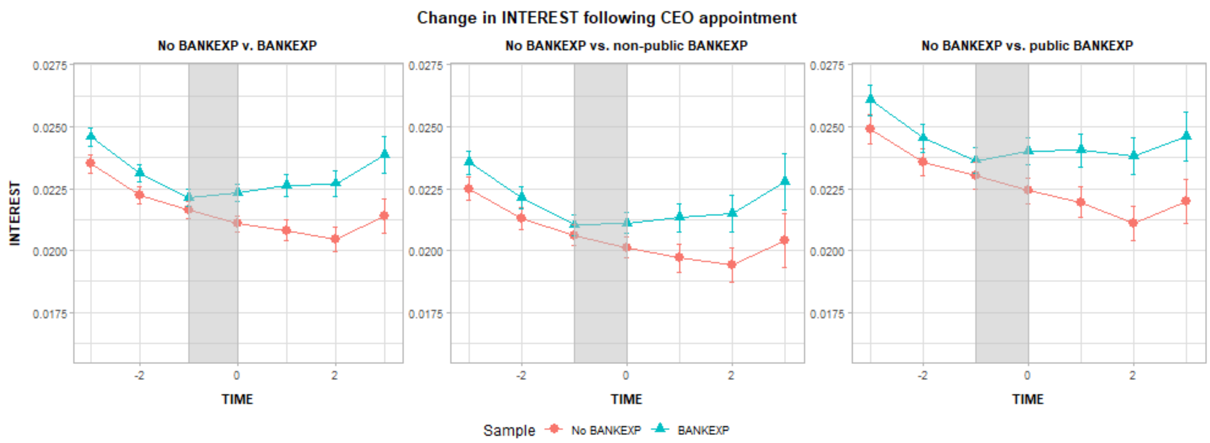
Table 2: Descriptive statistics

	Initial sample		BANKEXP = 1	BANKEXP = 0	
	Mean	Std. dev.	Mean	Mean	Diff.
	(1)	(2)	(3)	(4)	(5)
D.V.					
INTEREST	0.021	0.026	0.025	0.021	0.004***
FINDEBT	0.164	0.273	0.204	0.161	0.043***
BANK1	0.003	-	0.006	0.003	0.003***
BANK2	0.011	-	0.022	0.010	0.012***
BANK3	0.019	-	0.038	0.017	0.021***
AP	0.106	0.174	0.131	0.104	0.027***
NEWEQUITY	0.073	-	0.080	0.073	0.007***
I.V.					
SIZE	8.317	1.721	8.163	8.330	-0.167***
EBITDA	0.046	0.315	-0.013	0.051	-0.064***
CASH	0.236	0.267	0.172	0.241	-0.069***
LEV	0.810	1.048	1.080	0.788	0.292***
NI	0.020	0.340	-0.045	0.025	-0.070***
CFO	0.057	0.366	-0.001	0.062	-0.063***
GROWTH	0.028	0.312	0.025	0.029	-0.004***
TANGIBILITY	0.977	0.075	0.974	0.977	-0.003***
DIVIDEND	0.193	-	0.123	0.198	-0.075***
ZSCORE	6.525	26.61	4.830	6.661	-1.831***
BOARDSIZE	2.289	1.410	2.114	2.303	-0.189***
HHI	0.749	0.295	0.767	0.748	0.019***
AUDIT	0.100	-	0.167	0.094	0.073***
CEOAGE	49.98	11.01	52.15	49.80	2.350***
CEOEXP	4.171	7.963	6.290	4.000	2.290***
WC	0.601	0.359	0.555	0.605	-0.050***
CR	11.53	53.67	10.97	11.58	-0.610***
NEGEQ	0.127	-	0.217	0.119	0.098***
CFOL	0.305	1.776	0.209	0.313	-0.104***
LOSS2	0.286	-	0.381	0.278	0.103***
gNI	0.025	2.193	0.036	0.024	0.012**

This table presents descriptive statistics for the dependent (D.V.) and independent (I.V.) variables applied in this study. Column (1) presents the mean values for the initial sample, (2) presents the standard deviations for the initial sample, (3) presents the mean values for the firm-year observations in which the CEO has a bankruptcy record, (4) presents the mean values for the firm-year observations in which the CEO does not have a bankruptcy record, and (5) presents the difference (including significance) between columns (3) and (4). Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

findings suggest that the difference in the cost of debt increases when a firm hires a CEO with bankruptcy experience, regardless of whether the bankruptcy record is public.

Figure 1: Change in interest over time following a new CEO appointment



This figure shows mean values for *INTEREST* for firms that hire a CEO with bankruptcy experience (triangle – blue line) and firms that hire a CEO without bankruptcy experience (circle – red line). Error bars represent the 95% confidence interval. The figure differentiates between any type of bankruptcy record (left column), a non-public bankruptcy record (middle column), and a public bankruptcy record (right column). New CEOs are appointed between TIME -1 and 0 (grey area) and firms are matched on various covariates at TIME -1.

4.2. Main findings

Table 3 presents the findings from estimating the DID-model on a matched sample of firms that hire a new CEO. The dependent variable is *INTEREST* and the model is estimated both excluding (columns 1, 3, and 5) and including (columns 2, 4, and 6) control variables. For brevity, coefficients and t-statistics for the control variables are omitted. The results suggest that hiring a CEO with a public bankruptcy record is associated with a significant increase in cost of debt of more than 10 basis points, whereas the effect of a non-public bankruptcy record is insignificant. The difference between the effect of a public and non-public record is consequently significant. These findings provide evidence in favor of H1 and H2, by showing that cost of debt is higher for firms that employ a CEO with bankruptcy experience and that the effect is significantly larger when the bankruptcy is public.

Table 3: DID regression results for new hires

DV: INTEREST	(1)	(2)	(3)	(4)	(5)	(6)
P_BANKEXP	0.135***	0.114***			0.149***	0.127***
	(5.98)	(5.49)			(5.97)	(5.30)
NP_BANKEXP			-0.009	-0.007	0.042*	0.035
			(-0.44)	(-0.34)	(1.80)	(1.65)
CONTROLS	NO	YES	NO	YES	NO	YES
FE(YEAR)	YES	YES	YES	YES	YES	YES
FE(FIRM)	YES	YES	YES	YES	YES	YES
DIFF(P – NP)					0.107***	0.092***
N	527,362	453,324	527,362	453,324	527,362	453,324
Adjusted R^2	0.49	0.54	0.48	0.54	0.49	0.54

This table shows the regression results of estimating the DID-model on a matched sample firms that hire a new CEO. Columns (1), (3), and (5) are within-firm regressions excluding control variables and columns (2), (4) and (6) include control variables. The dependent variable is *INTEREST* across all regressions. Note that *INTEREST* is multiplied by a hundred and coefficients should be intercepted accordingly. The variables of interest are *P_BANKEXP* which indicates whether the CEO has a public bankruptcy record, while *NP_BANKEXP* indicates whether the CEO has a non-public bankruptcy record. DIFF(P-NP) represents the difference between *P_BANKEXP* and *NP_BANKEXP*. Control variables are specified in Section 3.1 and defined in Appendix I. T-statistics in parentheses are estimated using robust standard errors clustered by firm and year. Significance levels of 10%, 5% and 1% are represented by *, ** and ***.

Narrower event windows

To ensure that the findings are not driven by general firm effects, the DID model is estimated on narrower windows around the hiring event. The regression results are presented in Table 4. Columns (1) and (2) present the regression results excluding and including the control variables using an event window of plus/minus two years, while columns (3) and (4) present the same results using an event window of plus/minus three years. The findings remain qualitatively unchanged, as the coefficient for *P_BANKEXP* remains positive and significant, the coefficient for *NP_BANKEXP* remains insignificant, and the difference between the two coefficients remains positive and significant.

Table 4: DID regression results for new hires with a narrow event window

	Event window: ±2		Event window: ±3	
DV: INTEREST	(1)	(2)	(3)	(4)
P_BANKEXP	0.123***	0.104***	0.121***	0.101***
	(3.93)	(3.98)	(4.00)	(4.00)
NP_BANKEXP	0.058*	0.040	0.055*	0.035
	(1.98)	(1.70)	(2.05)	(1.62)
CONTROLS	NO	YES	NO	YES
FE(YEAR)	YES	YES	YES	YES
FE(FIRM)	YES	YES	YES	YES
DIFF(P – NP)	**	**	**	**
N	342,984	307,461	387,279	348,210
Adjusted R^2	0.49	0.55	0.50	0.55

This table shows the regression results of estimating the DID model on a matched sample of firms that hire a new CEO. Columns (1) and (2) are within-firm regressions excluding and including the control variables, where the dependent variable is *INTEREST* and the event window is two years before and after the hiring event. Columns (3) and (4) are within-firm regressions excluding and including the control variables, where the dependent variable is *INTEREST* and the event window is three years before and after the hiring event. Note that *INTEREST* is multiplied by 100 and the coefficients should be interpreted accordingly. The variables of interest are *P_BANKEXP*, which indicates whether the CEO has a public bankruptcy record, and *NP_BANKEXP*, which indicates whether the CEO has a non-public bankruptcy record. DIFF(P-NP) represents the difference between *P_BANKEXP* and *NP_BANKEXP*. The control variables are specified in Section 3.1 and defined in Appendix I. T-statistics in parentheses are estimated using robust standard errors clustered by firm and year. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

Concurrent bankruptcies

Next, the DID model is estimated on a matched sample of firms whose CEOs experience a concurrent bankruptcy and comparable peers whose CEOs do not experience a concurrent bankruptcy. The matching procedure is the same as for new hires and firms are matched on the year prior to the concurrent bankruptcy. The regression results are presented in Table 5. As all concurrent bankruptcies are initially public, the model is estimated using a general indicator for bankruptcy experience (*BANKEXP*) as well as the previous public and non-public indicators.[7] The findings are consistent with those for the sample looking at new hires, and they provide evidence in favor of H1 and H2. Interestingly, the regression results in column (4) also suggest

[7] It follows from this that observations where *NP_BANKEXP* = 1 are firms that have employed the same CEO that experienced a concurrent bankruptcy for more than five years.

that creditors do not forget, as the cost of debt is significantly higher even after the individual leaves the bankruptcy register.

Table 5: DID regression results for concurrent bankruptcies

DV: INTEREST	(1)	(2)	(3)	(4)
BANKEXP	0.151***	0.165***		
	(5.94)	(7.37)		
P_BANKEXP			0.172***	0.175***
			(6.44)	(7.67)
NP_BANKEXP			0.017	0.100***
			(0.41)	(3.40)
CONTROLS	NO	YES	NO	YES
FE(YEAR)	YES	YES	YES	YES
FE(FIRM)	YES	YES	YES	YES
DIFF(P – NP)	-	-	0.155***	0.075**
N	197,080	161,989	197,080	161,898
Adjusted R^2	0.48	0.54	0.48	0.54

This table shows the regression results of estimating the DID model on a matched sample of firms whose CEO experiences a concurrent bankruptcy and other comparable firms whose CEO does not experience a concurrent bankruptcy. Columns (1) and (2) are within-firm regressions excluding and including the control variables, where the dependent variable is *INTEREST* and the variable of interest is an indicator of whether the CEO has bankruptcy experience. Columns (3) and (4) are within-firm regressions excluding and including the control variables, where the dependent variable is *INTEREST*. The variables of interest are *BANKEXP*, indicating whether the CEO has previously been involved in a bankruptcy; *P_BANKEXP*, indicating whether the CEO has a public bankruptcy record; and *NP_BANKEXP*, indicating whether the CEO has a non-public bankruptcy record. DIFF(P-NP) represents the difference between *P_BANKEXP* and *NP_BANKEXP*. The control variables are specified in Section 3.1 and defined in Appendix I. T-statistics in parentheses are estimated using robust standard errors clustered by firm and year. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

As an additional test, the matched samples of new hires and concurrent bankruptcies are combined. Note that because one firm can be the control firm in both the new hire and concurrent samples, combining the datasets results in some duplicate control observations. This is effectively equivalent to matching with replacement. The findings are presented in Table 6. Again, the findings are consistent with bankruptcy experience being associated with a higher cost of debt and the effect is significantly greater when the bankruptcy record is publicly available. Likewise, the effect of a non-public bankruptcy record is again significant, suggesting that creditors do not forget that the sitting CEO previously experienced a bankruptcy.

Table 6: DID regression results for new hires and concurrent bankruptcies

DV: INTEREST	(1)	(2)	(3)	(4)
BANKEXP	0.111***	0.101***		
	(5.79)	(5.70)		
P_BANKEXP			0.159***	0.142***
			(7.92)	(7.94)
NP_BANKEXP			0.037*	0.044**
			(1.85)	(2.44)
CONTROLS	NO	YES	NO	YES
FE(YEAR)	YES	YES	YES	YES
FE(FIRM)	YES	YES	YES	YES
DIFF(P – NP)	-	-	0.122***	0.098***
N	724,444	615,246	724,444	615,246
Adjusted R^2	0.49	0.55	0.49	0.55

This table shows the regression results of estimating the DID model on a combined sample of both the new hires sample and the concurrent sample. Columns (1) and (2) are within-firm regressions excluding and including the control variables, where the dependent variable is *INTEREST* and the variable of interest is an indicator of whether the CEO has bankruptcy experience. Columns (3) and (4) are within-firm regressions excluding and including the control variables, where the dependent variable is *INTEREST*. The variables of interest are *BANKEXP*, indicating whether the CEO has previously been involved in a bankruptcy; *P_BANKEXP*, indicating whether the CEO has a public bankruptcy record; and *NP_BANKEXP*, indicating whether the CEO has a non-public bankruptcy record. DIFF(P-NP) represents the difference between *P_BANKEXP* and *NP_BANKEXP*. The control variables are specified in Section 3.1 and defined in Appendix I. T-statistics in parentheses are estimated using robust standard errors clustered by firm and year. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

Within-CEO specification

As CEOs switch jobs during the sample period, a within-CEO DID specification is utilized to further ensure that the findings are driven by CEO-specific factors and not firm effects. The DID model is specified as before, but firm fixed effects are replaced with CEO fixed effects. Notably, there is limited within-CEO variation with regards to bankruptcy experience, in part because CEOs tend to exit the executive labor market upon experiencing a bankruptcy (Eckbo, Thorburn, and Wang 2016; Gilson 1989). Table 7 presents the within-CEO regression results. Overall, the findings show that having a public bankruptcy record is associated with a higher cost of debt. Non-public bankruptcy records are no longer significant in the concurrent or combined samples, and the effect of public bankruptcy disclosure is consequently significant. This is consistent across all three samples, and the results again support H1 and H2.

Table 7: Within-CEO DID regression results across three samples

	Sample: new hires		Sample: concurrent		Sample: combined	
DV: INTEREST	(1)	(2)	(3)	(4)	(5)	(6)
BANKEXP	0.070		0.155***		0.082**	
	(1.79)		(4.36)		(2.70)	
P_BANKEXP		0.075*		0.154***		0.085**
		(1.87)		(4.29)		(2.72)
NP_BANKEXP		-0.021		0.074		-0.019
		(-0.46)		(1.37)		(-0.53)
CONTROLS	YES	YES	YES	YES	YES	YES
FE(YEAR)	YES	YES	YES	YES	YES	YES
FE(FIRM)	YES	YES	YES	YES	YES	YES
DIFF(P – NP)		0.096***		0.080*		0.104***
N	431,017	431,017	154,867	154,867	592,352	455,484
Adjusted R^2	0.51	0.51	0.51	0.51	0.51	0.51

This table shows the regression results of estimating a within-CEO DID model on three samples: new hires (columns 1 and 2), concurrent (columns 3 and 4), and both combined (columns 5 and 6). The dependent variable is *INTEREST* across all regressions. *INTEREST* is multiplied by 100 and coefficients should be interpreted accordingly. The variables of interest are *BANKEXP*, indicating whether the CEO has previously been involved in a bankruptcy; *P_BANKEXP*, indicating whether the CEO has a public bankruptcy record; and *NP_BANKEXP*, indicating whether the CEO has a non-public bankruptcy record. The control variables are specified in Section 3.1 and defined in Appendix I. T-statistics in parentheses are estimated using robust standard errors clustered by firm and year. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

Controlling for time since last bankruptcy

Finally, the variables *TIME_BANK* and the set of variables *TIME_BANK_N* are included to control for the fact that creditors may view old bankruptcies as less relevant, and that this drives the difference between public and non-public records. Table 8 presents the regressions results including controls for the time passed since the previous bankruptcy. Interestingly, as seen from column (1) the variable *TIME_BANK* is not significant when included in the model by itself. Columns (2) shows that both a public and non-public bankruptcy record is significantly associated with higher cost of debt when controlling for the time since the bankruptcy occurred. As in the previous analyses, the difference between the coefficient for *P_BANKEXP* and *NP_BANKEXP* is significant. Column (3) replaces *TIME_BANK* with the set of indicator variables *TIME_BANK_N*, and the findings show that the coefficient is positive and significant across the first five years, which are equivalent to the years where the bankruptcy experience is public. In contrast, the coefficients for *TIME_BANK_6* and beyond are not consistently significant. Again, the coefficient for *P_BANKEXP* remains significant when including this set of indicator variables, and the difference between *P_BANKEXP* and *NP_BANKEXP* is significant at the 10% level. This provides some evidence that the effect is not due to creditors considering older bankruptcies as less relevant.

Table 8: DID regression results including time since last bankruptcy

DV: INTEREST	(1)	(2)	(3)	(4)
P_BANKEXP		0.140***		0.104**
		(5.15)		(2.71)
NP_BANKEXP		0.075*		0.026
		(1.99)		(0.95)
TIME_BANK	0.002	-0.004		
	(1.06)	(-1.23)		
TIME_BANK_1			0.168***	0.064
			(4.77)	(1.15)
TIME_BANK_2			0.135***	0.031
			(3.17)	(0.87)
TIME_BANK_3			0.079**	-0.025
			(2.23)	(-0.52)
TIME_BANK_4			0.141***	0.037
			(3.38)	(0.80)
TIME_BANK_5			0.104**	
			(2.67)	
TIME_BANK_6			-0.021	-0.047
			(-0.52)	(-1.05)
TIME_BANK_7			0.058*	0.031
			(1.87)	(0.79)
TIME_BANK_8			0.049	0.023
			(1.43)	(0.70)
TIME_BANK_9			0.101**	0.074*
			(2.30)	(1.95)
TIME_BANK_10UP			0.026	
			(0.95)	
CONTROLS	YES	YES	YES	YES
FE(YEAR)	YES	YES	YES	YES
FE(FIRM)	YES	YES	YES	YES
DIFF(P – NP)		0.065**		0.078*
N	453,324	453,324	453,324	453,324
Adjusted R^2	0.54	0.54	0.54	0.54

This table shows the regression results of estimating the DID model on a matched sample of firms that hire a new CEO. All regressions are within-firm regressions where the dependent variable is *INTEREST*. The variables of interest are *BANKEXP*, indicating whether the CEO has previously been involved in a bankruptcy; *P_BANKEXP*, indicating whether the CEO has a public bankruptcy record; and *NP_BANKEXP*, indicating whether the CEO has a non-public bankruptcy record. The variable *TIME_BANK* is a discrete variable indicator the number of yeas since the CEO experiences their previous bankruptcy. *TIME_BANK_N* are indicator variables indicating whether the CEO experiences a bankruptcy *N* years ago. The remaining control variables are specified in Section 3.1 and defined in Appendix I. T-statistics in parentheses are estimated using robust standard errors clustered by firm and year. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

4.3. Additional analyses

Do firms seek alternative sources of financing?

Because creditors seemingly increase interest rates for firms that employ a CEO with a public bankruptcy record, firms may respond by seeking alternative sources of financing. Two alternative sources of funding are supplier credit and equity. Table 9 presents the results of regressing the within-firm DID model on *FINDEBT*, *AP* and *NEWEQUITY*. The findings in columns (1) and (2) show that firms do not decrease the level of debt to financial institutions.[8] However, the results in (3) and (4) suggest that firms increase the level of accounts payable upon employing a CEO with bankruptcy experience, both public and non-public. Here, a public disclosure effect is not evident, as the difference is only significant within the regression that does not include the control variables. There is some evidence that firms issue more equity upon hiring a CEO with bankruptcy experience, although it is only significant at the 10% level. Altogether, the direction of the results is consistent with what one would expect – firms seek alternative sources of income due to higher interest rates. However, the strength of these results is somewhat weaker than for the previous results.

Are creditors justified?

Finally, Table 10 shows the results of regressing whether or not the firm files for bankruptcy within one, two, or three years on the CEO's bankruptcy record. Generally, the findings show a significant and positive relationship between bankruptcy experience and the risk of filing for bankruptcy in the future. As such, creditors are seemingly justified in increasing the cost of debt for firms that employ a CEO with bankruptcy experience. Interestingly, the effect of bankruptcy

[8] In untabulated results, an alternative measure of *FINDEBT* is "debt to financial intuitions over total debt" rather than over total assets. However, the results remain qualitatively unchanged.

Table 9: DID regression results for sources of financing

	FINDEBT		AP		NEWEQUITY	
	(1)	(2)	(3)	(4)	(5)	(6)
P_BANKEXP	0.396	-0.028	0.977***	0.448**	0.006*	0.005
	(1.55)	(-0.14)	(4.80)	(2.64)	(2.11)	(1.74)
NP_BANKEXP	0.040	-0.086	0.456**	0.321*	0.004*	0.003*
	(0.23)	(-0.50)	(2.75)	(2.03)	(2.11)	(2.06)
CONTROLS	NO	YES	NO	YES	NO	YES
FE(YEAR)	YES	YES	YES	YES	YES	YES
FE(FIRM)	YES	YES	YES	YES	YES	YES
DIFF(P – NP)	0.356	0.060	0.521**	0.127	0.002	0.002
N	527,362	453,324	527,362	453,324	527,362	453,324
Adjusted R^2	0.64	0.71	0.57	0.66	0.16	0.19

This table shows the regression results of estimating the DID-model on a matched sample firms that hire a new CEO. Columns (1) and (2) are within-firm regressions excluding and including control variables where the dependent variable is *AP*. Columns (3) and (4) are within-firm regressions excluding and including control variables where the dependent variable is *NEWEQUITY*. Note that *AP* is multiplied by a hundred and coefficients should be intercepted accordingly. The variables of interest are *P_BANKEXP* which indicates whether the CEO has a public bankruptcy record, while *NP_BANKEXP* indicates whether the CEO has a non-public bankruptcy record. DIFF(P-NP) represents the difference between *P_BANKEXP* and *NP_BANKEXP*. Control variables are specified in Section 3.1 and defined in Appendix I. T-statistics in parentheses are estimated using robust standard errors clustered by firm and year. Significance levels of 10%, 5% and 1% are represented by *, ** and ***.

experience is greater when the CEO has a non-public bankruptcy record. This could suggest that having a public record does not adversely affect the firm's bankruptcy risk. In fact, having a public bankruptcy record could trigger stronger control and monitoring from stakeholders, which could help reduce the risk of bankruptcy. Finally, it should be noted that there is a possibility of reverse causality, in that higher cost of debt increases the risk of bankruptcy and that creditors may be more inclined to deem a firm bankrupt if the CEO has a bankruptcy record. This could consequently become a "self-fulfilling prophecy" that eventually leads to the firm filing for bankruptcy.

Table 10: DID regressions results for bankruptcy risk

	BANKRUPT1		BANKRUPT2		BANKRUPT3	
	(1)	(2)	(3)	(4)	(5)	(6)
BANKEXP	0.001		0.005***		0.005***	
	(1.56)		(3.78)		(3.86)	
P_BANKEXP		0.000		0.003*		0.003*
		(0.43)		(2.16)		(1.86)
NP_BANKEXP		0.001***		0.006***		0.007***
		(3.33)		(5.28)		(4.76)
CONTROLS	YES	YES	YES	YES	YES	YES
FE(YEAR)	YES	YES	YES	YES	YES	YES
FE(FIRM)	YES	YES	YES	YES	YES	YES
DIFF(P – NP)		-0.001*		-0.003**		-0.004**
N	453,324	453,324	453,324	453,324	453,324	453,324
Adjusted R^2	0.10	0.10	0.23	0.23	0.39	0.39

This table shows the regression results of estimating the DID model on a matched sample of firms that hire a new CEO. Columns (1) and (2) are within-firm regressions where the dependent variable is *BANKRUPT1*. Columns (3) and (4) are within-firm regressions where the dependent variable is *BANKRUPT2*. Columns (5) and (6) are within-firm regressions where the dependent variable is *BANKRUPT3*. The variables of interest are *BANKEXP*, indicating whether the CEO has previously been involved in a bankruptcy; *P_BANKEXP*, indicating whether the CEO has a public bankruptcy record; and *NP_BANKEXP*, indicating whether the CEO has a non-public bankruptcy record. The control variables are specified in Section 3.1 and defined in Appendix I. T-statistics in parentheses are estimated using robust standard errors clustered by firm and year. Significance levels of 10%, 5%, and 1% are represented by *, **, and ***, respectively.

5. Discussion

As a starting point, the findings in this paper provide robust evidence that creditors consider the bankruptcy history of CEOs when setting interest rates. Upon employing a CEO with a bankruptcy record, firms experience an increase in the cost of debt relative to firms that do not employ a CEO with such a record. This is the case when firms hire a new CEO with bankruptcy experience or when the current CEO experiences a concurrent bankruptcy, and the findings are robust to matching procedures and several controls. This is consistent with expectations, as creditors could view individuals with bankruptcy experience as less skilled or more risk-seeking, which could consequently affect the firms they manage. As such, the results support the hypothesis that creditors respond to firms that employ CEOs with bankruptcy experience by raising interest rates.

Crucially, the regression results show that the effect of bankruptcy experience on the cost of debt is limited to firms that employ a CEO with a public bankruptcy record in the bankruptcy register. The variable for public bankruptcy experience is consistently significant and positive across all model specifications and samples, and the effect of a public and non-public record is consistently significantly different. This is in line with expectations, as the bankruptcy register significantly reduces information-collection costs, ensuring that creditors have access to important information about the CEO's background. Furthermore, using a matched sample of concurrent bankruptcies, the results show a significant effect of a non-public bankruptcy record on the cost of debt. Under the concurrent sample, non-public observations are limited to CEOs who experience a concurrent bankruptcy and subsequently stay with the firm for more than four years. This suggests that creditors do not forget about the bankruptcy record of the sitting CEO. There is no such effect when looking at a sample of new hires, in part because most non-public observations are due to new hires (whose history the creditors have no knowledge of) rather than CEOs going from a public to non-public record. All in all, these findings support the hypothesis that the effect of bankruptcy history on the cost of debt is stronger when the record is publicly available, likely because information-collection costs are lower.

The findings from the additional analyses tell a similar story. Firms that employ CEOs with bankruptcy records tend to increase the level of accounts payable and there is some evidence that they issue more equity. This is an expected response to higher interest rates, as accounts payable often are interest-free and equity does not have a legally required rate of return. However, the difference between the public and non-public effect is not significant, although the coefficient for public bankruptcy experience is consistently higher. As such, the results of the additional analyses follow logically from the main results and provide additional support for the story.

Creditors also seem to be justified in their response, as employing a CEO with a bankruptcy record is significantly and positively associated with bankruptcy risk. This is consistent with our expectations, as bankruptcy experience could signal lower managerial ability or a greater appetite for risk. What is perhaps most interesting is that the risk of bankruptcy is greater when the bankruptcy record is not public. This could suggest that a public record may have a disciplinary effect, perhaps due to increased stakeholder monitoring or control. However, the effect size is relatively small and does not necessarily justify the information-collection costs associated with obtaining information about non-public bankruptcy experience. It could also be that the 10 to 20 basis-point increase in interest rates is disproportionate to the bankruptcy risk and could play a role in the increased bankruptcy risk.

It is difficult to completely ensure that the findings are not driven by other factors, and it is not possible to rule out other explanations beyond creditors taking the CEO's bankruptcy record into consideration. Some concerns should be mitigated by the matching procedures and robustness tests, but there could still be alternative explanations for the findings, such as firm-CEO matching. However, even if the findings are due, in part, to firm-CEO matching, this would suggest that creditors could view the hiring of a CEO with bankruptcy experience as a signal of the firm's financial state or future plans. The practical implications of this would be consistent with our general finding that employing a CEO with a bankruptcy record results in a higher cost of debt, although the mechanism would be different.

By illustrating that creditors incorporate bankruptcy experience into their decision making, the study contributes to the well-established literature on the cost of debt and, more specifically, to the growing stream of literature on the how personal characteristics of management affect creditors' decisions (e.g., De Franco et al. 2017; Donelson et al. 2017; Rahaman and Zaman 2013; Regenburg and Seitz 2021). This study also contributes to the growing body of literature on the effects bankruptcy experience among executives (e.g., Dittmar and Duchin 2016;

Gopalan et al. 2021; Guo, Lisic, Pittman, Seidel, Zhou, and Zhou 2022; Ivanova, Tylaite, and Zhu 2022). Finally, the study contributes to the literature on public disclosure by providing evidence of how a public bankruptcy register reduces information-collection costs and enables creditors to make more informed decisions. These findings are particularly of interest with regard to SME, as it might be more difficult to obtain information on the CEO's past employment history when the firm is small.

References

Ahmed, A. S., Christensen, B. E., Olson, A. J., & Yust, C. G. (2019). Déjà Vu: The Effect of Executives and Directors with Prior Banking Crisis Experience on Bank Outcomes around the Global Financial Crisis. Contemporary Accounting Research, 36(2), 958-998.

Bennedsen, M., Pérez-González, F., & Wolfenzon, D. (2020). Do CEOs Matter? Evidence from Hospitalization Events. The Journal of Finance, 75(4), 1877-1911.

Berger, A. N., & Frame, S. W. (2007). Small business credit scoring and credit availability. Journal of Small Business Management, 45, 5-22.

Bernile, G., Bhagwat, V., & Rau, P. R. (2017). What Doesn't Kill You Will Only Make You More Risk-Loving: Early-Life Disasters and CEO Behavior. The Journal of Finance, 72(1), 167-206.

Bertrand, M., & Schoar, A. (2003). Managing with Style: The Effect of Managers on Firm Policies. The Quarterly Journal of Economics, 118(4), 1169-1208.

Bhimani, A., Gulamhussen, M. A., & Lopes, S. R. (2013). The Role of Financial, Macroeconomic, and Non-financial Information in Bank Loan Default Timing Prediction. European Accounting Review, 22(4), 739-763.

Blankespoor, E., deHaan, E., & Marinovic, I. (2020). Disclosure processing costs, investors' information choice, and equity market outcomes: A review. Journal of Accounting and Economics, 70(2-3).

Cain, M., & McKeon, S. (2016). CEO Personal Risk-Taking and Corporate Policies. Journal of Financial and Quantitative Analysis, 51(1), 139-164.

De Franco, G., Hope, O., & Lu, H. (2017). Managerial ability and bank-loan pricing. Journal of Business Finance & Accounting, 44(9-10), 1315-1337.

Denrell, J., & March, J. G. (2001). Adaptation as Information Restriction: The Hot Stove Effect. Organization Science, 12(5), 523-538.

Dittmar, A., & Duchin, R. (2016). Looking in the Rearview Mirror: The Effect of Managers' Professional Experience on Corporate Financial Policy. The Review of Financial Studies, 29(3), 565-602.

Donelson, D. C., Jennings, R., & McInnis, J. (2017). Financial Statement Quality and Debt Contracting: Evidence from a Survey of Commercial Lenders. Contemporary Accounting Research, 34(4), 2051-2093.

Eckbo, B. E., Thorburn, K. S., & Wang, W. (2016). How costly is corporate bankruptcy for the CEO? Journal of Financial Economics, 121(1), 210-229.

Gilson, S. C. (1989). Management turnover and financial distress. Journal of Financial Economics, 25(2), 241-262.

Goldstein, I., & Yang, L. (2017). Information Disclosure in Financial Markets. Annual Review of Financial Economics, 9, 101-125.

Gopalan, R., Gormley, T. A., & Kalda, A. (2021). It's not so bad: Director bankruptcy experience and corporate risk-taking. Journal of Financial Economics, 142(1), 261-292.

Grunert, J., Norden, L., & Weber, M. (2005). The role of non-financial factors in internal credit ratings. Journal of Banking & Finance, 29(2), 509-531.

Guo, F., Lisic, L. L., Pittman, J., Seidel, T. A., Zhou, M., & Zhou, Y. (2022). Fool Me Once, Shame on You; Fool Me Twice, Shame on Me: The Long-Term Impact of Arthur Andersen's Demise on Partners' Audit Quality. Contemporary Accounting Research, Accepted manuscript.

Hertwig, R., & Erev, I. (2009). The description–experience gap in risky choice. Trend in Cognitive Sciences, 13(12), 517-523.

Hertwig, R., Barron, G., Weber, E. U., & Erev, I. (2004). Decisions from Experience and the Effect of Rare Events in Risky Choice. Psychological Science, 15(8), 534-539.

Hilbert, M. (2012). Toward a synthesis of cognitive biases: How noisy information processing can bias human decision making. Psychological Bulletin, 138(2), 211-237.

Hsu, A., Lee, C.-F., & Liu, S. (2022). Book-tax differences, CEO overconfidence, and bank loan contracting. Review of Quantitative Finance and Accounting, 58, 437-472.

Ivanova, M., Tylaite, M., & Zhi, L. (2022). Impact of client bankruptcies on auditors' judgment. Working paper.

Jenter, D., & Lewellen, K. (2015). CEO Preferences and Acquisitions. The Journal of Finance, 70(6), 2813-2852.

Jian, M., & Kee, K. W. (2011). Does CEO reputation matter for capital investments? Journal of Corporate Finance, 17(4), 929-946.

Jiang, J. X. (2008). Beating Earnings Benchmarks and the Cost of Debt. The Accounting Review, 83(2), 377-416.

Kallunki, J.-P., & Pyykkö, E. (2013). Do defaulting CEOs and directors increase the likelihood of financial distress of the firm? Review of Accounting Studies, 18, 228-260.

King, G., & Nielsen, R. (2019). Why Propensity Scores Should Not Be Used for Matching. Political Analysis, 27(4), 435-454.

Krishnan, G. V., & Wang, C. (2015). The Relation between Managerial Ability and Audit Fees and Going Concern Opinions. AUDITING: A Journal of Practice & Theory, 34(3), 139-160.

Malmendier, U., & Tate, G. (2005). CEO Overconfidence and Corporate Investment. The Journal of Finance, 60(6), 2661-2700.

Malmendier, U., & Tate, G. (2008). Who makes acquisitions? CEO overconfidence and the market's reaction. Journal of Financial Economics, 89(1), 20-43.

Malmendier, U., Tate, G., & Yan, J. (2011). Overconfidence and Early-Life Experiences: The Effect of Managerial Traits on Corporate Financial Policies. The Journal of Finance, 66(5), 1687-1733.

Ooghe, H., & De Prijcker, S. (2008). Failure processes and causes of company bankruptcy: a typology. Management Decision, 46(2), 223-242.

Petersen, M. A., & Rajan, R. G. (1994). The benefits of lending relationships: Evidence from small business data. Journal of Finance, 49, 3-37.

Rahaman, M. M., & Al Zaman, A. (2013). Management quality and the cost of debt: Does management matter to lenders? Journal of Banking & Finance, 37(3), 854-874.

Regenburg, K., & Seitz, M. N. (2021). Criminals, bankruptcy, and cost of debt. Review of Accounting Studies, 26, 1004-1045.

Roussanov, N., & Savor, P. (2014). Marriage and Managers' Attitudes to Risk. Management Science, 60(10), 2496-2508.

Schoar, A., & Zho, L. (2016). Does the Market Value CEO Styles? American Economic Review, 106(5), 262-266.

Schoar, A., & Zuo, L. (2017). Shaped by Booms and Busts: How the Economy Impacts CEO Careers and Management Styles. The Review of Financial Studies, 30(5), 1425-1456.

Sengupta, P. (1998). Corporate Disclosure Quality and the Cost of Debt. The Accounting Review, 73(4), 459-474.

Valta, P. (2012). Competition and the cost of debt. Journal of Financial Economics, 105(3), 661-682.

Appendix

Appendix I: Variable definitions

Variable		Definition
INTEREST	=	Interest expense over the mean value of net interest-bearing debt at t and t-1.
FINDEBT	=	Debt to financial institutions over total assets.
AP	=	Accounts payable over total assets.
NEWEQUITY	=	1 if the firm issues new equity within the fiscal year, 0 otherwise.
BANK1	=	1 if the firm files for bankruptcy within one year, 0 otherwise.
BANK2	=	1 if the firm files for bankruptcy within two years, 0 otherwise.
BANK3	=	1 if the firm files for bankruptcy within three years, 0 otherwise.
SIZE	=	Natural log of total assets.
EBITDA	=	Earnings before interest, taxes, depreciation, and amortization over total assets.
CASH	=	Cash and cash equivalents over total assets.
LEV	=	Total liabilities over total assets.
NI	=	Net income over total assets.
CFO	=	Cash flow from operations over total assets.
GROWTH	=	Sales minus lagged sales over the absolute value of sales plus lagged sales.
TANGIBILITY	=	Tangible assets over total assets.
DIVIDEND	=	1 if the firm paid a dividend, 0 otherwise.
ZSCORE	=	Altman's Z-score modified for private firms.
BOARDSIZE	=	Number of board members.
HHI	=	Firm ownership concentration as measured using an adapted Herfindahl-Hirschman Index.
AUDIT	=	1 if any adverse comment was mentioned in the audit opinion, 0 otherwise.
CEOAGE	=	Age of the CEO.
CEOEXP	=	Number of firms that the CEO has previously managed.
TIME_BANK	=	Discrete variable of the number of years since the CEO experienced their previous bankruptcy
TIME_BANK_N	=	1 if the CEO experiences a variable *N* years ago, 0 otherwise.
WC	=	Current assets minus current liabilities (working capital) over total assets.
CR	=	Current assets over current liabilities.
NEGEQ	=	1 if the firm's total liabilities are greater than total assets, 0 otherwise.
CFOL	=	Cash flow from operations over total liabilities.
LOSS2	=	1 if the firm has reported a loss over the two last fiscal years, 0 otherwise.
gNI	=	Net income minus lagged net income over the absolute value of net income plus lagged net income.