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When Executives Pledge Integrity: The Effect of the Accountant's Oath on Firms' Financial Reporting

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ABSTRACT: We study the effect of executives' pledges of integrity on firms' financial reporting outcomes by exploiting a 2016 regulation that requires holders of Dutch professional accounting degrees to pledge an integrity oath. We identify chief executive officers (CEOs) and chief financial officers (CFOs) required to take the integrity oath and find that firms reduce income-increasing discretionary accruals after executives took the oath. These firms also reduce discretionary expenditures, indicating that oath-taking executives reduce overall earnings management and do not merely substitute accruals-based with real-activities earnings management. These effects are concentrated in firms where the CFO took the oath. Overall, our results indicate that integrity oaths for executives improve firms' financial reporting quality.

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I. INTRODUCTION

Oaths have gained momentum as a tool to strengthen integrity among executives and other professionals (e.g., Khurana and Nohria 2008; Zingales 2015). The increasing popularity of these oaths is motivated by the insight that executives' integrity is a key determinant of firms' compliance with laws and regulations and that traditional corporate governance mechanisms regularly fail to ensure integrity among executives (e.g., Dikolli, Keusch, Mayew, and Steffen 2020; Guiso, Sapienza, and Zingales 2015). Despite the increasing prevalence of these oaths, there is no systematic evidence on whether executives' pledges of integrity affect firms' financial reporting quality. We exploit

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a 2016 law that introduced a requirement for holders of Dutch professional accounting degrees to pledge a one-time integrity oath.[1] The objective of this law was to improve firms' reporting quality and reduce fraud risk (Koninklijke Nederlandse Beroepsorganisatie van Accountants (NBA) 2014). We identify chief executive officers (CEOs) and chief financial officers (CFOs) subject to this law and examine whether and how their firms' financial reporting changes after the oath-taking.

This setting presents several advantages to identify the effect of integrity oaths on financial reporting. First, a common challenge in examining the effect of new regulations on behavior is that regulations typically include various new provisions and concurrent enforcement changes, making it difficult to isolate the effect of one specific provision, such as an integrity oath, on behavior. In contrast, the regulation we exploit did not include other changes, such as stronger penalties for accounting fraud, more enforcement, or new legal or ethical requirements. Second, new regulations typically affect all firms, making it difficult to identify an appropriate control group. In our setting, CEOs and CFOs who do not hold a Dutch professional accounting degree are not required to pledge the integrity oath, allowing us to use their firms as a control group. Using the Dutch public registry of professional accounting-degree holders, we identify 40 CEOs and 84 CFOs, working for 120 unique firms, who are required to take the oath. Many firms are led by CEOs or CFOs who do not have a professional accounting degree, forming the control group of 717 unique firms. Our final sample consists of 837 Dutch private and public firms from 2013 to 2019. Third, we can rely on a set of measures to capture reporting quality and can separately examine the effect of integrity oaths taken by CFOs who directly oversee the financial statements and CEOs who may be less directly involved in firms' reporting choices (e.g., Feng, Ge, Luo, and Shevlin 2011).

From a theoretical standpoint, the effect of executives' pledges of integrity on firms' financial reporting quality is unclear. Integrity oaths could enhance executives' compliance with regulations and standards for at least three reasons. First, oaths can serve as a reminder of the code of ethics and laws within a profession and introduce cues that make people think of their own moral standards when tempted to act dishonestly (e.g., Ariely 2012; Ayal, Gino, Barkan, and Ariely 2015; Khurana and Nohria 2008; Mazar, Amier, and Ariely 2008). These reminders can prevent individuals from justifying their dishonest behavior (Shalvi, Gino, Barkan, and Ayal 2015). Second, oaths can affect executives' understanding of acceptable conduct within a profession (Cialdini and Trost 1998). Thus, an executive whose behavior is noncompliant with the norms within a profession experiences disutility (e.g., de Quervain et al. 2004; Rilling et al. 2002). Third, an oath can create a commitment that binds the individual to behavioral acts of a specific type (e.g., Jacquemet, Luchini, Rosaz, and Shogren 2019). For example, prior work shows that oaths trigger commitments that increase truth-telling—even when the probability of detecting a lie is known to be zero (e.g., Mazar et al. 2008). In our setting, the oath could drive CEOs and CFOs to improve their firms' financial reporting quality. For example, Hugo Hollander (2016), the Director of a Dutch accounting firm, describes that oath-taking helped him to "understand what being an accountant is about." Under this view, we would expect that firms whose CEO or CFO takes the oath engage in less aggressive reporting choices.

In contrast, oaths could also be ineffective for several reasons. First, the oath does not introduce new ethical requirements or offer specific guidance through the many difficult decisions that accountants make. Instead, it is a mere reminder of what is already required by the professional code of conduct, laws, and regulations. Consistent with this, Pheijffer (2014), an accounting professor at a Dutch university, argues that the oath is unnecessary, as Dutch accountants already have a code of conduct. Second, the oath does not increase the legal penalties or detection rate of misconduct, and, thus, violations of the oath would often go undetected. Therefore, according to Becker's (1968) framework, executives' behavior should not change, as the expected costs of misconduct remain unchanged. Third, prior work shows that personal characteristics (e.g., Davidson, Dey, and Smith 2015; Malmendier and Tate 2005, 2015; Schrand and Zechman 2012), education, and the behavior of other executives (e.g., Kowaleski, Sutherland, and Vetter 2020; Zingales 2015) drive executives' behavior. Moreover, fraudulent executives often rationalize their behavior (Bazerman and Tenbrunsel 2011; Messick and Bazerman 1996). Under this alternative view, we would not expect to find a change in reporting choices for firms whose CEO or CFO takes the oath. Ultimately, whether and the extent to which executives' integrity oaths affect firms' financial reporting outcomes are empirical questions.

Empirically, we employ a difference-in-differences methodology that examines changes in reporting outcomes after the passage of the 2016 law requiring holders of Dutch professional accounting degrees to pledge an oath. To measure reporting outcomes, we use three different proxies for firms' reporting quality (Beneish 1999; Dechow, Sloan, and Sweeney 1995; Roychowdhury 2006).[2] Using the modified Jones (1991) model to estimate discretionary accruals, as per

[1] A professional accounting-degree holder in The Netherlands is comparable to a Certified Public Accountant (CPA) in the United States. The oath was implemented in response to a series of scandals and regulatory findings suggesting shortcomings in firms' financials (e.g., Brouwer 2015). The objective of the oath was to remind professional accountants that integrity, quality, and professional skepticism are paramount to ensure that financial statements provide a true and fair view of the business. We provide an overview of the setting in the "Institutional Setting" section.

[2] We do not use restatements or enforcement actions because data on these events are sparse in the Dutch setting (Bosman 2021).

Dechow et al. (1995), we find a reduction in income-increasing discretionary accruals after executives take the oath. After a CFO pledges the oath, income-increasing discretionary accruals decrease by approximately 0.26 standard deviations. We do not find an effect when only the CEO takes the pledge.

Interpreting these results as an overall improvement of firms' reporting quality is not straightforward because treated executives might simply switch from accruals-based to real-activities earnings management. Prior work shows that firms switch between these two strategies, depending on their costs (e.g., Zang 2012). To examine this possibility, we also examine the effect of taking the oath on real-activities earnings management using the model of Roychowdhury (2006). After a CFO pledges the oath, discretionary production costs decrease by 0.42 standard deviations, and discretionary expenses decrease by 0.50 standard deviations, highlighting that treated firms reduce both discretionary production costs and expenses. Similar to the previous results, we do not find an effect when only the CEO takes the pledge. These results suggest that treated executives do not substitute accruals-based with real-activities earnings management.

A question arising from these results is whether the oath also reduces egregious reporting practices, especially as that was an objective of the Dutch accounting oversight body (NBA 2014). To address this question, we use Beneish's (1999) M-Score, a commonly used fraud-prediction score (Beneish and Vorst 2022), to identify potential financial misstatements. We find that a firm's M-Score decreases by 0.16 standard deviations after the CFO takes the oath, suggesting that the oath also reduces more egregious accounting practices.

Potential concerns with our findings are that they might be driven by unobservable correlated omitted variables, are sensitive to our research-design choices, or are driven by concurrent events. We conduct three sets of tests to alleviate these concerns. First, we examine the time trends in treatment versus control firms around the year of the oath-taking requirement. These analyses suggest that there were no pre-trends before the oath-taking requirement. Similarly, we run falsification tests randomizing the treated firms and imposing placebo treatments on Belgian firms run by executives with an accounting degree. We do not find significant effects in these tests. In addition, we conduct tests to mitigate the concern that concurrent changes in the Dutch Corporate Governance or accounting rules explain our results. In sum, these tests alleviate the concern that other concurrent events drive our results. Second, we find consistent results using entropy balancing or when we define the control group using exclusively firms run by executives with an inactive accounting or business degree. These tests alleviate the concern that the effects are due to inherent differences between treated and control firms or executives. Finally, our results are robust to alternative research designs and estimation models (e.g., Chen, Hribar, and Melessa 2018; Dechow and Dichev 2002; Kothari, Mizik, and Roychowdhury 2016).

Finally, we also examine potential differences in the effect of oaths for executives of private as compared with public firms. We find that the oath has a similar effect on reducing earnings management in both private and public firms. We also do not find that firms whose executives took the oaths have lower future performance, alleviating the concern that oath-taking harms investors.

Our study makes three main contributions. First, professional integrity oaths are increasingly used as a cost-effective tool to enhance compliance with laws and regulations. Despite the prevalence of these oaths, little is known about their efficacy. Most closely related are a few studies examining the stock-market response to the Securities and Exchange Commission's (SEC's) (2002) oath requirement for executives (e.g., Bhattacharya, Groznik, and Haslem 2007; Chang, Chen, Lia, and Mishra 2006; Griffin and Lont 2005). However, these studies find mixed results, perhaps because it is empirically challenging to separate the effect of the oath from other provisions of the Sarbanes-Oxley Act that coincided with the oath and affected firms' reporting choices and stock-market responses. Although our study focuses on a different setting, it provides the first systematic evidence showing that executives' integrity pledges improve firms' financial reporting. Thus, our study offers a possible explanation for the positive stock-market response following executives' certification of their firms' financial statements documented by Chang et al. (2006); i.e., investors are responding to improved financial reporting quality. These results are particularly important in light of evidence suggesting that other instruments to strengthen professional integrity in corporations, such as codes of conduct or compliance trainings, are often ineffective (e.g., Kaptein and Schwartz 2008; Park 2020).

Second, our study contributes to the literature in behavioral economics examining the effect of oaths on individuals' behavior. Most studies in this area employ experiments presenting hypothetical scenarios outside of capital markets (e.g., Bénabou and Tirole 2011; Brandts and Charness 2003; Jacquemet et al. 2019; Wang and Murnighan 2017). Although this literature typically finds improvements in behavior after oath-taking, it is unclear whether these effects extend to individuals' actual on-the-job behavior (e.g., FeldmanHall et al. 2012) and to a capital-market context. By contrast, our study uses a large sample of executives and firms to examine on-the-job behavior by exploiting a 2016 law that introduces a one-time integrity oath as a quasi-natural experiment. Further, our study is the first to document the effect of oaths on financial reporting choices.

Finally, our results provide valuable insights to professional associations, regulators, and policymakers aiming to strengthen firms' compliance with accounting regulations. Our results are particularly informative for Dutch regulators

and policymakers, as they provide evidence on the effect of the Dutch integrity oath.[3] Our results are also useful for other professional associations, regulators, and policymakers that have debated the use of oaths to improve firms' financial reporting. For example, in the aftermath of the Enron scandal, the SEC required CEOs and CFOs of large firms to swear that their financial reports are accurate and complete. Although the SEC only required this oath once, our findings show that such one-time oaths can be an effective mechanism to improve firms' financial reporting. Particularly relevant to capital-market regulators is our finding that the oath reduces earnings management in both private and public firms and does not lead to lower future performance.

II. RELATED LITERATURE, INSTITUTIONAL SETTING, AND RESEARCH QUESTION

Related Literature

A large body of work in accounting, economics, finance, and law focuses on understanding which regulatory tools work to improve financial reporting. Within the accounting literature, prior studies show that regulatory resources (e.g., Kedia and Rajgopal 2011), increased transparency of regulatory oversight (e.g., Duro, Heese, and Ormazabal 2019), better controls (e.g., Heese and Pérez-Cavazos 2020), and greater accountability (e.g., Cohen, Dey, and Lys 2008) can improve firms' financial reporting. However, the prevalence of accounting scandals has resulted in calls to further strengthen the ethical conduct of managers (e.g., Zingales 2015). These calls are motivated by research showing that executives' integrity is an essential determinant of firms' compliance with laws and regulations (e.g., Dikolli et al. 2020; Guiso et al. 2015). Dikolli et al. (2020), for example, show that higher perceived CEO integrity reduces audit fees and option backdating.

To strengthen professional integrity, regulators, professional associations, and firms have implemented various new instruments over the last two decades, albeit with mixed success. For example, regulators have passed regulations, such as the Sarbanes-Oxley Act, requiring firms to develop codes of conduct. However, research suggests that codes of conduct are often ineffective (e.g., Kaptein and Schwartz 2008). Similarly, regulatory bodies of various professions, as well as companies, have also tried to improve integrity via ethics training and debated more broadly how to prevent rationalization of fraudulent activities. For example, since 2010, the qualification exam for investment advisers in the United States focuses more on ethics, and Kowaleski et al. (2020) show that this change reduced advisers' misconduct. In contrast, Park (2020) studies the effect of compliance trainings on misconduct in a large multinational firm and finds that only some compliance trainings affect behavior and that the effect is generally short-lived. Overall, regulators and firms are still trying to better understand which instruments work to enhance professional integrity and how different types of organizations and professionals respond to these instruments. Using the fraud triangle as a framework, Mintchik and Riley (2019) point out that little is known about perpetrators' rationalization of fraud and strategies that help to prevent such rationalization.

Recently, several professional associations and regulators have begun requiring professionals to take mandatory integrity oaths to strengthen compliance with laws and regulations. Oaths have a long tradition, dating as far back as the Hippocratic Oath, circa 400 BC, as an instrument to shape beliefs about acceptable conduct and commit members of a profession to a specific type of behavior. Although traditionally, oaths were only required of physicians, politicians, lawyers, and civil servants, more recently, accountants, engineers, financial advisers, and other professionals around the world are being asked to make formal pledges of their integrity. For example, since 2010, senior executives of Dutch banks have been required to pledge an integrity oath (Weitzel and Kirchler 2021). Similarly, several countries, such as Singapore (Institute of Singapore Chartered Accountants (ISCA) 2016), require all professional accounting-degree graduates to swear an integrity oath. In the United States, the Maryland Association of Certified Public Accountants requires all newly certified public accountants to swear an oath (Maryland Association of Certified Public Accountants (MACPA) 2010). Relatedly, CPA license holders in the United States are required to make a declaration about their ethical compliance when they renew their license. Managers of publicly listed U.S. companies are required to provide and sign a management representation letter to indicate the "true and fair" nature of the financial statements during the audit process. These various forms of pledges differ in frequency (e.g., one-time versus recurring) and form (e.g., verbal versus written) and take place in different institutional contexts, making it difficult to compare them.

Prior work in behavioral economics provides theoretical arguments in favor of oaths and documents improvements in individuals' behavior after oath-taking using experiments examining hypothetical scenarios primarily outside of

[3] Eijkelenboom (2016) describes that part of the problem in the discussion about the oath in The Netherlands was that proponents and critics of the oath had little research they could rely on to substantiate their arguments.

capital markets (e.g., Bénabou and Tirole 2011; Brandts and Charness 2003; Jacquemet et al. 2019; Wang and Murnighan 2017). This literature describes that an oath can change behavior by creating a tighter link between words and actions and providing a reminder about desirable behavior (e.g., Ariely 2012; Kartik 2009). For example, Ariely (2012) argues that moral-code reminders limit individuals' ability to cheat and still perceive themselves as honest, honorable people. For example, Lyon and Dorado (2008) show in an experiment that six- and seven-year-old children are more likely to report maltreatment after being asked to promise to tell the truth. Similarly, healthy food and nonsmoking pledges appear to affect behavior (Hallaq 1976; Raju, Rajagopal, and Gilbride 2010).

A few studies have also provided arguments in favor of integrity oaths for managers (e.g., Messick and Bazerman 1996; Khurana and Nohria 2008). However, the empirical evidence on the efficacy of oaths on executives' on-the-job behavior is scant. Most closely related are a few studies in accounting that examine the stock-market response to the 2002 SEC order requiring that CEOs and CFOs of certain publicly listed companies state under oath that the firm's financial reports are materially accurate, pursuant to the Securities Exchange Act of 1934. This order was a response to the accounting scandals of the early 2000s and part of a broader debate on the usefulness of oaths. If the oath-taking improves firms' financial reporting, investors should recognize this, triggering stock-market responses. However, the results from these studies are mixed. Some studies document a stock-market response around the oath-taking by executives (e.g., Chang et al. 2006; Griffin and Lont 2005), whereas others do not find that the oath-taking resulted in a stock-market response (Bhattacharya et al. 2007). One explanation for these mixed findings is that it is empirically difficult to separate the effect of the oath from other provisions of the Sarbanes-Oxley Act that coincided with the oath and affected reporting and stock-market responses.

Besides empirical challenges, prior literature also emphasizes that the effect of oaths varies with the specific content of the oath, the salience of the oath-taking, and the availability of sanctions in cases of oath violations (e.g., de Bruin 2016). The heterogeneity in the expected effect of integrity oaths further underscores the importance of considering each setting's specific features and calls for further research to understand such effects in capital markets.

Institutional Setting

Accounting professionals in The Netherlands must register with the Royal Netherlands Institute of Chartered Accountants, or the NBA.[4] The NBA is a public body and refers to the professional organization for all accounting professionals in The Netherlands. The NBA's members include over 21,000 professionals working at public accountancy practices, government agencies, as internal auditors, or in organizational management (Koninklijke Nederlandse Beroepsorganisatie van Accountants (NBA) 2020a).

In May 2014, in response to requests by members of the Dutch House of Representatives and the Finance Ministry, the NBA announced the formation of a working group to develop proposals to improve the accounting profession in The Netherlands (e.g., Brouwer 2015; Couwenbergh and Piersma 2014). These requests were a response to the belief that the audit profession in The Netherlands needed to be reformed. For example, reports of the Autoriteit Financiële Markten (AFM) (2010, 2014)—i.e., the Dutch capital-market regulator comparable to the SEC in the United States—had found shortcomings in the quality of a large percentage of the audits carried out by the Big 4 audit firms.[5] According to the AFM (2014), these shortcomings ultimately increased the risk of reporting errors, earnings management, and accounting fraud, reducing the quality of financial reports. In addition, several scandals at Dutch audit firms and a broader push within the European Union to improve audit quality also triggered the NBA to set up a working group (Brouwer 2015).

These events were also extensively covered by the Dutch press (e.g., Couwenbergh and Piersma 2014; de Jong 2016; Oudshoorn 2016; Piersma 2016). The working group submitted its proposals in September 2014, listing the integrity oath as the first proposal.[6] After consultation and public debate, on May 17, 2016, the NBA passed the regulation

[4] The Dutch name of the NBA is Koninklijke Nederlandse Beroepsorganisatie van Accountants, explaining the abbreviation NBA.

[5] In its 2014 report, the AFM stated that 45 percent of reviewed audits were inadequate. The shortcomings involved insufficient documentation of audits, insufficient collection of audit evidence, insufficient audits of internal controls, insufficient examination of accounting fraud red flags, and insufficient skepticism toward management's reporting choices. AFM reports issued in more recent years remained concerned about audit quality (Autoriteit Financiële Markten (AFM) 2017, 2019). Figure 1, which we discuss in more detail in the "Time Trends" section, does not show substantial improvement in the reporting quality of control firms after the passage of the integrity oath, consistent with the concerns expressed in the AFM reports.

[6] The Dutch professional accounting associations (i.e., the predecessors of the NBA) had long considered requiring an integrity oath for Dutch accountants. For example, shortly after its founding in 1895, the first professional association for Dutch accountants proposed an integrity oath for their members. At various other points throughout the 20th century, similar proposals were made. However, none of these proposals ever became a requirement (see Eijkelenboom 2016 for a detailed review of the oath's history). The NBA working group developed additional proposals that were aimed at audit firms and auditors, such as more extensive reporting on the risks of fraud in audit reports or the establishment of a supervisory board to oversee audit firms. Conversations with the NBA confirmed that only the oath directly affected executives with an active accounting degree (working for non-audit firms). In addition, our difference-in-differences design controls for any potential effects arising from these other changes, as they affected the whole audit profession and, hence, the audits of the control sample. See https://www.nba.nl/globalassets/projecten/in-het-publiek-belang/in-the-public-interest-measures-to-improve-quality.pdf for a summary of these proposals.

requiring that all active holders of professional accounting degrees, specifically Chartered Accountants ("Registeraccountant" in Dutch) or Accountant-Administration Consultants ("Accountant-Administratieconsulent" in Dutch), take a professional integrity oath (Koninklijke Nederlandse Beroepsorganisatie van Accountants (NBA) 2016).

The oath aimed to strengthen integrity among holders of professional accounting degrees to improve firms' reporting quality. According to the NBA (2014), the main problem at that time was that professional accountants often did not act with integrity, which reduced the quality of financial reports and undermined the confidence that shareholders and society had in those reports. The NBA (2014) considered that an integrity oath would remind these professionals of the profession's code of ethics, the relevant laws, and their responsibility to ensure that the financial statements provide a true and fair view of the business.[7] The NBA (2014) also clarified that improving the quality of financial reports is the responsibility of all professional accountants (and not only those working as auditors). The regulation became effective on June 1, 2016, and all registered active accountants had to take the oath before May 1, 2017 (NBA 2016). New holders would pledge the oath during their graduation ceremony, and current active holders would pledge the oath in separately arranged meetings, typically in a larger group of people. The specific oath is (Koninklijke Nederlandse Beroepsorganisatie van Accountants (NBA) 2020b):

> I am aware that as a professional accountant I am bound to act in the public interest. I will exercise my profession with an attitude of professional skepticism. When exercising my profession as a professional accountant I am guided by fundamental principles of integrity, objectivity, professional competence, and due care and confidentiality. I will comply with the laws and regulations applicable to my profession. My professionalism implies that I will not execute any acts of which I know or ought to know that these could bring the accountancy profession into disrepute. So help me God/I promise/I declare.

The fundamental principles referred to in the oath are described in the Regulation of Conduct and Professional Rules for Accountants, or VGBA (Koninklijke Nederlandse Beroepsorganisatie van Accountants (NBA) 2013). This regulation describes the Code of Ethics for holders of professional accounting degrees and has been in effect since January 1, 2006 (the code was most recently amended on June 14, 2010). Thus, the rule did not amend or introduce new ethics regulations or other provisions that could affect these professionals.

If the holder of a professional accounting degree refuses to take the oath, the NBA can cancel the degree holder's professional registration, prohibiting that individual from conducting business as a professional accounting-degree holder (Koninklijke Nederlandse Beroepsorganisatie van Accountants (NBA) 2019). Losing the license can be costly for professionals across all ranks, as active holders of the degree typically enjoy larger salaries. In addition, it can also result in reputational costs. For example, a CFO might want to avoid explaining to the company's board that they lost their license because they declined to take an integrity oath. Consistent with the conjecture that losing the license is costly, 98 percent of active degree holders took the oath within one year of its introduction (The Accountant 2017). Although we are not aware of executives in our sample who refused to take the oath, a few individuals refused to take the oath and lost their licenses as a result. For example, the NBA canceled the licenses of eight professionals who refused to take the oath (Koninklijke Nederlandse Beroepsorganisatie van Accountants (NBA) 2018). To keep their license active, degree holders must fulfill annual "permanent-education" requirements, but they do not need to declare their ethical compliance when renewing their license.[8]

Similar to other countries, executives in The Netherlands can be subject to severe penalties for engaging in accounting fraud. According to Section 336 of the Dutch Criminal Code, it is a crime for an executive to intentionally publish false financial statements or intentionally allow the publication of false financial statements. This crime can be punished with imprisonment of up to six years.

Hypothesis Development

Integrity oaths could improve firms' financial reporting quality for at least three reasons. First, oaths can serve as a reminder of the code of ethics and laws within a profession and introduce cues that make people's own moral standards salient (e.g., Ariely 2012; Ayal et al. 2015; Khurana and Nohria 2008). Evidence from the psychology literature shows that people take advantage of gray areas to justify their dishonest behavior (e.g., Shalvi et al. 2015). To explain such behavior, Mazar et al. (2008) develop and test a theory of self-concept maintenance, whereby individuals receive utility

[7] For example, the NBA emphasized the critical role of professional accountants in "the detection of and fight against fraud," and noted that "[d]oing nothing with regard to fraud is not in line with society's expectations. Therefore, [professional accountants]…should pay more time and resources on fraud" (NBA 2014, 60).

[8] See https://www.nba.nl/kwaliteitsbevordering/

from thinking of themselves as honest. Mazar et al. (2008, 635) show that inattention to ethical and legal standards allows people to obtain the rewards of dishonesty, while deluding themselves of their own integrity. Their findings indicate that "[it] is not whether people know that it is wrong to behave dishonestly but whether they think of these standards and compare their behavior with them in the moment of temptation."

Second, an integrity oath can change executives' understanding of the norms and rules within a profession (Cialdini and Trost 1998). In particular, oaths can shape beliefs about acceptable conduct, specifying behaviors that people within a profession approve or disapprove of. According to Khurana and Nohria (2008), oaths create an implicit social contract among the members of a profession, shaping the social capital of a profession in the eyes of society and facilitating peer monitoring. Relatedly, studies using brain imaging showed that individuals experience disutility when their behavior does not comply with the norms and values of their group (e.g., de Quervain et al. 2004; Rilling et al. 2002).

Third, an oath can act as a commitment device for members of a profession (Jacquemet et al. 2019). Jacquemet et al. (2019) argue that a central purpose of an oath is to get a person to *commit* to comply with certain norms and standards. Such commitment can establish a direct link between individuals' concrete offenses and their general perceptions of their morality (Ayal et al. 2015). In the context of our setting, Moling and Ham (2015), who are chairman and director of the Dutch Association of Accountants and Accountancy firms, argue that the Dutch accountant's oath can "oblige individual professionals to promise out loud that they will behave properly…[t]owards colleagues, customers, bosses, colleagues and other stakeholders."

Consistent with these arguments, Hugo Hollander (2016) described the impact the oath-taking had on him as follows: "And when you say that [oath], you understand what being an accountant is about. It makes you aware of the value of your profession…For me it means: once you have taken the oath, you as an organization and as an accountant actually have to ask yourself what that means for you. And how you can give substance to the oath yourself. It gives the profession a perspective for action…So practice what you preach."

In our setting, the oath requires professional accountants to pledge that their work will be guided by integrity and due care. This pledge could drive CEOs and CFOs, who determine their firms' reporting choices (e.g., Feng et al. 2011; Wells 2020), to improve their firms' financial reporting quality.[9] Under this view, we would expect that firms whose CEO or CFO takes the oath would show less aggressive reporting choices.

In contrast, oaths may be ineffective for several reasons. First, an oath does not introduce new ethical requirements, but is simply a reminder of what is already required by a profession's code of conduct, laws, and regulations. For example, accountants are already required to comply with principles of integrity, objectivity, professional due care and competency, and confidentiality, as described in the profession's code of conduct. Second, the oath does not introduce new legal requirements that increase the punishment or detection rate associated with misbehavior. Consequently, under Becker's (1968) framework, executives' cost-benefit analysis related to misconduct should remain unchanged.

Third, prior literature shows that executives' behavior is driven by personal characteristics (e.g., Davidson et al. 2015; Malmendier and Tate 2005, 2015) and that fraudulent executives often rationalize or fail to acknowledge their behavior, so as to deny any ethical lapse (Bazerman and Tenbrunsel 2011; Messick and Bazerman 1996). In fact, prior studies show that professions that require oaths are still rife with misconduct. For example, despite physicians pledging to act in their patients' best interests, thousands of physicians in the United States are convicted each year for mistreating patients (e.g., Jung, Lurie, and Wolfe 2006).

Finally, the oath is general and, therefore, does not provide a specific guide through the many difficult decisions that accountants make, potentially rendering it ineffective. Under this alternative view, we would not expect to find a change in reporting choices following the pledge of a CEO or CFO integrity oath. In sum, whether executives' integrity oaths improve firms' financial reporting is an empirical question.

III. DATA AND EMPIRICAL METHODOLOGY

Data

We obtain information on executives of Dutch private and public firms from Bureau van Dijk (BvD) (see Beuselinck, Elfers, Gassen, and Pierk 2023 for a comprehensive overview of studies using BvD). Using these data, we

[9] Feng et al. (2011), for example, emphasize that CFOs have a critical role in shaping their firm's reporting quality because they typically oversee the process of preparing the financial statements, serve as a watchdog for the firm's financial reporting, and are in a unique position to carry out accounting manipulations.

first identify individuals with Dutch titles for the executive position of CEO or CFO and the individuals' respective employers.[10] We find 7,579 CEOs and CFOs working for 7,373 companies that were appointed before 2016.

As described above, the NBA maintains a public register of all NBA members. We obtain data on all NBA members from this register, available at https://www.nba.nl/. The register includes NBA members' first name(s) initials and last name, their specific degree, the status of their degree (active versus inactive), the date they obtained their degree, and professional violations by the NBA member (including the size of the penalty). We begin with a total of 27,893 NBA members with initial registration years ranging from 1967 to 2020. We then identify whether each executive is an active accounting-degree holder by matching executives to registered accountants based on their last name and first and middle initials. We manually check all matched executives to ensure that they are indeed holders of professional accounting degrees and minimize false matches. We identify 40 CEOs and 84 CFOs, working for 120 firms, who are active holders of professional accounting degrees. We restrict the sample to CEOs and CFOs who (1) serve at the *same* firm and (2) are *active* accounting degree holders for the entirety of the sample period. Table 1, Panel A describes our sample composition. Our final sample consists of 948 executives working for 837 firms. Out of these 837 firms, 47 are public, and 790 are private firms.

Panel B shows the number of firm-years with treated CEOs and treated CFOs by industry. Treated executives—i.e., executives required to take the oath—belong to multiple industries. "Administrative, Professional, and Management Services," "Wholesale Trade, Retail Trade, and Transportation," and "Manufacturing" are the most common industries in our sample, representing 44.6 percent, 24.7 percent, and 15.6 percent of all observations in our sample. In relative terms (considering treated and control observations), firm-years with treated CEOs and CFOs represent the highest fraction of observations per industry in "Administrative, Professional, and Management Services," with 6.9 percent and 14.0 percent, respectively.

Research Design

We examine the effect of executives' pledges of integrity on firms' financial reporting using a difference-in-differences methodology.[11] The basic regression we estimate is as follows:

$$Earnings\ Management_{i,t} = \beta_1 Treatment_{i,t} + Controls_{i,t} + \gamma_i + \delta_t + \epsilon_{i,t}, \tag{1}$$

where the dependent variable *Earnings Management* is either discretionary accruals or real-activities earnings management. $Discretionary\ Accruals_{i,t}$ is the residual for firm i in year t from the following modified Jones (1991) discretionary accruals model as per Dechow et al. (1995) estimated for each industry-year (specifically at the two-digit NAICS level) with more than ten two-digit NAICS industry-year observations:

$$Total\ Accruals_{i,t} = \alpha_0 + \alpha_1 \frac{1}{AT_{i,t-1}} + \alpha_2 \frac{\Delta REV_{i,t} - \Delta REC_{i,t}}{AT_{i,t-1}} + \alpha_3 \frac{PPE_{i,t}}{AT_{i,t-1}} + \epsilon_{i,t}. \tag{2}$$

$Total\ Accruals_{i,t}$ for firm i in year t are calculated as $((\Delta Current\ Assets - \Delta Cash) - (\Delta\ Current\ Liabilities - \Delta Loans))$. ΔREV is the change in sales from year t+1 to year t; ΔREC is the change in accounts receivable from year t+1 to year t; $PPE_{i,t}$ is gross property, plant, and equipment. We scale $(\Delta REV - \Delta REC)$ and $PPE_{i,t}$ variables by lagged total assets (AT). We use signed discretionary accruals to capture income-increasing reporting choices.[12]

To measure real-activities earnings management, we either use abnormal production costs (*REM Prod*), abnormal discretionary expenses (*REM Disx*), or a summary measure of both (*REM*). We follow Roychowdhury (2006) and estimate abnormal production costs as deviations from the predicted values from the following industry-year regression, which is run at the two-digit NAICS level with more than ten firm-year observations:

[10] We define individuals with the following title as CEO: "Algemeen Directeur/Directrice," "Algemeen Direkteur/Direktrice," "General Director," "Directievoorzitter," "Voorzitter Raad van Bestuur," and "Chief Executive Officer." We define individuals with the following title as CFO: "Financieel Directeur/Directrice," "Financieel Direkteur/Direktrice," "Financial Director," and "Chief Financial Officer."

[11] A concern with our analysis is the potential violation of the perfect-compliance assumption, whereby individuals in either the treatment or control group select into treatment (Armstrong and Kepler 2018). As we discussed in the previous section, we are not aware of executives that refused to take the oath. Similarly, we are not aware of executives without an active accounting degree that voluntarily pledged the oath. Thus, it is unlikely that the perfect-compliance assumption is violated in our setting. However, we caution the reader to attribute our results to the 2016 Dutch regulation requiring an integrity oath (instead of the oath-taking action *per se*) (Armstrong and Kepler 2018).

[12] We also conduct tests using unsigned discretionary accruals and do not find a significant effect of oaths (untabulated). These tests indicate that although the oath reduces income-increasing accruals, it does not affect the overall level of discretionary accruals. Prior studies (e.g., Badertscher, Phillips, Pincus, and Rego 2009) show that the majority of restatements and accounting frauds relate to income-increasing reporting choices.

TABLE 1

Sample

Panel A: Sample Composition

	Number of Executives (1)	Number of Firms (2)	Firm-Years (3)
Current CEOs and CFOs with an appointment date before 2016	7,579	7,373	—
Less: missing observations to calculate test variables	6,093	6,025	—
Less: restrict to 2013–2019	17	16	806
Less: no observation after oath (2017)	310	298	763
Less: non-limited liability companies	147	138	570
Less: singletons	40	39	39
Less: financial industries	24	20	110
Final sample	**948**	**837**	**4,559**

This panel presents the sample composition for the period 2013–2019.

Panel B: Sample Composition by Industry

NAICS Code	Industry Description	Firm-Years with Treated CEO	% of Total	Firm-Years with Treated CFO	% of Total	Total	% of Total Firm-Years
11–29	Agriculture, Mining, Utilities, and Construction	18	3.7	44	9.1	485	10.6
31–33	Manufacturing	26	3.7	48	6.8	711	15.6
42–49	Wholesale Trade, Retail Trade, and Transportation	26	2.3	100	8.9	1,125	24.7
51–56	Administrative, Professional, and Management Services	140	6.9	284	14.0	2,035	44.6
61–62	Educational and Health Services	6	6.7	7	7.8	90	2.0
71–72	Accommodation, Entertainment, and Food Services	8	7.1	5	4.4	113	2.5
Total Firm-Years		224	4.9	488	10.7	4,559	100

This panel presents the distribution of treated and control firm-years in our sample for the period 2013–2019 by industry. Treated firm-years are firm-years of companies run by an executive required to take the oath.

$$\frac{Prod_{i,t}}{AT_{i,t-1}} = \alpha_0 + \alpha_1 \frac{1}{AT_{i,t-1}} + \alpha_2 \frac{Rev_{i,t}}{AT_{i,t-1}} + \alpha_3 \frac{\Delta Rev_{i,t}}{AT_{i,t-1}} + \alpha_4 \frac{\Delta Rev_{i,t-1}}{AT_{i,t-1}} + \varepsilon_{i,t}, \quad (3)$$

where *Prod* for firm *i* in year *t* is calculated as the sum of costs of goods sold (Cost) and the change in inventory (Stok), i.e., (Cost + (Stok − Stok_{t-1}))/AT_{t-1}. We measure abnormal discretionary expenses as deviations from the predicted values from the following industry-year regression:

$$\frac{Dis\ Exp_{i,t}}{AT_{i,t-1}} = \alpha_0 + \alpha_1 \frac{1}{AT_{i,t-1}} + \alpha_2 \frac{Rev_{i,t}}{AT_{i,t-1}} + \varepsilon_{i,t}, \quad (4)$$

where *Dis Exp* is calculated as the difference between gross profit (Gros) and operating profit (Oppl), i.e., (Gros − Oppl)/AT_{t-1}).[13] We further multiply *Dis Exp* by −1 so that higher amounts reflect a higher level of the firm cutting discretionary expenses. Consequently, both residual values of *Dis Exp* and *Prod* are positively correlated to earnings. Finally, we combine both measures into a composite measure of real-activities earnings management (e.g., Cohen and Zarowin 2010; Zang 2012) computed as:

$$REM = REM\ Prod + REM\ Disx. \tag{5}$$

Note that the sample in the tests using real-activities earnings management is smaller than the sample in the tests using discretionary accruals because data for the expense items are often unavailable. Similarly, the sample is smaller for abnormal production costs than abnormal discretionary expenses because data on production costs are missing more often.

The main explanatory variable, $Treatment_{i,t}$, takes the value of 1 for firm *i* if either the firm's CEO or CFO were required to take an oath in the three years following the passage of the Dutch regulation in 2016 that required holders of professional accounting degrees to pledge an integrity oath, and 0 in the three years before the passage of the regulation (the year of the regulation change, 2016, is also set to 0).[14] We focus on a six-year window around the passage of the regulation (i.e., three years prior and three years after the passage of the regulation) because 2019 is the most recent year for which we can obtain firms' financial data. The coefficient on $Treatment_{i,t}$ captures the effect of the executives' pledges of integrity on firms' financial reporting.

This identification strategy allows us to employ a difference-in-differences methodology exploiting the passage of the law in 2016. The first difference is the change in earnings management before and after the law. The second difference is the change in earnings management in firms run by executives required to take the oath compared with firms run by executives not required to take the oath. The effect of executives' pledges of integrity on firms' financial reporting is estimated as the difference between those two differences.

We include firm fixed effects to control for time-invariant firm characteristics and year fixed effects to control for general time trends. These fixed effects do not control for potential changes in governance characteristics and auditor type during our sample period. As we cannot directly control for these factors due to data limitations, it is important to interpret our results in light of this limitation. Our sample only includes firms whose CEOs and CFOs have served in those roles throughout the entire sample period. This restriction mitigates the concern that our results are driven by changes in executives. Standard errors are clustered at the firm level, which accounts for time-varying correlations within firms.

Similar to Zang (2012), our tests include *Controls* for *Market Share* at the beginning of the year (measured as the ratio of a company's sales to the total sales of its three-digit-NAICS industry in each year), net operating assets at the beginning of the year (*NOA*; measured as shareholders' equity less cash and marketable securities plus total debt at the beginning of the year divided by sales at the beginning of the year), *Size* (measured as the natural logarithm of total assets), *ROA* (measured as net income scaled by total assets), and *Leverage* (measured as the ratio of total debt to total assets).[15] Variables are defined in Appendix A.

Table 2, Panel A provides descriptive statistics. On average, 6 percent of the firm-years include firms run by CEOs or CFOs that are registered accountants and, hence, were required to take the oath. Specifically, 2 percent of the firm-years correspond to CEOs and 4 percent of the firm-years to CFOs (in a few instances, both the CEO and the CFO are registered accountants, explaining why the number of treated firms is smaller than the number of treated CEOs and CFOs). Perhaps not surprisingly, CFOs are more likely to be registered accountants than CEOs. The average firm in our sample has total assets of €730 million, a market share of 3 percent, NOA of 43 percent, ROA of 7 percent, and leverage of 61 percent.

[13] "Cost," "Stok," "Gros," and "Oppl" refer to the labels used by BvD. Whereas Roychowdhury (2006) calculates *Dis Exp* as the sum of R&D, advertising, and SG&A expenses, we calculate *Dis Exp* as the difference between gross and operating profit due to the limited granularity of profit and loss items in the BvD database.

[14] The findings are robust to excluding 2016, i.e., the year of the policy change (untabulated).

[15] Our control variables differ from Zang (2012) as follows. First, Zang (2012) includes control variables that require market data, which we cannot include, as our sample also includes private firms. Similarly, we cannot control for audit-firm tenure, as such data are not available in Amadeus. We also do not control for cash cycle, as the data to calculate this variable are often missing in our dataset. However, our results hold when controlling for cash cycle (untabulated). Third, time-invariant firm characteristics are subsumed by our firm fixed effects. Zang (2012) does not include firm fixed effects. Although our primary research design does not include unexpected and predicted levels of real-activities manipulation in the discretionary accruals model as per Zang (2012), our results hold when including these variables (untabulated).

TABLE 2

Summary Statistics

Panel A: Summary Statistics for Sample

	Firm-Years (n = 4,559)							
	Mean	**Std. Dev. (Pooled)**	**Std. Dev. (within Firm)**	**P10**	**P25**	**Median**	**P75**	**P90**
Treatment	0.06	0.24	0.00	0.00	0.00	0.00	0.00	1.00
Treatment_CEO	0.02	0.14	0.00	0.00	0.00	0.00	0.00	0.00
Treatment_CFO	0.04	0.20	0.00	0.00	0.00	0.00	0.00	1.00
Discretionary Accruals	0.00	0.13	0.12	−0.13	−0.05	0.00	0.06	0.13
M-Score	−2.54	0.99	0.87	−3.51	−3.04	−2.62	−2.14	−1.42
REM Prod (n = 1,868)	−0.03	0.43	0.15	−0.54	−0.20	0.04	0.23	0.39
REM Disx (n = 2,069)	−0.02	0.44	0.14	−0.51	−0.17	0.07	0.24	0.39
REM (n = 1,868)	−0.03	0.83	0.25	−0.99	−0.35	0.11	0.46	0.75
Market Share	0.03	0.11	0.02	0.00	0.00	0.00	0.01	0.05
NOA	0.43	1.32	0.91	0.03	0.25	0.45	0.62	0.74
Size	18.38	1.72	0.26	16.68	17.23	17.92	19.24	20.90
Total Assets in € mm	730.00	2,483.15	492.67	17.46	30.37	60.76	226.10	1,189.96
ROA	0.07	0.12	0.07	−0.03	0.02	0.06	0.11	0.19
Leverage	0.61	0.24	0.09	0.30	0.45	0.60	0.75	0.91

This panel reports the summary statistics on an annual basis of the variables used in our analyses. We tabulate pooled-sample as well as within-firm standard deviations of the variables used in our analyses.
All variables are defined in Appendix A.

Panel B: Summary Statistics for Treated versus Control Firms

	Firm-Years (n = 4,559)		
	Treated (n = 688) Mean (1)	**Control (n = 3,871) Mean (2)**	**Difference (1) – (2)**
Discretionary Accruals	0.00	0.00	0.00
M-Score	−2.60	−2.53	0.07*
REM Prod (n = 1,868)	0.01	−0.03	0.04
REM Disx (n = 2,069)	0.05	−0.03	0.08***
REM (n = 1,868)	0.06	−0.05	0.11**
Market Share	0.02	0.03	−0.01***
NOA	0.39	0.44	−0.05
Size	18.31	18.39	−0.08
ROA	0.08	0.06	0.02***
Leverage	0.62	0.61	0.01

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This panel reports the means of the variables used in our analyses separately for treated and control firms. It also displays the differences between the means of the variables.
All variables are defined in Appendix A.

Table 2, Panel B reports the means of the variables used in our analyses separately for treated and control firms. It also displays the differences between the means of the variables. As shown, treated and control firms are generally similar. For example, there are no significant differences in firm size, discretionary accruals, leverage, and net operating assets. Even for variables with statistically significant differences (e.g., *Market Share*), the economic magnitudes seem modest. That being said, we conduct specific tests discussed in Section V that address any remaining concerns about differences between treatment and control firms.

TABLE 3

The Accountants' Oath and Discretionary Accruals

Dependent Variable	*Discretionary Accruals*	
Treatment	CEO	CFO
Variables	(1)	(2)
Treatment	0.007	−0.031***
	(0.013)	(0.010)
Market Share	−0.001	−0.002
	(0.094)	(0.094)
NOA	−0.008	−0.008
	(0.006)	(0.006)
Size	0.042***	0.042***
	(0.011)	(0.011)
ROA	0.243***	0.246***
	(0.042)	(0.042)
Leverage	−0.192***	−0.195***
	(0.028)	(0.028)
Firm FE	Yes	Yes
Year FE	Yes	Yes
Adjusted R^2	0.02	0.02
Observations	4,559	4,559

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This table reports the estimation results from linear regressions of the following form:

$$Discretionary\ Accruals_{i,t} = \beta_0 + \beta_1 Treatment_{i,t} + \phi Controls_{i,t} + \varepsilon.$$

The models presented differ in the *Treatment* variable. The dependent variable is signed discretionary accruals obtained from the modified Jones (1991) model. In column (1), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant. In column (2), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

IV. RESULTS

Accruals-Based Earnings Management

We examine the effect of executives' pledges of integrity on firms' income-increasing discretionary accruals using Equation (1). If pledging the oath improves financial reporting, we expect that discretionary accruals decrease after an executive pledges the integrity oath.

Table 3 reports the results. Column (1) ((2)) reports the results using CEOs (CFOs) that took the integrity oath as *Treatment*. We find a negative and significant coefficient on *Treatment* in column (2) ($p < 0.01$). Firms managed by an oath-taking CFO reduce income-increasing discretionary accruals by 0.031 (Table 3, column (2)). This result indicates that the integrity oath decreases discretionary accruals by approximately 0.26 standard deviations.[16] However, the coefficient on *Treatment* is insignificant in column (1), indicating that when the CEO is required to take the oath, there is no effect on discretionary accruals.

Overall, the results from these analyses indicate that firms reduce their use of income-increasing discretionary accruals after their CFO takes an oath. These results are consistent with the view that integrity oaths can improve firms' financial reporting outcomes.

[16] We follow deHaan (2021) and use within-firm standard deviations to determine the economic magnitude in fixed-effects models.

TABLE 4

The Accountants' Oath and Real-Activities Earnings Management

Dependent Variable	*REM Prod*		*REM Disx*		*REM*	
Treatment	CEO	CFO	CEO	CFO	CEO	CFO
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	−0.009	−0.063**	−0.014	−0.070**	−0.012	−0.139***
	(0.051)	(0.030)	(0.039)	(0.031)	(0.087)	(0.053)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.84	0.84	0.87	0.87	0.88	0.88
Observations	1,868	1,868	2,069	2,069	1,868	1,868

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This table reports the estimation results from linear regressions of the following form:

$$REM\ Prod/REM\ Disx/REM\ = \beta_0 + \beta_1 Treatment_{i,t} + \phi Controls_{i,t} + \varepsilon.$$

The models presented differ in the dependent variables and *Treatment* variable. In columns (1) and (2), the dependent variable, *REM Prod*, is signed discretionary production costs obtained from the Roychowdhury (2006) model. In columns (3) and (4), the dependent variable, *REM Disx*, is signed discretionary expenses obtained from the Roychowdhury (2006) model. In columns (5) and (6), the dependent variable is the sum of *REM Prod* and *REM Disx*. In columns (1), (3), and (5), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant. In columns (2), (4), and (6), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

Real-Activities Earnings Management

Next, we examine whether integrity oaths also reduce firms' real-activities earnings management using Equation (1). These tests aim to distinguish whether integrity oaths simply result in a substitution from accruals-based earnings management to real-activities earnings management or whether they reduce earnings management activities altogether.

Table 4 reports the results for *REM Prod* (columns (1) and (2)), *REM Disx* (columns (3) and (4)), and *REM* (columns (5) and (6)). Columns (1), (3), and (5) ((2), (4), and (6)) report the results using CEOs (CFOs) that took the integrity oath as *Treatment*. We find a negative and significant coefficient on *Treatment* across all three real-activities earnings management measures when the CFO takes the oath. Treated firms reduce abnormal production costs by 0.063 (Table 4, column (2)) and discretionary expenses by 0.070 (Table 4, column (4)), representing a reduction in abnormal production costs (discretionary expenses) of approximately 0.42 (0.50) standard deviations.

These results indicate that firms engage in less real-activities earnings management after the passage of the oath-taking requirement. Similar to the tests in Table 3, the effects are concentrated in firms whose CFO took the oath. Overall, the evidence shows that integrity oaths affect both accruals-based and real-activities earnings management.

M-Score

Next, we examine whether integrity oaths also reduce egregious reporting practices, which was one of the NBA's goals. To capture such practices, we use the M-Score developed by Beneish (1999). The M-Score is a comprehensive measure to identify potential misstatements and commonly used in academic research (Beneish 1999; Beneish and Vorst 2022). We examine the effect of the oath on a firm's M-Score using Equation (1).[17]

Table 5 reports the results. Column (1) ((2)) reports the results using CEOs (CFOs) that took the integrity oath as *Treatment*. We find a negative and significant coefficient on *Treatment* when CFOs take the oath. These firms reduce their M-Score by 0.139 points, which represents a decrease of 0.16 standard deviations (based on Table 5, column (2)).

[17] Beneish and Vorst (2022) show that the M-Score is one of the few fraud-prediction models that provide a net benefit after considering the costs of false positives. Beneish, Lee, and Nichols (2013) also point out that the M-Score is useful in understanding a firm's earnings quality. We provide a detailed definition of the M-Score in Appendix A.

TABLE 5

The Accountants' Oath and M-Score

Dependent Variable	*M-Score*	
Treatment Variables	CEO (1)	CFO (2)
Treatment	0.185	−0.139**
	(0.121)	(0.070)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
Adjusted R^2	0.10	0.10
Observations	4,559	4,559

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This table reports the estimation results from linear regressions of the following form:

$$M\text{-}Score = \beta_0 + \beta_1 Treatment_{i,t} + \phi Controls_{i,t} + \varepsilon.$$

The models presented differ in the *Treatment* variable. The dependent variable, *M-Score*, is the M-Score according to Beneish (1999). In column (1), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant. In column (2), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

These results suggest that firms also reduce more egregious reporting choices after their CFOs take the oath. Again, the effects are concentrated in firms whose CFO took the oath.

V. ENHANCING IDENTIFICATION AND ADDITIONAL TESTS

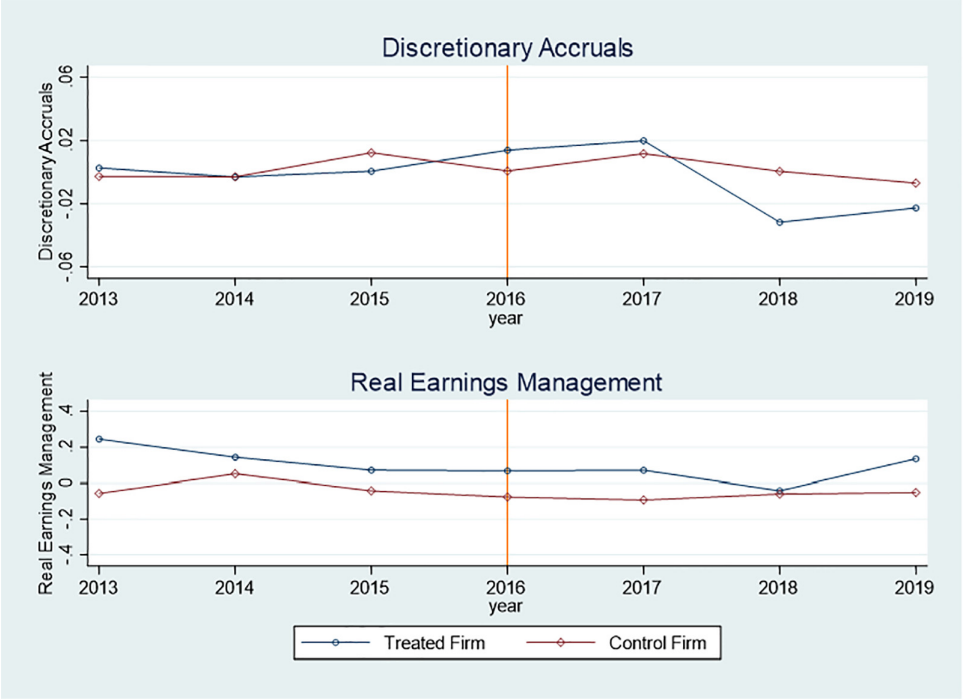
In this section, we describe a number of tests to address potential concerns that our findings might be driven by unobservable correlated omitted variables or concurrent events or are sensitive to our research-design choices.

Time Trends

We examine time trends in treatment versus control firms around the year of the oath-taking requirement (2016). Figure 1 plots the mean values of discretionary accruals and real earnings management separately for treatment (blue) and control (red) firms in the six-year window around the oath-taking requirement (i.e., from 2013 to 2019). Treatment firms are those whose CFO is an active registered accountant. We use the full sample for this figure. The figure suggests that there were no pre-trends before the oath-taking requirement. In addition, the effects seem to be driven by the treated firms instead of changes in the control firms.

Figure 1 also suggests that the effect of the integrity oath on discretionary accruals appears with a delay and persists for at least three years. A possible explanation for this pattern is that the oath leads to structural changes (such as the introduction of better accounting systems) that improve financial reporting quality, but take time to implement. Moreover, structural changes might lead to effects that persist for several years, even if the CFO is not actively making changes years after taking the oath. The oath might also trigger changes in reporting quality through indirect channels. For example, employees' trust in management may increase in response to the integrity oath, potentially contributing to better financial reporting outcomes in the long run (Garrett, Hoitash, and Prawitt 2014). In contrast, the effect of the oath on real earnings management is most pronounced two years after taking the oath, after which the effect appears to fade. It is important to note that both graphs present average values that are not adjusted for firm-specific characteristics. As a result, we also plot the coefficients on *Treatment_CFO* for each year using Equation (2) for both discretionary accruals and real earnings management in Figure 2. Consistent with Figure 1, Figure 2 suggests that there are no

FIGURE 1
Time Trends for Discretionary Accruals and Real Earnings Management



This graph plots the mean values of *Discretionary Accruals* and *REM* separately for treatment (blue) and control (red) firms around the oath-taking requirement (the vertical orange line marks the year of the oath requirement). Treatment firms are those whose CFO is an active registered accountant. We use the full sample for this figure.
(The full-color version is available online.)

pre-trends and that the effect of the oath persists for at least three years for discretionary accruals, whereas it appears to fade in later years for real earnings management.

Falsification Tests

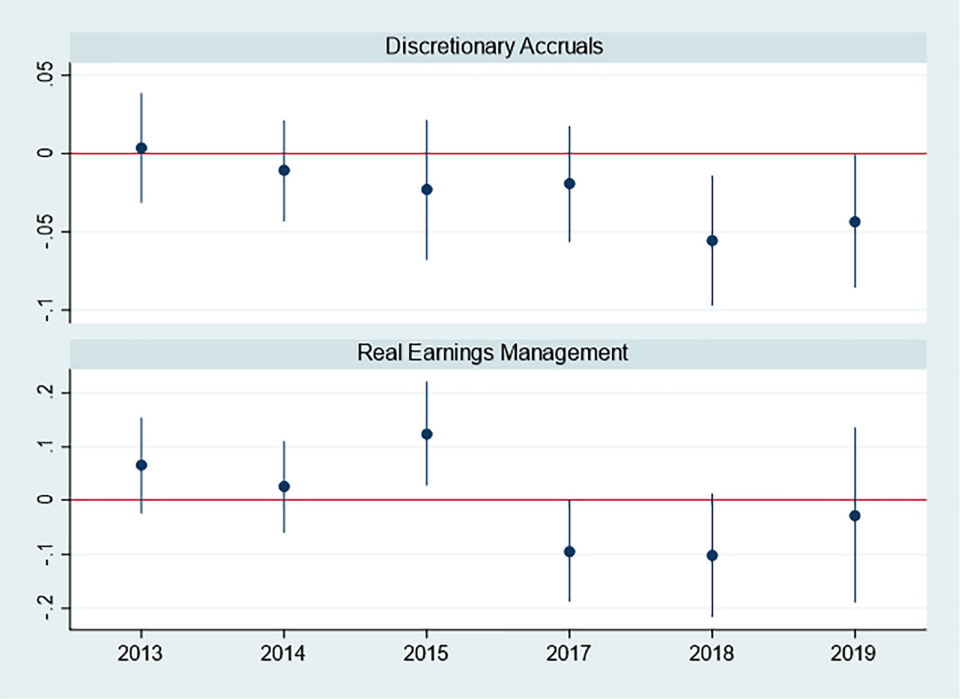
We run a falsification test using placebo treatments in Belgium. We focus on Belgium, as the country shares similarities with The Netherlands along various dimensions, including market size, population, and language. We use BoardEx to identify Belgian firms run by executives (i.e., CEOs or CFOs) with an accounting degree, which serve as our placebo treatment, given that those executives were not subject to an oath requirement in 2016. Belgian firms run by executives without an accounting degree constitute our control group. Our sample for these tests consists of 123 firms, out of which 20, or 16 percent, are run by an executive with an accounting degree, a frequency very similar to that in the Dutch sample. As shown in Table 6, we do not find a change in discretionary accruals or discretionary expenditures in these tests, further alleviating the concern that concurrent events drive our results.[18]

Entropy Balancing

As the descriptives in Table 2, Panel B indicate that treatment and control firms differ across some observable characteristics, we examine the robustness of the results presented in Tables 3 and 4 using entropy balancing. Entropy balancing reweights control sample observations along moments of the control variable distributions (Hainmueller 2012). In contrast to propensity-score matching, entropy balancing retains all observations (rather than discarding

[18] We also randomly assign treated CFOs to placebo firms and examine whether these placebo firms experience reductions in earnings management after the oath-taking (untabulated). We do not find reductions in earnings management, further alleviating the concern that concurrent events drive our results.

FIGURE 2
Treatment Effects for Discretionary Accruals and Real Earnings Management over Time



These graphs plot the coefficients on *Treatment_CFO* × *year*, where *year* takes the values of 2013 to 2019. *Treatment_CFO* takes the value of 1 for firms whose CFO is an active registered accountant. The top plot shows the coefficients (and the 90 percent confidence intervals) using *Discretionary Accruals* as the dependent variable. The bottom plot uses *REM* as the dependent variable. We use the full sample for these figures. Note that year 2016 is excluded from the graph, as it is the reference year.
(The full-color version is available online.)

"unmatched" observations). In addition, entropy balancing does not require research-design choices, such as setting a certain caliber or radius, to achieve covariate balance, alleviating concerns that the results are sensitive to model specification. In these tests, we focus on firms whose CFOs take the oath because the results are concentrated in those firms.

Table 7 presents the results. Panel A presents the first and second moments for the control variables before and after entropy balancing, and Panel B presents the results of estimating our primary tests using the entropy-balanced sample. Consistent with our main results, Table 7, Panel B shows that, after the treatment, treated firms have significantly lower discretionary accruals and lower real-activities manipulation. The economic magnitudes are similar to those reported in Tables 3 and 4.

Alternative Control Group

A related concern is that firms run by executives with an accounting or business degree might either be inherently different from firms run by executives without such degrees or respond differently to the concurrent events. Similarly, executives with or without an accounting degree might be inherently different. To alleviate these concerns, we limit our control firms to a sample of firms run by inactive accounting-degree holders and executives with a business degree, as those individuals (and the firms they run) are likely to be similar to executives with an active accounting degree.[19] We identify three firms run by executives with an inactive accounting degree (i.e., 1 (2) firm(s) run by a CEO (CFO) with an

[19] We acknowledge that it could be possible that an omitted personal attribute that is highly correlated with the possession of an active professional accounting degree (but not with an inactive or business degree) could drive a change in financial reporting quality surrounding the exact year in which the oath was introduced.

TABLE 6

Placebo Tests in Belgium

Dependent Variable	*Discretionary Accruals*		*REM*	
Treatment Variables	CEO (1)	CFO (2)	CEO (3)	CFO (4)
Treatment	−0.117	−0.042	−0.042	−0.079
	(0.123)	(0.082)	(0.067)	(0.085)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.18	0.18	0.72	0.72
Observations	725	725	396	396

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This table examines the robustness of the results presented in Tables 3 and 4 using placebo treatments. In columns (1) and (2), the dependent variable, *Discretionary Accruals*, is signed discretionary accruals obtained from the modified Jones (1991) model. In columns (3) and (4), the dependent variable, *REM*, is the sum of *REM Prod* and *REM Disx*. In columns (1) and (3), *Treatment* takes the value of 1 for firm-years after 2016 for Belgian companies whose CEO has an accounting degree, and 0 for firm-years of Belgian companies whose CEO does not have an accounting degree. In columns (2) and (4), *Treatment* takes the value of 1 for firm-years after 2016 for Belgian companies whose CFO has an accounting degree, and 0 for firm-years of Belgian companies whose CFO does not have an accounting degree. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

TABLE 7

Entropy Balancing

Panel A: Covariate Balance

	Before Reweighting				After Reweighting			
	Treatment		Control		Treatment		Control	
Variables	Mean (1)	Variance (2)	Mean (3)	Variance (4)	Mean (5)	Variance (6)	Mean (7)	Variance (8)
Market Share	0.014	0.002	0.033	0.014	0.014	0.002	0.014	0.002
NOA	0.388	0.066	0.436	1.950	0.388	0.066	0.387	0.067
Size	18.240	2.316	18.400	3.031	18.240	2.316	18.240	2.319
ROA	0.079	0.010	0.065	0.015	0.079	0.010	0.079	0.010
Leverage	0.621	0.034	0.607	0.062	0.621	0.034	0.621	0.034

This panel presents descriptive statistics for the control variables before and after reweighting to achieve covariate balance between treated and control firms.
Variables are defined in Appendix A.

(continued on next page)

inactive accounting degree) and 15 firms run by executives with a business degree using BoardEx (i.e., 11 (4) firms run by a CEO (CFO) with a business degree).[20] As shown in Table 8, we find consistent results.

[20] Due to the small number of executives with an inactive accounting degree in our sample, we also add executives with a business degree to our control group.

TABLE 7 (continued)

Panel B: Main Results Using Entropy Balancing

Dependent Variables	*Discretionary Accruals*	*REM*
Variables	(1)	(2)
Treatment	−0.024**	−0.117**
	(0.010)	(0.052)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
Adjusted R^2	0.12	0.89
Observations	4,557	1,868

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This panel examines the robustness of the results presented in Tables 3 and 4 using an entropy-balanced-matched sample. In column (1), the dependent variable, *Discretionary Accruals*, is signed discretionary accruals obtained from the modified Jones (1991) model. In column (2), the dependent variable, *REM*, is the sum of *REM Prod* and *REM Disx*. *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

TABLE 8

Alternative Control Group

Dependent Variable	*Discretionary Accruals*		*REM*	
Treatment	CEO	CFO	CEO	CFO
Variables	(1)	(2)	(3)	(4)
Treatment	−0.019	−0.061***	0.053	−0.489***
	(0.029)	(0.017)	(0.143)	(0.077)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.13	0.03	0.93	0.89
Observations	291	503	94	213

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This table examines the robustness of the results presented in Tables 3 and 4 using alternative control groups. In columns (1) and (2), the dependent variable, *Discretionary Accruals*, is signed discretionary accruals obtained from the modified Jones (1991) model. In columns (3) and (4), the dependent variable, *REM*, is the sum of *REM Prod* and *REM Disx*. In columns (1) and (3), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant, and 0 for firm-years of companies whose CEO is an inactive registered accountant or has a business background. In columns (2) and (4), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant, and 0 for firm-years of companies whose CFO is an inactive registered accountant or has a business background. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

Concurrent Events

Although the tests presented in the previous two sections help to mitigate concerns about potentially confounding events, we present additional tests to more directly address two specific events that could explain the results presented in Tables 3 and 4. First, the Dutch Corporate Governance Code, which contains corporate governance principles and best practices related to financial reporting for firms listed on Dutch stock exchanges, was amended in 2016. To mitigate the concern that the amended code drives the results presented in Tables 3 and 4, we rerun these tests using only private firms because they are not subject to this code. As shown in Table 9, Panel A, columns (1)–(4), the results hold.

TABLE 9

Additional Tests

Panel A: Concurrent Events

Dependent Variable	*Discretionary Accruals*		*REM*		*Discretionary Accruals*		*REM*		*Discretionary Accruals*		*REM*	
Sample	Private Firms				Firms without Intangible Assets or Extraordinary Income				Small Firms			
Treatment	CEO	CFO	CEO	CFO	CEO	CFO	CEO	CFO	CEO	CFO	CEO	CFO
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	0.007	−0.031***	−0.044	−0.149***	0.090	−0.042**	−0.078	−0.117*	0.003	−0.063***	0.068	−0.050
	(0.014)	(0.011)	(0.091)	(0.053)	(0.061)	(0.020)	(0.068)	(0.071)	(0.019)	(0.016)	(0.074)	(0.073)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.02	0.02	0.89	0.89	0.05	0.05	0.85	0.85	0.40	0.40	0.86	0.86
Observations	4,292	4,292	1,749	1,749	1,371	1,371	573	573	372	372	193	193

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

This panel examines the robustness of the results presented in Tables 3 and 4 using different samples. In columns (1), (2), (5). (6), (9), and (10), the dependent variable, *Discretionary Accruals*, is signed discretionary accruals obtained from the modified Jones (1991) model. In columns (3), (4), (7), (8), (11), and (12), the dependent variable, *REM*, is the sum of *REM Prod* and *REM Disx*. In columns (1)–(4), the sample is limited to private firms. In columns (5)–(8), the sample excludes firms with intangible assets or extraordinary income. In columns (9)–(12), the sample is limited to small firms, i.e., those with fewer than 50 employees. In columns (1), (3), (5), (7), (9), and (11), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant. In columns (2), (4), (6), (8), (10), and (12), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.

All variables are defined in Appendix A.

(continued on next page)

TABLE 9 (continued)

Panel B: Additional Fixed Effects

Dependent Variable	*Discretionary Accruals*		*REM*	
Treatment Variables	**CEO (1)**	**CFO (2)**	**CEO (3)**	**CFO (4)**
Treatment	0.014	−0.023**	0.007	−0.138***
	(0.014)	(0.010)	(0.064)	(0.040)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.04	0.04	0.90	0.90
Observations	4,551	4,551	1,860	1,860

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This panel examines the robustness of the results presented in Tables 3 and 4 using different fixed effects. In columns (1) and (2), the dependent variable, *Discretionary Accruals*, is signed discretionary accruals obtained from the modified Jones (1991) model. In columns (3) and (4), the dependent variable, *REM*, is the sum of *REM Prod* and *REM Disx*. In columns (1) and (3), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant. In columns (2) and (4), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and industry-year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

(continued on next page)

Second, the Title 9 provisions, which govern financial reporting practices in The Netherlands, were also amended in 2016. The amendments primarily relate to the reporting of goodwill and extraordinary income. We run two additional tests to mitigate the concern that the Title 9 amendments drive the results presented in Tables 3 and 4. First, we rerun our tests excluding firms with accounting items affected by the changes in the Title 9 provisions (i.e., intangible assets and extraordinary income). We exclude all firms with intangible assets, as goodwill is not separately reported in BvD. Second, we rerun the tests using only small and micro companies—i.e., firms with less than 50 employees—as these firms are also excluded from the Title 9 provisions. As shown in Table 9, Panel A, columns (5)–(12), the results hold.[21] Overall, these tests alleviate the concern that the amendments of the Dutch Corporate Governance Code or the Title 9 provisions drive the results presented in Table 3.

Different Fixed-Effects Structures

We also test whether the results are robust to adding interactions between industry fixed effects and year fixed effects to control for industry-specific shifts over time. The sample for these tests is smaller than those used in Tables 3 and 4, as the inclusion of industry-year fixed effects drops some singleton observations. As shown in Table 9, Panel B, the results hold.

Alternative Accruals and Real-Activities Earnings Management Models

Next, we examine the robustness of the results in Tables 3 and 4 to alternative accruals and real-activities earnings management models. First, we estimate discretionary accruals using the Dechow and Dichev (2002) model and control for current (*CFO*), past (CFO_{t-1}), and future cash flows (CFO_{t+1}) scaled by average total assets as determinants of discretionary accruals. We report the results in Table 9, Panel C, columns (1) and (2). This model determines discretionary accruals using cash-flow information, which is missing for many private firms in our sample, decreasing the sample size for these tests. We continue to find a negative and significant coefficient on *Treatment* if the CFO took the integrity pledge, indicating that the results are robust to this alternative model.[22]

[21] Note that the coefficient on *Treatment* in column (12) is not significant. This result might, in part, be explained by the lack of power in this rather small sample.

[22] In untabulated tests, we also keep firms in financial industries and use these firms as control group because executives in these industries have been subject to a similar oath in 2010. The results hold. In our primary tests, we follow Barton and Simko (2002) and do not consider depreciation when determining accruals in Equation (2). However, our results are robust to subtracting depreciation from total accruals in Equation (2) (untabulated).

TABLE 9 (continued)

Panel C: Alternative Accruals and Real-Activities Earnings Management Models

Dependent Variable	*Discretionary Accruals (DD)*		*Total Accruals*		*Discretionary Accruals (Time-Series-Adjusted)*		*REM (Time-Series-Adjusted)*	
Treatment	**CEO**	**CFO**	**CEO**	**CFO**	**CEO**	**CFO**	**CEO**	**CFO**
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treatment	−0.017	−0.032*	0.010	−0.031***	0.034	−0.259*	0.007	−0.117*
	(0.021)	(0.019)	(0.013)	(0.011)	(0.211)	(0.154)	(0.100)	(0.067)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.89	0.89	0.05	0.05	0.33	0.33	0.09	0.09
Observations	597	597	4,559	4,559	4,224	4,224	1,392	1,392

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

This panel examines the robustness of the results presented in Table 3 using alternative discretionary accruals and real-activities earnings management models. In columns (1) and (2), the dependent variable, *Discretionary Accruals (DD)*, is discretionary accruals as per the Dechow and Dichev (2002) model. In columns (3) and (4), the dependent variable, *Total Accruals*, is total accruals as per the Jones (1991) model, but estimating abnormal accruals in one step, as per Chen et al. (2018). In columns (5) and (6), the dependent variable, *Discretionary Accruals (Time-series-adjusted)*, is discretionary accruals based on the Kothari et al. (2016) framework. In columns (7) and (8), the dependent variable, *REM (Time-series-adjusted)* is the sum of *REM Prod* and *REM Disx* calculated based on the Kothari et al. (2016) framework. In columns (1), (3), (5), and (7), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant. In columns (2), (4), (6), and (8), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. In columns (1) and (2), *Controls* include *CFO*, CFO_{t+1}, CFO_{t-1}, *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. In columns (3) and (4), *Controls* include *Constant*, *ΔRevenues*, *PPE*, *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. In columns (5)–(8), *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.

All variables are defined in Appendix A.

TABLE 10

Private versus Public Firms

Dependent Variables		*Discretionary Accruals*		*REM*	
Treatment Variables		CEO (1)	CFO (2)	CEO (3)	CFO (4)
Treatment × *Public Firm*	α_3	0.007	0.001	0.098	0.046
		(0.014)	(0.027)	(0.102)	(0.050)
Treatment	α_1	−0.000	−0.031***	−0.053	−0.154***
		(0.018)	(0.011)	(0.090)	(0.053)
F-Test: $\alpha_1 + \alpha_3 > 0$		0.007	−0.030*	0.045	−0.108*
		[0.28]	[0.09]	[0.21]	[0.00]
Controls		Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Adjusted R^2		0.02	0.02	0.88	0.88
Observations		4,559	4,559	1,868	1,868

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This table analyzes cross-sectional variation in the results of Tables 3 and 4. *Public Firm* equals 1 if the firm is listed, and 0 otherwise. The models presented differ in the *Treatment* variable. In columns (1) and (2), the dependent variable is signed discretionary accruals obtained from the modified Jones (1991) model. In columns (3) and (4), the dependent variable, *REM*, is the sum of *REM Prod* and *REM Disx*. In columns (1) and (3), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant. In columns (2) and (4), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. *Public Firm* is absorbed by the firm fixed effects. All models include firm and year fixed effects. The sample spans the period 2013–2019. The p-values are displayed in brackets below the mean differences in coefficient estimates. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

Second, we follow Chen et al. (2018) and estimate abnormal accruals in one step using the modified Jones (1991) model (Table 9, Panel C, columns (3) and (4)). In that model, the dependent variable is *Total Accruals*. Consistent with the modified Jones (1991) model, we control for a constant scaled by total assets at the beginning of the year (*Constant*), the difference in the change of revenues and receivables scaled by total assets at the beginning of the year (Δ*Revenues*), and property, plant, and equipment scaled by total assets at the beginning of the year (*PPE*). We use signed accruals as the dependent variable, so a positive and significant coefficient on *Treatment* indicates reporting choices to increase reported earnings. To be consistent with our main research design, we require at least ten industry-year observations for this model. We find a negative and significant coefficient on *Treatment* if the CFO took the integrity pledge, indicating that the results are robust to this alternative model.

Third, following Kothari et al. (2016) and Siriviriyakul (2021), we estimate a time-series-adjusted version of the proxies for abnormal accruals and real-activities manipulation. In particular, we adjust each variable in Equations (2)–(4) by subtracting its lagged value before running the industry-year regressions. We find a negative and significant coefficient on *Treatment* if the CFO took the integrity pledge, indicating that the results are robust to this alternative model (Table 9, Panel C, columns (5)–(8)).[23]

Public versus Private Firms

As our sample includes both public and private firms, we also examine differences in the results presented in Tables 3 and 4 between these different types of firms. For example, differences in incentives to engage in earnings management and differences in accounting and auditing requirements for private and public firms could result in different effects of the oath on earnings management in public versus private firms (e.g., Burgstahler, Hail, and Leuz 2006).

[23] Using the Kothari et al. (2016) model, the within-firm standard deviations for abnormal accruals (real-activities manipulation) are 1.79 (0.33), implying a decrease in abnormal accruals (real-activities manipulation) by 0.14 (0.35) standard deviations when the CFO took the pledge. These magnitudes are similar to the ones in our main tests.

TABLE 11

The Accountants' Oath and Future Performance

Dependent Variable	ROA_{t+1}		ROA_{t+2}		$Sales_{t+1}$		$Sales_{t+2}$	
Treatment	CEO	CFO	CEO	CFO	CEO	CFO	CEO	CFO
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.005	0.017	0.013	0.025	−0.082	0.025	−0.252	0.00
	(0.022)	(0.012)	(0.034)	(0.017)	(0.160)	(0.054)	(0.358)	(0.149)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.590	0.590	0.600	0.600	0.900	0.900	0.900	0.900
Observations	3,467	3,467	2,734	2,734	3,467	3,467	2,734	2,734

*, **, *** Indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.
This table reports the estimation results from linear regressions of the following form:

$$Future\ Performance_{i,t} = \beta_0 + \beta_1 Treatment_{i,t} + \phi Controls_{i,t} + \varepsilon.$$

The models presented differ in the *Treatment* variable and dependent variable. In columns (1) and (2), the dependent variable is *ROA* in year t+1. In columns (3) and (4), the dependent variable is *ROA* in year t+2. In columns (5) and (6), the dependent variable is the natural logarithm of sales in year t+1. In columns (7) and (8), the dependent variable is the natural logarithm of sales in year t+2. In columns (1), (3), (5), and (7), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CEO is a registered accountant. In columns (2), (4), (6), and (8), *Treatment* takes the value of 1 for firm-years after 2016 for companies whose CFO is a registered accountant. *Controls* include *Market Share*, *NOA*, *Size*, *ROA*, and *Leverage*. All models include firm and year fixed effects. The sample spans the period 2013–2019. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients.
All variables are defined in Appendix A.

Overall, it is difficult to predict how these differences in incentives and reporting requirements affect the responses to the oath in private versus public firms.

Table 10 presents the results. We do not find a statistically significant coefficient on *Treatment* × *Public Firm*, indicating that the oath has a similar effect on reducing earnings management in both private and public firms, despite the differences in incentives and reporting requirements across public and private firms.[24] As a caveat, these results are based on a relatively small number of 47 public firms. Thus, it is possible that these tests lack power to capture differences in the effect of the oath on executives of private as compared with public firms.

Future Performance

A question that arises from our results is whether the requirement to take the oath harms investors and other stakeholders, as it could reduce future performance. For example, executives that take the oath may invest a substantial amount of time and resources on improving their firm's financial reporting (e.g., via improving their reporting processes or implementing new accounting systems), instead of focusing on managing the business, which could hurt future revenues and profitability. To explore this question, we examine whether firms whose executives took the oath have lower future profitability or revenues.[25]

We tabulate the results in Table 11. We do not find that firms whose executives took the oath have a lower return on assets or revenues in years t+1 or t+2. These results suggest that requiring executives to take the oath does not reduce future performance, appeasing the concern that the oath creates substantial costs for investors and other external stakeholders.[26]

[24] The negative and statistically significant result from the F-test in Table 10, columns (2) and (4), confirms that the oath also reduces earnings management in public firms when the CFO takes the oath.

[25] We focus on accounting-based measures of performance (instead of market-based measures) because our sample also includes private firms for which stock prices or other market data are not available.

[26] Another question is whether the oath effect is driven by firms that employed the highest levels of aggressive financial reporting practices in the pre-oath period or by conservative firms becoming even more conservative. In untabulated tests, we find that the effect of the oath is concentrated in firms with high levels of aggressive financial reporting practices in the pre-oath period (i.e., firms in the top quartile of each earnings management proxy), suggesting that the oath had the intended effect of reducing aggressive reporting practices.

VI. CONCLUSION

In this paper, we examine the effect of executives' pledges of integrity on firms' financial reporting outcomes. Using a 2016 regulation that requires Dutch professional accounting degree holders to pledge an integrity oath, we identify CEOs and CFOs required to take this oath. After taking the oath, we find that companies whose CFO was required to swear the oath exhibit lower accruals-based and real-activities earnings management. Although the traditional approach to reducing earnings management is to increase enforcement or apply harsher punishments, our analyses indicate that requiring executives to take an oath can also improve firms' financial reporting quality.

It is important to note three limitations of our study. First, the recency of this rule allows us to examine only the first three years after the oath-taking. Thus, the effects of the oath may fade over more extended periods. Second, our analyses are set in The Netherlands. Although The Netherlands shares many cultural similarities with other European countries and even the United States, cultural and legal characteristics may influence the magnitude of the effect. Despite these limitations, to our knowledge, this is the first study to examine, in isolation, the effect of an integrity oath on firms' financial reporting. Our results complement and extend important insights from experimental behavioral economics studies to executives' actual on-the-job behavior in an accounting reporting setting. Finally, our setting examines the effect of a one-time verbal oath on firms' reporting choices. However, other types of integrity pledges exist for accounting-degree holders outside of The Netherlands. For example, CPA license holders in the United States are required to make a declaration about their ethical compliance when they renew their license. Managers of publicly listed U.S. companies are required to provide and sign a management representation letter to indicate the "true and fair" nature of the financial statements during the audit process. Our study does not speak to the efficacy of these different integrity pledges or the mechanisms that could explain differences in efficacy (e.g., verbal versus written pledge, one-time versus recurring pledge). Relatedly, our study does not examine all direct and indirect channels that may explain why integrity pledges improve financial reporting quality. For example, integrity pledges may also increase employees' trust in management and, thus, indirectly increase reporting quality in the longer run (Garrett et al. 2014). We believe these additional questions provide fruitful avenues for future research.

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APPENDIX A

Variable Definitions

The following variables are constructed using data from Bureau van Dijk's Amadeus database (BvD) and publicly available data on Dutch auditors from the NBA website (https://www.nba.nl) (NBA).

Variables of Interest

Discretionary Accruals

Discretionary accruals are the signed residuals for firm i in year t from the following modified Jones (1991) discretionary accruals model as per Dechow, Sloan, and Sweeney (1995) estimated for each industry-year (at the two-digit NAICS level) with more than 10 firm-year observations:

$$Total\ Accruals_{i,t} = \alpha_0 + \alpha_1 \frac{1}{AT_{i,t-1}} + \alpha_2 \frac{\Delta REV_{i,t}\text{-}\Delta REC_{i,t}}{AT_{i,t-1}} + \alpha_3 \frac{PPE_{i,t}}{AT_{i,t-1}} + \epsilon_{i,t}$$

$Total\ Accruals_{i,t}$ for firm i in year t are calculated as $((\Delta current\ assets - \Delta\ cash) - (\Delta\ current\ liabilities - \Delta\ loans))$. ΔREV is the change in sales from year t+1 to year t; ΔREC is the change in accounts receivable from year t+1 to year t; $PPE_{i,t}$ is gross property, plant and equipment. We scale $(\Delta REV\text{-}\Delta REC)$ and $PPE_{i,t}$ variables by lagged total assets (AT). (BvD)

M-Score

The *M-Score* is calculated following Beneish (1999). In particular, the formula of the *M-Score* is: $M\text{-}Score = -4.84 + 0.92\ DSRI + 0.528\ GMI + 0.404\ AQI + 0.892\ SGI + 0.115\ DEPI - 0.172\ SGAI - 0.327\ LVGI + 4.679\ TATA$. DSRI is $(Net\ Receivables_t / Sales_t) / (Net\ Receivables_{t-1} / Sales_{t-1})$; GMI is $((Sales_{t-1} - COGS_{t-1}) / Sales_{t-1}) / ((Sales_t - COGS_t) / Sales_t)$; AQI is $(1 - (Current\ Assets_t + PPE_t) / Total\ Assets_t) / (1 - (Current\ Assets_{t-1} + PPE_{t-1}) / Total\ Assets_{t-1})$; SGI is $Sales_t / Sales_{t-1}$; $DEPI$ is $(Depreciation_{t-1} / (PPE_{t-1} + Depreciation_{t-1})) / (Depreciation_t / (PPE_t + Depreciation_t))$; $SGAI$ is $(SG\&A_t / Sales_t) / (SG\&A_{t-1} / Sales_{t-1})$; $LVGI$ is $((Current\ Liabilities_t + Long\text{-}Term\ Debt_t) / Total\ Assets_t) / ((Current\ Liabilities_{t-1} + Long\text{-}Term\ Debt_{t-1}) / Total\ Assets_{t-1})$; $TATA$ is $(Income\ from\ Operations_t - Cash\ Flow\ from\ Operations_t) / Total\ Assets_t$.

REM Disx

Abnormal discretionary expenditures are calculated following Roychowdhury (2006). We run the regression for each two-digit SIC code/year with at least ten observations. We multiply the residual by −1. (BvD)

REM Prod

Abnormal discretionary production calculated following Roychowdhury (2006). We run the regression for each two-digit SIC code/year with at least ten observations. (BvD)

REM

The sum of abnormal discretionary production and abnormal discretionary expenditures *REM Prod* + *REM Disx*. (BvD)

Treatment CEO

Indicator set to 1 for firms whose CEO is a registered accountant in the years after 2016, and 0 otherwise. (BvD and NBA)

Treatment CFO

Indicator set to 1 for firms whose CFO is a registered accountant in the years after 2016, and 0 otherwise. (BvD and NBA)

Controls

Market Share

Sales at the beginning of the year scaled by total sales of the firm's three-digit-NAICS-code industry at the beginning of the year. (BvD)

NOA

Shareholders' equity less cash and marketable securities plus total debt at the beginning of the year divided by sales at the beginning of the year. (BvD)

Size

The log of total assets. (BvD)

ROA

Net income scaled by total assets. (BvD)

Leverage

The ratio of total debt to total assets. (BvD)