

# **Accounting and Society: A Holistic Perspective on Corporate Social Responsibility**

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# Abstract

The aim of this thesis is to provide novel and holistic evidence on *why managers pursue corporate social responsibility (CSR)*. To achieve this aim, I conduct three independent but interrelated empirical studies. Motivated by endogeneity concerns and the lack of evidence on managers' *private* preferences (i.e., agency motives), the first study (Chapter II) examines how a truly exogenous increase in agency problems affects different types of CSR investment decisions. Using a natural experiment for identification, I find that an increase in agency problems causes managers to misinvest in immaterial CSR and over-invest in material CSR. This finding suggests that managers have an underling preference for *building a social empire*. In addition, I provide evidence that an increase in agency problems causes managers to issue CSR press releases more frequently, and these press releases have a more positive tone. This indicates that managers want the general public to notice their social empire. The findings of the first study contribute to the existing literature by providing more nuanced evidence on managerial preferences, offering improved identification of agency problems, and analyzing CSR press releases.

Motivated by the inconclusive evidence on managers' financial motives, the second study (Chapter III) provides more nuanced evidence by accounting for customer profile differences. Using an interaction model, I show that issuing a CSR report has a positive effect on financial performance if firms address end-consumers (i.e., are a B2C firm) and their profitability level is low. In contrast, if firms addresses other businesses (i.e., are a B2B firm) and their profitability level is low, issuing a CSR report has a negative effect on financial performance. This finding contributes to the existing literature by revealing that only managers of less profitable B2C firms have a financial motive to issue a CSR report.

Motivated by the scarcity of research on institutional forces that push or pull managers to pursue CSR, the third study (Chapter IV) examines how culture—an informal institutional force—affects managers' decisions regarding the credibility of CSR reports. Using a multi-methods approach,

I show that cultural rule orientation—that is, people’s proclivity for adhering to rules, laws, and regulations—has a positive effect on several corporate decisions that determine the credibility of CSR reports (e.g., whether to receive external assurance). Path analysis and qualitative comparative analysis reveal that the *direct* effect of cultural rule orientation is much stronger than its *indirect* effect via legal institutions. This finding contributes to the existing literature by showing that culture is a powerful institutional primitive that motivates managers to issue more credible CSR reports.

# I. Introduction

## 1. Motivation and aim

Firms increasingly portray themselves as socially responsible members of society (Jha and Cox, 2015). In doing so, they are allocating substantial proportions of their expense budgets to corporate social responsibility (CSR)—activities that appear to further some social good beyond what is legally required (McWilliams and Siegel, 2001).<sup>1</sup> In fact, large United States (US) firms are spending over \$40 billion annually on CSR projects (Di Giuli and Kostovetsky, 2014).<sup>2</sup> Examples of such CSR projects include abating pollution, giving to charity, supporting local businesses, recycling, and embodying products with social attributes (McWilliams and Siegel, 2001). In addition, many firms publish extensive stand-alone CSR reports or devote large sections of their annual reports to CSR. Specifically, over 90% of the world’s largest firms are currently publishing stand-alone CSR reports (KPMG, 2017). Reflecting the importance of CSR in firms, socially responsible investing (SRI) is also an increasingly relevant investment vehicle (Deng et al., 2013). In particular, the amount of professionally managed US assets tied to CSR through SRI is currently over \$3 trillion, and almost 1,000 institutional shareholders from all around the world are signatories to the United Nations Principles for Responsible Investing (Di Giuli and Kostovetsky, 2014; Deng et al., 2013). But not only managers and shareholders pay attention to CSR, also business school graduates, auditors, and management consultants increasingly recognize CSR. Business school graduates see job opportunities, auditors see a growing market for assuring CSR reports, and management consultants see an enormous potential for consulting CSR projects (Montgomery and Ramus, 2008; O’Dwyer, 2011; Skouloudis and Evangelinos, 2014).

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<sup>1</sup> The terms corporate sustainability, corporate sustainable development, and corporate accountability are often used instead of CSR (Eccles et al., 2014; Lys et al., 2015; Christensen, 2016). Generally, these terms can be used interchangeably. Due to its dominance in the literature, I use the term CSR in this thesis.

<sup>2</sup> In this thesis, all \$ signs refer to US dollars.

Despite the growing importance of CSR, the fundamental question of *why managers pursue CSR* is subject to much debate. The literature provides three different views that try to explain why managers pursue CSR. The first view, often called the stakeholder maximization view,<sup>3</sup> suggests that managers invest in CSR to maintain better relations with stakeholders (e.g., employees, suppliers, and customers), who then reward the firm. For example, offering CSR-related products—because the firm’s customers prefer products with CSR attributes—increases sales and, in turn, financial performance. This view considers CSR to be strategic because investments in CSR are financially motivated (Jha and Cox, 2015). Generally, studies arguing this viewpoint examine the value relevance of CSR—that is, the effect of CSR on financial performance measures. Empirical evidence on this viewpoint is mixed at best (Margolis et al., 2011). While some studies find a positive association between CSR and financial performance (Bachoo et al., 2013; Deng et al., 2013; Dhaliwal et al., 2014), others do not (Jones et al., 2007; Guidry and Patten, 2010; Manchiraju and Rajgopal, 2017).

The second view, often called the agency view,<sup>4</sup> suggests that managers pursue CSR at the expense of shareholders because they derive private benefits from it. For example, managers of firms with the highest levels of CSR are awarded prestigious prizes, receive extensive personal media coverage, and have buildings (and the like) named in their honor.<sup>5</sup> This view considers CSR to be an agency problem because CSR is motivated by managers’ private benefits (Masulis and Reza, 2015). Generally, studies arguing this viewpoint examine whether agency measures (e.g., ownership structures) are associated with CSR. Empirical evidence on this viewpoint is also mixed at best. While some studies find that more severe agency problems are associated with higher CSR (e.g., Surroca and Tribo, 2008; Barnea and Rubin, 2010; Masulis and Reza, 2015), others find an inverse association (e.g., Harjoto and Jo, 2011; Jo and Harjoto, 2012; Ferrell et al., 2016).

The third view, often called the institutional view,<sup>6</sup> suggests that institutional forces are responsible for pushing or pulling managers to pursue CSR (Ioannou and Serafeim, 2012). This view considers CSR to be a neutral endeavor, meaning that the motivation is not the benefit of some

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<sup>3</sup> The stakeholder maximization view is rooted in the (instrumental) stakeholder theory (see Donaldson and Preston, 1995).

<sup>4</sup> The agency view is rooted in traditional agency theory (see Jensen and Meckling, 1976).

<sup>5</sup> Please visit <http://www.csrwire.com> for extensive anecdotal evidence.

<sup>6</sup> The institutional view is rooted in (economics-based or sociology-based) institutional theories (Williamson, 2000; Scott, 2001).

party—shareholders or managers (Jha and Cox, 2015). Empirical evidence on this viewpoint is limited (Ioannou and Serafeim, 2012). The few studies arguing this viewpoint focus on formal institutions (e.g., legal institutions), but largely neglect informal institutions (e.g., culture). For example, Liang and Renneboog (2017) show that managers of firms located in civil law countries are more likely to pursue CSR. They argue that they behave this way because of the stakeholder orientation of these countries. Similarly, Simnett et al. (2009) show that firms located in civil law countries are more likely to issue more credible CSR reports.

Motivated by the inconclusive and limited evidence on these three views, the *aim* of this thesis is to provide novel and holistic evidence on *why managers pursue CSR*.

## 2. Objectives

To achieve the aim of providing novel and holistic evidence on why managers pursue CSR, I do not only consider the different views of CSR but also focus on the different aspects of CSR: investment, reporting, and assurance. In doing so, the *first objective* of this thesis is to provide more nuanced and truly causal evidence on managers’ agency motives (i.e., managerial preferences) in terms of CSR investments. Prior research examines whether CSR *per se* reflects an agency problem (Surroca and Tribo, 2008; Masulis and Reza, 2015; Ferrell et al., 2016). This thesis provides more nuanced evidence by decomposing firms’ overall CSR investments into CSR over-investments,<sup>7</sup> CSR under-investments,<sup>8</sup> CSR mis-investments,<sup>9</sup> and risky CSR investments.<sup>10</sup> Furthermore, prior studies use traditional agency proxies to measure agency problems (e.g., ownership structures), which do not allow drawing truly causal inferences (i.e., solve the endogeneity problem) (Surroca and Tribo, 2008; Barnea and Rubin, 2010; Borghesi et al., 2014). This thesis uses a natural experiment to provide truly causal evidence.

The *second objective* is to provide more nuanced evidence on managers’ financial motives (i.e., value relevance) in terms of CSR reporting. Survey evidence and prior empirical studies show that

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<sup>7</sup> CSR over-investments are defined as investments in material CSR projects (according to the Sustainability Accounting and Standards Board (SASB)) that exceed the optimal level.

<sup>8</sup> CSR under-investments are defined as forgone investments in material CSR projects (according to the SASB) below the optimal level.

<sup>9</sup> CSR mis-investments are defined as investments in immaterial CSR projects (according to the SASB).

<sup>10</sup> Risky CSR investments are defined as investments in green (environmental) innovation projects or product innovation projects.

the value relevance of CSR reporting is unclear, concluding that this is likely due to opposing value relevance effects of different firms (Haddock-Fraser and Fraser, 2008; KPMG, 2011; Margolis et al., 2011). This thesis provides more nuanced evidence by investigating whether customer profile differences explain the opposing value relevance effects of CSR reporting.

The *third objective* is to provide novel evidence on managers’ institutional motives in terms of credible CSR reporting (i.e., CSR assurance). Prior studies provide evidence that legal institutions (e.g., legal origin) well explain managers’ decisions regarding the credibility of CSR reports (i.e., CSR assurance decisions) (Simnett et al., 2009; De Beelde and Tuybens, 2015; Zhou et al., 2016). This study provides novel evidence by exploring the role of culture (i.e., an informal institution)—whether it affects credible CSR reporting directly or indirectly via legal institutions.

Taken together, these three objectives provide a holistic perspective on managers’ motives to pursue CSR by focusing on agency motives, financial motives, institutional (neutral) motives, CSR investments, CSR reporting, and CSR assurance.

### 3. Research design

This thesis relies on archival (panel) data,<sup>11</sup> which come from various (commercial) sources (e.g., MSCI ESG STATS, GRI Sustainability Disclosure Database, Thomson Reuters, Compustat, ExecuComp, AuditAnalytics, Factiva, SEC filings, Google Trends, House et al. (2004), RobecoSAM, and The World Bank DataBank).<sup>12</sup> Data from Compustat and ExecuComp are derived using the software SAS. Data from US Securities and Exchange Commission (SEC) filings and Google Trends are derived using the softwares R and Matlab. All other data are derived using web interfaces (e.g., AuditAnalytics) or are hand-collected from the source documents (e.g., House et al. (2004)). To match the different sources, I use identifiers (e.g., GVKEY, CUSIP, ISIN). If identifiers are not available or yield poor matching results, I use a fuzzy matching algorithm (programmed in R) based on firm information (e.g., firm name, state, industry).

The samples used in this thesis are different in each chapter, determined by data limitations and

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<sup>11</sup> Therefore, no ethics approval is necessary.

<sup>12</sup> I am grateful to Todd A. Gormley and David A. Matsa for providing data from their 2016 *Journal of Financial Economics* article; to John Nofsinger and Abhishek Varma for providing data from their 2014 *Journal of Banking & Finance* article; and to Raji Srinivasan, Gary L. Lilien, and Shrihari Sridhar for providing data from their 2011 *Journal of Marketing* article.



contextual factors. Chapter II focuses on all US firms included in the MSCI ESG STATS database between 1991 and 2013.<sup>13</sup> Chapter III focuses on all US firms included in the Standard & Poor’s 500 index between 2007 and 2011.<sup>14</sup> Chapter IV focuses on firms from 31 countries (with sufficient country-level data) included in Compustat and the GRI Sustainability Disclosure Database between 2012 and 2016.<sup>15</sup>

To analyze the data, I use the software Stata. As main models, I employ a linear difference-in-differences model with several fixed effects (using the `reg` command in Stata), a linear regression model with firm and time fixed effects (using the `xtreg` command in Stata), a linear path model with industry and time fixed effects (using the `sem` command in Stata), a qualitative comparative analysis (using the `fuzzy crisp` command in Stata), and a probit model with industry and time fixed effects (using the `probit` command in Stata). Apart from these main models, I also employ several alternative models for robustness and auxiliary analyses (e.g., instrumental variable probit model, hierarchical generalized linear model, and Weibull hazard model).

## 4. Outline of the thesis

This thesis is a collection of three empirical studies (Chapters II to IV). Table 1 outlines the three studies. Figure 1 shows how the three studies relate to the aim and the objectives of this thesis.

The first study (Chapter II)—entitled ‘*Building a social empire? Managerial preferences, shareholder litigation, and corporate social responsibility*’—examines how an exogenous increase in agency problems affects several CSR investment decisions. For empirical identification, I exploit the staggered adoption of universal demand (UD) laws across US states as a natural experiment. UD laws exogenously increase agency problems by insulating managers from shareholder litigation. Using over 23,000 firm-year observations, I find that, after managers are insulated by the adoption of a UD law, they both mis-invest in immaterial CSR and over-invest in material CSR. This finding suggests that managers have an underlying preference for *building a social empire* and rules out alternative managerial preferences (i.e., *supporting CSR pet projects*, *enjoying the quiet life*, and

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<sup>13</sup> I use US data because the MSCI ESG STATS database covers only US firms. In addition, this database is the only one that has data starting as early as 1991, which is important for our identification strategy.

<sup>14</sup> I focus on the US because analyzing a single market with identical societal and political circumstances facilitates a relatively homogeneous dataset (Gamerschlag et al., 2011).

<sup>15</sup> I use an international dataset because the objective of the study is to explain *cross-country* disparities in terms of the credibility of CSR reports by focusing on culture and legal institutions.

*playing it safe*). Further tests corroborate this initial suggestion: only managers who have greater ex ante incentives to empire build—for instance, when free cash flow is abundant—react to a UD law being adopted. Moreover, I show that, after a UD law is adopted, managers issue CSR press releases more frequently, and these press releases have a more positive tone. This finding suggests that managers want the general public to notice their social empire. Collectively, this study provides evidence that US managers build social empires to satisfy their hunger for status, fame, power, and prestige. In doing so, it addresses *the first objective* of this thesis: to provide more nuanced and truly causal evidence on managers’ agency motives (i.e., managerial preferences) in terms of CSR investments.

The second study (Chapter III)—entitled *‘Do customers affect the value relevance of corporate social responsibility reporting? Empirical evidence on stakeholder interdependence’*—examines whether the effect of CSR reporting on financial performance (i.e., the value relevance of CSR reporting) is affected by customer profile differences. Using a sample of US listed firms, I find that the value relevance of CSR reporting is indeed affected by customer profile differences. Further tests reveal that the customer profile effect is only predominant if firms’ profitability levels are low. In fact, I find that CSR reporting has a positive effect on financial performance only for less profitable firms that address end-consumers (so called B2C firms). This suggests that only managers of less profitable B2C firms have a financial motive to issue a CSR report. Taken together, the findings of this study address *the second objective* of this thesis: to provide more nuanced evidence on managers’ financial motives in terms of CSR reporting.

The third study (Chapter IV)—entitled *‘Cultural rule orientation, legal institutions, and the credibility of corporate social responsibility reports’*—examines the effect of cultural rule orientation—that is, people’s proclivity for adhering to laws, rules, and regulations (Venaik and Brewer, 2010)—on fundamental corporate decisions determining the credibility of CSR reports. Using an international sample, I find that managers of firms located in countries with stronger cultural rule orientation are more likely to decide to receive assurance on the firm’s CSR report, to receive assurance from an accounting firm, to receive assurance in accordance with an assurance standard, and to receive assurance on the firm’s *entire* CSR report. Path analysis shows that the *direct* effect of cultural rule orientation is much stronger than its *indirect* effect via legal institutions. In fact, the indirect effect, at most, accounts for 23% of the total effect, while the direct effect, at least,

accounts for 77% of the total effect. The dominance of the direct effect is confirmed in a qualitative comparative analysis. Overall, this study shows that cultural rule orientation pushes managers to issue more credible CSR reports. Thereby, it addresses *the third objective* of this thesis: to provide novel evidence on managers' institutional motives in terms of credible CSR reporting.

## 5. Contribution of the thesis

This thesis contributes to the literature by providing novel and holistic evidence on why managers pursue CSR. The first study (Chapter II) contributes to literature in several ways. First, and most notably, I contribute by systematically mapping out managerial preferences (i.e., managers' agency motives) in terms of CSR. Prior research only examines *whether* managers prefer to invest *more* or *less* in CSR than shareholders desire, but has not given much attention to the question of *why* managers behave a certain way. Second, I contribute to the literature by offering improved identification of agency problems. Prior research uses endogenous agency variables, such as managers' ownership, or relies on single regulatory events for identification. My identification strategy is innovative because it exploits the staggered occurrence of regulatory events, which helps reduce biases and noise that may be present when drawing inferences from endogenous agency variables or a single regulatory event. Third, I contribute to the literature by providing large scale textual evidence on press releases and SEC filings with respect to CSR.

The second study (Chapter III) makes two important contributions to the literature. First, I contribute by providing more nuanced evidence on the financial effects of CSR reporting. Particularly, I show that issuing a CSR report is not beneficial *per se* but depends on firms' customer profile. Second, I contribute by showing that not only the customer profile but also the level of profitability moderates the financial effect of CSR reporting.

The third study (Chapter IV) also contributes to the literature in several distinct ways and directly responds to some recently raised research questions. First, and most obviously, this study contributes to the literature explaining cross-country differences in the credibility of CSR reports. Thus far, scholars have focused on legal institutions, such as legal origin or rule of law. I add culture as an incrementally important determinant by providing evidence that also culture matters. Second, I contribute to the paucity of research examining the interdependence between culture and

legal institutions by showing that the *direct* effect of culture is much stronger than its *indirect* effect via legal institutions. Third, I contribute to the literature investigating corporate decisions related to the credibility of CSR reports. Thus far, the literature has focused on the assurance decision and the assurance provider. I consider three additional decisions to provide more comprehensive evidence on the credibility of CSR reports.

## 6. Publication details

***Study 1 (Chapter II).*** *Building a social empire? Agency problems, shareholder litigation, and corporate social responsibility*

- **Co-authors:** Andreas Hellmann (Macquarie University) and Max Goettsche (Catholic University Eichstaett-Ingolstadt). My contribution was 80 percent.
- **Publication:** Working paper
- **Presentations:** An earlier version of this study was presented at the University of Technology Sydney (research seminar) and at the *Annual Congress of the British Accounting and Finance Association* (London, 10–11 April 2018).

***Study 2 (Chapter III).*** *Do customers affect the value relevance of corporate social responsibility reporting? Empirical evidence on stakeholder interdependence*

- **Co-authors:** Max Goettsche (Catholic University Eichstaett-Ingolstadt) and Simon Gietl (Andersch AG). My contribution was 80 percent.
- **Publication:** A slightly different version of this study was published as the lead article in *Business Strategy and the Environment* (Vol. 25 No. 3, pp. 149–164, 2016).
- **Presentations:** Earlier versions of this study were presented at the University of St. Gallen (research seminar) and at the 37<sup>th</sup> *Annual Congress of the European Accounting Association* (Tallinn, 21–23 May 2014).
- **Miscellaneous:** Warth & Klein Grant Thornton research award (Matthäus Schwarz Förderpreis)

***Study 3 (Chapter IV).*** *Cultural rule orientation, legal institutions, and the credibility of corporate social responsibility reports*

- **Publication:** Working paper
- **Presentations:** Earlier versions of this study were presented at the Barcelona Graduate School of Economics (research seminar) and at the *2016 Australasian Conference of the Centre for Social and Environmental Accounting Research* (Adelaide, 7–9 December 2016).
- **Miscellaneous:** Best PhD paper award at the *2016 Australasian Conference of the Centre for Social and Environmental Accounting Research*

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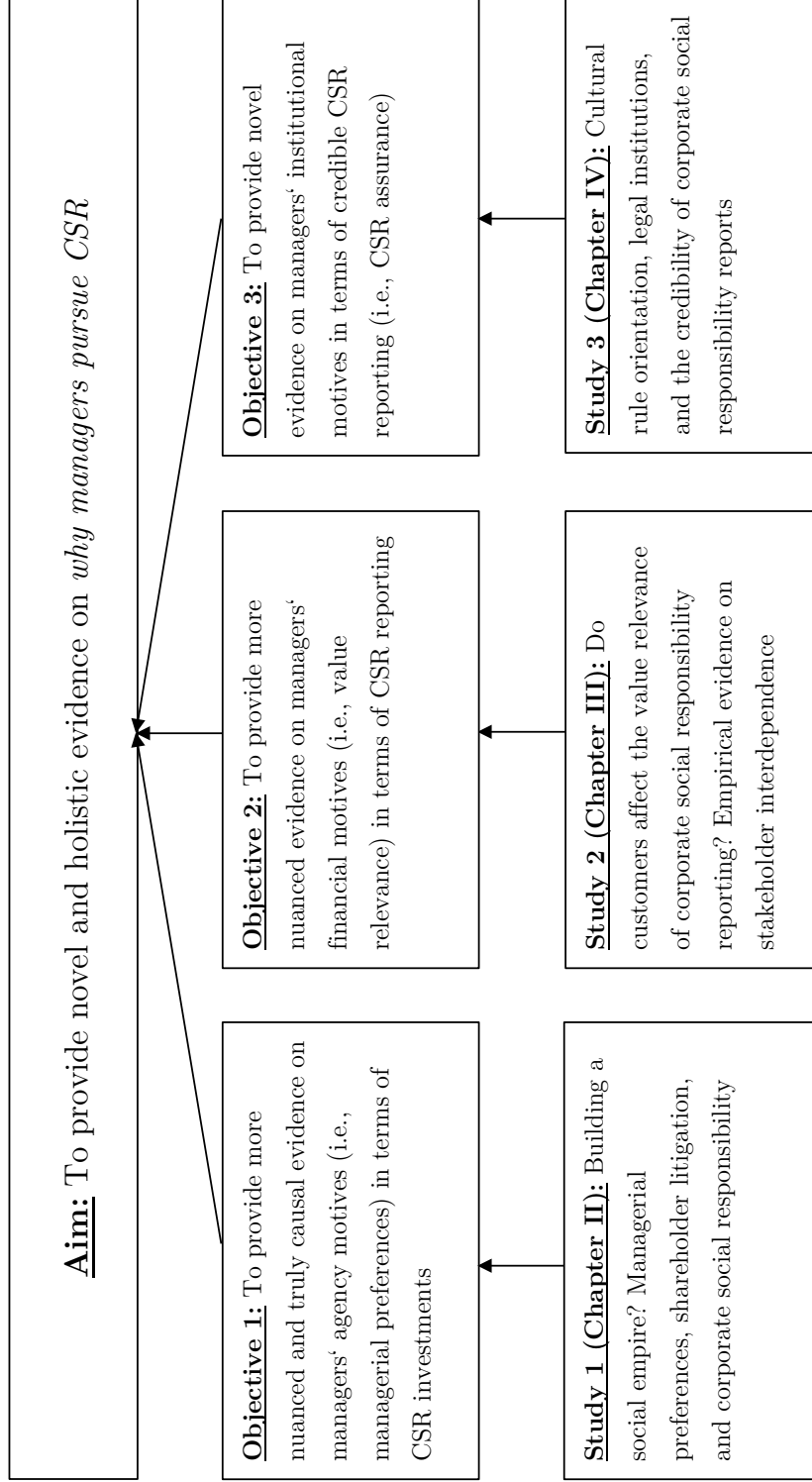
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**Table 1**  
Outline of the thesis

Study 1 (Chapter II): Building a social empire? Agency problems, shareholder litigation, and corporate social responsibility			
Objective	Theoretical foundation	Main related literature	Key findings
To provide more nuanced and truly causal evidence on managers' agency motives (i.e., managerial preferences) in terms of CSR investments	Agency theory	Barnea and Rubin (2010) Masulis and Reza (2015) Ferrell et al. (2016)	An exogenous increase in agency problems causes an increase in CSR mis-investment and CSR over-investment—i.e., managers have an underlying preference for <i>building a social empire</i> .
Study 2 (Chapter III): Do customers affect the value relevance of corporate social responsibility reporting? Empirical evidence on stakeholder interdependence			
Objective	Theoretical foundation	Main related literature	Key findings
To provide more nuanced evidence on managers' financial motives (i.e., value relevance) in terms of CSR reporting	(Instrumental) Stakeholder theory	Jones et al. (2007) Guidry and Patten (2010) Bachoo et al. (2013)	The value relevance of CSR reporting is affected by customer profile differences. Specifically, CSR reporting has a positive effect on financial performance only for less profitable B2C firms.
Study 3 (Chapter IV): Cultural rule orientation, legal institutions, and the credibility of corporate social responsibility reports			
Objective	Theoretical foundation	Main related literature	Key findings
To provide novel evidence on managers' institutional motives in terms of credible CSR reporting (i.e., CSR assurance)	(Economics-based and sociology-based) Institutional theory	Simmnett et al. (2009) Orij (2010) Zhou et al. (2016)	Cultural rule orientation shapes credible CSR reporting directly and indirectly via legal institutions. The direct effect is much stronger than the indirect effect.

**Figure 1**  
Aim, objectives, and studies (chapters) of the thesis



## II. Building a social empire? Managerial preferences, shareholder litigation, and corporate social responsibility\*

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### Abstract

We map out managerial preferences in terms of corporate social responsibility (CSR). For empirical identification, we exploit the staggered adoption of universal demand (UD) laws as a natural experiment. UD laws exogenously increase agency problems by insulating managers from shareholder litigation. We find that, after managers are insulated by the adoption of a UD law, they both misinvest in immaterial CSR and over-invest in material CSR. This suggests that managers have an underlying preference for *building a social empire*. Indeed, only managers who have greater ex ante incentives to empire build—for example, when free cash flow is abundant—react to a UD law being adopted. Beyond, after a UD law is adopted, managers issue CSR press releases more frequently, and these press releases have a more positive tone.

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# 1. Introduction

Traditional agency models emphasize managers’ underlying preferences for building empires, supporting pet projects, enjoying the quiet life, and playing it safe. In all these cases, managers take actions that are not in shareholders’ best interests. Empire-building managers systematically over-invest (Jensen, 1986; Humphery-Jenner, 2012), self-serving managers mis-invest in their own pet projects (Shleifer and Vishny, 1989; Laux and Mittendorf, 2011), lazy managers generally under-invest (Grossman and Hart, 1983; Bertrand and Mullainathan, 2003), and risk-averse managers take on too little risk (Holmström, 1999; Gormley and Matsa, 2016). Building on this literature, we ask: what are managers’ underlying preferences in terms of corporate social responsibility (CSR)?<sup>1</sup>

Theoretically, managers might prefer to systematically over-invest in CSR because running a social empire satisfies their hunger for fame, status, power, and prestige (Atkinson and Galaskiewicz, 1988). Only managers of firms with the highest levels of CSR are awarded prestigious prizes, receive extensive personal media coverage, and have buildings (and the like) named in their honor.<sup>2</sup> Alternatively, managers might prefer to mis-invest in their own CSR pet projects (Brown et al., 2006)—by investing in *specific* CSR projects that are close to their hearts, these managers gain a ‘warm glow’ from increased happiness and life satisfaction (Andreoni, 1990).<sup>3</sup> By contrast, managers might prefer to avoid the difficult decisions and costly efforts associated with CSR investments—that is, they prefer to enjoy the quiet life and, therefore, under-invest in CSR (Kock et al., 2012). Eventually, managers might prefer to play it safe. Motivated by career concerns and risk aversion, they forgo risky CSR projects, such as those involving green (environmental) innovation, even though these projects lead to a competitive advantage if successful (Carrion-Flores and Innes, 2010). Which of these managerial preferences actually dominates is ultimately an empirical question. The aim of this study is to empirically examine how an *exogenous* increase in agency problems affects

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<sup>1</sup> CSR can be defined as the investment that appears to further some social good beyond what is legally required (McWilliams and Siegel, 2001). Examples of CSR include going beyond legal requirements in abating pollution, recycling, giving to charity, supporting local businesses, and embodying products with social attributes (McWilliams and Siegel, 2001).

<sup>2</sup> Please visit <http://www.csrwire.com> for extensive anecdotal evidence.

<sup>3</sup> For example, a CEO who is a big fan of ballet and, therefore, makes generous corporate donations to ballet companies all over the world, enhances her reputation among a very specific group of people she values, is granted access to ballet celebrities, and receives tickets to debut performances (Liang and Renneboog, 2017a).

several CSR investment decisions and, in the process, we hope to gain deep and fine-grained insights into managers' underlying preferences in terms of CSR.

Two obstacles impede our attempt to empirically map out managerial preferences in terms of CSR. The first is how to adequately measure CSR investments. The standard approach in the literature is to use firms' *overall* CSR investment levels.<sup>4</sup> However, using overall investment levels does not allow us to detect the different sources of managerial preferences. This is because an increase in firms' overall CSR investments following an increase in agency problems could be either due to managers' preference for systematically over-investing in CSR, or to managers' preference for mis-investing in specific CSR pet projects. Likewise, a decrease in firms' overall CSR investments following an increase in agency problems could be either due to managers' preference for generally under-investing in CSR, or to managers' preference for solely forgoing risky CSR investments. To overcome this measurement obstacle, we decompose firms' overall CSR investments into *material* and *immaterial*. In doing so, we hand-map newly available industry-specific materiality classification from the Sustainability Accounting Standards Board (SASB) to MSCI ESG STATS (hereafter, MSCI; previously known as KLD), which provide firm-level investment ratings on 70 distinct CSR issues. The rationale behind this materiality decomposition is that not all CSR investments are equally material to all firms—rather, the materiality of different CSR investments varies systematically across firms and industries (Khan et al., 2016). For example, human rights and child labor projects are material to apparel firms, such as GAP or Nike, but immaterial to technology service firms, such as Google or Facebook. By contrast, data security and customer privacy projects are material to technology service firms, but immaterial to apparel firms. Because immaterial CSR projects are not related to firms' core business activities, we argue that they reflect managers' own CSR pet projects and, thus, are generally wasteful from a shareholder perspective (Grewal et al., 2017). Conversely, material CSR investments are in shareholders' best interests, but only as long as they do not exceed the optimal investment level (McWilliams and Siegel, 2001). In principle, the optimal investment level of material CSR investments is attained when managers are disciplined—that is, when agency problems are low. Therefore, if managers increase material CSR investments

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<sup>4</sup> Studies using overall CSR investment levels include, but are not limited to, Graves and Waddock (1994), Hong and Kostovetsky (2012), Deng et al. (2013), Jiraporn and Chintrakarn (2013), Servaes and Tamayo (2013), Eccles et al. (2014), Di Giuli and Kostovetsky (2014), Gao et al. (2014), Lys et al. (2015), Ferrell et al. (2016), Cronqvist and Yu (2017), Liang and Renneboog (2017b), and Lins et al. (2017).

following an increase in agency problems, we argue that they over-invest in material CSR.

While the decomposition into material and immaterial CSR allows us to distinguish between managers' preference to build a social empire and managers' preference to support their own CSR pet projects, it does not allow us to distinguish between managers' preference to enjoy the quiet life and managers' preference to play it safe. A decrease in firms' material CSR investments following an increase in agency problems could either be due to managers' preference to generally under-invest in material CSR, or to managers' preference to solely forgo risky material CSR investments. To circumvent this issue, we decompose material CSR investments into *risky* and *non-risky* based on in-depth evaluation of each material CSR issue.<sup>5</sup> If managers only reduce risky material CSR investments, following an increase in agency problems, we argue that they prefer to play it safe. However, if managers not only reduce risky material CSR investments but also material non-risky CSR investments, following an increase in agency problems, we argue that they prefer to enjoy the quiet life. Compared with prior studies, our decomposition into material and immaterial as well as risky and non-risky CSR investments allows us to detect the different sources of managerial preferences.

The second obstacle that impedes our attempt to empirically map out managerial preferences is the endogeneity of agency problems. Firms that differ in terms of agency problems may also differ in other, unobservable dimensions. Therefore, comparing managerial behavior between firms with high and low agency problems may capture the effects of these unobservable differences rather than the effects of agency problems. Likewise, changes in agency problems within a firm are potentially accompanied by other, unobservable changes. For example, changes in ownership structures might be accompanied by changes in management teams,<sup>6</sup> which makes it impossible to disentangle the effects of agency problems from those of management characteristics. To circumvent this endogeneity obstacle, we exploit the staggered adoption of universal demand (UD) laws across United States (US) states as a natural experiment. UD laws impose a substantial procedural hurdle to derivative lawsuits.<sup>7</sup> In particular, UD laws require shareholders to seek board approval prior to filing derivative lawsuits. Since boards almost always reject such approvals, derivative lawsuits become much

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<sup>5</sup> All identified risky CSR issues are related to (environmental or product) innovation activities.

<sup>6</sup> Since the seminal work of Jensen and Meckling (1976), ownership structure has become one of the standard proxies for measuring agency problems within a firm (e.g., Ang et al., 2000; Fahlenbrach and Stulz, 2009; Masulis and Reza, 2015).

<sup>7</sup> Shareholder derivative lawsuits target managers *personally* for breaching their fiduciary duties.

less likely. The reduced fear of shareholder litigation means that an important external corporate governance mechanism has become less effective, thereby increasing agency problems. Expressed another way, being insulated from shareholder litigation makes managers freer to act upon their underlying preferences that do not align with shareholders' interests. This reasoning is consistent with recent findings in the literature. For example, Appel (2016) shows that adopting a UD law reduces the incidence of shareholder derivative lawsuits, decreases firm performance, and causes managers to make privately beneficial investments. Compared with prior studies, exploiting the staggered adoption of UD laws allows us to provide truly causal evidence of the effect of agency problems on CSR investments.

Methodologically, we exploit the staggered adoption of UD laws using a difference-in-differences estimator, with the 'treatment' group comprising states that have adopted a UD law, and the 'control' group comprising states that have not.<sup>8</sup> To tighten identification, we control for both firm fixed effects and industry-by-year fixed effects. Because almost 70% of the firms in our sample are incorporated in a different state to the one where they are located, we are also able to control for unobserved, time-varying state of location-level factors, such as local business cycles. Therefore, we identify our estimates by comparing the differential CSR investment decisions of managers of two firms operating in the same industry and located in the same state, but when only one firm is incorporated in a state that has adopted a UD law.

Based on a sample of 23,190 firm-year observations between 1991 and 2013, we find that, after managers are insulated by a UD law being adopted, they increase both immaterial CSR investments and material CSR investments. Immaterial CSR investments increase by 19% of the pre-law standard deviation, and material CSR investments increase by 25% of the pre-law standard deviation. To better understand the economic significance of these estimates, we convert them into direct monetary costs (through Selling, General, and Administrative (SG&A) expenses). The conversion yields that, after a UD law is adopted, managers spend an extra \$5 million on immaterial CSR per year, and an extra \$3 million on material CSR per year. This suggests that building a social empire costs the average firm about \$8 million, which represents over 5% of its net income. We find no evidence that managers who are insulated by the adoption of a UD law change their

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<sup>8</sup> Our estimation approach closely follows, among others, Bertrand and Mullainathan (2003), Gormley and Matsa (2016), and Ni and Yin (2018).

investment behavior in terms of risky CSR.

These findings rule out managers' underlying preference for enjoying the quiet life (i.e., to reduce material CSR) and managers' underlying preference for playing it safe (i.e., to reduce risky CSR). However, what is less clear is whether some managers prefer to support their own CSR pet projects (i.e., increase immaterial CSR) while others prefer to build a social empire (i.e., increase material CSR), *or* whether increasing immaterial CSR is part of the average manager's preference for building a social empire. To examine complementary effects between material and immaterial CSR investments, we test whether the residuals of the two regressions correlate with each other, and find that the residuals of the two regressions correlate positively. This correlation is highly statistically significant—it indicates that, after a UD law is adopted, managers increase immaterial and material CSR investments *simultaneously*. Expressed another way, immaterial and material CSR investments are complements. Therefore, we can conclude that the average manager's preference is to build a social empire.

To strengthen the empire-building interpretation, we examine cross-sectional heterogeneity in managers' responses to UD laws by exploiting differences in managers' *ex ante* incentives to empire build. Agency theory predicts that managers have greater incentives to empire build when free cash flow is abundant, leverage is scarce, and their compensation is not hedged by fixed pay (Jensen, 1986; Kannianen, 2000). Consistent with the theory, we find that only managers of firms with above-median free cash flow, below-median leverage, and below-median fixed pay increase immaterial and material CSR investments after a UD law is adopted. Managers of firms with below-median free cash flow, above-median leverage, and above-median fixed pay do *not* increase immaterial and material CSR investments after a UD law is adopted. These findings corroborate our interpretation that managers mis-invest in immaterial CSR and over-invest in material CSR because they have an underlying preference for building a social empire.

Next, we explore an alternative explanation that challenges our assumption that the threat of shareholder litigation—that is, derivative lawsuits—disciplines managers. Some scholars argue that derivative lawsuits might be frivolous and primarily motivated by the settlement fees attorneys hope to extract (Romano, 1991; Fischel and Bradley, 1986; Macey and Miller, 1991). If this is the case, the threat of derivative lawsuits pressures rather than disciplines managers—that is, prior to a UD law being adopted, managers are pressured to under-invest in material CSR and, only after a UD law



is adopted, increase material CSR investments to reach a level that is in shareholders' best interests. To assess whether this is indeed the case, we conduct two sets of tests. First, we estimate firms' optimal level of material CSR investments. In doing so, we regress firms' material CSR investments on several determinants derived from the theoretical works of McWilliams and Siegel (2001) and Baron (2008). Based on the residuals obtained from yearly regressions, we construct two variables: one that captures over-investment and one that captures under-investment. Using these variables, we find that, after a UD law is adopted, managers do not correct under-investments, but rather over-invest. This finding supports our initial assumption that the threat of derivative lawsuits disciplines rather than pressures managers. In a second set of tests, we examine cross-sectional heterogeneity in managers' responses to UD laws by exploiting differences in firms' ex ante internal corporate governance. If derivative lawsuits pressure managers, then after a UD law is adopted, both managers of firms with strong internal corporate governance and managers of firms with weak internal corporate governance should increase material CSR investment to reach an optimal CSR investment level (that is in shareholders' best interests). However, if UD laws discipline managers, only managers of firms with weak internal corporate governance should react to a UD law being adopted. We find that only managers of firms with weak internal corporate governance increase material CSR investments after a UD law is adopted. This finding further supports our initial assumption that UD laws discipline rather than pressure managers.

Finally, we provide auxiliary evidence of managers' communication behavior in terms of CSR. Specifically, we analyze whether and how managers adjust their CSR communication via press releases and Securities and Exchange Commission (SEC) filings following UD law adoption. We focus on press releases because they capture active media and primarily address the general public, and on SEC filings because they capture disclosure management and primarily address shareholders. We find that after a UD law is adopted, managers issue more positive CSR press releases, and the tone of those press releases becomes more positive. By contrast, we find only weak evidence that after a UD law is adopted, managers are more likely to include positive CSR information in SEC filings. These findings suggest that managers want the general public to notice their social empire.

This study contributes to the literature in several ways. First, we contribute by systematically mapping out managerial preferences in terms of CSR. Prior studies only examine *whether* managers

prefer to invest *more* or *less* in CSR than shareholders desire, but have not given much attention to the question of *why* managers behave this way.<sup>9</sup> We show that managers prefer to invest more in CSR than shareholders desire *because* they have an underlying preference for building a social empire. We find no evidence that managers have an underlying preference for supporting specific CSR pet projects, enjoying the quiet life, or playing it safe.

Second, we contribute to the literature by offering improved identification of agency problems. Prior literature uses endogenous (instrumented) agency variables, such as managers' ownership,<sup>10</sup> or relies on a single regulatory event for identification.<sup>11</sup> Our identification approach is innovative because it exploits the staggered occurrence of regulatory events—that is, the staggered adoption of UD laws. This staggered structure helps reduce biases and noise that may be present when drawing inferences from endogenous agency variables or a single regulatory event (Roberts and Whited, 2013; Christensen et al., 2016). Our results provide causal, unbiased evidence through a well-identified natural experiment that is very close in spirit to an ideal experiment.

Third, we contribute to the literature by showing that managers prefer to disseminate CSR information frequently, with a positive tone, and preferably via informal channels (i.e., press releases) rather than formal channels (i.e., SEC filings). This means that managers prefer to talk frequently and positively about CSR in an informal manner, and want the general public to notice their social empire.<sup>12</sup>

Fourth, we contribute to the literature by supporting the notion that derivative lawsuits discipline rather than pressure managers.<sup>13</sup> We show that, after a UD law is adopted, managers do

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<sup>9</sup> By examining the effect of *ex ante* agency problems on CSR investments, the following prior studies show that managers prefer to invest *more* in CSR than shareholders desire: Bartkus et al. (2005), Brown et al. (2006), Surroca and Tribo (2008), Barnea and Rubin (2010), Hong et al. (2012), Walls et al. (2012), Jiraporn and Chintrakarn (2013), Borghesi et al. (2014), Dimson et al. (2015), Masulis and Reza (2015), Cheng et al. (2016), and Ng and Gul (2017). By examining the effect of *ex ante* agency problems on CSR investments, the following prior studies show that managers prefer to invest *less* in CSR than shareholders desire: Harjoto and Jo (2011), Jo and Harjoto (2012), Ferrell et al. (2016), and Liang and Renneboog (2017a).

<sup>10</sup> Prior studies using endogenous (instrumented) agency variables to examine the effect of *ex ante* agency problems on CSR investments include Bartkus et al. (2005), Brown et al. (2006), Surroca and Tribo (2008), Barnea and Rubin (2010), Harjoto and Jo (2011), Jo and Harjoto (2012), Walls et al. (2012), Jiraporn and Chintrakarn (2013), Borghesi et al. (2014), Dimson et al. (2015), Ferrell et al. (2016), Liang and Renneboog (2017a), and Ng and Gul (2017).

<sup>11</sup> Only very few studies use a single exogenous (regulatory) event to examine the effect of *ex ante* agency problems on CSR investments: Hong et al. (2012), Masulis and Reza (2015), and Cheng et al. (2016).

<sup>12</sup> These findings are most closely related to those of Chan et al. (2014) and Jizi et al. (2014), who examine the effect of *ex ante* agency problems on CSR disclosure practices. A limitation of these two studies is also the endogeneity of the agency variable. To the best of our knowledge, no study has investigated the effect of agency problems on CSR media management or coverage.

<sup>13</sup> Prior studies supporting the notion that derivative lawsuits discipline rather than pressure managers include Ferris et al. (2007), Erickson (2010), Donelson and Yust (2014), Houston et al. (2015), Appel (2016), Ni and Yin

not correct under-investment in material CSR to reach an optimal investment level; rather, managers over-invest in material CSR to build a social empire. This finding corroborates the notion that derivative lawsuits are an important external corporate governance mechanism and are not frivolous.

Finally, we contribute to the paucity of research addressing the materiality of CSR issues.<sup>14</sup> Prior studies conclude that material CSR investments are in shareholders' best interests, whereas immaterial CSR investments are not. We show that managers have an underlying preference for over-investing in material CSR, which is also not in the shareholders' best interest. Therefore, we contribute to the literature by empirically testing McWilliams and Siegel's (2001) theory, which predicts an optimal level of (material) CSR and that managers have a preference for diverging from that optimal level.

The rest of the paper is organized as follows. Section 2 discusses the identification strategy. Section 3 defines the CSR investment variables. Section 4 describes the sample and the data sources. Section 5 presents the main findings, explores the validity of the identification strategy, provides robustness analyses, presents supporting cross-sectional analyses, and rules out an alternative explanation. Section 6 provides auxiliary analyses regarding the communication of CSR. Section 7 concludes.

## 2. Identification strategy

One major obstacle that hinders any empirical attempt to study managerial preferences is the endogeneity of agency problems.<sup>15</sup> In this section, we discuss our identification strategy that circumvents this obstacle.

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(2018), and Li et al. (2017).

<sup>14</sup> We are only aware of three prior studies that empirically examine the concept of materiality in terms of CSR: Khan et al. (2016), Grewal et al. (2016), and Grewal et al. (2017).

<sup>15</sup> To date, one of the cleanest settings available for measuring exogenous variation in agency problems is the staggered adoption of business combination laws (anti-takeover laws). These laws, adopted by 33 US states between 1985 and 1997, restrict hostile takeovers. The reduced fear of hostile takeover means that an important external corporate governance mechanism has become less effective, thus increasing agency problems. Only two states adopted business combination laws after 1991. Because data on CSR investments are not available for the years prior to 1991, it is not feasible to exploit business combination laws, as a well-identified natural experiment, when studying CSR investment decisions.

## 2.1 *Derivative lawsuits*

In the standard formulation of US law, managers owe fiduciary duties to shareholders, meaning that they are legally obliged to manage the firm in shareholders' best interests (American Law Institute, 1994, sec. 2.01). However, in reality, managers do not always act in these interests because of the separation of ownership and control (Jensen and Meckling, 1976).<sup>16</sup> Therefore, US state law allows shareholders to initiate judicial proceedings to address such agency problems.

Judicial proceedings fall into one of two categories: direct lawsuits or derivative lawsuits. The purpose of a direct lawsuit is to remedy a wrong committed against a specific shareholder or subset of shareholders (Erickson, 2010). For instance, an allegation that managers have wrongfully excluded a subset of shareholders from profits is a wrong that would typically be pursued in a direct (class action) lawsuit (Ferris et al., 2007), and any monetary recovery would go directly to the harmed shareholders (Appel, 2016). However, the purpose of a derivative lawsuit is to remedy a wrong committed against all shareholders (Erickson, 2010). For instance, an allegation that managers have mis-managed the firm, causing a general decrease in shareholder value, is a wrong that would typically be pursued in a derivative lawsuit (Ferris et al., 2007). Since the firm itself would be the primary recipient of harm, shareholders would sue managers derivatively *on behalf of the firm* (Erickson, 2010). Any monetary recovery would go to the firm treasury; shareholders would not directly receive payment (Appel, 2016).

Derivative lawsuits are the focus of this study, mainly for two reasons. First, derivative lawsuits target managers *personally*,<sup>17</sup> thereby entailing financial and non-financial costs for the managers themselves. Financial costs are usually covered by directors and officers (D&O) insurance, but only as long as the allegations do not involve dishonesty, intentional misconduct, or a personal gain (Cox, 1999; Lin et al., 2013). In addition, insurers frequently deny coverage on the grounds that managers have failed to disclose important information when applying for insurance (Cox, 1999). Therefore, despite D&O insurance, financial costs can be notable. Probably more important than financial costs are the non-financial costs associated with derivative lawsuits, which include loss of time, loss of reputation, and the shame associated with being personally named in a lawsuit (Laux, 2010;

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<sup>16</sup> In the US, 90% of large public firm managers own less than 5% of the firm they manage (Ofek and Yermack, 2000).

<sup>17</sup> For example, Erickson (2010) shows that CEOs are named personally (alone or together with other officers, or members of the board of directors) in 97.2% of the cases covered by their study.

Donelson and Yust, 2014). In fact, recent survey evidence indicates that the single most important risk area about which managers are concerned is reputation risk (EisnerAmper, 2016). We echo the literature<sup>18</sup> by arguing that the financial and non-financial costs associated with derivative lawsuits deter managers *ex ante* from pursuing their own goals that are not in shareholders' best interests—that is, the threat of derivative lawsuits is an important external corporate governance mechanism that mitigates agency problems.

Second, we focus on derivative lawsuits because almost half the allegations of derivative lawsuits pertain to value-destroying investments or other issues related to mis-management (Ferris et al., 2007). In fact, over 25% of the derivative lawsuits in our sample relate to mis-management in terms of CSR in the broadest sense. One of the most prominent cases involving CSR mis-management—more precisely, corporate donations—is *Kahn v. Sullivan* (594 A.2d 48, 51, Del. 1991). Armand Hammer, the chief executive officer (CEO) of Occidental Petroleum Corp., was an art collector. In 1991, he proposed that the firm construct and fund an art museum. The board approved the proposal of the 'Armand Hammer Museum and Culture Center of Art', which would have cost \$86 million. The project was subsequently challenged through three derivative lawsuits.<sup>19</sup> Another example of a shareholder derivative lawsuit related to CSR (product quality and safety) is described in Table A7.

## 2.2 UD laws

A substantial procedural hurdle to derivative lawsuits is the 'demand requirement'. Before initiating a derivative lawsuit, shareholders must make a written demand of the firm's board to take suitable actions that address the allegations, which includes that the board itself files the derivative lawsuit. Such a demand involves an inherent conflict of interest, because board members are often named as defendants together with officers (Erickson, 2010). As a result, boards almost always reject the demand to proceed with a derivative lawsuit (Swanson, 1993). In this case, the court usually follows the board's decision and dismisses the lawsuit pursuant to the business judgment rule (Kinney,

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<sup>18</sup> For example, La Porta et al. (1998, pp. 1114, 1128) state that "[t]he rights attached to securities become critical when managers of companies act in their own interest. [...] [S]ome countries give minority shareholders legal mechanisms against perceived oppression by directors. [...] These mechanisms may include the right to challenge the [managers'] decisions in court (as in the American derivative suit)." Some legal scholars argue that derivative lawsuits pressure rather than discipline managers (Romano, 1991; Kinney, 1994). We return to this alternative argument in Section 5.5.

<sup>19</sup> Please see Fisch (2000) and Pearce II (2015) for more details.

1994).

However, shareholders can circumvent the 'demand requirement' by arguing 'demand futility', which is possible if shareholders can provide evidence that the board is too involved and cannot make an unbiased decision (Thompson and Thomas, 2004). In practice, shareholders prefer to argue 'demand futility' because the board almost always rejects the demand to proceed with a derivative lawsuit (Swanson, 1993; Houston et al., 2015).

As listed in Table 1, between 1989 and 2005, 23 US states adopted UD laws that impose the 'demand requirement' on *every* derivative lawsuit and no longer allow 'demand futility' to be argued. Therefore, UD laws are a substantial procedural hurdle to derivative lawsuits.<sup>20</sup> In fact, UD laws are associated with a drop of about one-third in the incidence of derivative lawsuits (Appel, 2016).<sup>21</sup>

The general tenor in recent literature is that "the adoption of a UD law weakens the deterrence function of derivative [lawsuits] and contributes to more severe agency problems" (Li et al., 2017, p. 10). Davis (2008) shows that after a UD law is adopted, managers increase related-party transactions. Appel (2016) documents that after a UD law is adopted, privately beneficial investments increase, firm performance declines, and governance provisions to which shareholders are commonly apposed rise. Houston et al. (2015) and Ni and Yin (2018) reveal that after a UD law is adopted, firms' cost of equity and cost of debt increase. Most recently, Li et al. (2017) find that after a UD law is adopted, managers decrease SG&A cost elasticity and increase SG&A cost asymmetry, proxies that capture managerial empire building. Echoing this literature, we argue that the adoption of a UD law increases agency problems by insulating managers from derivative lawsuits.

### 2.3 Empirical specification

We use the staggered adoption of UD laws as a natural experiment to examine managerial preferences in terms of CSR. In doing so, we employ a difference-in-differences estimator, with the 'treatment' group comprising states that have adopted a UD law, and the 'control' group comprising states that have not. The underlying identification assumption is that, apart from the law, the

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<sup>20</sup> Commentators widely view UD laws as a substantial hurdle to derivative lawsuits, declaring that such laws "make derivative litigation impossible to maintain in all cases" (*New York Times*, November 29, 1993) and calling them "a death knell" for derivative lawsuits (*ABA Journal*, March, 1994).

<sup>21</sup> In Section 5.2.1, we provide further evidence by showing that UD laws are also associated with a substantial drop in the incidence of derivative lawsuits related to CSR.

sets of firms in both groups would follow parallel trends—that is, the changes in the CSR investment variables (i.e., outcome variables) for firms incorporated in states that have adopted a UD law would have been the same as for firms incorporated in states that have not adopted a UD law. We closely follow Gormley and Matsa’s (2016) application of the difference-in-differences estimator by specifying the following regression equation:

$$y_{ijlst} = \beta_1(UD\ Law_{st}) + \delta_i + \lambda_{jt} + \omega_{lt} + \varepsilon_{ijlst} \quad (1)$$

where  $y$  denotes the outcome variables for firm  $i$  in industry  $j$ , state of location  $l$ , state of incorporation  $s$ , and year  $t$ .  $UD\ Law$  is the ‘treatment’ variable—that is, a dummy variable that equals one if state  $s$  has adopted a UD law by year  $t$ , and zero otherwise.  $\delta_i$  are firm fixed effects.  $\lambda_{jt}$  are two-digit Standard Industrial Classifications (SIC) industry-by-year fixed effects.  $\omega_{lt}$  are state of location-by-year fixed effects.  $\varepsilon$  is the error term. The firm fixed effects control for any unobserved, time-invariant differences across firms. The industry-by-year fixed effects control for any unobserved, time-varying differences across industries. The state of location-by-year fixed effects control for any unobserved, time-varying differences across states of location. We account for serial correlation of the error term by clustering the standard errors at the state of incorporation level.<sup>22</sup>

Including firm fixed effects, industry-by-year fixed effects, and state of location-by-year fixed effects ensures that our difference-in-differences estimator is robust to many types of omitted, unobservable variables that could otherwise bias our estimates (Gormley and Matsa, 2014). A particularly appealing feature of our specification is the inclusion of state of location-by-year fixed effects. We are able to include these because almost 70% of the firms in our sample are incorporated in

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<sup>22</sup> As suggested by Bertrand et al. (2004), we also use alternative methods to account for serial correlation of the error term. The first alternative method is a parametric approach, assuming that the error term follows an AR(1) process. We regress the residuals from Equation (1) on their lagged values to obtain the first-order autocorrelation coefficient. We then generate an estimate of the block-diagonal variance-covariance matrix of the residuals and re-estimate Equation (1) using generalized least squares. The second alternative method is a block bootstrapping approach. We generate a large number (200) of bootstrap samples by drawing with replacement states of incorporation. For each bootstrap sample, we estimate Equation (1) and store the coefficients. Subsequently, we calculate the standard errors based on the empirical distribution of the 200 sets of coefficients. The third alternative method is a two-step ‘residual collapsing’ approach. In the first step, we regress our outcome variables, respectively, on fixed effects. We store the residuals for the treated firms only and compute the average residuals for the pre-treatment and post-treatment periods. In the second step, we regress the average residuals on the  $UD\ Law$  dummy, using White standard errors to account for heteroskedasticity. Un-tabulated estimates show that our main findings (reported in Table 3) are robust to using any of these alternative methods.

a different state to the one in which they are located. These fixed effects tighten our identification because they allow us to control for any shocks specific to a state of location and year. For a simple illustration, consider two pharmaceutical firms: both are located in California, but one is incorporated in Pennsylvania and the other is incorporated in Delaware. Since Pennsylvania adopted a UD law in 1997, we are able to compare the changes in the Delaware-incorporated firm’s CSR investment decisions with those of the Pennsylvania-incorporated firm. Because both firms are located in California, they are affected by roughly similar political and economic shocks, but only the firm incorporated in Pennsylvania is affected by a UD law being adopted. Thus, we are able to control for any business cycle or political factors that may coincide with, or lead to, a UD law being adopted. In essence, the estimates obtained from Equation (1) are identified by comparing the outcome variables of two firms that operate in the same industry,  $j$ , and are located in the same state,  $l$ , but when only one firm is incorporated in a state,  $s$ , that has adopted a UD law.

We deliberately do not include firm-level control variables in Equation (1) because they are likely endogenous to legislative changes and, therefore, may lead to inconsistent estimates (Bertrand and Mullainathan, 2003; Appel, 2016; Gormley and Matsa, 2016). However, in robustness tests we include controls that are *unlikely* to be affected by a UD law being adopted.

### 3. Measuring CSR investments

In the literature, the standard approach for measuring CSR investments is to construct an overall (aggregated) CSR score based on data from MSCI.<sup>23</sup> MSCI covers six CSR categories: Community, Diversity, Employee relations, Environment, Human rights, and Product.<sup>24</sup> Within each of these categories, MSCI rates firms in terms of several CSR strengths and CSR concerns. In total, MSCI rates 70 strengths and concerns across all six categories. For example, in the ‘Community’ category, MSCI assigns a *strength* score if the firm supports housing initiatives for the economically disadvantaged. By contrast, MSCI assigns a *concern* score if a firm has been involved in serious controversies

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<sup>23</sup> Prior studies constructing an overall CSR investment measure based on MSCI data include, but are not limited to, Graves and Waddock (1994), Hong and Kostovetsky (2012), Deng et al. (2013), Jiraporn and Chintrakarn (2013), Servaes and Tamayo (2013), Di Giuli and Kostovetsky (2014), Gao et al. (2014), Davis et al. (2016), Cronqvist and Yu (2017), and Lins et al. (2017).

<sup>24</sup> MSCI also rates firms in terms of corporate governance and further controversial business practices, such as alcohol or tobacco. Prior studies usually do not consider these, because corporate governance is different from CSR, and controversial business practices primarily capture whether firms are operating in a ‘sin’ industry, which cannot be changed via investments but only via quitting operations in such industries.



with indigenous people. The overall CSR score is computed by adding up the individual strengths scores and subtracting the individual concerns scores. Specifically:

$$CSR\ Overall = \sum_{k=1}^K MSCI\ Strength_k - \sum_{k=1}^K MSCI\ Concern_k \quad (2)$$

As a result, a higher value of the variable *CSR Overall* corresponds to a higher overall CSR investment level.

Using overall CSR investment levels does not allow us to detect the different sources of managers' preferences in terms of CSR. This is because an increase in firms' overall CSR investments, following a UD law being adopted, could either be due to managers' preference for systematically over-investing in CSR (empire-building preference), or to managers' preference for mis-investing in specific CSR projects (pet-project preference). Likewise, a decrease in firms' overall CSR investments, following a UD law being adopted, could either be due to managers' preference for generally under-investing in CSR (quiet-life preference), or to managers' preference for solely forgoing risky CSR investments (playing-it-safe preference).

To overcome this measurement obstacle, we decompose firms' overall CSR investments into *material* and *immaterial*.<sup>25</sup> The rationale behind this decomposition is that not all CSR investments (i.e., not all MSCI CSR strengths and concerns) are equally material to all firms—rather, the materiality of different CSR investments varies systematically across firms and industries (Khan et al., 2016). To identify which MSCI strengths and concerns are material to a specific firm, we use the newly available SASB industry-specific materiality classification.

The SASB, an independent 501(c)(3) non-profit organization, adopts a shareholder viewpoint;<sup>26</sup> for this purpose, it follows the materiality definition of the SEC, which, in turn, is built on the US Supreme Court's interpretation of materiality.<sup>27</sup> The shareholder focus and legal character of the SASB's approach to materiality makes their materiality classification particularly well suited to our study, mainly for two reasons. First, CSR investments that are identified as immaterial, according to the Supreme Court's own interpretation of materiality, are easier to challenge (e.g., in a derivative

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<sup>25</sup> The decomposition procedure generally follows Khan et al. (2016), but uses the latest version of the SASB industry classification (79 industries instead of 45) as well as the newly available SASB industry 'Look-up Tool'.

<sup>26</sup> The SASB's shareholder viewpoint is narrower compared with that of other organizations, such as the Global Reporting Initiative, which adopts a multi-stakeholder viewpoint (Khan et al., 2016).

<sup>27</sup> *TSC Industries v. Northway Inc.* (426 U.S. 438, 449, 1976). See also *Basic Inc. v. Levinson* (485 U.S. 224, 1988).

lawsuit). Second, CSR investments that are immaterial to shareholders reflect privately optimal investments. Specifically, the SASB classifies a CSR issue as immaterial if, among other criteria, it is not related to firms' core business activities. For example, the CSR issue 'Biodiversity impacts' is not related to the core business activities of firms operating in the technology service industry, and is therefore, classified as immaterial. As a result, we argue that immaterial CSR issues reflect investments in managers' own CSR pet projects that are *unrelated* to firms' business activities.

By 2017, the SASB had classified 23 CSR issues for 79 industries within 10 sectors into material and immaterial (see Figure A1). Because a firm's industry affiliation is central to the materiality classification, the SASB has established its own industry classification system, which rearranges the industries based on the similarity of firms' CSR challenges and CSR investment opportunities, rather than on traditional financial considerations.<sup>28</sup> We use the SASB industry 'Look-up Tool' to identify firms' SASB industry affiliation.<sup>29</sup> For those firms not included in the SASB database, we hand-map the SASB industry code to the SIC code obtained from Compustat.

Next, we hand-map each of the 70 MSCI strengths and concerns to the 23 SASB CSR issues outlined in Figure A1. For example, the MSCI concern 'Hazardous waste' (ENV-con-A) is mapped to the SASB CSR issue 'Waste and hazardous materials management', the MSCI strength 'Gay & lesbian policies' (DIV-str-G) is mapped to the SASB CSR issue 'Diversity and inclusion', and the MSCI strength 'Support for housing' (COM-str-C) is mapped to the SASB CSR issue 'Access and affordability'.<sup>30</sup>

Based on the industry assignment and the MSCI materiality classification, we construct one material CSR score and one immaterial CSR score for each firm-year, respectively. In doing so, we follow the standard procedure used in the literature by adding up strengths and subtracting concerns, as described in the first paragraph of this section. Specifically, we compute the two

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<sup>28</sup> <https://sasb.org/innovation-scenes-sics>

<sup>29</sup> The SASB industry 'Look-up Tool' includes the SASB industry affiliation of 13,000+ US publicly listed firms. The SASB industry 'Look-up Tool' can be accessed via <https://sasb.org/approach/sics>.

<sup>30</sup> MSCI strengths and concerns, which are too broad to be mapped to a specific SASB CSR issue, are mapped to an SASB CSR *category*. For example, the MSCI strength 'Other environment strengths' (ENV-str-X) is mapped to the SASB CSR category 'Environment' and not to a specific SASB CSR issue *within* the category 'Environment'. Therefore, if any of the seven specific CSR issues within the overarching CSR category 'Environment' is material, the MSCI strength ENV-str-X is material.

materiality variables as follows:

$$CSR\ Material = \sum_{k=1}^K \text{Material MSCI Strength}_k - \sum_{k=1}^K \text{Material MSCI Concern}_k \quad (3)$$

$$CSR\ Immaterial = \sum_{k=1}^K \text{Immaterial MSCI Strength}_k - \sum_{k=1}^K \text{Immaterial MSCI Concern}_k \quad (4)$$

As a result, a higher value of the variable *CSR Material* corresponds to higher material CSR investments, and a higher value of the variable *CSR Immaterial* corresponds to higher immaterial CSR investments. These two variables allow us to distinguish between managers' preference for building a social empire and managers' preference for supporting their own CSR pet projects. Because immaterial CSR investments are *unrelated* to firms' core business activities, we argue that they reflect managers' own CSR pet projects (Grewal et al., 2017). By contrast, material CSR investments are *related* to firms' core business activities and, therefore, are generally in shareholders' best interests. However, McWilliams and Siegel (2001) argue that there is an optimal level of (material) CSR investments. For example, they posit that when a firm is dependent on a highly skilled labor force, sells more differentiated products, or can exploit scale economies, their optimal level of CSR investments is higher. The optimal level of material CSR investments is, in principle, attained when managers' are disciplined—that is, when agency problems are low. Therefore, if managers increase material CSR investments after a UD law is adopted, we argue that they over-invest in material CSR, which reflects their underlying preference for building a social empire.

While the decomposition into material and immaterial CSR allows us to distinguish between managers' preference for building a social empire and managers' preference for supporting their own CSR pet projects, it does not allow us to distinguish between managers' preference for enjoying the quiet life and managers' preference for playing it safe. A decrease in firms' material CSR investments following a UD law being adopted, could either be due to managers' preference for generally under-investing in material CSR (quiet-life preference), or to managers' preference for solely forgoing material CSR investments that are *risky* (playing-it-safe preference). To circumvent this issue, we decompose material CSR investments into *risky* and *non-risky* based on in-depth evaluation of each

MSCI CSR issue. We identify five individual MSCI strengths as particularly risky,<sup>31</sup> and all of these are related to either green (environmental) innovation activities or product innovation activities. Theories and empirical evidence underscore the riskiness of innovation projects and the tolerance for failure in motivating innovation (Azoulay et al., 2011; Tian and Wang, 2014; Ghosh et al., 2017). We compute our *CSR Risky* variable as follows:

$$CSR\ Risky = \sum_{k=1}^K \text{Risky Material MSCI Strength}_k \quad (5)$$

As a result, a higher value of the variable *CSR Risky* corresponds to higher material CSR investments that are particularly risky.

## 4. Sample, data, and summary statistics

The sample in this study consists of all firms covered by the MSCI database between 1991 and 2013. The sample period starts in 1991 because MSCI does not provide data for earlier years.<sup>32</sup> We exclude financial firms,<sup>33</sup> firms located or incorporated outside the US, firms that have changed their state of incorporation,<sup>34</sup> firms with missing SIC codes, and firm-year observations with either missing or negative total assets or sales. This yields a sample of 23,190 firm-year observations.

We obtain data on CSR investments from MSCI, which we hand-map to the industry-specific materiality classification of the SASB, as described in the previous section. Further, we obtain financial data and SIC codes from Compustat. Finally, we obtain state of incorporation data and state of location data from various sources, as described below.

Whether a firm is subject to a UD law depends on its state of *incorporation*. Because firms can change their state of incorporation, it is important to correctly identify firms' *historical* state of incorporation. Unfortunately, Compustat only provides firms' current state of incorporation.

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<sup>31</sup> The five MSCI strengths identified as particularly risky are ENV-str-A, ENV-str-B, ENV-str-C, ENV-str-D, and PRO-str-B.

<sup>32</sup> To the best of our knowledge, no other database provides CSR investment data for years prior to 1991. In fact, most other databases, such as Thomson Reuters' ASSET4 database, start their data coverage much later.

<sup>33</sup> We exclude financial firms because of their exposure to regulatory oversight. In un-tabulated tests, we also exclude utilities firms because they too are exposed to some regulatory oversight. The un-tabulated estimates are qualitatively similar to the ones reported in Table 3, which indicates that our inferences are not driven by the inclusion of utility firms.

<sup>34</sup> We exclude firms that have changed their state of incorporation to avoid endogenous changes—i.e., firms may change their state of incorporation *because of* a UD law being adopted. Approximately 4% of the firms included in our sample have changed their state of incorporation.

Therefore, we collect information on firms’ historical state of incorporation from various sources. For the years 1991–2006, we obtain historical state of incorporation data from Gormley and Matsa (2016), who collected this data from SEC disclosure compact discs, Compustat back-tapes, SEC Analytics, 10-K SEC filings, Moody’s Manuals, and the legacy version of Compustat. For the years 1997–2010, we obtain historical state of incorporation data from Bill McDonald,<sup>35</sup> who collected this data from the headers of firms’ 10-K SEC filings. For the years 2011–2013, we follow Bill McDonald and hand-collect historical state of incorporation data from the headers of firms’ 10-K SEC filings. We obtain information on firms’ state of *location* in the same manner.

For some tests (e.g., robustness tests), we use alternative samples and require data from further sources, including Execucomp, Thomson Reuters, Audit Analytics, Factiva, and the National Bureau of Economic Research (NBER). We describe these samples and data as they arise.

The summary statistics in Table 2 show that firms incorporated in states that have adopted a UD law are similar to firms in other states three years prior to a UD law being adopted. This helps satisfy the parallel trends assumption, which is implicit in a difference-in-differences estimator. Column (1) of Table 2 reports the variable means and standard deviations for firms incorporated in states adopting a UD law. Column (2) of Table 2 reports the variable means and standard deviations for firms incorporated in states *not* adopting a UD law. Column (3) of Table 2 reports the  $p$ -values from  $t$ -tests for differences between the sample means. The  $p$ -values, which are all well below the 0.05 margin, indicate no statistically significant differences between ‘UD law’ firms and ‘No UD law’ firms three years prior to the states adopting a UD law in terms of CSR investments, size, leverage, and SG&A expenses.

## 5. Empirical findings

### 5.1 Main findings

Table 3 presents our main findings. All regressions are based on Equation (1). Column (1) of Table 3 shows that managers increase *overall* CSR investments after a UD law is adopted. The positive coefficient on *UD Law*, which captures the difference-in-differences treatment effect, is statistically significant at the 1% level and economically meaningful. After a UD law is adopted, managers

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<sup>35</sup> <https://www3.nd.edu/~mcdonald/10-K-Headers/10-K-Headers.html>

increase *overall* CSR investments by about 20% ( $= 0.386/1.950 \times 100$ ) of the pre-law standard deviation. While this finding suggests that managers derive private benefits from investing in CSR, their underlying preference is less clear. Do they increase CSR investments because they prefer to support *specific* CSR pet projects, or because they prefer to *systematically* over-invest in CSR to build a social empire?

Column (1) of Table 3 shows that managers increase *immaterial* CSR investments after a UD law is adopted. The positive coefficient on *UD Law* is statistically significant at the 1% level and economically meaningful. After a UD law is adopted, managers increase investments in immaterial CSR by about 19% ( $= 0.292/1.571 \times 100$ ) of the pre-law standard deviation. Because immaterial CSR investments are unrelated to firms' business activities, this finding implies that managers have an underlying preference for supporting their own CSR pet projects.

Turning to Column (3) of Table 3, we find that managers also increase *material* CSR investments after a UD law is adopted. The positive coefficient on *UD Law* is statistically significant at the 1% level and economically meaningful. After a UD law is adopted, managers increase investments in material CSR by about 25% ( $= 0.270/1.062 \times 100$ ) of the pre-law standard deviation. This finding suggests that managers over-invest in material CSR—that is, invest beyond the optimal level—because they have an underlying preference for building a social empire.

The findings in Columns (2) and (3) of Table 3 can be interpreted in two different ways. First, *some* managers prefer to support their own CSR pet projects (i.e., increase immaterial CSR), while *others* prefer to build a social empire (i.e., increase material CSR). Second, increasing immaterial CSR is part of the *average* manager's preference for building a social empire. To find which of these two interpretations holds empirically, we test for interrelations (complementary effects) between material and immaterial CSR investment decisions. In doing so, we follow Abernethy et al. (2015) and test whether the residuals of the two regressions—Column (2) and Column (3) of Table 3—correlate with each other,<sup>36</sup> and find that the residuals strongly positively correlate with each other ( $\rho = 0.512$ ). The correlation is also statistically significant at the 1% level. This finding indicates that after a UD law is adopted, managers increase immaterial CSR investments and material CSR

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<sup>36</sup> We correlate the residuals of the ordinary least squares (OLS) regressions with each other, and do not run a seemingly unrelated regression (SUR), because both regressions include the same set of explanatory variables. In such a case, OLS and SUR yield the same estimates and, hence, correlating the residuals of the OLS regressions is appropriate, i.e., yields the same  $\rho$  as under SUR.

investments *simultaneously*. Expressed another way, immaterial and material CSR investments are complements and not substitutes, which, in turn, suggests that immaterial CSR investments are part of the average manager’s preference for building a social empire.

To better understand the economic significance of our results, we follow Di Giuli and Kostovetsky (2014) and convert the effect of *UD Laws* on *CSR Immaterial* and *CSR Material*, respectively, into direct monetary costs.<sup>37</sup> In doing so, we regress the natural logarithm of SG&A expenses on *CSR Immaterial* and *CSR Material*, respectively.<sup>38</sup> As expected, Table A2 shows that both *CSR Immaterial* and *CSR Material* increase SG&A expenses, respectively. In particular, a one unit increase in *CSR Immaterial* is associated with a 2.3% increase in SG&A expenses, which comes to \$16 million ( $= 0.023 \times \$679$  million) for the average pre-law firm. A one unit increase in *CSR Material* is associated with a 1.7% increase in SG&A expenses, which comes to \$12 million ( $= 0.017 \times \$679$  million) for the average pre-law firm. Accordingly, the increase in *CSR Immaterial* caused by the adoption of UD laws costs the average pre-law firm approximately \$5 million ( $= 0.292 \times \$16$  million). The increase in *CSR Material* caused by the adoption of UD laws costs the average pre-law firm approximately \$3 million ( $= 0.270 \times \$12$  million). Overall, building a social empire costs the average pre-law firm an extra \$8 million per year, which represents over 5% of its net income. These numbers demonstrate that the costs of building a social empire are not trivial.

Finally, Column (4) of Table 3 shows that after a UD law is adopted, managers do not change their investment behavior in terms of *risky* CSR. This indicates that managers do not have a preference for playing it safe. Collectively, the results of Table 3 suggest that managers have an underlying preference for building a social empire. By contrast, supporting pet projects, enjoying the quiet life, or playing it safe are not the norm when it comes to CSR.

For the sake of brevity, we focus on the effect of *UD Law* on *CSR Immaterial* and *CSR Material*, respectively, in all further tests.

## 5.2 Validity

For the results of Table 3 to be valid, our identification strategy must fulfill two requirements: the inclusion restriction and the exclusion restriction. While we have provided arguments in favor of

<sup>37</sup> Many CSR projects show up in higher SG&A expenses, such as corporate donations, childcare projects, pollution prevention projects, or employee health projects (Di Giuli and Kostovetsky, 2014).

<sup>38</sup> Analyzing the economic significance of our estimates in terms of SG&A fits our study well because Chen et al. (2012) show that SG&A reflect managerial empire building.

both requirements in Section 2, we now run some tests to empirically support these arguments.

### 5.2.1 *Inclusion restriction*

To satisfy the inclusion restriction, the treatment (i.e., the adoption of UD laws) must reduce the incidence of derivative lawsuits to reduce managers' exposure to, or threat of, derivative lawsuits. In addition, if UD laws reduce the incidence of derivative lawsuits, this reduction must not be offset by an increase of other forms of litigation, such as direct (class action) lawsuits.

Appel (2016) shows that adopting a UD law does indeed yield substantial decreases in derivative lawsuits and, at the same time, does not increase the incidence of direct (class action) lawsuits. Building on these findings, we test whether adopting a UD law decreases the incidence of *CSR-related* derivative lawsuits, and whether adopting a UD law increases *CSR-related* direct class action lawsuits.<sup>39</sup> In doing so, we follow Appel (2016) and collect data on derivative lawsuits from Audit Analytics and 10-K SEC filings. To identify as many derivative lawsuits as possible, we base this analysis on the Compustat universe between 1994 (the first year for which electronic SEC filings are available) and 2013. Over 25% of the identified derivative lawsuits are related to CSR issues.<sup>40</sup> The sample of direct class action lawsuits is also based on the Compustat universe between 1996 (adoption of the Private Securities Litigation Reform Act) and 2013. We collect data on direct class action lawsuits from Audit Analytics and the Stanford Securities Class Action Clearinghouse (SCAC) database. Approximately 15% of the identified direct class action lawsuits are related to CSR.

Column (1) of Table 4 shows that adopting a UD law reduces the likelihood of derivative lawsuits. The coefficient on *UD Law* is negative and statistically significant at the 5% level. The magnitude of this coefficient is also sizable, suggesting that UD laws have a significant effect on the prevalence of shareholder derivative lawsuits. Column (2) of Table 4 shows that adopting a UD law has no effect on the likelihood of direct class action lawsuits, suggesting that reduced likelihood of derivative lawsuits is not offset by an increase in the likelihood of direct class action lawsuits. Taken together, these findings empirically underscore the satisfaction of the inclusion restriction.

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<sup>39</sup> Although derivative lawsuits are particularly well suited to targeting CSR issues, direct (class action) lawsuits are also used to target CSR issues, as the current Rana Plaza class action lawsuit (*Das v. George Weston Limited*, ONSC 4129, 2017) shows.

<sup>40</sup> Please see Table A7 for an example of a derivative lawsuit that is related to CSR (product quality and safety).



### 5.2.2 *Exclusion restriction*

To satisfy the exclusion restriction, the treatment (i.e., adoption of UD laws) must be truly exogenous with respect to CSR investments. We have argued that UD laws are exogenous to the extent that they are adopted by states and, therefore, are not endogenously driven by firm-specific characteristics. In this section, we discuss three potential identification concerns that could still drive our results.

**Lobbying.** A remaining identification concern is that managers use their political connections to lobby for the adoption of UD laws. In particular, if managers who prefer to mis-invest in immaterial CSR and over-invest in material CSR, lobbied for UD laws to be adopted, then our results would be driven by endogeneity (i.e., reverse causation). To rule out this concern, we search the database of the Center for Responsive Politics, which contains information about US firms' lobbying behavior from 1998 onward; we find no evidence for lobbying activities related to UD laws. While the absence of such qualitative evidence helps mitigate lobbying concerns, it does not provide rigorous empirical evidence. Therefore, we perform three additional empirical tests to mitigate lobbying concerns.

First, we apply a Weibull hazard model in the spirit of Kroszner and Strahan (1999) to test whether higher levels of pre-existing material and immaterial CSR investments, respectively, affect the timing of states adopting UD laws. If managers of firms with higher levels of material and immaterial CSR lobbied to adopt a UD law, then we would expect that the states where these firms are incorporated would adopt a UD law earlier. The dependent variable of our Weibull hazard model is the natural logarithm of the time expected until UD laws are adopted, or 'time until deregulation'. The explanatory variable is the mean of firms' material and immaterial investment levels at the state of incorporation level, respectively. The hazard ratios, as reported in Table 5, represent the likelihood that a state adopts a UD law in year  $t$ , given that it has not yet adopted a UD law. The hazard ratios on  $State(CSR\ Immaterial)$ , reported in Column (1) of Table 5, and the hazard ratio on  $State(CSR\ Material)$ , reported in Column (2) of Table 5, are both not statistically significant at conventional levels. This indicates that pre-existing material and immaterial CSR investments do not affect the timing of UD laws being adopted, which mitigates lobbying concerns.

Second, we examine the dynamics of the treatment effect (i.e., adoption of UD laws). To do this, we add four pre-treatment dummy variables indicating the four years prior to the actual UD

laws being adopted (*UD Law* (−1), *UD Law* (−2), *UD Law* (−3), and *UD Law* (−4)). If managers’ anticipate a UD law being adopted because they lobbied for it, then the coefficients on these pre-treatment variables should be positive and statistically significant. We find no evidence for such pre-existing trends. The coefficients on all pre-treatment variables in Table 6 (both columns) are not statistically significant at conventional levels. The precise timing of the changes in CSR investments (material and immaterial, respectively) suggests that these changes are, in fact, caused by a UD law being adopted, which, in turn, mitigates our lobbying concerns.

Third, we restrict the sample of treated firms to an environment in which lobbying concerns are likely muted. Specifically, we restrict the sample of treated firms to those incorporated in Pennsylvania, where the Supreme Court of Pennsylvania decided to adopt a UD law for the sake of consistency with judicial precedent<sup>41</sup> and, hence, was arguably unlikely the result of lobbying activities. In Table 7, we restrict the sample to the years 1992–2002—that is, five years before and five years after the UD law was adopted in Pennsylvania. Because of the substantially smaller sample size, we only include firm fixed effects and year fixed effects. The coefficients on *UD Law* in Table 7 are in the same direction as in the full sample and remain statistically significant at the 1% level. Again, this finding is reassuring because it further mitigates our lobbying concerns. Taken together, the qualitative analysis and three empirical tests suggest that our main results are unlikely to be driven by lobbying or, on a more general note, reverse causality.

**Political economy.** Another concern related to the exclusion restriction is that changes in the local economy may drive both the adoption of a UD law *and* the increases in CSR investments. For example, if a state’s economy is booming, politicians may seize this opportunity to adopt a UD law. At the same time, the booming economy may spur CSR investments. We have already minimized this concern by controlling for state of location-by-year fixed effects—that is, our estimates are identified using only the differential response to an UD law being adopted for firms located in the same state. However, what if politicians are more sensitive to changes in the local economy that affect firms *located in their state* compared with those that affect firms *located in other states*? If this is the case, (political economy-driven) endogeneity may still affect our results. We address this concern with an additional test that isolates the differential within-state responses to UD laws.

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<sup>41</sup> *Cuker v. Mikalauskas* (547 Pa. 600, 692 A.2d 1042, 1997). The fact that only four months passed between the initiation of the derivative lawsuit in *Cuker* and the court’s decision further mutes lobbying concerns.

Specifically, we follow Gormley and Matsa (2016) and obtain estimates by allowing for a differential effect for firms incorporated and located in the same state, and for firms incorporated and located in different states. Table 8 reports these estimates. We find that both managers of firms that are incorporated and located in the same state, as well as managers of firms that are incorporated and located in different states, increase material and immaterial CSR investments following an UD law being adopted. These findings, particularly those for firms incorporated and located in different states, mitigate concerns that our results are driven by local political economic factors—that is, political economy-driven endogeneity.

**Confounding laws.** Our final concern related to the exclusion restriction is that the adoption of a UD law may coincide with the adoption of another state-level law. We are particularly concerned about one specific legal change: the adoption of constituency statutes. Constituency statutes provide managers “with a legally enforceable mechanism—beyond case law and the business judgment rule—for considering stakeholder interests” (Flammer and Kacperczyk, 2016, p. 1987). This means that if the adoption of a UD law coincides with the adoption of a constituency statute, it is not possible to disentangle our empire-building interpretation from a stakeholder-oriented interpretation.<sup>42</sup> Between 1984 and 2006, 34 US states adopted a constituency statute. During our sample period, three states adopted both a UD law and a constituency statute: Connecticut, North Carolina, and Texas. To alleviate the concern that our results are driven by a constituency statute, rather than a UD law being adopted, we 1) control for the adoption of constituency statutes and 2) exclude the three states that adopted both a constituency statute and a UD law during our sample period. In Table 9, Panel A, when we control for the adoption of UD laws, the coefficients on *UD Law* are positive and remain statistically significant at the 1% level. In Table 9, Panel B, when we exclude Connecticut, North Carolina, and Texas, the coefficients on *UD Law* are, again, positive and remain statistically significant at the 1% level. Collectively, these findings alleviate our concerns about confounding laws, particularly constituency statutes.

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<sup>42</sup> Stakeholder orientation means that managers increase CSR investments because they focus more on stakeholders’ demands.

## 5.3 Robustness

### 5.3.1 Including control variables

In our main specification, Equation (1), we deliberately exclude firm-level control variables because they are likely endogenous to legislative changes, which, in turn, can lead to inconsistent estimates (Bertrand and Mullainathan, 2003; Appel, 2016; Gormley and Matsa, 2016). As robustness analyses, we include firm-level control variables that are *unlikely* to be endogenous to UD laws. Appel (2016) shows that adopting a UD law has *no* effect on the following firm-level variables: *Size*, *Leverage*, *Capex*, and *Dividend*.<sup>43</sup> As shown in Table A3, Panel A, our main results are robust to the inclusion of these firm-level variables. Further, Appel (2016) shows that adopting a UD law has also *no* effect on *CEO Total Pay*.<sup>44</sup> As shown in Table A3, Panel B, the results are robust to the inclusion of *CEO Total Pay* along with the firm-level control variables already included in Panel A.

### 5.3.2 Restricting the sample of control firms

An important characteristic of the US corporate landscape is that over half of the public firms are incorporated in Delaware (Bebchuk and Cohen, 2003). Since Delaware did not adopt a UD law during our sample period, firms incorporated in Delaware are in the control group. Hence, if Delaware firms are investing less in CSR over time, our main results could be spurious, merely reflecting a 'Delaware effect'. To address this concern, we exclude firms incorporated in Delaware. Because of the substantially smaller sample size, we only include firm fixed effects and year fixed effects. Table A4, Panel A reports the results. The coefficients on *UD Laws* are positive and remain statistically significant at the 1% level, suggesting that our results are not driven by a 'Delaware effect'.

Another interesting observation is that most of the states have adopted a UD law closely related to the Model Business Corporation Act (MBCA), a model set of corporate laws created by the American Bar Association (Appel, 2016). By restricting our sample of control firms to those incorporated in states that closely follow the MBCA,<sup>45</sup> we address the concern that our results

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<sup>43</sup> The data source for all these variables is Compustat. Please see Table A1 for variable definitions.

<sup>44</sup> The data source for this variable is Execucomp. The variable is defined in A1.

<sup>45</sup> Specifically, the control sample comprises firms incorporated in MBCA states: Alabama, Colorado, Illinois, Kentucky, Maryland, New Mexico, North Dakota, Oregon, South Carolina, Tennessee, and Washington. The list of MBCA states is from Appel (2016).

could be spurious, resulting from incorporation in an MBCA state rather than from adoption of a UD itself. Again, because of the substantially smaller sample size, we only include firm fixed effects and year fixed effects. Table A4, Panel B reports the results. The coefficients on *UD Laws* are positive and remain statistically significant at the 1% level, suggesting that our results are not driven by an 'MBCA effect'.

### 5.3.3 *Alternative measures of CSR investments*

Measuring (quantifying) CSR investments is difficult. The only CSR data available to date are CSR investment ratings. An issue with all ratings is that they are subjectively influenced by rating analysts or the rating process itself (Dilly and Mählmann, 2015; Krüger, 2015). Therefore, it is possible that our results are driven by the MSCI rating process rather than reflecting CSR investments. In our setting, using alternative CSR ratings is not feasible because data coverage does not start before 2002 (e.g., ASSET4). What we do instead is focus on related CSR data, namely corporate donations and green (environmental) patents. These data are particularly suitable for testing the robustness of our results because donations and patents are 'raw' data and thus not subject to a rating process. In addition, corporate donations are particularly prone to agency problems (Masulis and Reza, 2015), and green patents reflect the outcome of risky CSR investments (Carrion-Flores and Innes, 2010). Therefore, for our main results to be robust to measurement concerns, UD laws must positively affect corporate donations, but not affect green patents. We hand-collect data on corporate donations from the National Directory of Corporate Giving (NDCG).<sup>46</sup> Data on patents come from the NBER database.<sup>47</sup> Our main classification of green patents follows that of Carrion-Flores and Innes (2010). Table A5 presents the results of the regressions with the two alternative dependent variables.<sup>48</sup> Consistent with our main results, we find that adopting a UD law positively affects corporate donations (Column (1) of Table A5), but does not affect green patents (Column (2) of Table A5).

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<sup>46</sup> The sample comprises Standard & Poor's (S&P) 500 firms for the years 1997–2008. Unfortunately, since data for 1998 and 2001 are not available, we linearly interpolate the data to fill in the missing years.

<sup>47</sup> The sample comprises firm-year observations from 1976 to 2006 (the last year of available NBER data).

<sup>48</sup> In Column (1) of Table A5, we only include firm and year fixed effects because of the substantially smaller sample size.

## 5.4 *Managers' ex ante incentives to empire build*

To strengthen our empire-building interpretation, we examine cross-sectional heterogeneity in managers' responses to UD laws by exploiting differences in their ex ante incentives to empire build.

### 5.4.1 *Empirical specification for identifying cross-sectional heterogeneity*

Methodologically, we have to modify our main empirical specification so we can compare managers' responses based on ex ante cross-sectional differences. We particularly want to test whether managers with different incentives in the year prior to a UD law being adopted, denoted as year  $T-1$ , respond differently to the adoption of that UD law. Because of the staggered adoption of UD laws, we have multiple years in which UD laws have been adopted, meaning that no unique  $T-1$  period exists for each firm in the panel. This makes it difficult to test for heterogeneous responses. To overcome this difficulty, we follow Gormley and Matsa (2011) and use a matching difference-in-differences estimator.<sup>49</sup> For each year in which a new UD law has been adopted, we identify firms incorporated in states that have adopted a UD law that year, and compare them with firms incorporated in the other states. The sample of this matching approach consists of cohorts, with each cohort covering firm-year observations in the three years before and the three years after a UD law being adopted.<sup>50</sup> We allow firms to be selected as matches in multiple cohorts (i.e., we match with replacement). By generating such matched cohorts, we are able to account for  $T-1$  cross-sectional differences; in doing so, we could run separate regressions for each cohort that account for  $T-1$  cross-sectional differences and report the average effect across all regressions. Fortunately, separate regressions for each cohort are not necessary in practice. Instead, we can pool the data across all cohorts and estimate the average effect based on the following specification:

$$y_{icjlst} = \beta_2(UD\ Law_{st} \times d_{icjlsT-1}^H) + \beta_3(UD\ Law_{st} \times d_{icjlsT-1}^L) + \gamma_c + \delta_i + \lambda_{jt} + \omega_{lt} + \varepsilon_{icjlst} \quad (6)$$

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<sup>49</sup> Switching to the matching difference-in-differences estimator does change our main findings. The un-tabulated coefficients on *UD Law* are the same as in Table 3 in terms of sign and significance level, and similar in terms of magnitude.

<sup>50</sup> Prior studies generally focus on the 10 years before and after each law's adoption. However, using such a long time span would leave us with only five UD law adoptions (instead of 14) because CSR (MSCI) data are not available for years prior to 1991. Therefore, we use the shorter three year time span to maximize the number of UD law adoptions included in our analyses.

where  $y$  denotes the outcome variables for firm  $i$ , in cohort  $c$ , industry  $j$ , state of location  $l$ , state of incorporation  $s$ , and year  $t$ . As before, *UD Law* is the 'treatment' variable.  $d_{icjlsT-1}^H$  is the first partitioning variable: a dummy variable equal to one if  $d$ , a firm-level variable such as free cash flow, is *equal to or above* the sample median one year prior to a UD law being adopted ( $T-1$ ), and zero otherwise.  $d_{icjlsT-1}^L$  is the second partitioning variable: a dummy variable equal to one if  $d$  is *below* the sample median one year prior a UD law being adopted, and zero otherwise.  $\gamma_c$  are cohort fixed effects. All remaining fixed effects are the same as before.  $\varepsilon$  is the error term. The interaction terms (i.e.,  $UD\ Law \times d^H$  and  $UD\ Law \times d^L$ ) allow us to examine heterogeneity in the effect of UD laws. The corresponding coefficients of interest are  $\beta_2$  and  $\beta_3$ . For example, the two coefficients (together with a 'coefficient equality test') indicate whether managers of firms with high versus low free cash flows respond differently to a UD law being adopted.

In essence, this matching difference-in-differences estimator with two interaction terms allows us to examine cross-sectional heterogeneity in managers' responses to a UD law being adopted, even if the partitioning variable ( $d$ ) itself is affected by that UD law adoption.

#### 5.4.2 *Free cash flow, leverage, and fixed CEO pay*

Agency theory predicts that managers have greater incentives to empire build when free cash flow is abundant, leverage is scarce, and their compensation is not hedged by fixed pay (Jensen, 1986; Kanniainen, 2000). Consistent with this theory, we find that only managers of firms with above-median free cash flow, below-median leverage, and below-median fixed pay increase immaterial and material CSR investments after a UD law is adopted. Managers of firms with below-median free cash flow, above-median leverage, and above-median fixed pay do *not* increase immaterial and material CSR investments after a UD law is adopted. The estimates are reported in Table 10. In Panel A, Column (1), we see that the coefficient on  $UD\ Law \times High\ Free\ Cash\ Flow$  is positive (0.363) and statistically significant at the 5% level. By contrast, the coefficient on  $UD\ Law \times Low\ Free\ Cash\ Flow$  is positive, but very small in terms of magnitude (0.083) and not statistically significant at conventional levels. The corresponding 'coefficient equality test' shows that the coefficients are statistically significant different from each other, although only at the 10% level ( $p$ -value = 0.065). In Panel A, Column (2), we see that the coefficient on  $UD\ Law \times High\ Free\ Cash\ Flow$  is also positive (0.428) and even statistically significant at the 1% level. By contrast, the coefficient on  $UD\ Law \times Low\ Free\ Cash\ Flow$

is positive but, again, very small in terms of magnitude (0.047) and not statistically significant at conventional levels. The corresponding 'coefficient equality test' shows that the coefficients are, again, statistically significant different from each other ( $p$ -value = 0.054). These results, as well as the results of Panels B and C of Table 10, corroborate our interpretation that managers mis-invest in immaterial CSR and over-invest in material CSR because they have an underlying preference for building a social empire.

### 5.5 *Alternative explanation: Does the threat of shareholder litigation pressure managers?*

A pivotal implication of seminal work in law and finance is that the threat of shareholder litigation helps resolve agency problems arising from the separation of ownership and control (Shleifer and Vishny, 1997; La Porta et al., 1998; La Porta et al., 2000). Building on this literature, recent studies show that, in particular, the threat of *derivative lawsuits* disciplines managers (Ferris et al., 2007; Davis, 2008; Erickson, 2010; Donelson and Yust, 2014; Houston et al., 2015; Appel, 2016; Ni and Yin, 2018; Li et al., 2017.) However, some scholars argue that derivative lawsuits pressure rather than discipline managers because they are frivolous, and primarily motivated by the settlement fees attorneys hope to extract (Fischel and Bradley, 1986; Macey and Miller, 1991; Romano, 1991; Lin et al., 2016).<sup>51</sup> If this is indeed the case, then prior to a UD law being adopted, managers (are pressured to) under-invest in material CSR and only increase material CSR investments to reach a level that is in shareholders' best interests after a UD law is adopted. To assess this alternative explanation, we conduct two sets of tests. In our first set, we estimate firms' optimal level of material CSR investments.<sup>52</sup> In doing so, we regress firms' material CSR investments on several determinants derived from the theoretical works of McWilliams and Siegel (2001) and Baron (2008).

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<sup>51</sup> Motivated by settlement fees, attorneys might urge shareholders to file a derivative lawsuit even though evidence indicates that breach of fiduciary duty is only minimal (Fischel and Bradley, 1986; Macey and Miller, 1991).

<sup>52</sup> This approach is related to the following studies: Richardson (2006), Biddle et al. (2009), and Lys et al. (2015).



Specifically, we run *yearly* regressions based on the following specification:

$$\begin{aligned}
CSR\ Material_{ijls} = & \varphi_1(Size_{ijls}) + \varphi_2(Profit_{ijls}) + \varphi_3(Capex_{ijls}) + \varphi_4(Diversification_{ijls}) \\
& + \varphi_5(R\&D_{ijls}) + \varphi_6(Advertising_{ijls}) + \varphi_7(Top\ Brand_{ijls}) + \varphi_8(Labor\ Union_{ijls}) \\
& + \varphi_9(Customer\ Concentration_{ijls}) + \varphi_{10}(Labor\text{-}to\text{-}Capital_{ijls}) + \varphi_{11}(SRI_{ijls}) \\
& + \varphi_{12}(Government\ Contract_{ijls}) + \varphi_{13}(Long\text{-}Term\ Shareholders_{ijls}) + \zeta_j + \eta_{ijls}
\end{aligned} \tag{7}$$

where  $i$  indexes firms,  $j$  indexes industries,  $l$  indexes state of location, and  $s$  indexes state of incorporation. Table A1 defines the dependent variable and all explanatory variables.  $\zeta_j$  are four-digit SIC industry fixed effects.  $\eta$  is the error term. We account for heteroskedasticity in the yearly regressions by using White standard errors. Although we use the estimates of the *yearly* regressions to construct our over-investment and under-investment variables, the results of a pooled regression are reported in Table A6.<sup>53</sup> All variables have the predicted sign and are statistically significant at the 10% level, at least. The model fit is good ( $R^2 = 0.33$ ), which mitigates the concern about important omitted variables contributing to explain the optimal level of *CSR Material* (Lys et al., 2015).<sup>54</sup> The residuals of the yearly regressions reflect the 'deviation' from the optimal investment level. Positive residuals reflect over-investment, while negative residuals reflect under-investments. Following Richardson (2006), we use the residuals of the yearly regressions to construct two variables. First, we replace negative residuals (which reflect under-investment) with the value of zero to generate our over-investment variable (*CSR Overinvest*). Second, we replace positive residuals (which reflect over-investment) with the value of zero to generate our under-investment variable (*CSR Underinvest*). Using these two variables separately in our main specification allows us to detect whether managers reduce under-investment or increase over-investment after a UD law is adopted. A reduction in under-investment indicates that UD laws pressure managers to under-invest in material CSR, while an increase in over-investment indicates that UD laws discipline managers. As shown in Column (1) of Table 11, managers increase over-investment in material CSR after a UD law is adopted. The coefficient on *UD Law* is positive and significant at the 1% level. By contrast, as shown in Column (2) of Table 11, managers do not reduce under-investment

<sup>53</sup> In the pooled regression, we also include time fixed effects and cluster standard errors at the firm level.

<sup>54</sup> In a robustness specification, we add three variables to Equation (7): *Firm Age*, *Earnings Volatility*, and *Market Share*. Table A1 defines these variables. The un-tabulated model fit ( $R^2 = 0.33$ ) does not change when we add these three variables.

in material CSR after a UD law is adopted. The coefficient on *UD Law* is not statistically significant at conventional levels.<sup>55</sup> To further strengthen the over-investment results, we construct a dummy variable that captures severe over-investment in material CSR. In particular, the newly constructed dummy variable, *CSR Overinvest Dummy*, equals one if the variable *CSR Overinvest* is above the 75<sup>th</sup> percentile (P75) of the sample, and zero otherwise. As shown in Column (3) of Table 11, managers severely over-invest in material CSR (i.e., they switch to an investment level that is in the top 25% of the sample) after a UD law is adopted. Finally, we take a look at the raw data. In Figure 1, we plot the means of the residuals for UD law firms and non-UD law firms separately. The figure shows that for non-UD law firms, the mean investment level roughly reflects an optimal investment level—that is, close to the value of zero. By contrast, for UD law firms, the mean clearly reflects over-investment—that is, the mean is clearly positive. The difference between the means is statistically significant at the 1% level. Therefore, the raw data support the argument that managers switch from a roughly optimal CSR investment level to a CSR investment level that reflects over-investment. Collectively, this first set of tests suggests that UD laws discipline rather than pressure managers.

In a second set of tests, we examine cross-sectional heterogeneity in managers' responses to UD laws by exploiting differences in firms' ex ante internal corporate governance. We argue that if shareholder derivative lawsuits pressure manager to under-invest in material CSR, then after a UD law is adopted, both managers of firms with weak internal corporate governance *as well as* managers of firms with strong internal corporate governance should increase material CSR investments (to reach a level that is in shareholders' best interests). However, if UD laws discipline managers, then after a UD law is adopted, *only* managers of firms with weak internal corporate governance should increase material CSR investments. We use the Entrenchment Index (*E-Index*) developed by Bebchuk et al. (2009) to measure cross-sectional differences in firms' internal corporate governance one year prior to a UD law being adopted. Methodologically, we rely on the specification put forth in Equation (6). Consistent with the disciplining notion, Table 12 shows that only managers of firms with weak internal corporate governance (i.e., *High E-Index*) increase material CSR investments after a UD law is adopted. Specifically, the coefficient on *UD Law*  $\times$  *High E-Index* is positive and

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<sup>55</sup> Because the variables *CSR Overinvest* and *CSR Underinvest* are censored variables (left- and right-censored, respectively), we re-estimate the regressions using a Tobit estimator. The un-tabulated coefficients are equal in terms of sign and significance level.

statistically significant at the 5% level. By contrast, managers of firms with strong internal corporate governance do not react to a UD law being adopted. The coefficient on  $UD\ Law \times Low\ E-Index$  is not statistically significant at conventional levels. Although the 'coefficient equality test' is not statistically significant, the difference between the two coefficients is qualitatively substantial (0.348 versus 0.002). These findings further support the notion that UD laws discipline rather than pressure managers.

## 6. UD laws, CSR press releases, and CSR disclosures

Managers prefer to build a social empire because it satisfies their hunger for fame, status, power, and prestige (Atkinson and Galaskiewicz, 1988), much of which arises from people knowing that those managers are running a social empire. This means that excessively investing in CSR is only half of the story; the other half is letting people know about these investments.<sup>56</sup> In this section, we take a look at the second half of the story by examining whether managers change their communication behavior in terms of CSR after a UD law is adopted. We focus on press releases because they capture active media management and primarily address the general public (Ahern and Sosyura, 2014), and on SEC filings because they capture disclosure management and primarily address shareholders (Griffin, 2003).

We collect CSR press releases published by the top three newswires (Reuters News, Dow Jones News Services, and Business Wire) from Factiva. To identify positive CSR press releases, we use 76 search terms that relate to positive CSR.<sup>57</sup> Based on the search results, we construct the variable *CSR Press*, which is the number of positive CSR press releases published by the top three newswires. Next, we perform a textual analysis in the spirit of Krüger (2015) to capture the tone of those press releases. Using the 1,915 words classified as positive by the Harvard IV-4 dictionary,<sup>58</sup> we construct the variable *CSR Press Tone*, which is the fraction of positive words in positive CSR newswire articles published by the top three newswires. Last, we construct our CSR disclosure variable *CSR*

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<sup>56</sup> From individual social responsibility research, we know that *anonymous* charitable donations represent only 1% of the total number of donations made (Glazer and Konrad, 1996). People buy social prestige: they are more likely to behave pro-socially if their friends and neighbors know about it (Bénabou and Tirole, 2010; Funk, 2010).

<sup>57</sup> The CSR search terms come from the descriptions of the 39 MSCI strengths. By using search terms related to the MSCI data, we connect our press release variables with our investment variables (which are based on MSCI data).

<sup>58</sup> For example, the following words are classified as positive by the Harvard IV-4 dictionary: ability, accomplish, achievement, admiration, and advantage. Please visit <http://wjh.harvard.edu/~inquirer> for more information on the Harvard IV-4 dictionary.

*10-K*, which is a dummy variable equal to one if the firm’s 10-K SEC filing covers positive CSR.<sup>59</sup> We use the same 76 search terms as before to identify whether positive CSR information is included in a firm’s 10-K SEC filing.<sup>60</sup>

The results reported in Table 13 show that after a UD law is adopted, managers issue more positive CSR press releases (Column (1)), and the tone of those press releases is more positive (Column (2)). The coefficients on *UD Law* are both economically meaningful and statistically significant (1% level). By contrast, we find only weak evidence that managers are more likely to include positive CSR information in 10-K SEC filings after a UD Law is adopted. The coefficient on *UD Law* in Column (3) is economically negligible and statistically significant at the 10% level only. Together, these findings suggest that managers want the general public to notice their social empire. They build social empires to satisfy their hunger for fame, status, power, and prestige.

## 7. Conclusion

This study systematically maps out managerial preferences in terms of CSR. For empirical identification, we exploit the staggered adoption of UD laws as a natural experiment. UD laws exogenously increase agency problems by insulating managers from shareholder litigation. We find that, after a UD law is adopted, managers both mis-invest in immaterial CSR and over-invest in material CSR. This suggests that managers have an underlying preference for building a social empire. Further analyses strengthen our empire-building interpretation and rule out alternative explanations. In auxiliary analyses, we show that communicating CSR to the general public is an integral part of managers building a social empire.

Knowing that managers have an underlying preference for building a social empire has important implications for designing effective CSR contracts.<sup>61</sup> In particular, effective CSR contracts either discourage empire-building managers from excessively investing in CSR, prevent self-serving managers from investing in their own CSR pet projects, motivate lazy managers to invest in funda-

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<sup>59</sup> An alternative approach would be to construct a disclosure index (Mallin et al., 2014). However, such an approach is more suitable for analyzing stand-alone CSR reports.

<sup>60</sup> CSR information included in 10-K SEC filings is hand-collected from electronic 10-K SEC filings. Because electronic SEC filings are not available prior to 1994, the sample period for any analysis that includes the variable *CSR 10-K* is confined to the years 1994–2013.

<sup>61</sup> CSR contracts link managers’ compensation to specific CSR criteria. By 2013, 37% of the S&P 500 firms had adopted CSR contracting (Flammer et al., 2016).

mental CSR, or encourage risk-averse managers to invest in risky CSR. While extremely valuable if well designed, CSR contracts that fail to countervail managers' underlying preferences can aggravate, rather than alleviate, agency problems. Therefore, by knowing that managers have an underlying preference for building a social empire, CSR contracts can be effectively designed to curtail such behavior.

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**Table 1**  
UD laws

State	Year
Georgia	1989
Michigan	1989
Florida	1990
Wisconsin	1991
Montana	1992
Virginia	1992
Utah	1992
New Hampshire	1993
Mississippi	1993
North Carolina	1995
Arizona	1996
Nebraska	1996
Connecticut	1997
Maine	1997
Pennsylvania	1997
Texas	1997
Wyoming	1997
Idaho	1998
Hawaii	2001
Iowa	2003
Massachusetts	2004
Rhode Island	2005
South Dakota	2005

This table reports the states that have adopted a UD law. To identify when states adopted a UD law, we use the dates reported in Appel ([2016](#)).

**Table 2**

Summary statistics before the adoption of UD laws

	(1)	(2)	(3)
	UD law	No UD law	<i>p</i> -value of difference
<i>CSR Overall</i>	−0.187 (1.950)	−0.211 (2.018)	0.590
<i>CSR Immaterial</i>	−0.155 (1.571)	−0.186 (1.719)	0.171
<i>CSR Material</i>	−0.036 (1.062)	−0.022 (1.066)	0.703
<i>CSR Risky</i>	0.209 (0.535)	0.215 (0.518)	0.571
<i>Size</i>	7.076 (1.448)	7.105 (1.540)	0.367
<i>Leverage</i>	0.206 (0.169)	0.210 (0.180)	0.336
<i>SG&amp;A</i> (\$ millions)	679.400 (1648.585)	765.726 (1877.358)	0.243

This table reports summary statistics in the three years prior to a UD law being adopted. Column (1) reports means and standard deviations (in parentheses) for firms incorporated in states adopting a UD law. Column (2) reports means and standard deviations (in parentheses) for firms incorporated in states not adopting a UD law. Column (3) reports the *p*-values from *t*-tests of the differences between affected and unaffected firms. Please see Table A1 for variable definitions.

**Table 3**

Main findings: UD laws and CSR investments

	(1) <i>CSR</i> <i>Overall</i>	(2) <i>CSR</i> <i>Immaterial</i>	(3) <i>CSR</i> <i>Material</i>	(4) <i>CSR</i> <i>Risky</i>
<i>UD Law</i>	0.386*** (0.128)	0.292*** (0.099)	0.270*** (0.071)	0.019 (0.074)
Firm fixed effects	yes	yes	yes	yes
Location $\times$ Year fixed effects	yes	yes	yes	yes
Industry $\times$ Year fixed effects	yes	yes	yes	yes
$R^2$	0.70	0.65	0.66	0.63
N	23,190	23,190	23,190	23,190
Correlation of residuals ( $\rho$ )	—	0.512***	—	—

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year observations from 1991 to 2013. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 4**

UD laws, CSR-related derivative lawsuits, and CSR-related direct class action lawsuits

	(1) <i>CSR</i> <i>Derivative</i> <i>Lawsuit</i>	(2) <i>CSR</i> <i>Direct</i> <i>Lawsuit</i>
<i>UD Law</i>	-0.010** (0.004)	0.001 (0.006)
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
$R^2$	0.21	0.19
N	65,678	52,542

This table reports coefficients and standard errors (in parentheses) from OLS regressions. In Column (1), the sample comprises firm-year observations for 1994 to 2013. In Column (2), the sample comprises firm-year observations for 1996 to 2013. Fixed effects are included as indicated. Industry fixed effects are based on three-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the firm level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 5**

Timing of adopting a UD law and pre-existing CSR investments

	(2) <i>Adoption Time</i>	(3) <i>Adoption Time</i>
<i>State(CSR Immaterial)</i>	0.655 (0.282)	— —
<i>State(CSR Material)</i>	— —	1.195 (0.272)
N	136	136

This table reports hazard ratios and standard errors (in parentheses) from Weibull hazard regressions. The sample comprises state of incorporation-year observations from 1991 up to and including the year in which a UD law is adopted. States that adopted a UD law before 1991 are excluded. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 6**

Dynamics of the treatment effect

	(1) <i>CSR Immaterial</i>	(2) <i>CSR Material</i>
<i>UD Law</i>	0.313** (0.132)	0.260** (0.115)
<i>UD Law</i> (−1)	0.249 (0.164)	0.087 (0.092)
<i>UD Law</i> (−2)	−0.210 (0.201)	−0.140 (0.103)
<i>UD Law</i> (−3)	−0.182 (0.187)	−0.099 (0.065)
<i>UD Law</i> (−4)	−0.242 (0.245)	−0.148 (0.101)
Firm fixed effects	yes	yes
Location × Year fixed effects	yes	yes
Industry × Year fixed effects	yes	yes
$R^2$	0.65	0.66
N	23,190	23,190

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year observations from 1991 to 2013. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.



**Table 7**  
Pennsylvania analysis

	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
<i>UD Law</i>	0.314*** (0.031)	0.147*** (0.026)
Firm fixed effects	yes	yes
Year fixed effects	yes	yes
$R^2$	0.79	0.81
N	3,829	3,829

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year observations from 1992 to 2002, i.e., five years before and five years after UD law adoption in Pennsylvania. Treated firms are restricted to those incorporated in Pennsylvania. Fixed effects are included as indicated. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 8**  
Within-state variation

	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
[1] <i>UD Law</i> $\times$ <i>Incorporated in Location State</i>	0.297*** (0.091)	0.266*** (0.071)
[2] <i>UD Law</i> $\times$ <i>Incorporated in Other State</i>	0.275** (0.138)	0.285** (0.114)
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
$R^2$	0.65	0.66
N	23,190	23,190
$p$ -value: [1] = [2]	0.874	0.853

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year observations from 1991 to 2013. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 9**

Confounding laws: The role of constituency statutes

Panel A: Controlling for the adoption of constituency statutes		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
<i>UD Law</i>	0.290*** (0.099)	0.266*** (0.070)
<i>Constituency Statutes</i>	yes	yes
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
$R^2$	0.65	0.66
N	23,190	23,190
Panel B: Excluding firms incorporated in Connecticut, North Carolina, and Texas		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
<i>UD Law</i>	0.237*** (0.075)	0.258*** (0.063)
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
$R^2$	0.65	0.66
N	22,104	22,104

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year observations from 1991 to 2013. In Panel B, firms incorporated in Connecticut, North Carolina, and Texas are excluded. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 10**

Managers' ex ante incentives to empire build

Panel A: Free cash flow		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
[1] <i>UD Law</i> $\times$ <i>High Free Cash Flow</i>	0.363** (0.140)	0.428*** (0.140)
[2] <i>UD Law</i> $\times$ <i>Low Free Cash Flow</i>	0.083 (0.068)	0.047 (0.085)
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
Cohort fixed effects	yes	yes
$R^2$	0.80	0.81
N	34,399	34,399
$p$ -value: [1] = [2]	0.065	0.054
Panel B: Leverage		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
[1] <i>UD Law</i> $\times$ <i>High Leverage</i>	0.021 (0.195)	0.188 (0.174)
[2] <i>UD Law</i> $\times$ <i>Low Leverage</i>	0.435*** (0.137)	0.393*** (0.107)
Firm fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
$R^2$	0.80	0.81
N	34,513	34,513
$p$ -value: [1] = [2]	0.030	0.159

(continued on next page)

Panel C: CEO fixed pay		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
[1] <i>UD Law</i> $\times$ <i>High CEO Fixed Pay</i>	0.025 (0.156)	0.094 (0.078)
[2] <i>UD Law</i> $\times$ <i>Low CEO Fixed Pay</i>	0.428** (0.172)	0.542** (0.220)
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
Cohort fixed effects	yes	yes
$R^2$	0.80	0.81
N	24,244	24,244
$p$ -value: [1] = [2]	0.007	0.069

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year-cohort observations in the three years before and three years after the adoption of each UD law. The first cohort surrounds the UD law adoption in North Carolina in 1995. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table [A1](#) for variable definitions.

**Table 11**  
Over-investment versus under-investment

	(1)	(2)	(3)
	<i>CSR</i> <i>Overinvest</i>	<i>CSR</i> <i>Underinvest</i>	<i>CSR</i> <i>Overinvest</i> <i>Dummy</i>
<i>UD Law</i>	0.201*** (0.045)	0.043 (0.083)	0.140*** (0.030)
Firm fixed effects	yes	yes	yes
Location $\times$ Year fixed effects	yes	yes	yes
Industry $\times$ Year fixed effects	yes	yes	yes
$R^2$	0.56	0.56	0.51
N	22,658	22,658	22,658

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year observations from 1991 to 2013. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 12**  
Cross-sectional firm-level corporate governance differences

	(1) <i>CSR</i> <i>Material</i>
[1] <i>UD Law <math>\times</math> High E-Index</i>	0.348** (0.142)
[2] <i>UD Law <math>\times</math> Low E-Index</i>	0.002 (0.331)
Firm fixed effects	yes
Industry $\times$ Year fixed effects	yes
Location $\times$ Year fixed effects	yes
Cohort fixed effects	yes
$R^2$	0.81
N	18,582
$p$ -value: [1] = [2]	0.190

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year-cohort observations in the three years before and three years after the adoption of each UD law. The first cohort surrounds the UD law adoption in North Carolina in 1995. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

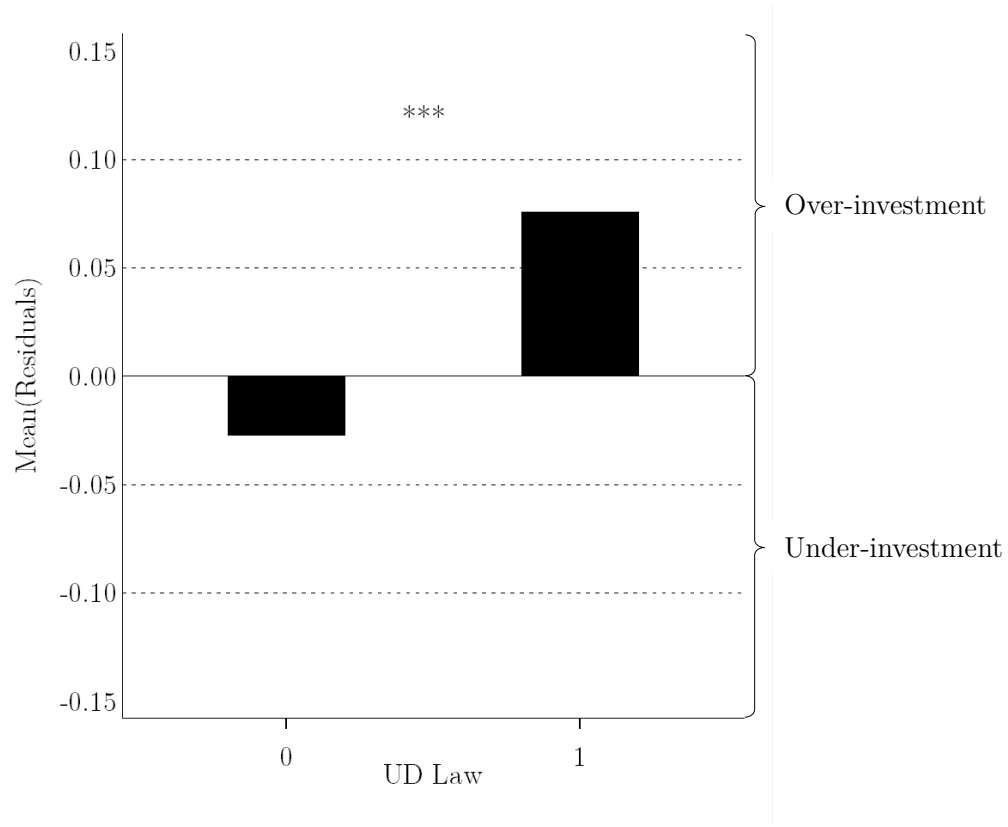
**Table 13**

UD laws, CSR press releases, and CSR disclosures

	(1)	(2)	(3)
	<i>CSR</i> <i>Press</i>	<i>CSR</i> <i>Press</i> <i>Tone</i>	<i>CSR</i> <i>10-K</i>
<i>UD Law</i>	0.473*** (0.110)	0.019*** (0.005)	0.008* (0.004)
Firm fixed effects	yes	yes	yes
Location $\times$ Year fixed effects	yes	yes	yes
Industry $\times$ Year fixed effects	yes	yes	yes
$R^2$	0.52	0.51	0.46
N	23,190	23,190	22,083

This table reports coefficients and standard errors (in parentheses) from OLS regressions. In columns (1) and (2), the sample comprises firm-year observations from 1991 to 2013. In Column (3), the sample comprises firm-year observations from 1994 to 2013. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Figure 1**  
Mean comparison of residuals



This figure plots the means of the residuals from yearly regressions based on Equation (7) for non-UD law firms ( $UD\ Law = 0$ ) and UD law firms ( $UD\ Law = 1$ ), respectively. \*\*\* indicates that the difference of the sample means is statistically significant at the 1% level. Please see Table A1 for variable definitions.

## Appendix

**Table A1**  
Variable definitions

Variable name	Definition
CSR variables:	
<i>Corporate Donations<sub>ijlst</sub></i>	$Ln(1 + \text{total corporate donations/sales}) \times 10^3$ . Data source: NDCG, Compustat
<i>CSR Derivative Lawsuit<sub>ijlst</sub></i>	Dummy variable equal to one if managers of the firm were sued in a CSR-related derivative lawsuit, and zero otherwise. Data source: Audit Analytics, SEC 10-K filings
<i>CSR Direct Lawsuit<sub>ijlst</sub></i>	Dummy variable equal to one if the firm was sued in a CSR-related direct class action lawsuit, and zero otherwise. Data source: Audit Analytics, SCAC database
<i>CSR Immaterial<sub>ijlst</sub></i>	The difference between the firm's immaterial strengths and immaterial concerns in the following CSR categories: Community, Diversity, Employee relations, Environment, Human rights, and Product. Data source: Khan et al. (2016), SASB Materiality Map, MSCI
<i>CSR Material<sub>ijlst</sub></i>	The difference between the firm's material strengths and material concerns in the following CSR categories: Community, Diversity, Employee relations, Environment, Human rights, and Product. Data source: Khan et al. (2016), SASB Materiality Map, MSCI
<i>CSR Overinvest<sub>ijlst</sub></i>	The residual obtained from regressing <i>CSR Material</i> on <i>Size</i> , <i>Profit</i> , <i>Capex</i> , <i>Diversification</i> , <i>R&amp;D</i> , <i>Advertising</i> , <i>Top Brand</i> , <i>Customer Concentration</i> , <i>Labor Union</i> , <i>Government Contract</i> , <i>SRI</i> , <i>Long-term Shareholders</i> , and industry fixed effects (four-digit SIC codes). For each year we run separate regressions. A negative residual is replaced by the value zero.
<i>CSR Overinvest Dummy<sub>ijlst</sub></i>	Dummy variable equal to one if <i>CSR Overinvest</i> is equal to or above the 75 <sup>th</sup> percentile (P75) of the sample, and zero otherwise.
<i>CSR Overall<sub>ijlst</sub></i>	The difference between the firm's strengths and concerns in the following CSR categories: Community, Diversity, Employee relations, Environment, Human rights, and Product. Data source: MSCI
<i>CSR Press<sub>ijlst</sub></i>	The number of positive newswire articles covering CSR, published by the top three newswires (Reuters News, Dow Jones News Service, and Business Wire). Data source: Factiva
<i>CSR Press Tone<sub>ijlst</sub></i>	The fraction of positive words in positive newswire articles covering CSR, published by the top three newswires (Reuters News, Dow Jones News Service, and Business Wire). Firms with no positive newswire articles covering immaterial CSR receive a score of zero. Data source: Factiva, Harvard IV-4 dictionary
<i>CSR Risky<sub>ijlst</sub></i>	Firm's material strengths that are classified as risky CSR (ENV-str-A, ENV-str-B, ENV-str-C, ENV-str-D, and PRO-str-B). Data source: Khan et al. (2016), SASB Materiality Map, MSCI

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$CSR\ Underinvest_{ijlst}$	The residual obtained from regressing <i>CSR Material</i> on <i>Size</i> , <i>Profit</i> , <i>Capex</i> , <i>Diversification</i> , <i>R&amp;D</i> , <i>Advertising</i> , <i>Top Brand</i> , <i>Customer Concentration</i> , <i>Labor Union</i> , <i>Government Contract</i> , <i>SRI</i> , <i>Long-term Shareholders</i> , and industry fixed effects (four-digit SIC codes). For each year we run separate regressions. A positive residual is replaced by the value zero.
$CSR\ 10-K_{ijlst}$	Dummy variable equal to one if the firm's 10-K SEC filing covers positive CSR, and zero otherwise. Data source: SEC 10-K filings
$Green\ Patents_{ijlst}$	Natural logarithm of one plus the number of the firm's green patents. Data source: NBER, Carrion-Flores and Innes (2010)
$State(CSR\ Immaterial)_{st}$	State of incorporation-mean of the variable <i>CSR Immaterial</i> .
$State(CSR\ Material)_{st}$	State of incorporation-mean of the variable <i>CSR Material</i> .
Treatment variable:	
$UD\ Law_{st}$	Dummy variable equal to one if the firm is incorporated in a state that has adopted a UD law, and zero otherwise. Data source: Appel (2016)
Other variables:	
$Adoption\ Time_{st}$	Natural logarithm of the expected time to adopting a UD law (as of 1991). Data source: Appel (2016)
$Advertising_{ijlst}$	Advertising expenditures divided by total sales. We assume that advertising expenditures are zero if data are missing. Data source: Compustat
$Cape_{ijlst}$	Capital expenditures divided by total sales. Data source: Compustat
$CEO\ Fixed\ Pay_{ijlst}$	CEO's salary plus bonus divided by total compensation. Data source: Execucomp
$CEO\ Total\ Pay_{ijlst}$	Natural logarithm of the CEO's total compensation. Data source: Execucomp
$Customer\ Concentration_{ijlst}$	Dummy variable equal to one if the firm has at least one customer that accounts for 10% or more of its sales, and zero otherwise. We assume that no customers accounts for 10% or more, if data are missing. Data source: Compustat
$Constituency\ Statutes_{st}$	Dummy variable equal to one if the firm is incorporated in a state that has adopted a constituency statute. Data source: Flammer and Kacperczyk (2016)
$Diversification_{ijlst}$	Industry-based entropy measure. Following Jacquemin and Berry (1979), we calculate the entropy measure as follows: $\sum_{b \in B} P_{ib} \ln(1/P_{ib})$ , where $B$ denotes the set of business segments (with different four-digit SIC codes) reported by firm $i$ . $P_{ib}$ denotes the share of the firm's sales attributed to business segment $b$ . $\ln(1/P_{ib})$ is the weight of each business segment $b$ . Data source: Compustat
$Dividend_{ijlst}$	Common dividends divided by total assets. Data source: Compustat
$E-Index_{ijlst}$	Entrenchment index. Data source: Online appendix of Bebchuk et al. (2009)
$Earnings\ Volatility_{cijlst}$	Natural logarithm of one plus the ex ante time-series standard deviation of earnings per share, using a rolling window of 10 years. Data source: Compustat
$Firm\ Age_{ijlst}$	Number of years the firm has been covered by Compustat.
$Free\ Cash\ Flow_{ijlst}$	Free cash flow. Data source: Compustat
$Government\ Contract_{ijlst}$	Dummy variable equal to one if the firm obtained a government procurement contract, and zero otherwise. Data source: Federal Procurement Data System – Next Generation

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<i>High CEO Fixed Pay</i> $_{icjlsT-1}$	Dummy variable equal to one if <i>CEO Fixed Pay</i> is equal to or above the sample median one year prior to a UD law being adopted, and zero otherwise.
<i>High E-Index</i> $_{icjlsT-1}$	Dummy variable equal to one if <i>E-Index</i> is equal to or above the sample median one year prior to a UD law being adopted, and zero otherwise.
<i>High Free Cash Flow</i> $_{icjlsT-1}$	Dummy variable equal to one if <i>Free Cash Flow</i> is equal to or above the sample median one year prior to a UD law being adopted, and zero otherwise.
<i>High Leverage</i> $_{icjlsT-1}$	Dummy variable equal to one if <i>Leverage</i> is equal to or above the sample median one year prior to a UD law being adopted, and zero otherwise.
<i>Incorporated in Location State</i> $_{lst}$	Dummy variable equal to one if the firm is incorporated in its state of location, and zero otherwise. Data source: Gormley and Matsa (2016), <a href="https://www3.nd.edu/~mcdonald/10-K-Headers/10-K-Headers.html">https://www3.nd.edu/~mcdonald/10-K-Headers/10-K-Headers.html</a> , SEC 10-K filings
<i>Incorporated in Other State</i> $_{lst}$	Dummy variable equal to one if the firm is not incorporated in its state of location. Data source: Gormley and Matsa (2016), <a href="https://www3.nd.edu/~mcdonald/10-K-Headers/10-K-Headers.html">https://www3.nd.edu/~mcdonald/10-K-Headers/10-K-Headers.html</a> , SEC 10-K filings
<i>Labor Union</i> $_{ijlst}$	Dummy variable equal to one if the firm reports collective bargaining coverage in its SEC 10-K filing, and zero otherwise. For the years 1991–1993, we use the values from 1994 (the first year for which SEC filings are available electronically). Data source: SEC 10-K filings
<i>Labor-to-Capital</i> $_{ijlst}$	Natural logarithm of labor-to-capital ratio, where labor is the number of employees and capital is the net property, plant, and equipment (PPE). Data source: Compustat
<i>Leverage</i> $_{ijlst}$	Sum of long-term debt and debt in current liabilities divided by total assets. Data source: Compustat
<i>Long-term Shareholders</i> $_{cijlst}$	Shareholder investment turnover. Following Gaspar et al. (2005), we calculate shareholder investment turnover as follows: $\sum_{v \in S} w_{ivq} (\frac{1}{4} \sum_{r=1}^4 CR_{vq-r+1})$ , where $S$ denotes the set of institutional shareholders in firm $i$ . $w_{ivq}$ denotes the weight of institutional shareholder $v$ in the total percentage held by institutional shareholders at quarter $q$ . $CR$ is the churn rate of institutional shareholder $v$ at quarter $q$ , calculated as follows: $\frac{\sum_{f \in Q}  N_{fvq} P_{fq} - N_{fvq-1} P_{fq-1} - N_{fvq-1} \Delta P_{fq} }{\sum_{f \in Q} \frac{N_{fvq} P_{fq} + N_{fvq-1} P_{fq-1}}{2}}$ , where $Q$ denotes the set of shares held by institutional shareholder $v$ . $P_{fq}$ and $N_{fvq}$ denote the price and number of shares, respectively, of firm $f$ held by institutional shareholder $v$ at quarter $q$ . We multiply shareholder investment turnover by $(-1)$ , so, that a high variable score indicates that the firm is held by shareholders with a longer-term investment horizon. Data source: Thomson Reuters
<i>Low CEO Fixed Pay</i> $_{icjlsT-1}$	Dummy variable equal to one if <i>CEO Fixed Pay</i> is below the sample median one year prior to a UD law being adopted, and zero otherwise.
<i>Low E-Index</i> $_{icjlsT-1}$	Dummy variable equal to one if <i>E-Index</i> is below the sample median one year prior to a UD law being adopted, and zero otherwise.
<i>Low Free Cash Flow</i> $_{icjlsT-1}$	Dummy variable equal to one if <i>Free Cash Flow</i> is below the sample median one year prior to a UD law being adopted, and zero otherwise.
<i>Low Leverage</i> $_{icjlsT-1}$	Dummy variable equal to one if <i>Leverage</i> is below the sample median one year prior to a UD law being adopted, and zero otherwise.
<i>Market Share</i> $_{ijlst}$	Total sales divided by the total sales of all firms in the same (two-digit SIC code) industry. Data source: Compustat
<i>Profit</i> $_{ijlst}$	Return on assets. Data source: Compustat

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$R\&D_{ijlst}$	Research and development (R&D) expenditures divided by total assets. We assume that R&D expenditures are zero, if data are missing. Data source: Compustat
$SG\&A_{ijlst}$	SG&A expenses. Data source: Compustat
$Size_{ijlst}$	Natural logarithm of total assets. Data source: Compustat
$SRI_{ijlst}$	Number of shares held by socially responsible funds divided by the total number of shares outstanding. Data source: Nofsinger and Varma (2014), Thomson Reuters
$Top\ Brand_{ijlst}$	Dummy variable equal to one if the firm owns a top brand (based on brand equity), and zero otherwise. Data source: Harris Poll EquiTrend

This table defines the variables used in this study.  $c, i, j, l, k, s, t$ , and  $T-1$  denote cohort, firm, industry, state of location, county of location, state of incorporation, year, and year prior to a UD law being adopted indexes, respectively. All variables with no natural lower and upper bounds are winsorized at the 1<sup>st</sup> and 99<sup>th</sup> percentiles.

**Table A2**  
CSR investments and SG&A expenses

	(1) Ln( $SG\&A$ )	(2) Ln( $SG\&A$ )
$CSR\ Immaterial$	0.023*** (0.006)	—
$CSR\ Material$	—	0.017*** (0.004)
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
$R^2$	0.94	0.94
N	20,468	20,468

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year observations from 1991 to 2013. The dependent variable is the natural logarithm of  $SG\&A$ . Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table A3**

Including control variables

Panel A: Including firm controls		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
<i>UD Law</i>	0.321*** (0.101)	0.272*** (0.067)
Firm controls	yes	yes
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
$R^2$	0.65	0.66
N	22,722	22,722
Panel B: Including firm controls and total CEO pay		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
<i>UD Law</i>	0.330*** (0.109)	0.313*** (0.076)
<i>CEO Total Pay</i>	yes	yes
Firm controls	yes	yes
Firm fixed effects	yes	yes
Location $\times$ Year fixed effects	yes	yes
Industry $\times$ Year fixed effects	yes	yes
$R^2$	0.67	0.67
N	14,928	14,928

This table reports coefficients and standard errors (in parentheses) from OLS regressions. In Panel A, the sample comprises firm-year observations from 1991 to 2013. In Panel B, the sample comprises firm-year observations from 1992 to 2013. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. In Panels A and B, all columns, the firm controls include *Size*, *Leverage*, *Capex*, and *Dividend*. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table A4**

Restricting the sample of control firms

Panel A: Excluding firms incorporated in Delaware		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
<i>UD Law</i>	0.295*** (0.098)	0.200*** (0.057)
Firm fixed effects	yes	yes
Year fixed effects	yes	yes
$R^2$	0.58	0.59
N	8,926	8,926
Panel B: Control firms restricted to firms incorporated in MBCA states		
	(1) <i>CSR</i> <i>Immaterial</i>	(2) <i>CSR</i> <i>Material</i>
<i>UD Law</i>	0.309*** (0.094)	0.242*** (0.058)
Firm fixed effects	yes	yes
Year fixed effects	yes	yes
$R^2$	0.61	0.62
N	5,816	5,816

This table reports coefficients and standard errors (in parentheses) from OLS regressions. The sample comprises firm-year observations from 1991 to 2013. In Panel A, firms incorporated in Delaware are excluded. In Panel B, control firms are restricted to those incorporated in states that follow the MBCA. Fixed effects are included as indicated. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table A5**  
Alternative dependent variables

	(1) <i>Corporate Donations</i>	(2) <i>Green Patents</i>
<i>UD Law</i>	0.328** (0.141)	−0.018 (0.016)
Firm fixed effects	yes	yes
Location × Year fixed effects	no	yes
Industry × Year fixed effects	no	yes
Year fixed effects	yes	no
(Pseudo) $R^2$	0.72	0.79
N	3,489	55,961

This table reports coefficients and standard errors (in parentheses) from OLS regressions. In Column (1), the sample is restricted to S&P 500 firms and comprises firm-year observations from 1997 to 2008. In Column (2), the sample comprises firm-year observations from 1976 to 2006. Fixed effects are included as indicated. Industry fixed effects are based on two-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the state of incorporation level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table A6**  
Economic determinants of material CSR investments

	Expected sign	(1) <i>CSR</i> <i>Material</i>
<i>Size</i>	+	0.126*** (0.016)
<i>Profit</i>	+	0.782*** (0.143)
<i>Capex</i>	+	0.690** (0.293)
<i>Diversification</i>	+	0.067* (0.040)
<i>R&amp;D</i>	+	0.083*** (0.022)
<i>Advertising</i>	+	2.623*** (0.557)
<i>Top Brand</i>	+	0.437** (0.176)
<i>Customer Concentration</i>	–	–0.088*** (0.032)
<i>Labor Union</i>	+	0.067** (0.033)
<i>Labor-to-Capital</i>	+	0.054*** (0.016)
<i>Government Contract</i>	+	0.172** (0.076)
<i>SRI</i>	+	10.672*** (2.577)
<i>Long-Term Shareholders</i>	+	1.656*** (0.199)
Industry fixed effects		yes
Year fixed effects		yes
$R^2$		0.33
Mean VIF		1.66
N		22,658

This table reports coefficients and standard errors (in parentheses) from OLS regression. The sample comprises firm-year observations from 1991 to 2013. The expected signs are based on the theoretical works of McWilliams and Siegel (2001) and Baron (2008). Fixed effects are included as indicated. Industry fixed effects are based on four-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the firm level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table A7**Example of shareholder derivative lawsuit: Johnson & Johnson

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Starting in April 2010, a number of shareholder derivative lawsuits were filed in the United States District Court for the District of New Jersey against certain current and former directors and officers of Johnson & Johnson. Johnson & Johnson is named as a nominal defendant. These actions were consolidated in August 2010 into one lawsuit: *In re Johnson & Johnson Derivative Litigation*. Additionally, in September 2010, another shareholder derivative lawsuit was filed by Michael Wolin in New Jersey Superior Court against certain current and former directors and officers of Johnson & Johnson. Johnson & Johnson is named as a nominal defendant in this action as well. The parties to this action have stipulated that it shall be stayed until the *In re Johnson & Johnson Derivative Litigation* is completely resolved.

These shareholder derivative actions are similar in their claims and collectively they assert a variety of alleged breaches of fiduciary duties, including, among other things, that the defendants allegedly engaged in, approved of, or failed to remedy or prevent defective medical devices, improper pharmaceutical rebates, improper off-label marketing of pharmaceutical and medical device products, violations of current good manufacturing practice regulations that resulted in product recalls, and that they failed to disclose the aforementioned alleged misconduct in the Company's filings under the Securities Exchange Act of 1934. Each complaint seeks a variety of relief, including monetary damages and corporate governance reforms. Johnson & Johnson moved to dismiss these actions on the grounds, *inter alia*, that the plaintiffs failed to make a demand upon the Board of Directors. In September 2011, *In re Johnson & Johnson Derivative Litigation* was dismissed without prejudice and with leave to file an amended complaint. [...]

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Source: Johnson & Johnson, 2013 SEC 10-K filing (Exhibit 13), <https://sec.gov/Archives/edgar/data/200406/000020040613000038/ex13-form10xk20121230.htm>

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**Figure A1**  
Sector-level materiality map



## SASB Materiality Map™

SASB's Materiality Map identifies likely material sustainability issues on an industry-by-industry basis. This map serves as a snapshot of likely material sustainability issues at the time of our initial analysis and may be subject to change as issues and industries are ever-evolving. Click on a highlighted cell at the sector-level and then on any highlighted cell at the industry-level to see suggested accounting metrics and additional information for each issue.

### Sector Level Map

- Issue is likely to be material for more than 50% of industries in sector
- Issue is likely to be material for less than 50% of industries in sector
- Issue is not likely to be material for any of the industries in sector

### Industry Level Map

- Not likely a material issue for companies in the industry
- Likely a material issue for companies in the industry

ISSUES	Click to expand									
	Health Care	Financials	Technology and Communications	Non-Renewable Resources	Transportation	Services	Resource Transformation	Consumption	Renewable Resources & Alternative Energy	Infrastructure
<b>Environment</b>										
GHG emissions										
Air quality										
Energy management										
Fuel management										
Water and wastewater management										
Waste and hazardous materials management										
Biodiversity impacts										
<b>Social Capital</b>										
Human rights and community relations										
Access and affordability										
Customer welfare										
Data security and customer privacy										
Fair disclosure and labeling										
Fair marketing and advertising										
<b>Human Capital</b>										
Labor relations										
Fair labor practices										
Employee health, safety and wellbeing										
Diversity and inclusion										
Compensation and benefits										
Recruitment, development and retention										
<b>Business Model and Innovation</b>										
Lifecycle impacts of products and services										
Environmental, social impacts on assets & operations										
Product packaging										
Product quality and safety										

This figure shows the sector-level SASB materiality map, available at <http://materiality.sasb.org>. Dark (light) grey color indicates that the issue is material for more (less) than 50% of the industries within the sector. White color indicates that the issue is immaterial for any of the industries within the sector. Please visit <http://materiality.sasb.org> to access the more detailed industry-level materiality map.

# III. Do customers affect the value relevance of corporate social responsibility reporting?

## Empirical evidence on stakeholder interdependence\*

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### Abstract

In spite of the strategic importance of corporate social responsibility (CSR) reporting in current business practice and the resulting increase in research on its value relevance, studies accounting for stakeholder interdependence are scarce. On the basis of the instrumental stakeholder theory, we investigate whether customers have an impact on the value relevance of CSR reporting. Using a sample of US listed firms, we show that the value relevance of CSR reporting is affected by customer profile differences, thereby confirming customer-shareholder interdependence. However, customer profile effects are only predominant if firms' profitability levels are low and disappear as profitability increases. Overall, our findings provide a more nuanced understanding of the value relevance of CSR reporting. Therefore, we offer managers a fine-grained guidance for value relevant CSR reporting.

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*Keywords:* Corporate social responsibility reporting, customer profile differences, stakeholder interdependence, stakeholder theory, value relevance

*JEL classification:* M14, M32, M31, M41, G32

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<sup>‡</sup>The published version is slightly different from the version included in the dissertation.

# 1. Introduction

In recent years, we have witnessed explosive growth in the strategic importance of corporate social responsibility (CSR) reporting,<sup>1</sup> due to the widespread belief that it enhances firm value and, thereby, affects value relevance. However, empirical evidence on this topic is inconclusive (e.g., Allayannis and Weston, 2001; Berthelot et al., 2012; Cardamone et al., 2012; Guidry and Patten, 2010). Thus, more nuanced research is needed to determine the conditions under which CSR reporting is actually effective. Some researchers have already shown that certain market conditions play a crucial role in the value relevance of CSR reporting (e.g., Bachoo et al., 2013; Kaspereit and Lopatta, 2016), but no study has yet examined whether the value relevance of CSR reporting is affected by customer profile differences, i.e., business-to-consumer (B2C) versus business-to-business (B2B) focused firms. This is surprising for several reasons. First, many B2C firms have experienced rapid growth through CSR campaigns, e.g., Ben & Jerry's or The Body Shop (Ketola, 2010; Wheale and Hinton, 2007; Dennis et al., 1998). Second, B2C firms have a higher exposure to the media compared to their B2B counterparts (Haddock-Fraser and Fraser, 2008; Demers and Lewellen, 2003). Third, the implementation of CSR strategies is mostly driven by end-consumer demand, such as in the cases of Honda, BMW, or Timberland (Sisodia et al., 2007). Consequently, we conclude that proximity to end-consumers can be intuited as an important factor influencing the value relevance of CSR reporting.

Relying on instrumental stakeholder theory—and based on the argument that different customer groups possess different sensitivity levels regarding firms' CSR—we hypothesize that the value relevance of CSR reporting is dependent on customer profile differences (H1). In addition, we argue that the value relevance of CSR reporting is dependent on customer profile differences *and* is moderated by the level of profitability (H2). To test our hypotheses we employ fixed effect regressions, using firm value as the dependent variable and the dichotomy of issuing versus non-issuing a CSR report as our explanatory variable of interest. By so doing, we concentrate on CSR reports that are issued in alignment with the Global Reporting Initiative (GRI) guidelines, *the* leading framework guiding the preparation of CSR reports emphasizing stakeholder engagement (Hess,

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<sup>1</sup> According to the Global Reporting Initiative (GRI), CSR reporting, also called sustainability reporting, is defined as the reporting about "economic, environmental and social impacts caused by [an organization's] everyday activities." (<http://globalreporting.org>)

2008; Gray, 2006). In addition, CSR reports prepared in accordance with reporting guidelines are perceived as more credible. In 2011, 80% of the 250 largest global firms issued GRI-aligned CSR reports (KPMG, 2011).

Our study makes two important contributions to the literature. Above all, we contribute to the literature by showing that shareholders of US firms value the issuance of a CSR report differently, depending on whether a firm addresses end-consumers (B2C) or other businesses (B2B). Furthermore, we find that the level of profitability moderates the differing valuation effects. These findings suggest that managers should carefully consider stakeholder interdependencies, i.e., customer-shareholder interdependence, as well as corporate abilities, i.e., profitability, when evaluating the costs and benefits of CSR reporting.

The remainder of this paper is organized as follows. Section 2 reviews the related literature. Section 3 provides the theoretical foundation of our study and develops the hypotheses. Section 4 defines the sample, describes the data, and outlines the main variables. Section 5 specifies our empirical model. Section 6 presents our empirical results. Section 7 explores the robustness of our empirical results. Finally, section 8 summarizes, concludes, and raises topics for further research.

## 2. Related literature

Most value relevance literature, on non-financial information, focuses on CSR investments in general rather than on CSR reporting in particular (e.g., Charlo et al., 2013; Martinez-Ferrero and Frias-Aceituno, 2013; Fifka, 2013; Wang and Choi, 2013; Baird et al., 2012; Lourenco et al., 2012; Nelling and Webb, 2009; Callan and Thomas, 2009; Goodfrey et al., 2009; Orlitzky et al., 2003). Concentrating on CSR reporting ensures capturing firms' special effort and commitment to *communicating* their CSR investments. In addition, detailed CSR reports potentially contain supplementary information beyond that covered by summary ratings (Dhaliwal et al., 2011). However, most CSR reports tend to contain only positive news or a primarily immaterial information (e.g., Holder-Webb et al., 2009; Hackston and Milne, 1996),<sup>2</sup> reinforcing the impression that CSR reporting is primarily a public relations activity (e.g., Porter and Kramer, 2006; Esrock and Leichty, 1998). Consequently, our study is obviously related to, but different from, research using

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<sup>2</sup> An informal content analysis of the CSR reports included in our study corroborates the statement that firms disclose almost exclusively positive CSR information.

external investment measures, such as MSCI ESG STATS (previously known as KLD), instead of self-disclosures.

Over the years, CSR reports have been incrementally replacing environmental reports (Kolk, 2008; Perez and Sanchez, 2009), extending the scope of non-financial disclosures from purely environmental to a triple bottom-line approach that covers not only environmental but also the social and economic dimensions of firms' strategies. Thus, literature analyzing the value relevance of triple bottom-line CSR reporting is recent but rather limited.

In the following paragraph, we review the literature addressing the value relevance of CSR reporting. For the years from 2001 to 2008, Guidry and Patten (2010) find no direct association between the first-time release of a CSR report and firm value when investigating a small sample of US firms. Kaspereit and Lopatta (2016) concentrate on the largest European firms for the years 2001–2011. While they reveal a definite positive effect of CSR investments on firm value, they find hardly any evidence for a positive effect of GRI-aligned reporting. Similarly, Buys et al. (2001) could not confirm a definite positive association between GRI-aligned reporting and firm value when analyzing a developing economy from 2002 to 2009. By comparison, Berthelot et al. (2012) find explicit evidence for a positive relationship between the issuance of a CSR report and firm value when drawing on a cross-sectional sample (financial year 2007) of publicly listed Canadian firms. Likewise, Schadewitz and Niskala (2010) analyze the Finnish market from 2002 to 2005 and show that shareholders positively value the issuance of a GRI-aligned CSR report. They conclude that earnings only partially cover relevant valuation information. Bachoo et al. (2013) confirm a clear positive relationship between the quality of CSR reporting and firm value, observing firms listed on the ASX from 2003 to 2005. Similarly, Al-Najjar and Anfimiadou (2012) investigate CSR reporting as part of an eco-efficient business strategy in the U.K. for the period 1999 to 2008. They provide evidence that CSR reporting and external environmental certification (ISO 14001) are positively related to firm value. Conversely, negative associations between CSR reporting and firm value are detected by Jones et al. (2007), Cardamone et al. (2012), and Gietl et al. (2013). Jones et al. (2007) include, but do not focus solely on, stand-alone CSR reports, when investigating the Australian market in 2004. They find that the level of CSR reporting is negatively associated with firm value. Cardamone et al. (2012) consider Italian listed firms between 2002 and 2008 and report an overall negative effect. The most recent time period, i.e., 2007–2010, is analyzed by Gietl et al. (2013),

who support a negative association between the highest level of GRI-aligned reporting (GRI A+) and firm value for EUROSTOXX 600 firms. All in all, the findings of our reviewed literature are obviously mixed and inconclusive.

### 3. Theoretical foundation and hypotheses

The stakeholder theory (Donaldson and Preston, 1995; Freeman, 1984) constitutes a solid basis for analyzing the value relevance of CSR reporting, due to the fact that the issuance of a CSR report is a strategic decision influenced by multiple stakeholders. By definition, stakeholders are groups who can affect a firm's performance or are affected by its actions (Freeman, 1984). Clarkson (1995) distinguishes between primary stakeholder groups (e.g., customers, shareholders) and secondary stakeholder groups (e.g., media, NGOs). Without the support and participation of primary stakeholder groups a firm cannot survive, whereas secondary stakeholder groups are not directly involved in a firm's transactions and are therefore not essential to its survival (Haddock-Fraser and Tourelle, 2010). Accordingly, managers' priorities must include balancing the demands and expectations of primary stakeholder groups in order to ensure a firm's existence and success (e.g., Gonzalez-Benito and Gonzalez-Benito, 2006; Freeman and Liedtka, 1991). Balancing the demands and expectations of primary stakeholder groups can be a strategic challenge, since the interests of different stakeholder groups may be conflicting. A specific primary stakeholder group may view addressing the demands of certain other primary stakeholder groups as unnecessary or even as a wastefulness of resources (Groening and Kanuri, 2013). Hence, the awareness of stakeholder interdependencies is an essential component of a successful business strategy.

The interdependencies between stakeholder groups, as well as their power and influence, are greatly responsible for pushing or pulling firms' strategies towards CSR (e.g., Onkila, 2011; Harvey and Schaefer, 2001; Madsen and Ulhoi, 2001; Grafe-Buckens and Hinton, 1998). In a shareholder oriented business culture, such as the US, shareholders exert more influence over strategic decisions, such as the issuance of a CSR report, compared to other primary stakeholder groups. Therefore, we use the instrumental approach of the stakeholder theory as a theoretical basis (Donaldson and Preston, 1995; Jones, 1995). Contrary to the descriptive or normative approach, the instrumental approach involves the financial consequences that emerge from addressing stakeholder demands.

Therefore, shareholders only reward firms if addressing the needs of other stakeholder groups is beneficial. Otherwise, shareholders are likely to show no reaction or to even punish the firm. Consider a market where customers possess a certain level of CSR awareness. In such a case, the communication of a firm's commitment towards CSR, through the issuance of a CSR report, may enhance its reputation and brand value, which can improve the positioning of its products (services) (Brown and Dacin, 1997). In turn, a better reputation and brand value in terms of CSR may allow firms to charge a higher price for their products (services) (McWilliams and Siegel, 2001), and they may even experience an increase in sales (Lev et al., 2010). This process of addressing customer needs is likely to be valued positively by shareholders, resulting in a higher firm value.

However, firms with a superior CSR reputation appeal only to customer groups that care about CSR (Lev et al., 2010). To address this issue of different customer profiles, we use a common industry dichotomy: consumer oriented (B2C) versus business oriented (B2B) firms. Several reasons support this distinction. First, customers of B2C firms look more for psychological benefits (Chitturi et al., 2008). In contrast, B2B customers focus more on price, quality, and satisfaction (Bolton et al., 2008) because their buying decisions must often be justified (Bunn, 1993). Accordingly, customers of B2C firms are likely to pay a premium for the products of socially responsible firms, whereas customers of B2B firms are only willing to do so if the portfolio of efforts is cost-effective (Drumwright, 1994). Second, CSR affects the buying decisions of B2B customers less frequently (Drumwright, 1994). Third, positive corporate social events, such as charitable events, are used to a greater degree by B2C firms than by B2B firms to attract customers (Groening and Kanuri, 2013). Fourth, B2B firms are able to shield questionable activities behind B2C trademarks, with which end-consumers more readily identify (Gonzalez-Benito and Gonzalez-Benito, 2010). Fifth, the overall pressure regarding CSR proactivity increases as the proximity to end-consumers increases (Gonzalez-Benito and Gonzalez-Benito, 2006). Hence, firms at the end of a supply chain face the greatest pressure to promote and communicate their CSR activities (Gonzalez-Benito and Gonzalez-Benito, 2010). Sixth, B2B firms receive pressure mainly from internal stakeholders, e.g., employees (Buyse and Verbeke, 2003), while B2C firms are presumed to feel pressure primarily from external stakeholders, e.g., customers. Eventually, the strategic motivation towards CSR is greater for B2C firms since they do not just aim for direct costs benefits, i.e., eco-efficiency, but also for reputational benefits (Haddock-Fraser and Tourelle, 2010). This is in line with research on corporate reputation,

suggesting that strategic CSR enhances reputation (e.g., Tang et al., 2012; Kang and Hur, 2012), particularly for B2C oriented firms (Melo and Garrido-Morgado, 2012). Consequently, we formally state our first hypothesis as follows.

**Hypothesis 1 (H1):** The impact of CSR reporting on firm value is dependent on customer profile differences.

However, the effect of CSR reporting on firm value may not only depend on customer profile differences but also on profitability. Clarkson (1995) argues that a firm's continuing profitability is based on the satisfaction of its (primary) stakeholder groups. Accordingly, the level of profitability can be regarded as a function of how well a firm fulfills the needs of its primary stakeholders, particularly its customers. In cases of lower levels of profitability, B2C firms may obtain profitability increases from the satisfaction of their customers' (primary) psychological needs. Furthermore, less profitable B2C firms may proactively communicate their CSR activities in order to explicitly target niche customer groups (Murray et al., 2006). Satisfying those psychological customer needs is expected to be valued positively by shareholders of B2C firms. Conversely, less profitable B2B firms may not experience an increase in profitability due to the communication of their CSR activities, because the efficiency and cost issues that matter most to their customers should be addressed first. Hence, shareholders are likely to punish less profitable B2B firms that communicate their CSR activities. At higher levels of profitability, B2B firms are expected to have mastered efficiency and cost issues. Therefore, communicating their CSR activities may create some additional value for their customers. In such cases, CSR reporting may be valued positively—or at least not punished by shareholders. In contrast, the increasing profitability of B2C firms may signal that customers have been psychologically satisfied. Like many other corporate activities, the communication of firms' CSR activities may display diminishing returns. Therefore, shareholders of profitable B2C firms are likely to view additional expenditures associated with CSR reporting as opportunity costs and, for this reason, may trade shares at a discount. If so, we have the following hypothesis.



**Hypothesis 2 (H2):** The impact of CSR reporting on firm value is dependent on customer profile differences and is moderated by the level of profitability.

## 4. Sample selection, data, and variable definitions

This study investigates the US market by focusing on the Standard & Poor’s (S&P) 500 index. Analyzing a single market with identical societal and political circumstances facilitates a relatively homogenous data set (Gamerschlag et al., 2011). The investigated time period ranges from 2007 to 2011, resulting in 2,500 firm-year observations. From this initial data set we exclude data on firms with non-US International Securities Identification Numbers (ISINs). Moreover, we eliminate all firms operating in the financial services industry due to their exposure to regulatory oversight. In addition, we delete all firm-year observations for which any data is missing. Consequently, our final sample includes 384 firms and comprises 1,742 firm-year observations. On average, each firm appears for approximately 4.5 years in the data set.

### 4.1 *Firm value*

To measure firm value, we use the forward-looking market-based measure Tobin’s  $q$ . We approximate Tobin’s  $q$  by market-to-book ratio, since more sophisticated versions, such as the Lindenberg and Ross (1981) or the Lewellen and Badrinath (1997) procedures, require certain assumptions about depreciation and inflation rates that decrease objectivity. Prior literature also supports this approximation approach. Chung and Pruitt (1994) show that at least 96% of the variance of a more sophisticated Tobin’s  $q$  can be explained by market-to-book ratio. Allayannis and Weston (2001) discover high associations between several Tobin’s  $q$  proxies, including market-to-book ratio. In the context of CSR reporting, Gietl et al. (2013) prove the robustness of market-to-book ratio by finding a statistically significant negative association between GRI A+ reporting and firm value for different Tobin’s  $q$  measures. Rountree et al. (2008) call the market-to-book ratio approach *the* common methodology in the literature. The data source for market-to-book ratio is Thomson Reuters Datastream.

## 4.2 *Customer profile differences*

To divide firms' customer profile into B2C and B2B, we draw on the work of Palepu (1985) and follow Srinivasan et al. (2011), using primary four-digit Standard Industrial Classification (SIC) codes based on the business description provided by the Occupational Safety and Health Administration. Primary four-digit SIC codes are assigned by Thomson Reuters Datastream according to sales breakdown. Examples of B2C firms are the four-digit SIC codes 3144 (women's footwear) and 7011 (hotels and motels), examples of B2B firms are 8742 (management consulting services) and 3366 (copper foundries). Overall, we identify 102 B2C and 282 B2B firms in our data set.

## 4.3 *CSR reporting*

As mentioned previously, we focus on CSR information disclosed by firms themselves, rather than relying on CSR investment ratings available through third parties. This ensures capturing firms' commitment to communicating their CSR activities, which, in turn, is of great importance when focusing not only on shareholders but also on customers. Issuances of CSR reports are often accompanied by press releases (Guidry and Patten, 2010), corroborating our assumption that CSR reporting is an appropriate proxy for firms' commitment to communicating their CSR activities. In particular, we concentrate on CSR reports issued in alignment with GRI. In doing so, we consider all GRI G3<sup>3</sup> application levels, i.e., A, B, and C,<sup>4</sup> and do not distinguish between externally assured (+) and externally non-assured reports. Furthermore, we include reports "that make explicit reference to being based on the GRI guidelines but for which there is no GRI content index" (GRI, 2012, p. 9) or application level available (GRI-ref). Finally, for the financial years 2007, 2008, and 2009, we consider reports issued in accordance with GRI G2, the predecessor of GRI G3. We choose this broad scope of GRI-aligned reporting due to the heterogeneous disclosure practices (cf. Figure A1), and to ensure a reasonable number of CSR reports in our data set. Table 1 shows that the number of firms preparing a CSR report in alignment with GRI has increased substantially over time, from 35 (B2C: 7, B2B: 28) in 2007 to 126 (B2C: 31, B2B: 95) in 2011. In total, 95 GRI-aligned CSR reports

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<sup>3</sup> The GRI G3 guidelines were released in 2006. An updated version, i.e., GRI G3.1, was launched in 2011 (GRI, 2013).

<sup>4</sup> Some previous studies (e.g., Gietl et al., 2013; Kaspereit and Lopatta, 2016) have explicitly focused on these application levels. However, such an approach would be outdated from a present view, since under the new GRI G4 standard, launched in May 2013, the lettered application levels are gone. Hence, by including all GRI G3 CSR reports, regardless of application level, we ensure the temporal relevance of our study.

have been issued by B2C firms, while B2B firms have released 315 reports. The proportion of GRI to non-GRI reports (95:356) is, with over 26%, almost as high for B2C firms as it is for B2B firms (315:976, 32%).<sup>5</sup> The overall proportion of GRI to non-GRI reports (410:1,332) is, with almost 31%, higher than it is in comparable studies (e.g., Dhaliwal et al., 2012; Schadewitz and Niskala, 2010). The data on CSR reporting are extrapolated from the GRI CSR Disclosure Database.<sup>6</sup>

#### 4.4 Control variables

We control for a series of observable time varying firm characteristics, which are expected to influence CSR reporting and firm value: firm size, profitability, capital market accessibility, future investment opportunities, geographic diversification, and capital structure differences. These selected control variables are consistent with prior literature (Wang and Choi, 2013; Gietl et al., 2013; Bris et al., 2009; Rountree et al., 2008; Jin and Jorion, 2006; Villalonga and Amit, 2006; Allayannis and Weston, 2001; Morck and Yeung, 1991). The data source for all control variables is Thomson Reuters Datastream.

### 5. Model specification

As a starting point, we specify the following base regression model:

$$\begin{aligned}
 FV_{it} = & \beta_1 GRI_{it} + \gamma_1 SIZE_{it} + \gamma_2 PROFIT_{it} + \gamma_3 DIV_{it} + \gamma_4 GROW_{it} + \gamma_5 RD_{it} \\
 & + \gamma_6 FOREIGN_{it} + \gamma_7 DEBT_{it} + \sum_{j=1}^4 \delta_j YEAR_{jt} + \lambda_i + \epsilon_{it}
 \end{aligned} \tag{1}$$

where  $i$  and  $t$  are firm and time indices, respectively. The dependent variable  $FV_{it}$  denotes firm value, proxied by the natural logarithm of market-to-book ratio. The explanatory variable  $GRI_{it}$  equals one if the firm issues a CSR report in alignment with GRI, and zero otherwise. Also included in Equation (1) are further variables, in order to control for time-variant heterogeneity across firms:

$SIZE_{it}$  = natural logarithm of total sales (firm size),

$PROFIT_{it}$  = return on assets (profitability),

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<sup>5</sup> Contrary to the findings of Haddock-Fraser and Fraser (2008), our data set provides no evidence for a relationship between customer profile differences and CSR reporting, since  $\chi^2$ -test results for the final two columns of Table 1 are statistically insignificant ( $\chi^2 = 2.07$ , p-value = 0.151).

<sup>6</sup> <http://database.globalreporting.org>

$DIV_{it}$  = one if the firm pays a dividend, and zero otherwise (capital market accessibility),  
 $GROW_{it}$  = capital expenditures divided by total sales (future investment opportunities – growth),  
 $RD_{it}$  = research and development expenditures divided by total assets (future investment opportunities – research and development),  
 $FOREIGN_{it}$  = foreign sales divided by total sales (geographic diversification),  
 $DEBT_{it}$  = debt-to-equity ratio (capital structure).

In order to control for factors that vary over time but are constant across firms (e.g., global economic crisis), we include time fixed effects by adding year dummy variables ( $YEAR_{jt}$ ) (Wang and Choi, 2013). Furthermore, we introduce firm fixed effects ( $\lambda_i$ ) to control for time-invariant heterogeneity across firms.<sup>7</sup> Customer profile differences ( $B2C_i$ ,  $B2B_i$ ) are such time-invariant heterogeneous firm factors and, hence, already captured by the parameter  $\lambda_i$ . However, even when controlling for customer profile differences, a fixed effect estimator may still be appropriate, since  $\lambda_i$  includes much more than simply customer profile differences. Therefore, as in Boyce and Wood (2011), a fixed effect estimator is used in Equation (2) (extended model) to test whether the association between  $FV_{it}$  and  $GRI_{it}$  is dependent on customer profile differences, a subset of  $\lambda_i$ .

$$FV_{it} = \psi_1 GRI_{it} \times B2C_i + \psi_2 GRI_{it} \times B2B_i + \sum_{k=1}^7 \gamma_k CV_{kit} + \sum_{j=1}^4 \delta_j YEAR_{jt} + \lambda_i + \epsilon_{it} \quad (2)$$

where  $B2C_i$  equals one if the firm addresses end-consumers, and zero otherwise. Correspondingly, the variable  $B2B_i$  equals one if the firm addresses other businesses, and zero otherwise. As fixed effect estimators build their estimation only on within-variation but not on between-variation, time-invariant variables, i.e.,  $B2C_i$  and  $B2B_i$ , cannot be included directly in a fixed effect model. However, by letting time-invariant variables interact with variables that do vary across time, it is still possible to control indirectly for time-invariant variables in a fixed effect model (Boyce and

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<sup>7</sup> The prevailing approach to cope with issues of (unobservable) firm-specific heterogeneity is to perform fixed effect regressions (Boyce, 2010). However, it might still be possible that a change in GRI is accompanied by unobservable time-varying factors, such as a change in the management team that causes the changes in GRI and FV. We address this issue in the robustness section by running a two stage Heckman model and a regression discontinuity model. In addition, our finding that customer-profile differences moderate the effect of GRI on FV also helps mitigate the omitted variable concern because for an omitted (time-varying firm) variable to explain our results, it has to affect both GRI and FV negatively when the firm addresses B2B firms. Even more restrictive, it has to affect both GRI and FV negatively when the firm has a low profitability level and addresses B2B firms and positively when the firm has a low profitability level and addresses B2C firms.

Wood, 2011). There are two approaches to incorporate such interactions between two dummy variables into a regression model: the partition approach and the base approach. As suggested by Yip and Tsang (2007), we use the partition approach in Equation (2) by entering the (multiplicative) interactions between  $GRI_{it}$  and both  $B2C_i$  and  $B2B_i$  into our extended model, i.e.,  $GRI_{it} \times B2C_i$  and  $GRI_{it} \times B2B_i$ . This is possible because  $GRI_{it}$  possesses enough within-variation (cf. Table A1). Besides the two interaction terms of interest, we include the same time-varying control variables ( $CV_{kit}$ ) as shown in Equation (1). We again control not only for firm fixed effects but also for time fixed effects. All variables with no natural lower and upper bounds are winsorized at extreme percentiles.

## 6. Empirical results

### 6.1 Descriptive analysis

Table 2 provides basic summary statistics and Pearson pair-wise correlations. Considering the entire sample, we find a relatively low negative correlation between  $GRI$  and  $FV$  ( $-0.02$ ).  $B2C$  shows a positive correlation with  $FV$  ( $0.17$ ).<sup>8</sup> In line with prior literature, all correlation coefficients between the time-varying control variables and the dependent variable,  $FV$ , have the expected sign, except for  $GROW$  ( $-0.18$ )<sup>9</sup> and  $DEBT$  ( $0.16$ ). Taking a look at Column (2), we find that  $SIZE$ ,  $DIV$ , and  $FOREIGN$  are (obviously) positively correlated with  $GRI$ , while all other variables show rather low correlations with  $GRI$ . In fact, we do not find any high correlations among the explanatory variables, indicating that multicollinearity is not a serious problem. Additionally, the variance inflation factors (VIF) do not detect any multicollinearity either.<sup>10</sup>

### 6.2 Univariate analysis

Our univariate analysis (Table 3) compares the mean  $FV$  of GRI reporters with those of non-GRI reporters for B2B and B2C firms, respectively. Considering the B2C firms in Panel A, the mean  $FV$  of GRI reporters is 27 basis points higher than of non-GRI reporters. For B2B firms we observe

<sup>8</sup> Logically, for the variable  $B2B$  the opposite is observed ( $-0.17$ ).

<sup>9</sup>  $RD$  as well as  $GROW$  both control for future investment opportunities but only  $RD$  takes into account intangible as well as hidden assets, which is probably responsible for the different signs.

<sup>10</sup> We calculate the VIFs after running a pooled OLS regression based on Equation (1). The highest VIF amounts to 1.56 and the mean VIF to 1.29.

the opposite. The mean *FV* of GRI reporters is 12 basis points lower than of non-GRI reporters. The differences are highly significant. These preliminary univariate results support H1.

In Panels B and C, we conduct mean comparison tests for two different subsamples. Specifically, we divide our data set into less profitable and profitable firms by using the median of the control variable *PROFIT* as splitting criterion. Panel B displays mean comparison tests for the less profitable subsample. Similarly to the results for the entire sample, we can show that B2C firms issuing a GRI-aligned CSR report have a mean *FV* that is 28 basis points higher than the one of their non-reporting counterparts. Again, for B2B firms we observe the opposite: The mean *FV* of GRI reporters is 15 basis points lower than of non-GRI reporters. Both differences are highly significant. For the profitable subsample in Panel C, we again find that B2C GRI reporters have a mean *FV* that is higher (14 basis points) than that of B2C non-GRI reporters, while B2B GRI reporters again have a lower mean *FV* (10 basis points) compared to B2B GRI-non reporters. However, these differences are, in absolute moduli, much lower compared to the ones in Panels A and B, and are not statistically significant. Consequently, the preliminary findings of Panels B and C show support for H2.

### 6.3 Regression analysis

Table 4 sets out our main regression results and begins in Column (1) with the fixed effect estimates of the base model shown in Equation (1). In line with another US study (Guidry and Patten, 2010), we are not able to detect a statistically significant impact of *GRI* on *FV* ( $-0.039$ ). This finding is inconsistent with non-US work that posits either a statistically significant negative (Gietl et al., 2013; Cardamone et al., 2012; Jones et al., 2007) or positive (Bachoo et al., 2013; Al-Najjar and Anfimiadou, 2012; Berthelot et al., 2012; Schadewitz and Niskala, 2010) association between CSR reporting and firm value. This inconsistency may be due to the application of different estimation strategies, the use of different proxies for *GRI* or *FV*, and/or the investigation of different markets. The latter is probably the key driver, suggesting that value relevance studies regarding CSR reporting cannot be generalized beyond the market examined, most likely due to substantial cross-country differences.

Column (2) highlights the importance of controlling for heterogeneous factors within firms by predicting the individual firm fixed effect residual ( $\lambda$ ), obtained from Column (1), using customer

profile differences (*B2C*). The coefficient of determination  $R^2$  depicts that customer profile differences explain only 10% of firm heterogeneity. This leaves a substantial unexplained component, affirming that firm fixed effects are indispensable and cannot be disregarded. In the robustness section we elaborate on alternative estimation strategies.

Column (3) displays the estimates for the extended model shown in Equation (2). We find that the effect of *GRI* on *FV* is on average positive (0.065) for B2C firms, though not statistically significant. By comparison, for B2B firms, the effect of *GRI* on *FV* is on average negative ( $-0.078$ ) and statistically significant at the 5% level. This implies that shareholders penalize B2B firms for issuing a CSR report but tolerate it as a corporate activity of B2C firms. The highly significant F-statistic (1%) of an equality test of the interaction effects provides evidence that the impact of *GRI* on *FV* differs between B2C and B2B firms. This, in turn, demonstrates that customer profile differences contribute to the explanation of *FV*. Consequently, the outcome of Column (3) verifies H1.

In the final two columns of Table 4, we estimate the same model as in Column (3) but for two different subsamples, less profitable and profitable firms.<sup>11</sup> In Column (4), we examine the less profitable firms. The results show that the effect of  $GRI \times B2C$  on *FV* is on average positive (0.183) and highly significant (1%), suggesting that shareholders of less profitable B2C firms value CSR reporting positively. The impact of our second interaction variable of interest,  $GRI \times B2B$ , has on average a statistically significant (5%) negative impact ( $-0.101$ ) on *FV*, indicating that shareholders of less profitable B2B firms punish CSR reporting. The highly significant (1%) F-statistic for the interaction effects signals that customer profile differences are important to explain the *FV* of less profitable firms. In contrast, the estimates for the profitable subsample (Column (5)) show a completely different picture: Neither for B2C nor for B2B firms can a statistically significant association between *GRI* and *FV* be observed. The F-statistic for the interaction effects is also not statistically significant at conventional levels, suggesting that the effect of *GRI* on *FV* does not differ between profitable B2C and B2B firms. With respect to the results of Column (4) and (5), we can verify H2, affirming that the relationship between *GRI* and *FV* depends on customer profile differences and is tempered at higher levels of profitability.

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<sup>11</sup> As stated previously, we divide our data set into less profitable and profitable firms by using the median of the control variable *PROFIT* as splitting criterion.

As far as the control variables are concerned, all statistically significant coefficients are in line with the correlation results and only *DEBT* differs from our expectations. The F-statistics of joint significance tests of the time fixed effects, i.e., year dummies, are highly significant (1%), indicating the importance of controlling for time-specific factors. With  $R^2$  ranging from 0.40 to 0.46, the model fits of the respective fixed effect regressions seem generally good.

All in all, Table 4 provides evidence that shareholders value CSR reporting differently, depending on firms' customer profile. The difference appears to be even more obvious when considering only less profitable firms. However, the differing effects disappear at higher levels of profitability. With this in mind, managers are advised to carefully consider the interdependence between customers and shareholders in the light of profitability when making strategic decisions over CSR reporting. From a methodological point of view, we conclude that it is essential to control for time-invariant firm heterogeneity (firm fixed effects), even if a part of that heterogeneity, i.e., customer profile differences, can be observed and is explicitly controlled for in the regression.

## 7. Robustness

This section explores the robustness of our results to several alternative estimation strategies and regression specifications. For the sake of brevity, the outputs of our robustness checks are not tabulated. We begin our robustness testing by re-estimating Equation (2), using, *first*, a pooled ordinary least squares (OLS) estimator<sup>12</sup> and, *second*, a random effect technique.<sup>13</sup> While the signs of our coefficients of interest, i.e.,  $GRI \times B2C$  and  $GRI \times B2B$ , are consistent with our main findings (Table 4), the significance levels are systematically higher and the associations are systematically stronger than those obtained under the fixed effect technique. Hence, our initial results can be regarded as conservative. *Third*, we employ a contested three-stage procedure, known as fixed effect vector decomposition (FEVD), allowing the estimation of time-invariant variables in a panel data model with fixed effects ( $\lambda$ ) (Pluemper and Troeger, 2007). The first two stages are equivalent to the regressions employed in Columns (1) and (2) of Table 4. In a third stage, we estimate Equation (2), but instead of controlling for firm fixed effects we include the error from Column (2). The corresponding parameter estimates of interest for the final stage reveal that significance levels and

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<sup>12</sup> Regression without firm fixed effects but with time fixed effects.

<sup>13</sup> Regression with firm random effects and time fixed effects.



magnitudes appear to be even higher than under the RE and OLS approach. Thus, our main results can, again, be considered as conservative. *Fourth*, we account for endogenous self-selection bias using a Heckman-type correction (e.g., Heckman, 1978).<sup>14</sup> For this purpose, we estimate a probit model of the probability of a firm issuing a CSR report, predicted by year fixed effects, industry fixed effects, our previously defined control variables, as well as the following instrumental variables: CSR reporting industry pressure,<sup>15</sup> political affiliation,<sup>16</sup> and social capital.<sup>17, 18</sup> Subsequently, we include the inverse Mills ratio calculated from the probit model in our main regressions.<sup>19</sup> By doing so, the parameter estimates for our variables of interest represent the extent to which the relationship between *GRI* and *FV* is incremental to that of a firm selected at random which faces a similar industry CSR reporting pressure, has a similar political orientation, and is surrounded by similar social capital. The inverse Mills ratio is statistically insignificant in all specifications,<sup>20</sup> alleviating endogenous self-selection concerns. *Fifth*, we further address potential endogeneity concerns by employing regression discontinuities of shareholder proposals.<sup>21</sup> Specifically, we compare the effect of shareholder proposals regarding CSR reporting that pass or fail by a small margin of votes. The passage of such 'close calls' can be regarded as a random assignment of CSR reporting to firms. Since the number of proposals related to CSR reporting is very small and all approved shareholder proposals are made for B2C firms, we can only test the effect of CSR reporting on *FV* for B2C

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<sup>14</sup> This approach addresses concerns about omitted (time-varying) variables and reverse causality.

<sup>15</sup> CSR reporting industry pressure is the number of firms in the industry of the focal firm in year  $t$  that issue a GRI CSR report.

<sup>16</sup> Following Di Giuli and Kostovetsky (2014), political affiliation is defined as the CEO's cumulative contributions to Democrats divided by the cumulative contributions to both parties. If no contributions are found for the CEO in the Federal Election Commission (FEC) database (<https://fec.gov>), the value is set to 0.5.

<sup>17</sup> Following Jha and Cox (2015), we use the social capital measure developed by Rupasingha et al. (2006). We linearly interpolate the data to fill gaps. A higher variable score indicates a higher degree of social capital in the firm's county of location. Data source: Online appendix of Rupasingha et al. (2006).

<sup>18</sup> We argue that that CSR reporting industry pressure, political affiliation, and social capital are appropriate instruments as they are plausibly exogenous to the firm, significantly explain *GRI*, and can be validly excluded from our main regressions (second stage regressions). While political affiliation and social capital can be regarded as truly exogenous variables, CSR industry reporting pressure likely includes an exogenous as well as endogenous component. However, CSR industry reporting pressure is very powerful in terms of affecting *MV* only via *GRI* and not via another channel. We include all instrumental variables simultaneously as well as one by one, thereby, addressing the robustness of the selection model (Lennox et al., 2012). In all specifications of the first stage, the significance levels and the magnitudes of the instrumental variables are virtually the same. In the main regressions, the significance levels and the magnitudes of our variables of interest are also very similar. Including the Mills ratio may cause multicollinearity issues (Lennox et al., 2012). Hence, we calculate the VIFs for the second stage to check whether our selection model may suffer from multicollinearity. The correlations of the inverse Mills ratio with the other explanatory variables are well below 0.8 and all VIFs are well below the critical value of 10, alleviating multicollinearity concerns.

<sup>19</sup> As the selection regression, the main regression also includes industry fixed effects instead of firm fixed effects.

<sup>20</sup> If we include all three instrumental variables simultaneously or separately in the selection regression.

<sup>21</sup> We follow the regression discontinuity estimation approach of Cnat et al. (2012). Data on shareholder proposals are obtained from RiskMetrics and SharkRepellent.

firms. We find that the effect of  $FV$  on  $GRI$  is statistically insignificant, corroborating the coefficient  $GRI \times B2C$  in Table 4 Column (3). *Sixth*, we use a 1-year-lagged  $GRI$  variable with respect to the dependent variable  $FV$ . In reference to our main results, we find qualitatively similar results for our variables of interest. Please note that we lose one cross-sectional unit when applying a 1-year-lag structure. *Last* but not least, we rerun our main regressions using an alternative customer profile differentiation based on an individual in-depth evaluation of each firm’s activities,<sup>22</sup> i.e., product and service portfolio (Haddock-Fraser and Fraser, 2008). If a firm has B2C as well as B2B activities, such as ExxonMobil Corp., the firm is defined as B2C (Haddock-Fraser and Tourelle, 2010). This method involves much more judgment than the initial SIC code classification adopted from Srinivasan et al. (2011). Nonetheless, the correlation between the two classification procedures is high at 91%, and the estimation results are also largely consistent, showing only slightly stronger negative associations for the interaction term  $GRI \times B2B$ , when using the alternative, and more judgmental, customer profile differentiation.

Overall, our robustness analysis suggests that our main findings of Table 4 are either conservative or qualitatively similar when using alternative estimation strategies or different empirical specifications. Therefore, the validity and reliability of our derived conclusions regarding our hypotheses are corroborated.

## 8. Conclusion

This interdisciplinary study sheds light on a critical yet neglected knowledge gap: stakeholder interdependence between customers and shareholders in the realm of CSR reporting. To address this issue, we focus on customer profile differences by using a common industry dichotomy: business-to-consumer (B2C) versus business-to-business (B2B) oriented firms. Our results demonstrate that shareholders of B2B firms value the issuance of a GRI-aligned CSR report negatively, while shareholders of B2C firms show no reaction to the issuance of such a report. Consistent with our conjecture, these valuation effects differ significantly from each other. Furthermore, we show that, at lower levels of profitability, the negative valuation effect for B2B firms appears to be stronger, and B2C firms even experience a positive valuation effect. The effects, again, differ significantly

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<sup>22</sup> The information regarding firms’ activities is obtained from annual reports or corporate websites.

from each other. In contrast, at higher levels of profitability, shareholders of B2C as well as B2B firms show no reaction to the issuance of a CSR report. Collectively, we provide evidence that the value relevance of CSR reporting is much more heterogeneous than suggested by previous literature.

Several managerial implications stem from our findings. Above all, we show that CSR reporting is not beneficial *per se* and, thereby, call attention to the inherent pitfalls and traps of CSR reporting. When firms do not address end-consumers, our findings show that CSR reporting actually decreases firm value. Thus, it is essential for managers to consider customer profile differences when determining their CSR reporting strategy. Specifically, managers of B2B firms may rethink their strategic decision of issuing a CSR report, especially in cases of lower levels of profitability. In contrast, managers of less profitable B2C firms may become more proactive in communicating their CSR activities. However, as profitability increases, strategic implications are less explicit; hence, managers are advised to carefully analyze the interdependence between customers and shareholders prior to deciding on CSR reporting. Even though our findings provide evidence that CSR reporting seems to be a double-edged sword, our work in no way implies that 'doing good' is harmful. We can only affirm that for B2B firms the effort put into *communicating* their 'goodness' may be disadvantageous from a shareholder's viewpoint. Taken together, this study provides a more nuanced understanding of the value relevance of CSR reporting, and therefore, offers managers a fine-grained guidance for value relevant CSR reporting.

A few caveats of our study are worth noting. First, there is a remote possibility that we have omitted some GRI-aligned CSR reports, simply because firms may not register their report with GRI and, consequently, do not appear in the GRI database. Thus, the control group may include firms that actually issue a GRI-aligned CSR report. This potential misclassification is most likely to be biased against, rather than in support of, our results. Second, we are cautious to claim that our results provide causal evidence. Although we apply a fixed effect estimator, use lagged explanatory variables, apply a Heckman-type correction, and employ regression discontinuities, we think that we are not able to *fully* rule out endogeneity concerns—we would rather say that we are able to curtail severe endogeneity concerns. Third, our customer profile differentiation does not account for the fact that some firms address both end-consumers as well as other businesses.

These caveats notwithstanding, we believe that our findings provide interesting insights and open venues for future research. For example, as the number of CSR reports increases, the investiga-

tion of more differentiated customer profiles would be worthwhile. Furthermore, the political debate about mandatory CSR reporting may allow to better address endogeneity issues by exploiting the (staggered) adoption of laws (that mandate CSR reporting) as a natural experiment.

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**Table 1**  
Summary of issued GRI reports

		2007		2008		2009		2010		2011		2012	
		<i>B2C</i>	<i>B2B</i>	<i>B2C</i>	<i>B2B</i>	<i>B2C</i>	<i>B2B</i>	<i>B2C</i>	<i>B2B</i>	<i>B2C</i>	<i>B2B</i>	<i>B2C</i>	<i>B2B</i>
<i>GRI</i>	7	28	10	49	21	62	81	26	81	31	95	95	315
Non- <i>GRI</i>	84	227	76	204	71	199	68	174	174	57	172	356	976

Table A2 outlines definitions and data sources for the variables *GRI*, *B2C*, and *B2B*. The sample includes 1,742 firm-year observations.

**Table 2**  
Summary statistics and correlation matrix

Variable	Obs.	Mean	SD	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1) <i>FV</i>	1,742	0.99	0.61									
(2) <i>GRI</i>	1,742	0.24	0.42	-0.02								
(3) <i>B2C</i>	1,742	0.26	0.44	0.17	-0.03							
(4) <i>SIZE</i>	1,742	2.11	1.15	-0.14	0.31	0.15						
(5) <i>PROFIT</i>	1,742	8.53	7.09	0.49	0.04	0.10	-0.06					
(6) <i>DIV</i>	1,742	0.72	0.45	-0.09	0.18	0.08	0.35	-0.01				
(7) <i>GROW</i>	1,742	0.08	0.12	-0.18	0.02	-0.21	-0.14	-0.16	0.07			
(8) <i>RD</i>	1,742	0.03	0.04	0.20	0.03	-0.25	-0.20	0.08	-0.33	-0.16		
(9) <i>FROEIGN</i>	1,742	0.33	0.27	0.24	0.16	-0.19	-0.02	0.21	-0.06	-0.18	0.46	
(10) <i>DEBT</i>	1,742	84.08	109.94	0.16	-0.02	0.06	0.09	-0.20	0.08	0.05	-0.19	-0.10

Columns (1)–(9) display Pearson pair-wise correlation coefficients. Table A2 outlines definitions and data sources for the variables.

**Table 3**  
Mean analysis of *FV*

	Non- <i>GRI</i> (1)	<i>GRI</i> (2)	Difference (2)–(1)
Panel A. Entire Sample			
<i>B2C</i>	1.11	1.38	<b>0.27</b>
<i>B2B</i>	0.96	0.84	<b>−0.12</b>
Panel B. Profit<Median			
<i>B2C</i>	0.72	1.00	<b>0.28</b>
<i>B2B</i>	0.71	0.56	<b>−0.15</b>
Panel C. Profit≥Median			
<i>B2C</i>	1.39	1.53	0.14
<i>B2B</i>	0.96	0.84	−0.10

This table reports mean comparison tests for *FV* across non-*GRI* (1,332 obs.) vs. *GRI* (410 obs.) reporters, and *B2C* (451 obs.) vs. *B2B* (1,291 obs.) firms. Mean comparison tests are conducted for the entire sample as well as two subsamples: less profitable (871 obs.) and profitable (871 obs.) firms, respectively. Table A2 outlines definitions and data sources for the variables *FV*, *GRI*, *B2C*, and *B2B*. Values in boldface denote statistical significance at the 1% level.

**Table 4**  
Regression results

	(1)	(2)	(3)	(4)	(5)
	Base model	FE residual	Extended model	Profit<Median	Profit≥Median
<i>GRI</i>	−0.039 (−1.33)				
<i>GRI</i> × <i>B2C</i>			0.065 (1.44)	0.183*** (3.55)	−0.054 (−1.06)
<i>GRI</i> × <i>B2B</i>			−0.078** (−2.29)	−0.101*** (−2.15)	−0.066 (−1.22)
<i>SIZE</i>	−0.180*** (−3.08)		−0.179*** (−3.05)	−0.133** (−2.23)	−0.268** (−2.12)
<i>PROFIT</i>	0.011*** (4.83)		0.011*** (4.82)	0.007*** (2.82)	0.022*** (4.49)
<i>DIV</i>	0.040 (0.66)		0.039 (0.65)	0.098 (1.29)	−0.052 (−0.58)
<i>GROW</i>	−0.266 (−0.98)		−0.258 (−0.95)	−0.100 (−0.25)	−0.638 (−0.89)
<i>RD</i>	4.810*** (4.34)		4.794*** (4.33)	5.307** (2.42)	4.675 (3.56)
<i>FOREIGN</i>	0.089 (0.75)		0.090 (0.74)	−0.012 (−0.09)	0.036 (0.18)
<i>DEBT</i>	0.002*** (7.87)		0.002*** (7.94)	0.002*** (9.89)	0.001* (1.87)
<i>B2C</i>		0.364*** (6.28)			
Firm fixed effects	yes	no	yes	yes	yes
Time fixed effects	yes	no	yes	yes	yes
F-statistics					
Interaction effects ( $H_0 : GRI \times B2C = GRI \times B2B$ )			7.14***	18.42***	0.03
Time fixed effects ( $H_0 : YEAR_1 = YEAR_2 = YEAR_3 = YEAR_4 = 0$ )	129.26***		129.00***	63.33***	74.79***
$R^2$	0.40 (within)	0.10	0.40 (within)	0.46 (within)	0.44 (within)
Observations	1,742	384	1,742	871	871

This table reports estimates and t-statistics (in parenthesis) that are based on robust standard errors. Firm and time fixed effects are not provided. The dependent variable in Columns (1), (3), (4), and (5) is *FV*. The dependent variable in Column (2) is the individual firm fixed effect residual ( $\lambda$ ) obtained from Column (1). The F-statistics are reported for equality tests of the coefficients of the interaction effects ( $H_0 : GRI \times B2C = GRI \times B2B$ ), and for joint significance tests of the coefficients of the time fixed effects ( $H_0 : YEAR_1 = YEAR_2 = YEAR_3 = YEAR_4 = 0$ ). Table A2 outlines definitions and data sources for the variables. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively.

## Appendix

**Table A1**

Extended summary statistics for *GRI*, *B2C*, and *B2B*

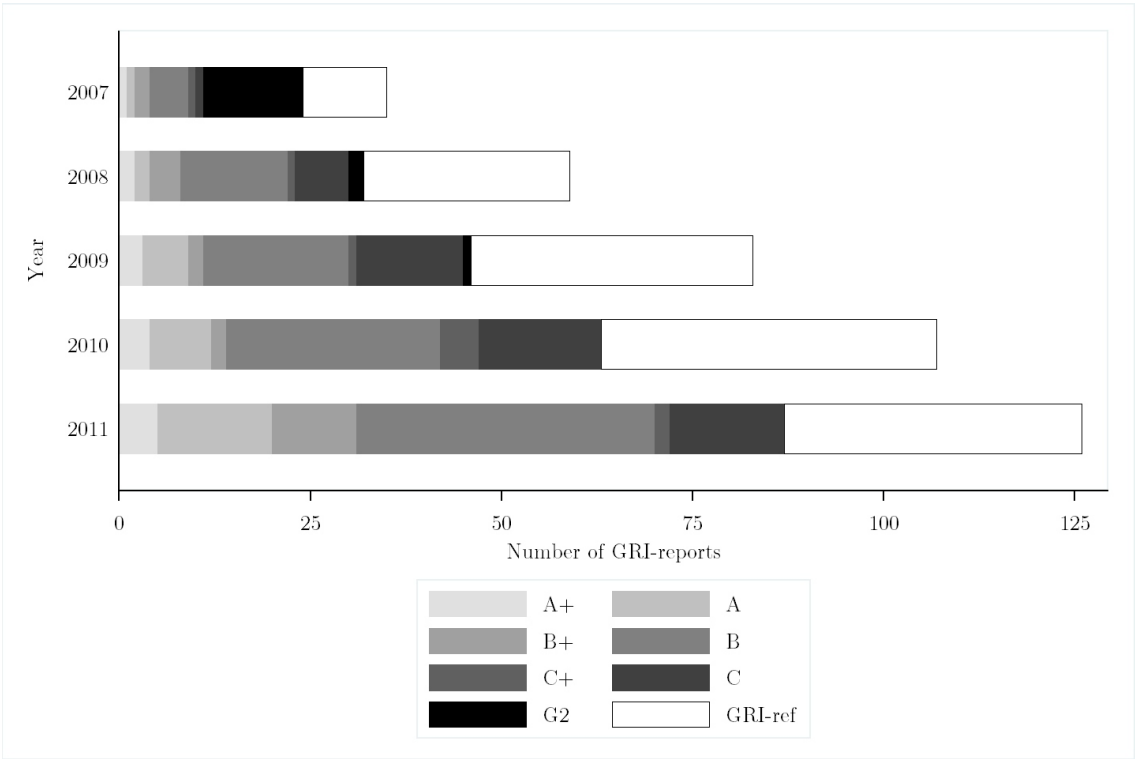
		Mean	SD	Min.	Max.	Obs.
<i>GRI</i>	overall	0.235	0.424	0	1	1,742
	between		0.358	0	1	
	within		0.229	−0.565	1.035	
<i>B2C</i>	overall	0.259	0.438	0	1	1,742
	between		0.442	0	1	
	within		0	0.259	0.259	
<i>B2B</i>	overall	0.741	0.438	0	1	1,742
	between		0.442	0	1	
	within		0	0.741	0.741	

**Table A2**

Variable definitions and data sources

Variable name	Definition	Source
<i>FV</i>	Natural logarithm of market-to-book ratio	Thomson Reuters Datastream
<i>GRI</i>	Dummy variable that equals one if the firm issues a GRI report, and zero otherwise.	GRI Sustainability Disclosure Database
<i>B2C</i>	Dummy variable that equals one if the firm addresses end-consumers, and zero otherwise.	Based on Srinivasan et al. (2011); Thomson Reuters Datastream
<i>B2B</i>	Dummy variable that equals one if the firm addresses other businesses, and zero otherwise.	Based on Srinivasan et al. (2011); Thomson Reuters Datastream
<i>SIZE</i>	Natural logarithm of total sales	Thomson Reuters Datastream
<i>PROFIT</i>	Return on assets	Thomson Reuters Datastream
<i>DIV</i>	Dummy variable that equals one if the firm pays a dividend, and zero otherwise	Thomson Reuters Datastream
<i>GROW</i>	Capital expenditures divided by total sales	Thomson Reuters Datastream
<i>RD</i>	Research and development expenditures divided by total assets	Thomson Reuters Datastream
<i>FOREIGN</i>	Foreign sales divided by total sales	Thomson Reuters Datastream
<i>DEBT</i>	Debt-to-equity ratio	Thomson Reuters Datastream

**Figure A1**  
Types of GRI reports issued



# IV. Cultural rule orientation, legal institutions, and the credibility of corporate social responsibility reports\*

Tobias Steindl†

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## Abstract

Culture matters for *credible* corporate social responsibility (CSR) reporting. I show that firms located in countries with stronger cultural rule orientation are more likely (1) to receive assurance on their CSR report, (2) to receive assurance from an accounting firm, (3) to receive assurance in accordance with an assurance standard, and (4) to receive assurance on their *entire* CSR report. Path analysis reveals that the *direct* effect of cultural rule orientation is much stronger than its *indirect* effect via legal institutions. In fact, the indirect effect, at most, accounts for 23% of the total effect, while the direct effect, at least, accounts for 77% of the total effect. I confirm the dominance of the direct effect in a qualitative comparative analysis. My findings survive several estimation approaches that address endogeneity concerns (e.g., an instrumental variable approach). Finally, I provide auxiliary evidence from a quasi-natural experiment, showing that firms located in countries with stronger cultural rule orientation respond stronger to the adoption of Directive 2014/95/EU (the so-called EU CSR reporting directive).

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*Keywords:* Assurance, corporate social responsibility, culture, legal institutions, non-financial reporting, rule orientation, uncertainty avoidance

*JEL classification:* M14, M41, M42, Z1

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*"Culture must be one of the foundations for world understanding."*—Albert Einstein, 1951, p. 32

*"Max Weber was right. If we learn anything from history [...], it is that culture makes almost all of the difference."*—David Landes, 2000, p. 2

## 1. Introduction

Corporate social responsibility (CSR) reporting has gained momentum.<sup>1</sup> To date, over 90% of the world's largest firms are issuing CSR reports (KPMG, 2017). Yet, the *credibility* of these reports varies substantially, particularly between countries (Moser and Martin, 2012; Cahan et al., 2016; KPMG, 2017). In fact, the percentage of firms in a country receiving assurance on their CSR reports ranges from 0% to over 90%.<sup>2</sup> To understand why these cross-country disparities are so large and persistent, scholars have turned to legal institutions for an explanation (Simnett et al., 2009; De Beelde and Tuybens, 2015; Zhou et al., 2016). In this study, I explore the role of culture and its interdependence with legal institutions to provide a more holistic explanation.<sup>3</sup>

The idea that culture matters goes back to at least Max Weber (1930), who argues that culture is a central ingredient of economic development. However, economics scholars have been very hesitant to integrate culture into their theoretical work. A notable exception is Oliver E. Williamson (2000), who provides a prominent theory that cautiously accounts for culture. Specifically, he theorizes that culture imposes constraints on legal institutions (e.g., laws, regulations, constitutions), which, in turn, shape corporate decision-making. Accordingly, culture indirectly affects corporate decisions via legal institutions. Building on Williamson's economics-based theory, I hypothesize (H1) that culture *indirectly*—via legal institutions—shapes firms' tendencies toward credible CSR reporting.

Sociology scholars have been much more progressive in integrating culture into their theoretical work. Most prominently, W. Richard Scott (2001) theorizes that both culture and legal institutions *directly* shape corporate decisions. Specifically, he argues that culture dictates socially appropriate

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<sup>1</sup> CSR reporting can be defined as the reporting about "economic, environmental, and social impacts caused by [an organization's] everyday activities" (<http://globalreporting.org>). CSR reporting is also referred to as (corporate) non-financial reporting, (corporate) sustainability reporting, and (corporate) accountability reporting (O'Dwyer et al., 2005; Dhaliwal et al., 2012; Lys et al., 2015).

<sup>2</sup> Please see Table 1 for exact numbers. Studies covering more countries provide similar statistics (KPMG, 2017).

<sup>3</sup> I follow Guiso et al. (2006) to define culture as the societal beliefs and social norms that are transmitted fairly unchanged from generation to generation.

thoughts, feelings, and actions, thereby directly shaping corporate decision-making. Legal institutions, he argues, also directly shape corporate decisions because they prescribe which actions are and are not condoned by the legal authority of a country. Building on Scott’s sociology-based theory, I hypothesize (H2) that culture *directly* shapes firms’ tendencies toward credible CSR reporting.

To test these two competing (but not mutually exclusive) hypotheses, I rely on the following sequence of decisions that determine the *credibility* of CSR reports. To begin with, firms decide (1) whether to receive assurance on their CSR report. Next, firms select (2) the assurance provider: an accounting or non-accounting firm. Together with the chosen assurance provider, firms then decide (3) whether the assurance is conducted in accordance with an assurance standard, (4) whether the entire or only parts of their CSR report is assured, and, finally, (5) whether the level of assurance is limited or high. A unique feature of these five fundamental decisions is that they are not regulated in terms of CSR reporting, but they are strictly regulated in terms of financial reporting.<sup>4</sup> Taking advantage of this feature, I conjecture that managers of firms located in a country with a stronger *rule orientation culture* are more likely to make CSR reporting decisions that comply with financial reporting regulations because they are mentally programmed to rely on laws, rules, and regulations when making decisions (Venaik and Brewer, 2010; Salvato et al., 2014).

I measure *cultural rule orientation* by using the Uncertainty Avoidance (UA) index from House et al. (2004) (i.e., the GLOBE project). A higher UA index score means that people in a country strive to avoid uncertainty by “seeking orderliness, consistency, structure, formalized procedures, and laws to cover situations in their daily lives” (House et al., 2004, p. 603). In other words, a higher UA index score indicates that people feel comfortable in regulated environments, rely on rules and regulations when making decisions, and believe that matters that can be regulated should not be left to chance (House et al., 2004). Compared to the UA index from Hofstede (1980) that captures stress orientation, the UA index from House et al. (2004), in essence, “represent[s] a single rule orientation component of UA” (Venaik and Brewer, 2010, p. 1304).

To measure legal institutions, I construct a factor variable in the spirit of Dhaliwal et al. (2012)

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<sup>4</sup> By 2016, China, Denmark, Malaysia, South Africa, and India had mandated CSR reporting in some way (Chen et al., 2017; Ioannou and Serafeim, 2017; Manchiraju and Rajgopal, 2017). However, no country mandates that firms issue a stand-alone CSR report in alignment with the Global Reporting Initiative (GRI) standards, or mandates assurance on firms’ CSR reports. Since this study focuses on corporate decisions related to the assurance process of GRI-aligned CSR reports, all decisions are completely voluntary around the world. In robustness tests, I exclude the aforementioned countries and obtain similar results, which indicates that my main results are not driven by countries mandating CSR reporting in some way.

that captures legal institutions of particular relevance to CSR reporting. Specifically, I compute a factor variable based on legal origin, labor laws, environmental laws, and CSR reporting regulations in a country.<sup>5</sup> A higher variable score indicates stronger legal institutions in terms of CSR.

The baseline sample of this study comprises all CSR reports issued in alignment with the Global Reporting Initiative (GRI) between 2012 and 2016. I focus on GRI-aligned CSR reports (hereafter, GRI reports) because no country in the world mandates the issuance of a GRI report, but the vast majority of firms issue such a report.<sup>6</sup> Further, detailed information on the assurance process is only available (from 2012) for GRI reports. I identify 6,589 GRI reports issued by 1,620 firms located in 31 countries; 2,389 of the identified GRI reports are assured.

The decisions analyzed in this study are sequential in nature. Only if a firm decides to receive assurance on its GRI report, does it move on to the decisions related to the assurance process (e.g., whether to receive assurance in accordance with an assurance standard). Hence, the empirical tests on the decision whether to receive assurance are based on the baseline sample comprising all 6,589 firm-years with GRI reports. All further decisions related to the assurance process are based on the confined sample comprising 2,389 firm-years with assured GRI reports. Since these samples are not random (i.e., they cover only firm-years with GRI reports and only firm-years with assured GRI reports), one might worry that my findings are affected by a self-selection bias. I address this issue in a robustness test.

I conduct three main empirical tests. First, I employ a path analysis approach that allows me to disentangle the direct effect of cultural rule orientation from its indirect effect via legal institutions. I find that cultural rule orientation shapes credible CSR reporting both directly and indirectly via legal institutions. Specifically, firms located in countries with stronger cultural rule orientation are more likely (1) to receive assurance on their GRI report, (2) to receive assurance from an accounting firm, (3) to receive assurance in accordance with an assurance standard, and (4) to receive assurance on their *entire* GRI report. I find no evidence that firms located in countries with stronger cultural rule orientation are more likely (5) to receive assurance on a high level. The *direct* effects of cultural rule orientation on the first four decisions account for between 77% and 93% of the total

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<sup>5</sup> All CSR regulations are related to CSR reporting and no regulation is related to the assurance of a CSR report. In addition, no CSR regulation mandates that firms issue a stand-alone CSR report in alignment with the GRI standards.

<sup>6</sup> In fact, in 2015, 89% of the world's largest firms issued a GRI-aligned CSR report (KPMG, [2017](#)).

effects. In contrast, the *indirect* effects of cultural rule orientation only account for between 7% and 23% of the total effects. These findings strongly suggest that the direct effect of culture on the credibility of CSR reporting is much stronger than its indirect effect via legal institutions. Hence, the hypothesized indirect effect (H1) is weakly supported, while the hypothesized direct effect (H2) is strongly supported.

Second, I employ a qualitative comparative analysis approach. The advantage of this approach is that, instead of using the previously described factor variable to measure legal institutions, I can use all four variables individually and examine their interdependence with culture and each other. In terms of the assurance provider choice, where culture has the strongest indirect effect in the path analysis, I find that the five variables together—that is, the four legal institutional variables and the cultural rule orientation variable—have a coverage of 22%, which means that *together* they explain 22% of the outcome. The consistency of this combination is very high, with 85%. However, I also find that cultural rule orientation alone has a coverage of 63%, with a consistency of 81%, indicating that the cultural variable alone explains 41% more than its combination with the legal institutional variables. For all other decisions, I find weaker effects for the combination of all variables, but stronger effects for the cultural variable alone. Hence, the findings from the qualitative comparative analysis also indicate that the direct effect of cultural rule orientation on the CSR reporting credibility is stronger than its indirect effect via legal institutions.

Third, I employ a probit regression approach to better understand the economic significance of the direct cultural effect. I find that moving from the country with the weakest rule orientation culture to the country with the strongest rule orientation culture increases (1) the likelihood of receiving assurance on a GRI report by 65%, (2) the likelihood of receiving assurance from an accounting firm by 59%, (3) the likelihood of receiving assurance in accordance with an assurance standard by 36%, and (4) the likelihood of receiving assurance in the *entire* GRI report by 42%. These magnitudes show that the direct cultural effects are economically sizable.

A major concern of any cross-country study is endogeneity in terms of *omitted variables*. It is possible that countries that differ in terms of cultural rule orientation also differ on other unobservable factors. Hence, my estimates might simply capture the effect of those other factors, rather than the cultural effect itself. To confidently mitigate this concern, I employ three alternative approaches. First, I control for several alternative sets of country-level control variables. Second,

I employ a hierarchical generalized linear regression approach, which accounts for the multi-level structure (country- and firm-level) of the dataset by performing an intercept-as-outcomes random effects model. Third, I employ an instrumental variable approach, using genetic distance as the instrument. The chosen genetic distance variable plausibly satisfies the exclusion and inclusion restriction, as discussed in detail in the endogeneity section. My main findings are robust to all three alternative approaches. Hence, I am confident that my findings are not spurious, capturing the effect of other country-level factors instead of the cultural effect itself.

Another concern, particularly of cultural studies, is endogeneity in terms of *measurement*. It is possible that the cultural rule orientation variable does not capture rule orientation, but something else. Given the absence of alternative measures that explicitly capture cultural rule orientation, I develop a novel variable based on the Google search volume in a country. The rationale behind this variable is that people with a stronger preference for rules are more likely to search for legal terms on Google. In other words, because of their intrinsic urge to comply with the law, they are more likely to search for laws on Google. My results are robust to this big-data-based alternative cultural variable. Further, I use another cultural variable that does not explicitly capture rule orientation, but a concept related to rule orientation: tightness. As expected, the effect of this cultural measure is not as clear as the effect of cultural rule orientation but still points in the same direction. Taken together, these findings indicate that the main cultural rule orientation variable captures rule orientation and not something else.

Finally, I exploit a quasi-natural experiment based on the adoption of Directive 2014/95/EU—the so-called EU CSR reporting directive. This directive provides an exogenous shock to the (legal) importance of (credible) CSR reporting within the European Union (EU) because it *mandates* CSR reporting and *encourages* assurance. Using a difference-in-difference estimator, I find that, after the adoption of the directive, firms located in countries with stronger cultural rule orientation are more likely (1) to receive assurance on their GRI report, (2) to receive assurance from an accounting firm, (3) to receive assurance in accordance with an assurance standard, (4) to receive assurance on their *entire* GRI report, and (5) to receive assurance on a high level, compared to firms located in countries with weaker cultural rule orientation. In essence, these findings suggest that firms located in countries with stronger cultural rule orientation respond more strongly to the EU CSR reporting directive, compared to firms located in countries with weaker cultural rule orientation.

This study contributes to the literature in several distinct ways and directly responds to some recently raised research questions. First, and most obviously, this study contributes to the literature explaining cross-country differences in the credibility of CSR reports. Thus far, scholars have focused on legal institutions, such as legal origin or rule of law (Simnett et al., 2009; Kolk and Perego, 2010; Herda et al., 2014; De Beelde and Tuybens, 2015; Fernandez-Feijoo et al., 2015; Zhou et al., 2016). I add culture as an incrementally important determinant by providing evidence that culture directly and (to a lesser extent) indirectly affects the credibility of CSR reports. In doing so, I respond to the recently raised question: "how does culture affect accounting outcomes in practice?" (Schatt et al., 2016, p. 33).<sup>7</sup>

Second, I contribute to the few studies examining the interdependence between culture and legal institutions (Licht et al., 2005; Licht et al., 2007; Gorodnichenko and Roland, 2017). By showing that the *direct* effect of cultural rule orientation on credible CSR reporting is much stronger than its *indirect* effect via legal institutions, I respond to the following questions: "how does [culture] relate to legal institutions? [...] Which type of norms—cultural or legal—are more effective in pursuing a certain policy?" (Guiso et al., 2015, pp. 336, 337). Further, by being the first to explicitly focus on cultural rule orientation, I address the issue raised by La Porta et al. (2013, p. 461), who argue that the interdependence between culture and legal institutions can only be appropriately studied if "something like *legal culture*" is considered. In addition, by using path analysis and qualitative comparative analysis to test for the interdependence between culture and legal institutions, I directly respond to Leuz and Wysocki (2016, p. 596), who suggest that these methods should be applied in future research. In particular, they state that these methods "help identify which factors are more likely to be economic primitives that directly affect outcomes and which factors are associated outcomes or second-order mediating factors."<sup>8</sup>

Third, I contribute to the nascent literature that uses culture to explain corporate decision-making.<sup>9</sup> I show that culture also matters for corporate decisions related to the credibility of CSR

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<sup>7</sup> A similar growing interest in culture is observed in finance. Zingales (2015, p. 3) states that "[t]he 'cultural revolution' in finance has just started, and it opens an infinite set of possibilities."

<sup>8</sup> Alesina and Giuliano (2015) also encourage the use of structural estimation approaches such as path analysis to better understand the interdependence between culture and legal institutions.

<sup>9</sup> For example, prior studies show that culture has a direct effect on corporate investment decisions (Shao et al., 2013), earnings management (Han et al., 2010), corporate cash holding (Chen et al., 2015), mergers (Ahern et al., 2015), CSR investment decisions (Ioannou and Serafeim, 2012), auditor choice (Hope et al., 2008), CSR reporting (Orij, 2010; Cahan et al., 2016; Luo and Tang, 2016), corporate misconduct (Liu, 2016), accounting conservatism (Kanagaretnam et al., 2014), integrated reporting (Garcia-Sanchez et al., 2013), management control systems (Van

reports. Further, by focusing on cultural rule orientation instead of UA, I contribute to the literature by providing more fine-grained evidence on how UA affects corporate decision-making.<sup>10</sup>

Fourth, I contribute to the literature investigating corporate decisions related to the credibility of CSR reports. Thus far, the literature has focused on the following two decisions: whether to receive assurance on a CSR report, and which assurance provider to select.<sup>11</sup> I consider three additional corporate decisions: whether the assurance is conducted in accordance with an assurance standard, whether the entire or only parts of a CSR report is assured, and whether the level of assurance is limited or high. In doing so, I dig deeper and provide more comprehensive evidence on the credibility of CSR reports.

Beyond these contributions, this study may be of interest to the general public. Data from Google web search activities, which are plotted in Figure A1, indicate that people simultaneously search for 'culture' and 'how it affects'. In addition, they simultaneously search for 'culture' and 'the law'. These Google search patterns suggest that the general public is interested in both how culture affects decisions and how culture is related to the law.

The rest of the paper is organized as follows. Section 2 develops the hypotheses. Section 3 defines the variables measuring credible CSR reporting. Section 4 defines the cultural rule orientation variable. Section 5 defines the legal institutional variable. Section 6 describes the sample and the data sources. Section 7 presents the main findings. Section 8 addresses endogeneity. Section 9 presents findings from a quasi-natural experiment. Section 10 concludes.

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der Stede, 2003), and corporate tax evasion (De Backer et al., 2015).

<sup>10</sup> Studies based on the general concept of UA include Han et al. (2010), Garcia-Sanchez et al. (2013), Kanagaretnam et al. (2014), Chen et al. (2015), Cahan et al. (2016), and Dou et al. (2016).

<sup>11</sup> Studies investigating one or both of these decisions include Simnett et al. (2009), Kolk and Perego (2010), Herda et al. (2014), De Beelde and Tuybens (2015), Casey and Grenier (2015), Fernandez-Feijoo et al. (2015), Gillet-Monjarret (2015), and Peters and Romi (2015).

## 2. Theoretical foundation and hypotheses<sup>12</sup>

### 2.1 *Defining culture and legal institutions*<sup>13</sup>

I follow Guiso et al. (2006) and define *culture* as the societal beliefs and social norms that are transmitted fairly unchanged from generation to generation. This definition acknowledges the intrinsic and extrinsic nature of culture. While societal beliefs are predominantly generated by genetics, social norms are predominantly generated by participation in networks (Henreich et al., 2005; Collier, 2016). Further, this definition emphasizes intergenerational transmission—the reason why culture changes so slowly (Guiso et al., 2016).

I follow North (1991) and define *legal institutions* as the formal regulations and rules that govern behavior, such as laws, constitutions, and property rights.

### 2.2 *Economics-based theory*

Economics scholars have been hesitant to integrate culture into their theoretical work (Guiso et al., 2006).<sup>14</sup> A notable exception is Oliver E. Williamson (2000), who cautiously accounts for culture in his seminal article 'The new institutional economics'.<sup>15</sup> In this article, Williamson theorizes that culture imposes constraints on legal institutions, which, in turn, shape resource allocation (i.e., corporate decision-making). This means that culture only *indirectly*—via legal institutions—shapes corporate decision-making. The rationale behind Williamson's theory is that if they are not supported by the country's cultural system, legal institutions either do not survive or are ineffective. In other words, if laws conflict with societal beliefs and social norms, compliance and enforcement is weaker. Anecdotal evidence supports this reasoning. For example, many developing countries imitated the United States (US) Constitution to prosper, but they failed because the cultural

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<sup>12</sup> Similar to the theories used in this section, Gray (1988) predicts that culture is directly and indirectly (via institutional consequences) related to a country's accounting system. However, his predictions are less clear in terms of legal institutions and the direction of the relations. Dougnik and Tsakumis (2004) extend Gray's model to (firm-level) accounting decisions but they are also unclear about legal institutions and the direction of the relations.

<sup>13</sup> Some scholars distinguish between informal and formal institutions rather than culture and legal institutions. I follow Alesina and Giuliano (2015, p. 902), and others, who use culture and legal institutions because these terms are "more appropriate and less confusing".

<sup>14</sup> The focus has been on legal institutions. La Porta et al. (2008) and La Porta et al. (2013) systematically review the literature on the economic consequences of legal institutions (i.e., legal origins).

<sup>15</sup> Williamson (2000) builds on the work of North (1991), who stresses the interdependence of culture and legal institutions but does not specify the nature of the interdependence (i.e., *how* culture and legal institutions are related).



foundation of the law did not support the power of a jury to disregard the law in convicting or absolving (Guiso et al., 2015).

Two strands of empirical literature are related to Williamson’s theory. The first strand, comprising just three studies, examines the *direct* effect of culture on legal institutions. Licht et al. (2005) show that several dimensions of culture affect the legal protection of investors. Similarly, Licht et al. (2007) provide evidence that several dimensions of culture affect the rule of law. Most recently, Gorodnichenko and Roland (2017) show that the cultural dimension individualism affects the legal protection against expropriation risk.

The second strand, which comprises numerous studies, examines the *direct* effect of legal institutions on corporate decision-making.<sup>16</sup> For example, Simnett et al. (2009) show that the rule of law and legal origin directly shape firms’ tendencies toward credible CSR reporting. Similarly, Kolk and Perego (2010) provide evidence that legal origin and rule of law shape firms’ tendencies toward credible CSR reporting. Most recently, Zhou et al. (2016) show that the rule of law and legal origin directly shape firms’ tendency towards credible greenhouse gas reporting.

Although no study has connected these two strands of literature (i.e., has examined the *indirect* effect of culture on corporate decision-making via legal institutions), the separate findings point toward an overall indirect effect. Building on Williamson’s economics-based theory and the related empirical literature, I formulate my first hypothesis as follows.

**Economics-based hypothesis (H1):** Culture shapes firms’ tendencies toward credible CSR reporting *indirectly* via legal institutions.

### 2.3 *Sociology-based theory*

Compared with economics scholars, sociology scholars have been much more progressive in integrating culture into their theoretical work. Most prominently, W. Richard Scott (2001) considers culture as the most fundamental ‘institutional pillar’ in his seminal work ‘Institutions and organization’. Specifically, he theorizes that culture dictates appropriate thoughts, feelings, and actions,

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<sup>16</sup> Simnett et al. (2009), Kolk and Perego (2010), and Zhou et al. (2016) are most closely related to my study. Further studies examining the effect of legal institutions on accounting-related corporate decision-making include Ball and Robin (2000), Leuz et al. (2003), Bushman et al. (2004), Bushman and Piotroski (2006), Gaio (2010), Herda et al. (2014), De Beelde and Tuybens (2015), Fernandez-Feijoo et al. (2015), Isidro and Marques (2015), Cahan et al. (2016), and De Villiers and Marques (2016).

thereby, *directly* shaping corporate decision-making. Culturally-motivated decisions are particularly persistent because they are made unconsciously, reflecting taken-for-granted traits and routines. In terms of legal institutions, Scott theorizes that they also *directly* shape corporate decisions because they prescribe which actions are and are not condoned by the legal authority of a country. However, legally-motivated decisions are less persistent because they are made consciously, reflecting rational behavior based on sanctions and conformity. According to Scott’s theory, both culture and legal institutions *directly* shape corporate decision-making with culture being the dominating force.<sup>17</sup>

Several recent studies provide evidence that culture directly shapes corporate decision-making;<sup>18</sup> three of which are particularly relevant to my study because they cover the decision whether to voluntarily disclose CSR-related information. Orij (2010) shows that several dimensions of culture affect the likelihood of voluntarily disclosing CSR information. Similarly, Cahan et al. (2016) provide evidence that two cultural dimensions (i.e., UA and individualism) affect the likelihood of voluntarily disclosing CSR information. Focusing on environmental disclosures, Luo and Tang (2016) show that several cultural dimensions affect the likelihood of voluntarily disclosing carbon-related information. Building on Scott’s sociology-based theory and the related empirical literature, I formulate my second hypothesis as follows.

**Sociology-based hypothesis (H2):** Culture *directly* shapes firms’ tendencies toward credible CSR reporting.

### 3. Measuring credible CSR reporting

*Financial reporting* is strictly regulated around the world. To ensure credibility, firms are required (1) to receive assurance on their financial report, (2) to select a qualified assurance provider (i.e., an accounting firm), (3) to receive assurance in accordance with specific assurance standards (e.g., International Standards on Auditing (ISA)), (4) to receive assurance on their *entire* financial report,

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<sup>17</sup> To make Scott’s sociology-based theory more appealing to economics scholars, I provide a parsimonious formal model in Appendix B. The model proposes that culture directly shapes corporate decision-making.

<sup>18</sup> For example, recent studies show that culture has a *direct* effect on corporate investment decisions (Shao et al., 2013), earnings management (Han et al., 2010), corporate cash holding (Chen et al., 2015), CSR investment decisions (Ioannou and Serafeim, 2012), auditor choice (Hope et al., 2008), corporate misconduct (Liu, 2016), accounting conservatism (Kanagaretnam et al., 2014), integrated reporting (Garcia-Sanchez et al., 2013), management control systems (Van der Stede, 2003), and corporate tax evasion (De Backer et al., 2015).

and (5) to receive assurance on a high level (i.e., the assurance risk has to be below a certain threshold).

In contrast, *CSR reporting* is barely regulated around the world.<sup>19</sup> Firms themselves decide (1) whether to receive assurance on their CSR report, (2) whether to receive assurance from an accounting or non-accounting firm,<sup>20</sup> (3) whether the assurance is conducted in accordance with an assurance standard (e.g., AccountAbility 1000 Assurance Standard (AA1000AS)), (4) whether the entire or only parts of the CSR report are assured, and (5) whether the assurance level is high or low.<sup>21</sup>

To measure the degree of credibility of a CSR report, I create five dummy variables based on the five decisions described in the previous paragraph. First, *Assurance* equals one if a firm receives assurance on its GRI report, and zero otherwise. Second, *Assurance Provider* equals one if a firm receives assurance on its GRI report from an accounting firm, and zero otherwise. Third, *Assurance Standard* equals one if a firm receives assurance on its GRI report in accordance with an international CSR assurance standard (i.e., AA1000AS and/or International Standard on Assurance Engagements 3000 (ISAE 3000)). Fourth, *Assurance Scope* equals one if a firm receives assurance on its *entire* GRI report, and zero otherwise. Fifth, *Assurance Level* equals one if a firm receives assurance on its GRI report at a high level, and zero otherwise. Table 1 summarizes these five decisions by country.

## 4. Measuring culture

Culture is a complex and multi-dimensional construct. Hence, it is important to focus on a specific cultural dimension "so that it becomes easier to identify a causal link from culture to economic outcomes" (Guiso et al., 2006, p. 23). In this study, I focus on the cultural dimension *rule orientation*.

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<sup>19</sup> By 2016, China, Denmark, Malaysia, South Africa, and India mandated CSR reporting in some way (Chen et al., 2017; Ioannou and Serafeim, 2017; Manchiraju and Rajgopal, 2017). However, no country mandates that firms issue a stand-alone CSR report in alignment with the GRI standards, or mandates assurance on firms' CSR reports. Since this study focuses on corporate decisions related to the assurance process of GRI-aligned CSR reports, all decisions are completely voluntary around the world.

<sup>20</sup> Non-accounting firms range from engineering firms to CSR service firms (Peters and Romi, 2015). Simnett et al. (2009), Casey and Grenier (2015), and Peters and Romi (2015) discuss why and show that accounting firms provide higher (perceived) assurance quality than non-accounting firms. The higher audit quality increases the credibility of CSR reports to a greater extent.

<sup>21</sup> Please refer to O'Dwyer (2011) and Mori Junior et al. (2014) for more detailed information on the assurance process of CSR reports.

In general, cultural rule orientation emphasizes people’s proclivity for adhering to laws, rules, and regulations (Venaik and Brewer, 2010; Salvato et al., 2014). More precisely, people from stronger rule-oriented cultures seek ”orderliness, consistency, structure, formalized procedures, and laws to cover situations in their daily lives” (House et al., 2004, p. 603). They are mentally programmed to feel comfortable in regulated environments (Salvato et al., 2014). They rely on rules, laws, and regulations when making decisions (Venaik and Brewer, 2010). They believe that matters that can be regulated should not be left to chance (House et al., 2004). In contrast, people from less rule-orientated cultures have an emotional ’horror of rules’ (House et al., 2004). They believe that rules should be established and followed only in case of absolute necessity, because they believe that many issues can be solved without formal rules (Venaik and Brewer, 2010).

The concept of cultural rule orientation is particularly well-suited for my study because it is related to both legal institutions *and* credible CSR reporting. In terms of legal institutions, I conjecture that countries with a stronger rule orientated culture have more laws and regulations in place. In terms of credible CSR reporting, I conjecture that managers of firms located in countries with a stronger rule orientated culture are more likely to make CSR reporting decisions that comply with financial reporting regulations, even though these regulations do not apply to CSR reporting. For example, I conjecture that firms from stronger rule orientated cultures are more likely to receive assurance on their GRI reports because assurance is mandatory (i.e., mandated by the law) for financial reports.

To measure cultural rule orientation, I use the UA index from House et al. (2004) because it ”represent[s] a single rule orientation component of UA” (Venaik and Brewer, 2010, p. 1304).<sup>22</sup> More precisely, the variable *UA Rule Orientation* represents the UA index from House et al. (2004), with a higher variable score indicating stronger cultural rule orientation. As reported in Table 1, *UA Rule Orientation* ranges from 3.39 (Greece) to 5.37 (Switzerland).

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<sup>22</sup> The most prominent measurement systems for culture are Hofstede (1980) and House et al. (2004) (i.e., GLOBE). ”Hofstede has originated one of the most influential frameworks in international business research. No less impressive is the scale of the GLOBE project, which is probably the most sophisticated project undertaken in international business research” (Leung, 2006, p. 881). While both provide data on identical cultural dimensions (e.g., UA), they acknowledge substantial differences between identical cultural dimensions (Hofstede, 2006; Javidan et al., 2006). For example, Hofstede’s UA index captures stress orientation, while GLOBE’s UA index captures rule orientation (Venaik and Brewer, 2010). A unique feature of the GLOBE project is that, for each cultural dimension, it provides a *practices* index and a *values* index. I use GLOBE’s UA *practices* index rather than its UA values index because the practices index captures how cultural rule orientation *is* practiced in a country (labeled ’as is’ culture), while the values index captures how cultural rule orientation *should be* practiced in a country (labeled as ’should be’ culture).

The variable *UA Rule Orientation* is measured at the country level. Although using country-level cultural variables is the standard approach in the literature,<sup>23</sup> a potential concern of this approach is that country-level cultural variables do not reflect the culture of firms' managers. For example, if the manager of a firm located in the US was born in another country, it is very likely that the culture of her country of birth traveled with her. In such a case, using the *UA Rule Orientation* score for the US would be inappropriate. Evidence suggests that such migration patterns are unlikely to bias my findings because 98% of chief executive officers (CEOs) in the US are US citizens, 90% of CEOs in Germany are German citizens, and 91% of CEOs in Italy are Italian citizens (Ahern et al., 2015).

## 5. Measuring legal institutions

Guided by Simnett et al. (2009) and Dhaliwal et al. (2012), I use four legal institutional variables that are of particular relevance to CSR reporting: *Civil Law*, *Labor Laws*, *Environmental Laws*, and *CSR Reporting Regulations*. The first variable, *Civil Law*, is a dummy variable equal to one if a firm is located in a country with a civil law origin, and zero otherwise. Countries with a civil law origin are characterized by stakeholder orientation and state intervention through rules and regulations (e.g., an ex ante delineation of appropriate behavior) (La Porta et al., 1997; Ball and Robin, 2000; Liang and Renneboog, 2017). In contrast, countries with a common law origin are characterized by shareholder orientation and a more discretion-oriented system that emphasizes following legal procedures over rules (La Porta et al., 1997; Ball and Robin, 2000; Liang and Renneboog, 2017). Since a civil law origin supports stakeholder orientation and rules that specify appropriate (stakeholder oriented) behavior, prior studies predict and provide evidence that firms located in countries with a civil law origin issue more credible CSR reports (Simnett et al., 2009; Kolk and Perego, 2010; Fernandez-Feijoo et al., 2015).

The second variable, *Labor Laws*, is the mean rank score of the following four indexes: (1) human rights laws, an index for human rights protection; (2) social security laws, an index of social security benefits based on (a) sickness and health benefits, (b) disability, old age, and death benefits,

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<sup>23</sup> Studies using country-level cultural variables include Hope et al. (2008), Chui et al. (2010), Han et al. (2010), Orij (2010), Ioannou and Serafeim (2012), Garcia-Sanchez et al. (2013), Shao et al. (2013), Kanagaretnam et al. (2014), Ahern et al. (2015), Cahan et al. (2016), Chen et al. (2015), Pevzner et al. (2015), Dou et al. (2016), Luo and Tang (2016), and Gorodnichenko and Roland (2017).

and (c) unemployment benefits; (3) collective relations laws, an index of the protection of collective relations based on (a) collective disputes and (b) labor union power; and (4) employment laws, an index of the protection of employment and labor based on (a) dismissal procedures, (b) the cost of firing workers, (c) the cost of increasing working hours, and (d) alternative employment contracts (Dhaliwal et al., 2012; Dhaliwal et al., 2014). Countries with a higher *Labor Laws* variable score are characterized by stronger laws that target stakeholder orientation in terms of employees. Prior studies show that firms located in countries with stronger labor laws are more likely to issue CSR reports (Dhaliwal et al., 2012; Dhaliwal et al., 2014).

The third variable, *Environmental Laws*, is the number of environmental laws in a country. Examples of environmental laws are the Australian Water Act 2007, the Indian Air (Prevention and Control of Pollution) Act 1981, and the Finland Forest Act 1996. Countries with a higher *Environmental Laws* variable score are characterized by stronger laws that target stakeholder orientation in terms of environment. Cahan et al. (2016) provide evidence that firms located in (European) countries with stronger environmental laws are more likely to issue CSR reports.<sup>24</sup>

The fourth variable, *CSR Reporting Regulations*, is the number of voluntary (soft) and mandatory (hard) CSR reporting regulations (for non-financial firms) in a country. Mandatory regulations are given a weight of two. Examples of mandatory CSR reporting regulations are the Revision of the Danish Financial Statements Act (The Social Responsibility for Large Businesses Law) 2008, the Brazilian Law Project n°3613/2008, and the South African King III Report 2009.<sup>25</sup> Examples of voluntary CSR reporting regulations are the Chilean Guide for Preparing Sustainability Reports 2003, the Austrian Reporting about Sustainability Guidelines 2003, and the Malaysian CSR Framework for Voluntary Reporting. Countries with a higher *CSR Reporting Regulations* variable score are characterized by stronger regulations that target CSR reporting. Prior research shows that firms located in countries with stronger CSR reporting regulations are more likely to issue CSR reports (Dhaliwal et al., 2012; Dhaliwal et al., 2014).

To obtain my main legal institutional variable—that is, *CSR Law*—I perform a factor analysis based on the four variables described above. Table A2 reports the results from the factor analysis. As expected, all four variables load positively on the principal factor (i.e., *CSR Law*). In addition,

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<sup>24</sup> Specifically, Cahan et al. (2016) use an environmental performance index that includes a legal component (i.e., environmental performance index from the view of law).

<sup>25</sup> No country mandates the issuance of a GRI report and/or mandates assurance.

all loadings are well above the critical value of 0.5, with *Labor Laws* having the strongest loading (0.924) and *CSR Reporting Regulations* having the weakest loading (0.630). The Kaiser criterion suggests retaining those factors with eigenvalues higher than one. Only the principal factor has an eigenvalue greater than one (2.360).<sup>26</sup> Hence, retaining only the principal factor is appropriate. The principal factor explains 59% of the total variance. As reported in Table 1, the principal factor (*CSR Law*) ranges from  $-1.82$  (Malaysia) to  $1.45$  (Sweden).

## 6. Sample and data

The sample in this study comprises all GRI reports covered by the GRI Sustainability Disclosure Database (GRI SDD) between 2012 and 2016.<sup>27</sup> The sample period starts in 2012 because information on the assurance process (i.e., assurance standard, assurance score, and assurance level) is not available for earlier years.<sup>28</sup> I exclude GRI reports from firms operating in the financial services industry,<sup>29</sup> GRI reports from firms with missing location codes, GRI reports from firms with missing Standard Industrial Classifications (SIC) codes, and GRI reports from firms with missing data on firm-level variables. Finally, I exclude countries with less than 10 GRI reports and missing data on country-level variables. This yields a final sample (i.e., reporting sample) of 6,589 GRI reports issued by 1,620 firms located in 31 countries. Of the 6,589 GRI reports, 2,389 (36%) are assured (i.e., assurance sample). Table A3 breaks these numbers down by country.

The decisions analyzed in this study are sequential in nature. Only if a firm decides to receive assurance on its GRI report (i.e., *Assurance* = 1), it moves on to the decisions related to the assurance process (i.e., *Assurance Provider*, *Assurance Standard*, *Assurance Scope*, and *Assurance Level*). Hence, all empirical tests with *Assurance* as the outcome variable are based on the reporting sample (6,589 firm-year observations). All further decisions are based on the confined assurance sample (2,497 firm-year observations).<sup>30</sup> Since the reporting sample and the assurance sample are

<sup>26</sup> The eigenvalues of the other three factors (un-tabulated) range from 0.225 to 0.792.

<sup>27</sup> The GRI SDD is the most comprehensive database in terms of GRI reports coverage.

<sup>28</sup> The GRI SDD is the only database that covers information on assurance standards, assurance scope, and assurance level.

<sup>29</sup> I exclude firms operating in the financial services industry because of their exposure to regulatory oversight. In un-tabulated tests, I also exclude firms operating in the utilities industry because they too are exposed to some regulatory oversight. The un-tabulated estimates are qualitatively similar to the ones reported in Table 2, Table 3, and Table 4, indicating that our inferences are not driven by the inclusion of utility firms.

<sup>30</sup> Prior studies use the same sequential samples for their empirical analyses (Simnett et al., 2009; Casey and Grenier, 2015; Peters and Romi, 2015; Zhou et al., 2016).

clearly not random, one might worry that my main findings are affected by a self-selection bias. I address this issue in Section 8.4.

I obtain data for all five outcome variables—*Assurance*, *Assurance Provider*, *Assurance Standard*, *Assurance Scope*, and *Assurance Level*—from GRI SDD. I obtain data for my main cultural variable, *UA Rule Orientation*, from House et al. (2004). To construct my main legal institutional variable, *CSR Law*, I obtain data from Humana (1992), La Porta et al. (1997), Botero et al. (2004), Dhaliwal et al. (2014), Maniora and Ernstberger (2016), and several online sources. For my country-level control variables, I obtain data from Bushman et al. (2004), Boolaky and Soobaroyen (2017), the World Bank DataBank, Compustat, and Thomson Reuters. For my firm-level control variables, I obtain data from Compustat and RobecoSAM.

I use the location code from Compustat to identify where firms are located. An issue with this approach is that Compustat only reports firms’ *current* country of location and not firms’ *historic* country of location. Because international relocations are not very frequent,<sup>31</sup> I believe that using Compustat’s current location code is appropriate.

Some analyses (e.g., robustness analyses) are based on alternative samples and require data from further sources (e.g., Google Trends, Gelfand et al. (2011), and Acemoglu et al. (2001)). I describe these samples and data as they arise.

## 7. Main empirical analyses

### 7.1 *Path analysis*

I start by performing a path analysis to analyze *how* cultural rule orientation shapes firms’ tendencies toward credible CSR reporting.

#### 7.1.1 *Model specification*

A path analysis decomposes the *total effect* of a source variable on an outcome variable into an *indirect effect* (via a mediating variable) and a *direct effect* (Wright, 1934). This decomposition allows

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<sup>31</sup> I randomly select 100 firms (10 German, 30 US, 10 French, 10 United Kingdom (UK), 10 Austrian, 10 Australian, 10 New Zealand, 10 Canadian, and 10 Swiss) and check whether these firms relocated internationally between 2012 and 2016. None of the 100 firms relocated internationally and only one relocated nationally. In addition, Pirinsky and Wang (2006) find that only 2% of US firms relocated *within the US* over 15 years.



me to distinguish between the two possible channels through which cultural rule orientation affects credible CSR reporting. On the one hand, I can test whether the effect of cultural rule orientation on credible CSR reporting is due to cultural rule orientation improving legal institutions—and improved legal institutions *subsequently* improve the credibility of CSR reports. On the other hand, I can test whether the effect of cultural rule orientation on credible CSR reporting is due to cultural rule orientation improving the credibility of CSR reports *on its own*. In the first channel, cultural rule orientation is hypothesized to have an indirect (mediated) effect on the credibility of CSR reports via legal institutions (H1). In the second channel, cultural rule orientation is hypothesized to have a direct effect on the credibility of CSR reports (H2).

I closely follow Pevzner et al. (2015) and De Fond et al. (2016) by specifying the following linear path model:<sup>32, 33</sup>

$$CSR\ Laws_c = \alpha_1(UA\ Rule\ Orientation_c) + \varepsilon_{ijct} \quad (1)$$

$$d_{ijct} = \beta_1(UA\ Rule\ Orientation_c) + \beta_2(CSR\ Laws_c) + \beta_3'\mathbf{X}_{ijct} + \beta_4'\mathbf{Z}_{c(t)} + \psi_j + \omega_t + \varepsilon_{ijct} \quad (2)$$

where  $i$  indexes firms,  $j$  indexes industries,  $c$  indexes countries, and  $t$  indexes years.  $d$  denotes the outcome variables (i.e., *Assurance*, *Assurance Provider*, *Assurance Standard*, *Assurance Scope*, *Assurance Level*), *UA Rule Orientation* denotes the source variable, and *CSR Laws* denotes the mediating variable.  $\mathbf{X}$  is a vector of firm-level control variables,  $\mathbf{Z}$  is a vector of country-level control variables,  $\psi$  are industry fixed effects based on one-digit SIC codes,<sup>34</sup> and  $\omega$  are year fixed effects.<sup>35</sup>  $\varepsilon$  is the error term. I account for serial correlation of the error term by clustering the standard errors at the country level. I account for the uneven country representation by estimating a weighted linear path model, with the weights equal to the inverse of number of firm-year observations in each country.

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<sup>32</sup> In performing the linear path model, I estimate a maximum likelihood (ML) structural equation model (SEM).

<sup>33</sup> Although all five outcome variables are dummy variables, I specify a linear path model for computational reasons (following De Fond et al., 2016). In Section 7.3, I specifically account for the fact that the outcome variables are dummy variables by specifying a probit model.

<sup>34</sup> For the sake of consistency, I do not include higher dimensional industry fixed effects (i.e., two, three, or four-digit SIC codes) because this would lead to an incidental parameters problem in a probit regression (see Section 7.3). However, I re-estimate the linear path model with fixed effects based on four-digit SIC codes. The un-tabulated estimates are similar to the ones reported in Table 2.

<sup>35</sup> Because *UA Rule Orientation* and *CSR Laws* are time-invariant variables, it is not feasible to control for country fixed effects or firm fixed effects.

The path coefficient  $\alpha_1 \times \beta_2$  is the magnitude of the indirect effect (total mediated path) from *UA Rule Orientation* to *d* mediated through *CSR Laws*. The statistical significance of the indirect effect is estimated using the Sobel (1982) test statistic. The path coefficient  $\beta_1$  is the magnitude of the direct effect (direct path) from *UA Rule Orientation* to *d*. Figure 1 depicts the posited direct and indirect effects (paths) for the linear path model specified above, along with the respective coefficients.

### 7.1.2 Findings

Table 2 reports the findings from the path analysis. In Column (1), the path coefficient estimate between *UA Rule Orientation* and *CSR Laws* is positive ( $\hat{\alpha}_1 = 0.600$ ) and statistically significant at the 1% level, indicating that cultural rule orientation improves CSR-related legal institutions. The path coefficient estimate between *CSR Laws* and *Assurance* is also positive ( $\hat{\beta}_2 = 0.053$ ) and statistically significant at the 1% level, indicating that improved CSR-related legal institutions increase the likelihood of firms receiving assurance on their CSR reports. Consequently, the total mediated path coefficient estimate, which is the product of these two path coefficient estimates, is positive ( $\hat{\alpha}_1 \times \hat{\beta}_2 = 0.032$ ) and statistically significant at the 5% level. This means that *UA Rule Orientation* has an indirect effect on *Assurance* via *CSR Law*, supporting H1: culture indirectly shapes firms' tendencies toward credible CSR reporting via legal institutions. The direct path coefficient estimate between *UA Rule Orientation* and *Assurance* is positive ( $\hat{\beta}_1 = 0.297$ ) and statistically significant at the 1% level. This means that *UA Rule Orientation* has a direct effect on *Assurance*, supporting H2: culture directly shapes firms' tendencies toward credible CSR reporting.

To examine whether the indirect effect or the direct effect is stronger, I compute the proportion of the total effect attributed to the indirect effect and the direct effect, respectively. The proportion attributed to the indirect effect is about 10% ( $= 0.032/(0.032+0.297) \times 100$ ), while the proportion attributed to the direct effect is about 90% ( $= 0.297/(0.032+0.297) \times 100$ ). This finding clearly suggests that the direct effect dominates in shaping firms' tendencies toward receiving assurance on their CSR reports, which means that H1 is weakly supported while H2 is strongly supported.

Columns (2) to (4) of Table 2 provide similar findings. *UA Rule Orientation* has an indirect effect via *CSR Law* as well as a direct effect on (2) *Assurance Provider*, (3) *Assurance Standard*, and (4) *Assurance Scope*. The proportions of the total effects attributed to the indirect effects

are about 23% for *Assurance Provider*, 7% for *Assurance Standard*, and 10% for *Assurance Scope*. Correspondingly, the proportions of the total effects attributed to the direct effects are about 77% for *Assurance Provider*, 93% for *Assurance Standard*, and 90% for *Assurance Scope*. These findings clearly suggest that the direct effect dominates in shaping firms' tendencies toward credible CSR reporting, which means that H1 is weakly supported while H2 is strongly supported.

Column (5) of Table 2 shows that *UA Rule Orientation* neither indirectly nor directly affects *Assurance Level* because both coefficient estimates (i.e.,  $\hat{\alpha}_1 \times \hat{\beta}_2$ ,  $\hat{\beta}_1$ ) are not statistically significant at conventional levels.

All models, Columns (1) to (5), are well-fitted with standardized root mean square residual (SRMR) statistics being below the critical value of 0.05 (Hu and Bentler, 1999).<sup>36</sup>

## 7.2 Qualitative comparative analysis

To better understand the mechanisms behind the indirect effect, I employ a qualitative comparative analysis (QCA). A QCA "allows the identification of multiple pathways which are not detectable via standard [path] analysis" (Leuz and Wysocki, 2016, p. 597). Specifically, a QCA enables me to use all four legal institutional variables (i.e., *Labor Laws*, *Civil Law*, *Environmental Law*, and *CSR Reporting Regulations*) individually. Thereby, I can examine how their interdependence with culture as well as with each other contributes to a certain outcome (i.e., *Assurance*, *Assurance Provider*, *Assurance Standard*, *Assurance Scope*, and *Assurance Level*).

### 7.2.1 Methodology

A QCA proceeds in four steps (Ragin, 1987; Fiss, 2011). In the first step, I have to decide whether to perform a crisp or fuzzy QCA. In a crisp QCA, all variables have to be transformed into dummy variables. In a fuzzy QCA, all (non-dummy) variables have to be transformed into fuzzy scores that range from zero to one. I use a crisp QCA because all my outcome variables as well as *Civil Law* are already dummy variables.<sup>37</sup> I transform the remaining variables (i.e., *Labor Laws*, *Environmental Laws*, *CSR Reporting Regulations*, and *UA Rule Orientation*) into dummy variables by using the

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<sup>36</sup> Other goodness-of-fit statistics (i.e., root mean square error of approximation (RMSEA), adjusted goodness-of-fit (AGFI), and comparative fit index (CFI)) cannot be computed because of weighting and clustered standard errors. However, when I re-estimate the linear path models without weighting and clustered standard errors, the RMSEA, AGFI, and CFI also indicate good model-fit.

<sup>37</sup> The findings are similar when I perform a fuzzy QCA.

sample median as splitting criterion. In the second step, I have to construct a data matrix, also referred to as a truth table, with  $2^k$  rows, where  $k$  is the number of possible combinations of attributes. Each variable has two attributes (i.e., 0 and 1). In my analysis, I have 32 rows ( $= 2^5$ ), which means I have 32 possible combinations of attributes. In the third step, I reduce the number of rows based on two conditions: (1) the minimum number of observations required for each combination of attributes, and (2) the minimum consistency level required for each combination of attributes. The minimum consistency level should be set between 0.85 and 0.75 (Ragin, 2006).<sup>38</sup> In the third step, I use Boolean algebra to simplify the combinations of attributes to paths. For each path and for the entire solution (i.e., all paths together), a consistency level and a coverage rate are computed. The coverage rate simply shows (in %) how much of the outcome is covered by each path as well as the entire solution.<sup>39</sup>

### 7.2.2 Findings

Table 3 reports the findings from the QCA for the outcome variable *Assurance Provider*.<sup>40</sup> Following the notation of Fiss (2011), black circles indicate the presence of an attribute (i.e., variable = 1), white circles indicate the negation of an attribute (i.e., variable = 0), and blank spaces indicate the absence of an attribute (i.e., the variable does not contribute to the outcome). In Panel A, I set the minimum consistency level at 0.84. I identify only one path, which includes all legal institutional variables as well as culture. Specifically, this path indicates that a strong rule-oriented culture, strong labor laws, strong environmental laws, strong CSR reporting regulations, and a civil law origin are necessary for selecting an accounting firm as assurance provider. The solution consistency at 0.848 is very high, but the coverage at 0.222 is rather low. The solution consistency indicates that the presence of attributes of the variables *UA Rule Orientation*, *Labor Laws*, *Environmental Laws*, *CSR Reporting Regulations*, and *Civil Law* together explain only 22.2% of the outcome variable *Assurance Provider*.

In Panel B, I set the minimum consistency level at 0.82. I identify three paths. *UA Rule*

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<sup>38</sup> The minimum consistency level can be compared to the minimum statistical significance level in regression analyses, which conventionally range from 0.01 (1%) to 0.05 (5%) (Delmas and Pekovic, 2017).

<sup>39</sup> The coverage rate can be compared to the magnitude of a coefficient (effect size) in regression analyses (Delmas and Pekovic, 2017).

<sup>40</sup> I focus on the findings for *Assurance Provider* because the path analysis reveals that the indirect effect is strongest for this variable.

*Orientation* is part of all three paths, *CSR Reporting Regulations* does not contribute to the first path, *Civil Law* does not contribute to the second path, and *Labor Laws* and *Environmental Laws* do not contribute to the third path. This means that culture is the underlying variable of all paths, while the institutional variables can substitute each other. The solution consistency is 0.833. The solution coverage increases to 0.372, indicating that the three paths together explain 37.2% of the outcome variable *Assurance Provider*.

In Panel C, I set the minimum consistency level at 0.80. I identify one path, which only comprises *UA Rule Orientation*. This means that *UA Rule Orientation* alone is necessary for selecting an accounting firm as assurance provider. The solution consistency is 0.812. The solution coverage amounts to 0.630, indicating that *UA Rule Orientation* alone explains 63% of *Assurance Provider*—almost three times as much as the solution of Panel A.

Taken together, these findings are in line with the findings from the path analysis. Since the solution coverage is akin to effect size (magnitude) in regression analysis (Delmas and Pekovic, 2017), I can conclude that, as in the path analysis, the effect of cultural rule orientation *on its own* is much stronger than its effect complemented by legal institutions (0.222 (Panel A) versus 0.630 (Panel C)).

For the sake of brevity, I only briefly discuss the un-tabulated findings for the other three outcome variables (*Assurance*, *Assurance Standard*, *Assurance Score*, and *Assurance Level*). Consistent with Panel C of Table 3, I find that *UA Rule Orientation* on its own well explains all three outcome variables, respectively. The coverage ranges from 0.52 to 0.65. However, solutions including legal institutional variables are much weaker. In fact, I find no solution that covers culture as well as all other legal institutional variables (as in Panel A of Table 3), and the coverage of solutions including (at most two) legal institutional variables is much lower (0.174 at most).

### 7.3 *Probit regression analysis*

Path analysis and QCA have established that the direct effect of culture dominates. To better understand the economic significance of this direct effect, I perform a probit regression analysis.

### 7.3.1 Model specification

In line with Equation (2), I specify the following probit model:<sup>41</sup>

$$\begin{aligned} \Pr\{d_{ijct}\} = & \gamma_1(UA \text{ Rule Orientation}_c) + \gamma_2(CSR \text{ Laws}_c) + \gamma'_3\mathbf{X}_{ijct} + \gamma'_4\mathbf{Z}_{c(t)} \\ & + \psi_j + \omega_t + \varepsilon_{ijct} \end{aligned} \quad (3)$$

where  $i$  indexes firms,  $j$  indexes industries,  $c$  indexes countries, and  $t$  indexes years.  $d$  denotes the outcome variables (i.e., *Assurance*, *Assurance Provider*, *Assurance Standard*, *Assurance Scope*, and *Assurance Level*). *UA Rule Orientation* is the cultural variable of interest. *CSR Laws* controls for legal institutions related to CSR.<sup>42</sup>  $\mathbf{X}$  is a vector of firm-level control variables.  $\mathbf{Z}$  is a vector of country-level control variables.  $\psi$  are industry fixed effects based on one-digit SIC codes.<sup>43</sup>  $\omega$  are year fixed effects.  $\varepsilon$  is the error term. I account for serial correlation of the error term by clustering the standard errors at the country level. I account for the uneven country representation by estimating a weighted probit model, with the weights equal to the inverse of number of firm-year observations in each country.

### 7.3.2 Findings

Table 4 reports the findings from the probit regressions. In Columns (1) to (4), the coefficient estimates for the variable *UA Rule Orientation* are positive and statistically significant at the 1% level. They are also economically significant. In Column (1), the marginal effect (at means) indicates that a one unit increase in *UA Rule Orientation* increases the probability of receiving assurance by about 37 percentage points. In Column (2), the marginal effect indicates that a one unit increase in *UA Rule Orientation* increases the probability of receiving assurance from an accounting firm by about 33 percentage points. In Column (3), the marginal effect indicates that a one unit increase

<sup>41</sup> Prior studies with a very similar research setting use logit models (Simnett et al., 2009; Casey and Grenier, 2015; Peters and Romi, 2015; Zhou et al., 2016). I use a probit model instead of a logit model because some of my robustness tests require a probit specification (e.g., Heckman selection model).

<sup>42</sup> Gow et al. (2016, p. 485) stress that including a mediating variable as a control variable yields more conservative estimates "if [and only if] the indirect effect via mediators is of the same sign as the direct (i.e., unmediated) effect." My path analysis reveals that this is the case. Hence, including *CSR Laws* in Equation (3) ensures that I by no means overestimate the economic significance of the coefficient  $\gamma_1$ .

<sup>43</sup> Using higher dimensional industry fixed effects (i.e., based on one, two, or three-digit SIC codes) would lead to an incidental parameters problem. To alleviate concerns about omitted industry effects, I re-estimate an ordinary least squares (OLS) model with industry fixed effects based on four-digit SIC codes. The un-tabulated signs and statistical significance levels for *UA Rule Orientation* and *CSR Laws* are the same as in Table 4.

in *UA Rule Orientation* increases the probability of receiving assurance in accordance with an assurance standard by about 22 percentage points. In Column (4), the marginal effect indicates that a one unit increase in *UA Rule Orientation* increases the probability of having the *entire* GRI report assured by about 22 percentage points. In Column (5), the coefficient estimate for the variable *UA Rule Orientation* is not statistically significant, indicating that cultural rule orientation does not explain the decision whether to receive assurance on a high level.

Figure A2 plots the predicted probabilities for *UA Rule Orientation*. In Figure (a), the predicted probability (at means) of *Assurance* = 1, *UA Rule Orientation* = 3.39 (its minimum) is about 9%. The predicted probability of *Assurance* = 1, *UA Rule Orientation* = 5.37 (its maximum) is about 74%. Correspondingly, the change in predicted probability as *UA Rule Orientation* moves from its minimum to its maximum is about 64%. For *Assurance Provider* (Figure (b)), the change in predicted probability is about 59%. For *Assurance Standard* (Figure (c)), the change in predicted probability is about 36%. For *Assurance Scope* (Figure (d)), the change in predicted probability is about 42%. These interpretations further illustrate that the economic significance of cultural rule orientation is substantial. Beyond this, Figure A2 shows that the relationship between *UA Rule Orientation* and all outcome variables is linear.

In terms of the legal institutional variable *CSR Laws*, Table 4 shows that the marginal effects (at means) are also economically meaningful but much weaker, ranging from 6 percentage points to 9 percentage points for the statistically significant coefficient estimates.

Turning to the control variables, Table 4 shows that the signs are mostly consistent with prior research.<sup>44, 45</sup> Two of the control variables are not included in all columns. First, the variable *Assurance Market* is only included in Column (2). I find that firms located in countries with a high assurance provider concentration (for assurance on financial reports) are more likely to select an accounting firm as assurance provider for assuring their GRI report. Second, the variable *ISA* is

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<sup>44</sup> The cross-country studies (examining the assurance and assurance provider decisions) with similar control variables are Simnett et al. (2009), Kolk and Perego (2010), Herda et al. (2014), De Beelde and Tuybens (2015), Fernandez-Feijoo et al. (2015), and Zhou et al. (2016).

<sup>45</sup> The only variable for which the sign of a statistically significant coefficient estimate changes is *Size*. I find that larger firms are more likely to receive assurance on their GRI report (Column (1)), are more likely to select an accounting firm as assurance provider (Column (2)), but are less likely to receive assurance on their *entire* CSR report (Column (3)). While the positive coefficient estimates in Columns (1) and (2) are in line with prior studies, the negative coefficient estimate in Column (3) is also plausible. For larger firms, having *all* CSR processes assured is more difficult, takes more time, and is costlier. Hence, firms may choose to have only parts (i.e., specific processes) assured.

only included in Column (3). I find that firms located in countries that have adopted the ISAs (i.e., standards guiding the assurance process of financial reports) are more likely to receive assurance on their GRI report in accordance with an international CSR assurance standard.

All models are well-fitted with coefficients of determination (Pseudo  $R^2$ s) ranging from 0.180 to 0.358.<sup>46</sup> Further, multicollinearity is not an issue, indicated by the low mean Variance Inflation Factors (VIFs).<sup>47</sup>

## 8. Endogeneity

In this section, I conduct several tests that address four sources of endogeneity that may bias my findings: omitted variables, reverse causality, measurement error, and self-selection (Larcker and Rusticus, 2007; Lennox et al., 2012; Roberts and Whited, 2013).

### 8.1 Omitted variables

Omitted (unobservable) variables are a major concern of this study, as of any cross-country study. It is possible that countries differing in terms of cultural rule orientation also differ on other unobservable factors. Hence, my estimates might simply capture the effect of those other unobservable factors rather than the direct effect of cultural rule orientation itself. I employ three approaches to mitigate this endogeneity concern.

First, I test whether the direct effect of cultural rule orientation on credible CSR reporting is robust to alternative sets of country-level control variables.<sup>48</sup> Table 5 reports the estimates from probit regressions with three alternative sets of control variables. In Panel A, I include the following five additional country-level control variables: *GDP*,<sup>49</sup> *Political Orientation*, *Trade Openness*, *Assurance Fee*, and *Assurance Risk*. The inclusion of these additional control variables does not alter the coefficient estimates for *UA Rule Orientation*. In Panel B, I control for four additional cultural dimensions: *Power Distance*, *Collectivism*, *Gender Egalitarianism*, and *Future Orientation*.

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<sup>46</sup> Prior studies have similar Pseudo  $R^2$ s (Kolk and Perego, 2010; Herda et al., 2014; Zhou et al., 2016).

<sup>47</sup> VIFs are based on linear regressions. All un-tabulated individual VIFs are well below the critical value of 10.

<sup>48</sup> Larcker and Rusticus (2010, p. 196) stress that an important first step is to "incorporate additional control variables [...] that mitigate the endogeneity problem."

<sup>49</sup> In addition to controlling for a country's economic development by including the variable *GDP*, I re-estimate the probit regressions based on a confined sample of the 22 Organization for Economic Co-operation and Development (OECD) countries. The un-tabulated coefficient estimates for *UA Rule Orientation* remain unaltered.



Controlling for these additional cultural dimensions does not alter the coefficient estimates for *UA Rule Orientation*. In Panel C, I use three country-level factor variables (constructed from 68 individual country-level variables) instead of the individual country-level variables included in Table 4. According to Isidro et al. (2016), these factors capture all important economic, geopolitical, and regulatory country-level factors that shape financial reporting decisions. Using these factors does not alter the coefficient estimates for *UA Rule Orientation*. Taken together, the results of Table 5 suggest that the direct effect of cultural rule orientation on credible CSR reporting is robust to several alternative sets of country-level control variables.

Second, I test whether the direct effect of cultural rule orientation on credible CSR reporting is robust to a hierarchical generalized linear model (HGLM) approach.<sup>50</sup> This approach can *partly* control for unobservable country factors because it includes country random effects and cleanly separates the effects taking place at the country level from those taking place at the firm level (Lee and Nelder, 1996; Eun et al., 2015). Table 6 reports the results from the HGLM. In all columns, the coefficient estimates for the variable *UA Rule Orientation* remain unaltered. The variances of the random effects are statistically significant at the 1% level, indicating that the country random effects contribute to the explanation of the outcome variables.

Third, I test whether the direct effect of cultural rule orientation on credible CSR reporting is robust to an instrumental variable (IV) approach.<sup>51</sup> The success of an IV approach critically hinges on the validity of the chosen IV. A valid IV must fulfill two conditions: the *relevance condition* and the *exclusion condition* (Roberts and Whited, 2013). The relevance condition requires that the IV must have a strong effect on the endogenous explanatory variable (Larcker and Rusticus, 2007; Larcker and Rusticus, 2010). Ideally, the predicted effect of the IV on the endogenous explanatory variable is deeply rooted in theory (Larcker and Rusticus, 2010; Gow et al., 2016). Fortunately, the relevance condition is empirically testable by regressing the endogenous explanatory variable on the IV, along with all other explanatory variables from the outcome equation (Roberts and Whited, 2013).<sup>52</sup> The exclusion condition requires that the IV must be truly exogenous and *only* affect the outcome variable via the endogenous explanatory variable, meaning that the IV must affect

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<sup>50</sup> In particular, I specify an intercept-as-outcomes random effects HGLM with a probit link function.

<sup>51</sup> In particular, I specify a weighted ML IV probit model for continuous endogenous explanatory variables and dummy variable outcomes.

<sup>52</sup> The outcome equation is often referred to as the structural equation.

the endogenous explanatory variable in the reduced-form equation but must *not* affect the error term in the outcome equation. Unfortunately, this condition is not empirically testable because the error term is unobservable (Roberts and Whited, 2013). Hence, it is crucial to provide compelling arguments why the IV does *not* affect the outcome variable in any way other than via the endogenous explanatory variable (Larcker and Rusticus, 2010; Gow et al., 2016).

Roberts and Whited (2013, p. 514) state that "[g]ood IVs can come from biological or physical events or features". In this study, I use a biology-based instrument, *Genetic Distance*, which is the natural logarithm of one plus the Mahalanobis distance ( $M_{ST}$ ) of the frequency of blood types A and B between the dominant populations of two countries. A higher  $M_{ST}$  score indicates greater distance from the US. The choice of this variable is deeply rooted in co-evolutionary theory, which treats genetics and culture as intertwined evolutionary forces (Durham, 1991; Henreich et al., 2005; Feldman and Laland, 1996). More precisely, scholars predict, and provide evidence indicating, that genetic changes precede cultural changes (Vallender et al., 2008; Chiao and Blizinsky, 2010; Fisher and Ridley, 2013). Table 7, Panel A, reports the coefficient estimates of the reduced-form equation from the IV probit model. Consistent with co-evolutionary theory, I find that *Genetic Distance* has a positive effect on *UA Rule Orientation*. All coefficient estimates are positive and statistically significant at the 1% level. The coefficient estimates are also economically meaningful.<sup>53</sup> These theoretically rooted empirical findings indicate that my chosen IV variable, *Genetic Distance*, satisfies the relevance condition.

I also argue that *Genetic Distance* satisfies the exclusion condition. Variables based on genetics are truly exogenous because it is impossible to 'choose' genetics, such as blood type. In addition, blood types are 'neutral' genetic markers that do not determine evolutionary fitness (i.e., the ability to run, work, think, etc.) (Cavalli-Sforza et al., 1994; Gorodnichenko and Roland, 2017). Therefore, it is reasonable to argue that *Genetic Distance* does not *directly* affect corporate decision-making. However, *Genetic Distance* might affect corporate decision-making *indirectly* through cultural dimensions other than *UA Rule Orientation*. Gorodnichenko and Roland (2017) provide evidence suggesting that *Genetic Distance* affects the cultural dimension of collectivism. But since *Collectivism* does not affect credible CSR reporting (see coefficient estimates for *Collectivism* in Panel B

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<sup>53</sup> For example, in Column (1) of Panel A, a 1% change in *Genetic Distance* increases *UA Rule Orientation* by about 8% ( $= 0.485 \times \ln(1.01)/0.577 \times 100$ ) of its standard deviation.

of Table 5), it is reasonable to argue that *Genetic Distance* does not affect credible CSR reporting indirectly through *Collectivism*. The only cultural dimension, apart from *UA Rule Orientation*, that systematically affects credible CSR reporting is *Gender Egalitarianism* (see coefficient estimates for *Gender Egalitarianism* in Panel B of Table 5). To mitigate the concern that *Genetic Distance* affects credible CSR reporting indirectly through *Gender Egalitarianism*, I test whether *Genetic Distance* affects *Gender Egalitarianism*. I find no empirical support for such an effect.<sup>54</sup> Taken together, I am confident that *Genetic Distance* is a valid instrument,<sup>55</sup> plausibly satisfying both the relevance condition as well as the exclusion condition.

Table 7, Panel B, reports the coefficient estimates of the outcome equation from the IV probit model. The direct positive effect of *UA Rule Orientation* on credible CSR reporting is highly robust. With reference to the main findings reported in Table 4, all coefficient estimates for *UA Rule Orientation* have the same sign and statistical significance level. The magnitudes of the coefficient estimates are considerably larger, indicating that my main findings are rather conservative. In the last row of Table 7, Panel B, I provide *p*-values for a Wald test of exogeneity. In all columns, the null hypothesis of no endogeneity is rejected.

## 8.2 Reverse causality

Reverse causality is not a major concern when it comes to examining the direct link between cultural rule orientation and credible CSR reporting. It is simply implausible that credible CSR reporting has the power to change the culture of a country. Further, *UA Rule Orientation* is built on survey data that were collected between 1994 and 1997—years in which CSR reporting was not yet an important issue. In fact, only 12% of the world’s largest firms issued a CSR report in 1994 (KPMG, 2017), and the first generation of the GRI standards was not issued before 2000.<sup>56</sup>

<sup>54</sup> Specifically, I estimate the following OLS regression:  $Gender\ Egalitarianism_c = \tau_1(Genetic\ Distance_c) + \varepsilon_c$ . Standard errors are robust standard errors adjusted for clustering at the continent level. The coefficient estimate for *Gender Egalitarianism* is  $-0.134$  with a corresponding *p*-value of  $0.362$ .

<sup>55</sup> Larcker and Rusticus (2010, p. 197) note that “[i]t is also informative to demonstrate that different IVs [...] provide the same substantive results. If this is not the case, the researcher should question the validity of the IVs.” I use two alternative genetic distance IVs. The first is based on DNA variation (data source: online appendix of Spolaore and Wacziarg (2009)). The second is based on somatic variation (data source: online appendix of Guiso et al. (2009)). Both of them are ‘neutral’ genetic markers. The un-tabulated coefficient estimates of the reduced-form equation from the IV probit model show that both alternative IVs have a statistically significant (at least at the 5% level) and economically meaningful effect on *UA Rule Orientation*. The un-tabulated coefficient estimates of the outcome equation from the IV probit model are similar to the ones reported in Table 7.

<sup>56</sup> <https://globalreporting.org/information/about-gri/gri-history>

However, reverse causality is a concern when it comes to examining the link between cultural rule orientation and legal institutions. It is plausible that changes in legal institutions drive changes in culture (and not vice versa),<sup>57</sup> as theorized by Williamson (2000) and predicted in my path model. For example, a civil law origin might be responsible for the development of a strong rule-oriented culture. To identify the direction of causality—from culture to legal institutions or from legal institutions to culture—I specify the following two outcome equation of an IV model:<sup>58</sup>

$$CSR\ Laws_c = \delta_1(UA\ Rule\ Orientation_c) + \delta'_2 \mathbf{Z}_c + \varepsilon_c \quad (4)$$

$$UA\ Rule\ Orientation_c = \eta_1(CSR\ Laws_c) + \eta'_2 \mathbf{Z}_c + \varepsilon_c \quad (5)$$

where  $c$  indexes countries. *CSR Laws* is the legal institutional variable. *UA Rule Orientation* is the cultural variable.  $\mathbf{Z}$  is a vector of country-level control variables.<sup>59</sup>  $\varepsilon$  is the error term. Standard errors are robust standard errors clustered at the continent level. In Equation (4), *UA Rule Orientation* is, as before, instrumented with *Genetic Distance* by specifying the following reduced-form equation:  $UA\ Rule\ Orientation_c = \delta_3(Genetic\ Distance_c) + \delta'_4 \mathbf{Z}_c + \varepsilon_c$ . In Equation (5), *CSR Law* is instrumented with *Settler Mortality* by specifying the following reduced-form equation:  $CSR\ Laws_c = \eta_3(Settler\ Mortality_c) + \eta'_4 \mathbf{Z}_c + \varepsilon_c$ , where *Settler Mortality* is the settler mortality index from Acemoglu et al. (2001).

*Settler Mortality* is also a biology-based IV, defined as the mortality rate of the first settlers in a colony. The rationale behind this IV is that in places (colonies) where the settlers faced high mortality rates (due to an unfavorable disease environment), they could not install and enforce the legal institutions of their country of origin (Acemoglu et al., 2001).<sup>60</sup> Since the (un-tabulated) coefficient estimate for *Settler Mortality* ( $\hat{\eta}_3$ ) is 0.044 with a corresponding  $p$ -value of 0.003, I confidently conclude that *Settler Mortality* satisfies the relevance condition. I am also confident that *Settler Mortality* satisfies the exclusion condition because mortality is truly exogenous. In

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<sup>57</sup> Guiso et al. (2015, p. 337) acknowledge the possibility of a two-way causality, stating that "culture underpins and colors the law, but legal institutions can shape cultural norms."

<sup>58</sup> In particular, I estimate a limited information maximum likelihood (LIML) IV model for continuous endogenous explanatory variables and continuous outcome variables.

<sup>59</sup> The country-level control variables are *Rule of Law*, *Financial Transparency*, *Financial System*, and *GDP*. *Financial System* and *GDP* are the median over the sample period.

<sup>60</sup> The rationale can be schematically summarized as follows: settler mortality  $\rightarrow$  settlements  $\rightarrow$  early legal institutions  $\rightarrow$  current legal institutions  $\rightarrow$  current decision-making (Acemoglu et al., 2001).

addition, all legal institutional variables are grounded in legal origin (Isidro et al., 2016), which makes *Civil Law* the primary channel through which *Settler Mortality* works.<sup>61</sup> In summary, I am confident that *Settler Mortality* is a valid IV.<sup>62</sup>

The (un-tabulated) coefficient estimate for *UA Rule Orientation* is statistically significant and economically meaningful ( $\hat{\delta}_1 = 1.017$ ,  $p\text{-value} = 0.000$ ), indicating that causality runs from cultural rule orientation to legal institutions. By contrast, the (un-tabulated) coefficient estimate for *CSR Laws* is not statistically significant ( $\hat{\eta}_1 = -0.015$ ,  $p\text{-value} = 0.852$ ), indicating that causality does *not* run from legal institutions to cultural rule orientation. These findings are in line with Williamson’s (2000) theory and the specification of my path model.

### 8.3 Measurement error

Measurement error—the discrepancy between a proxy and its unobservable ‘true’ counterpart—is a concern of any cultural study because identifying cultural dimensions and quantifying them is challenging (Caprar et al., 2015). I employ three alternative indexes to mitigate measurement error concerns.

First, I construct a novel cultural rule orientation index based on Google search patterns in a country.<sup>63</sup> Specifically, I use the median of the yearly Google search volumes (between 2005 and 2011) for eight legal terms (looked up in the country’s official language(s)) divided by the number of people regularly using the Internet.<sup>64, 65</sup> The rationale behind this index is that people with a stronger preference for rules, laws, and regulations are more likely to search for legal terms on Google. In other words, because of their (intrinsic) urge to comply with the law, they are more likely to search for laws on Google. Table 8, Panel A, reports the results for this alternative, big-

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<sup>61</sup> The findings do not change if I use *Civil Law* instead of *CSR Laws*, which corroborates the validity of *Settler Mortality*.

<sup>62</sup> As before, I follow the suggestion of Larcker and Rusticus (2010) and use an alternative IV to test the robustness of *Settler Mortality*. I use *British Rule*, which can be regarded as an exogenous shock to a country’s legal institutions. Although this variable assumes that just being a colony of Britain changes the legal institutions and does not capture the implementation of them (as *Settler Mortality* does), it is a variable related to *Settler Mortality* and, hence, is well-suited to test the robustness of *Settler Mortality*. The un-tabulated coefficient estimate for *British Rule* in the reduced-form equation is highly statistically significant (1% level), meaning that it fulfills the relevance condition. The un-tabulated coefficient estimate for *CSR Laws* in the outcome regression is not statistically significant, indicating that the causal effect does not run from *CSR Laws* to *UA Rule Orientation*.

<sup>63</sup> The methodological approach is inspired by Preis et al. (2012), who construct a cultural future orientation index based on Google search patterns.

<sup>64</sup> I use the median of seven years instead of yearly values to mitigate the concern that a major legal change in a country in a specific year biases my cultural variable.

<sup>65</sup> I use the following legal terms: law, regulation, directive, legislation, statute, treaty, constitution, and legal case.

data-based, cultural rule orientation index (*BD Rule Orientation*). The results strongly support a direct effect of cultural rule orientation on credible CSR reporting. All coefficient estimates for *BD Rule Orientation* are positive, economically meaningful, and statistically significant at conventional levels.

Second, I use the cultural tightness index from Gelfand et al. (2011). Countries with a high cultural tightness index "have many strong norms and a low tolerance of deviant behavior" (Gelfand et al., 2011, p. 1100). Although tightness does not *explicitly* capture rule orientation, it is related to it. Many of the survey items used to construct the tightness index capture concepts closely related to rule orientation, such as regulatory strength or need for structure (Gelfand et al., 2011). Therefore, I expect that the effect of tightness points in the same direction as rule orientation. Table 8, Panel B, reports the results for the cultural tightness index (*Tightness*). As expected, they point in the same direction as the ones for rule orientation but are much weaker and much more inconsistent. Specifically, all coefficient estimates for *Tightness* are positive, but only the ones in Columns (1) and (3) are statistically significant. The economic significance of the coefficient estimates is also weaker.

Finally, I use the UA index from Hofstede (1980). This measure captures stress orientation rather than rule orientation (Venaik and Brewer, 2010). Since it is difficult to think of arguments that support a link between stress orientation and credible CSR reporting, I expect to find no statistically significant results. Table 8, Panel C, reports the results for Hofstede's UA index (*UA Stress Orientation*). All coefficient estimates are not statistically significant, indicating that cultural stress orientation does not explain the credibility of CSR reports. This supports the finding of Venaik and Brewer (2010) that the UA indexes from Hofstede (1980) and House et al. (2004) measure different components of UA.

## 8.4 Self-selection

The samples of this study (i.e., the reporting sample and the assurance sample) are not random because firms themselves decide (i.e., self-select) whether to issue a GRI report and, subsequently, whether to receive assurance on their GRI report. This endogenous sampling raises the concern of a self-selection bias. The standard approach to control for a self-selection bias is to perform a Heckman (1979) self-selection model. Because all my outcome variables are dummy variables, I

perform a Heckman probit (Heckprobit) self-selection model (Van de Ven and Van Praag, 1981). A convincing implementation of any Heckman-type model hinges on the validity of the chosen selection instrumental variable (SIV) (Lennox et al., 2012). A valid SIV must fulfill the same two conditions as an IV: the inclusion condition and the exclusion condition (Lennox et al., 2012).

The SIV for the self-selection decision whether to issue a GRI report—that is, determining the reporting sample—is *Peers(GRI Report)*, defined as the country-industry-year percentage of firms (excluding the focal firm) issuing a GRI report. The SIV for the self-selection decision whether to issue an assured GRI report—that is, determining the assurance sample—is *Peers(Assurance)*, defined as the country-industry-year percentage of firms (excluding the focal firm) receiving assurance on their GRI report.

I argue that these variables are relevant because theoretical work predicts and empirical evidence shows that firms are pressured by their peers to make similar decisions in terms of financial reporting and CSR (Dye and Sridhar, 1995; Cao et al., 2017). Table A4 reports the results of the selection equations from the Heckprobit self-selection model. Column (1) shows that the SIV *Peers(GRI Report)* has a positive, economically meaningful, and highly statistically significant effect on the self-selection decision whether to issue a GRI report (*GRI Report*).<sup>66</sup> Similarly, Column (2) shows that the SIV *Peers(Assurance)* has a positive, economically meaningful, and highly statistically significant effect on the self-selection decision whether to issue an assured GRI report (*Assurance*). These findings indicate that my chosen SIVs satisfy the relevance conditions.

I am also confident that my SIVs satisfy the exclusion condition. Following prior studies, I argue that these variables are exogenous to the extent that they capture decisions made by *other* firms and not by the focal firm itself (Cheng et al., 2014; Ferrell et al., 2016; Zhou et al., 2016). In addition, it is unlikely that they affect the outcome variable through any channel other than the sample selection variable itself (Zhou et al., 2016), because both SIVs represent, by construction, an (at least partly) exogenous subset of their respective sample selection variable.<sup>67</sup>

Table 9 reports the results of the outcome equation from the Heckman self-selection model. The *p*-values for a Wald test of random sample selection reject the null hypothesis of no self-selection.

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<sup>66</sup> In Column (1), the sample comprises all firm-year observations included in Compustat between 2012 and 2016 that fulfill the following criteria: (1) a non-missing location code from one of the 31 countries included in this study, (2) a non-missing, non-financial SIC code, and (3) non-missing firm-level control variables.

<sup>67</sup> The sample selection variable refers to the dependent variable of the selection equation (i.e., *GRI Report* and *Assurance*, respectively).



Nonetheless, all coefficient estimates for the variable *UA Rule Orientation* are in line with those reported in Table 9, mitigating the concern that my main findings are driven by a self-selection bias.<sup>68</sup>

## 9. Evidence from the Directive 2014/95/EU

The empirical evidence so far shows that firms located in countries with a stronger rule-oriented culture issue more credible CSR reports. This reflects an *average effect*. To examine the *differential effect* of cultural rule orientation, I exploit a quasi-natural experiment based on the adoption of the Directive 2014/95/EU—the so-called CSR reporting directive. This directive was adopted on 22 October 2014 and entered into force on 1 January 2017, *mandating* large listed firms to disclose CSR information either in their (integrated) financial report or in a stand-alone CSR report.<sup>69</sup> Although the directive does not mandate assurance, it highly *encourages* it.

Because managers of firms located in countries with a strong rule-oriented culture are mentally programmed to rely on rules, laws, and regulations when making decisions (Venaik and Brewer, 2010; Salvato et al., 2014), I conjecture that they *respond stronger* to the directive than managers of firms located in countries with a weak rule-orientated culture. More specifically, I conjecture that they increase the credibility of their GRI reports *more* because the directive highly encourages a thorough assurance process.

To test this conjectured differential effect, I closely follow Liang and Renneboog (2017) by specifying the following linear difference-in-differences model with a single regulatory event and its interaction with a time-invariant country-level dummy variable:<sup>70</sup>

$$d_{ijct} = \lambda_1(\text{High UA Rule Orientation}_c) \times (\text{EU CSR Directive}_{ct}) + \lambda_2(\text{EU CSR Directive}_{ct}) + \lambda'_3\mathbf{X}_{ijct} + \lambda'_4\mathbf{Z}_{ct} + \varphi_c + \psi_j + \omega_t + \varepsilon_{ijct} \quad (6)$$

<sup>68</sup> Lennox et al. (2012) suggest testing whether the inferences are robust to minor changes in the SIVs because Heckman self-selection models are often fragile. I construct three alternative SIVs. Instead of the country-industry-year percentage, I use the country-year percentage, the industry-year percentage, and the cumulative country-industry-year number of firms issuing a GRI report (issuing an assured GRI report). The un-tabulated coefficient estimates are very similar to the ones reported in Table A4 and Table 9.

<sup>69</sup> The adoption of the CSR reporting directive provides a plausible exogenous shock to the (legal) importance of (credible) CSR reporting in the EU. Because the directive did not enter into force during my sample period, the shock is without any regulatory consequences.

<sup>70</sup> I estimate the difference-in-differences model using OLS because interaction terms in non-linear models, such as a probit model, could be biased (Ai and Norton, 2003).



where  $i$  indexes firms,  $j$  indexes industries,  $c$  indexes countries, and  $t$  indexes years.  $d$  denotes the outcome variables (i.e., *Assurance*, *Assurance Provider*, *Assurance Standard*, *Assurance Scope*, *Assurance Level*). *High UA Rule Orientation* denotes the cultural variable—that is, a dummy variable equal to one if *UA Rule Orientation* is above the sample mean, and zero otherwise.<sup>71</sup> *EU CSR Directive* is the ‘treatment’ variable—that is, a dummy variable equal to one if country  $c$  is a member of the EU and year  $t$  is 2015 or 2016, and zero otherwise.<sup>72</sup>  $\mathbf{X}$  is a vector of firm-level control variables.  $\mathbf{Z}$  is a vector of time-variant country-level control variables.  $\varphi$  are country fixed effects.  $\psi$  are industry fixed effects based on one-digit SIC codes.  $\omega$  are year fixed effects.  $\varepsilon$  is the error term. I account for serial correlation of the error term by clustering the standard errors at the country level. I account for the uneven country representation by estimating a weighted model, with the weights equal to the inverse of number of firm-year observations in each country.

An appealing feature of this model is that it allows me to control for all time-invariant unobservable country-level factors (by including country fixed effects) and, at the same time, to examine the differential effect of the time-invariant country-level variable *High UA Rule Orientation* (that is,  $\hat{\lambda}_1$ ).

Table 10 reports the results. All coefficient estimates for the interaction term *High UA Rule Orientation*  $\times$  *EU CSR Directive* are positive, economically nontrivial, and statistically significant at conventional levels. These positive differential effects indicate that, after the adoption of the EU CSR reporting directive, firms located in countries with a stronger rule-oriented culture are more likely (1) to receive assurance on their GRI report, (2) to receive assurance from an accounting firm, (3) to receive assurance in accordance with an assurance standard, (4) to receive assurance on their *entire* GRI report, and (5) to receive assurance on a high level, compared to firms located in countries with a weaker rule-oriented culture. In essence, these findings suggest that firms located in countries with a stronger rule-oriented culture are more responsive to the EU CSR reporting directive, compared to firms located in countries with a weaker rule-oriented culture.<sup>73</sup>

Beyond this, the results of Table 10 further mitigate concerns about omitted variables (because

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<sup>71</sup> I construct a dummy variable for the sake of interpretation.

<sup>72</sup> Many studies use the notation  $Post_t \times Treated_c$ , where  $Post$  is a dummy variable equal to one if year  $t$  is 2015 or 2016, and zero otherwise; and  $Treated$  is a dummy variable equal to one if country  $c$  is a member of the EU, and zero otherwise.

<sup>73</sup> My findings are robust to an alternative control group (determined via propensity score matching). In addition, insignificant coefficient estimates for placebo treatment years indicate that the adoption of the directive is indeed exogenous.

of the inclusion of country fixed effects) and causality (because of the use of an exogenous regulatory event).

## 10. Conclusion

”How does culture affect accounting outcomes in practice?” (Schatt et al., 2016, p. 33). ”[H]ow does [culture] relate to legal institutions? [...] Which type of norms—cultural or legal—are more effective in pursuing a certain policy?” (Guiso et al., 2015, pp. 336, 337). ”[W]hich factors[—cultural or legal—]are more likely to be economic primitives that directly affect outcomes and which factors are associated outcomes or second-order mediating factors” (Leuz and Wysocki, 2016, p. 596).

This study sheds light on these recently raised questions by examining how culture—in particular, cultural rule orientation—shapes firms’ tendencies toward credible CSR reporting. I develop two competing but not mutually exclusive hypotheses. My first (economics-based) hypothesis (H1) predicts that cultural rule orientation shapes firms’ tendencies toward credible CSR reporting *indirectly* via legal institutions. My second (sociology-based) hypothesis (H2) predicts that cultural rule orientation *directly* shapes firms’ tendencies toward credible CSR reporting.

I find strong and consistent evidence that cultural rule orientation affects the credibility of CSR reports both *directly* as well as *indirectly* via legal institutions. Using path analysis, I find that the *direct* effect is much stronger than the *indirect* effect. Specifically, the direct effect accounts for only 23% of the total effect, while the indirect effect, at least, accounts for 77% of the total effect. I confirm the dominance of the indirect effect using QCA. The economic significance of the direct effect is identified using probit regression analysis. In essence, the hypothesized indirect effect (H1) is weakly supported, while the hypothesized direct effect (H2) is strongly supported.

A concern of my study is endogeneity. To mitigate this concerns, I employ several additional approaches, such as an IV model and a Heckman self-selection model. As auxiliary analysis, I exploit a quasi-natural experimental setting that allows me to test whether firms located in countries with a stronger rule-oriented culture *respond* differently to an exogenous legal shock—that is, the EU CSR reporting directive—compared to firms located in countries with a weaker rule-oriented culture. I find that firms located in countries with a stronger rule-oriented culture are more responsive to the EU CSR directive in terms of increasing the credibility of their CSR report. This finding further

mitigates endogeneity concerns.

The results of this study should encourage (European) supra-national and national regulators to consider the rule-oriented culture of a country when making decisions, because how corporate decisions are made and how regulations are perceived—such as the EU CSR reporting directive—appear to be driven by cultural rule orientation.

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**Table 1**  
Summary of key variables

Country	Assurance		Assurance Provider		Assurance Standard		Assurance Scope		Assurance Level		CSR Laus		UA Rule Orientation	
	0	1	0	1	0	1	0	1	0	1	0	1	0	1
Australia	92	101	49	52	28	73	76	25	89	12	-0.04		4.39	
Austria	37	41	5	36	19	22	27	14	39	2	0.46		5.16	
Brazil	190	79	22	57	35	44	38	41	65	14	0.23		3.60	
Canada	218	92	15	77	20	72	81	11	81	11	-0.74		4.58	
Colombia	33	24	9	15	7	17	14	10	24	0	0.31		3.57	
Denmark	32	34	3	31	15	19	18	16	31	3	1.06		5.22	
Finland	118	105	16	89	32	73	45	60	100	5	0.95		5.02	
France	100	133	24	109	49	84	89	44	103	30	1.35		4.43	
Germany	179	119	6	113	35	84	86	33	106	13	1.03		5.19	
Greece	45	12	5	7	8	4	9	3	10	2	0.54		3.39	
Hong Kong	23	44	25	19	14	30	21	23	35	9	-0.84		4.32	
India	94	170	66	104	23	147	93	77	155	15	-1.34		4.15	
Ireland	21	19	13	6	9	10	14	5	18	1	-0.76		4.30	
Israel	29	0	—	—	—	—	—	—	—	—	-0.98		4.01	
Italy	56	102	16	86	14	88	32	70	97	5	1.02		3.79	
Japan	762	168	58	110	73	95	151	17	162	6	0.42		4.07	
Malaysia	52	37	21	16	24	13	13	24	30	7	-1.82		4.78	
Mexico	56	38	13	25	16	22	29	9	31	7	0.35		4.18	
Netherlands	101	94	18	76	70	24	52	42	69	25	0.87		4.70	
New Zealand	14	12	7	5	9	3	9	3	12	0	-1.13		4.75	
Philippines	20	19	16	3	9	10	10	9	18	1	0.20		3.89	
Portugal	25	33	6	27	6	27	17	16	23	10	1.32		3.91	
Singapore	65	42	22	20	19	23	28	14	42	0	-1.49		5.31	
South Africa	396	150	51	99	31	119	136	14	116	34	-0.73		4.34	
Spain	39	86	20	66	26	60	27	59	68	18	1.33		3.97	
Sweden	103	106	14	92	70	36	44	62	86	20	1.45		5.32	
Switzerland	124	83	35	48	45	38	49	34	69	14	0.80		5.37	
Thailand	71	32	13	19	17	15	32	0	32	0	-1.35		3.93	
Turkey	52	13	7	6	10	3	12	1	13	0	-0.14		3.63	
United Kingdom	79	134	50	84	53	81	90	44	108	26	-1.03		4.65	
United States	974	267	187	80	182	85	213	54	232	35	-1.05		4.15	
Total/Mean	4,200	2,389	812	1,577	968	1,421	1,555	834	2,064	325	0.01		4.39	

Please see Table A1 for variable definitions.

**Table 2**  
Path analysis

$d =$		(1)	(2)	(3)	(4)	(5)
		<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
Direct path:						
Pa[ <i>UA Rule Orientation</i> ; $d$ ]	$\hat{\beta}_1$	0.297*** (0.038)	0.208*** (0.052)	0.159*** (0.059)	0.199*** (0.050)	0.046 (0.038)
Mediated path:						
Pa[ <i>UA Rule Orientation</i> ; <i>CSR Laws</i> ]	$\hat{\alpha}_1$	0.600*** (0.089)	0.529*** (0.129)	0.529*** (0.129)	0.529*** (0.129)	0.529*** (0.129)
Pa[ <i>CSR Laws</i> ; $d$ ]	$\hat{\beta}_2$	0.053*** (0.020)	0.115*** (0.022)	0.022* (0.012)	0.043** (0.017)	0.007 (0.013)
Total mediated path:						
Pa[ <i>UA Rule Orientation</i> ; <i>CSR Laws</i> ] $\times$ Pa[ <i>CSR Laws</i> ; $d$ ]	$\hat{\alpha}_1 \times \hat{\beta}_2$	0.032** (0.013)	0.061*** (0.018)	0.012* (0.006)	0.023** (0.011)	0.004 (0.007)
Control variables		yes	yes	yes	yes	yes
Industry fixed effects		yes	yes	yes	yes	yes
Year fixed effects		yes	yes	yes	yes	yes
SRMR		0.045	0.036	0.034	0.033	0.032
N		6,589	2,389	2,389	2,389	2,389

This table reports coefficients and standard errors (in parentheses) from weighted linear ML path regressions. Weights are equal to the inverse of the number of firm-year observations in each country. All continuous variables are standardized with a mean of zero and a standard deviation of one. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. In all Columns, the following control variables are included: *CSR Laws*, *Rule of Law*, *Financial Transparency*, *Financial System*, *Size*, *DJSI*, *Leverage*, *Profit*, *R&D*, *Capex*, *Market Share*, *Age*, and *Earnings Volatility*. In Column (2), the control variable *Assurance Market* is also included. In Column (3), the control variable *ISA* is also included. Standard errors are robust standard errors adjusted for clustering at the country level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 3**  
Qualitative comparative analysis

Panel A: Minimum consistency level = 0.84										
Path no.	<i>UA Rule Orientation</i>	<i>Labor Laws</i>	<i>Environment Laws</i>	<i>CSR Reporting Regulations</i>	<i>Civil Law</i>	Coverage			Solution	
						Raw	Unique	Consistency	Coverage	Consistency
1	●	●	●	●	●	0.222	0.222	0.848	0.222	0.848
Panel B: Minimum consistency level = 0.82										
Path no.	<i>UA Rule Orientation</i>	<i>Labor Laws</i>	<i>Environment Laws</i>	<i>CSR Reporting Regulations</i>	<i>Civil Law</i>	Coverage			Solution	
						Raw	Unique	Consistency	Coverage	Consistency
1	●	●	●		●	0.343	0.101	0.833	0.372	0.833
2	●	●	●	●		0.239	0.006	0.821		
3	●			●	●	0.222	0.001	0.848		
Panel C: Minimum consistency level = 0.80										
Path no.	<i>UA Rule Orientation</i>	<i>Labor Laws</i>	<i>Environment Laws</i>	<i>CSR Reporting Regulations</i>	<i>Civil Law</i>	Coverage			Solution	
						Raw	Unique	Consistency	Coverage	Consistency
1	●					0.630	0.630	0.812	0.630	0.812

This table reports the results from a qualitative comparative analysis. The sample comprises firm-year observations from 2012 to 2016 (N=2,389). In all panels, the outcome variable (outcome set) is *Assurance Provider*. The explanatory variables (explanatory sets) *UA Rule Orientation*, *Labor Laws*, *Environment Laws*, and *CSR Reporting Regulations* are transformed into dummy variables by using the sample median as splitting criterion. Black circles (●) indicate the presence of an attribute (variable=1). White circles (○) indicate the negation of an attribute (variable=0). Blank cells indicate the absence of an attribute (i.e., the variable does not contribute to the outcome). Please see Table A1 for variable definitions.

**Table 4**  
Probit regression analysis

	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>UA Rule Orientation</i>	1.011*** (0.119) [0.369]	0.915*** (0.198) [0.333]	0.509*** (0.176) [0.190]	0.643*** (0.171) [0.220]	0.192 (0.227) [0.028]
<i>CSR Laws</i>	0.168*** (0.061) [0.061]	0.351*** (0.095) [0.127]	0.073* (0.039) [0.010]	0.196*** (0.068) [0.067]	0.126 (0.077) [0.004]
<i>Rule of Law</i>	-0.499*** (0.090)	-0.040 (0.138)	-0.153 (0.127)	-0.177 (0.124)	-0.126 (0.191)
<i>Financial Transparency</i>	0.007 (0.011)	0.029** (0.014)	0.005 (0.011)	0.021 (0.014)	0.002 (0.015)
<i>Financial System</i>	5.417*** (0.751)	2.161* (1.159)	2.038** (0.894)	3.190*** (1.093)	2.118* (1.211)
<i>Size</i>	0.107** (0.050)	0.064* (0.038)	0.044 (0.071)	-0.155** (0.070)	-0.050 (0.103)
<i>DJSI</i>	0.653*** (0.109)	0.066 (0.146)	0.043 (0.136)	-0.022 (0.130)	0.166 (0.167)
<i>Leverage</i>	-0.402 (0.261)	0.248 (0.422)	-0.104 (0.412)	-0.458 (0.367)	-0.730 (0.461)
<i>Profit</i>	-0.475 (0.606)	0.384 (1.116)	0.388 (1.177)	-0.522 (1.032)	-0.515 (1.378)
<i>R&amp;D</i>	-0.735 (1.636)	-4.348* (2.222)	-0.348 (2.302)	-8.070*** (2.511)	0.929 (3.192)
<i>Capex</i>	0.636* (0.362)	0.383 (0.644)	0.103 (0.588)	-0.480 (0.706)	0.512 (0.865)
<i>Market Share</i>	42.714*** (11.195)	56.233*** (15.671)	28.745** (14.351)	29.553* (15.249)	57.606*** (19.438)
<i>Age</i>	-0.006 (0.006)	-0.007 (0.011)	-0.007 (0.010)	-0.006 (0.009)	-0.009 (0.013)
<i>Earnings Volatility</i>	0.036 (0.034)	-0.010 (0.058)	0.045 (0.046)	0.048 (0.049)	-0.010 (0.070)
<i>Assurance Market</i>	—	2.184*** (0.511)	—	—	—
<i>ISA</i>	—	—	0.210*** (0.080)	—	—
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Pseudo $R^2$	0.190	0.228	0.180	0.358	0.314
Mean VIF	1.73	1.83	1.84	1.85	1.85
N	6,589	2,389	2,389	2,389	2,389

This table reports coefficients, standard errors (in parentheses), and marginal effects [in brackets] from weighted probit regressions. Weights are equal to the inverse of the number of firm-year observations in each country. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the country level. VIFs are based on linear regressions. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.



**Table 5**

Alternative sets of control variables

Panel A: Controlling for additional country-level variables					
	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>UA Rule Orientation</i>	0.643*** (0.174)	1.016*** (0.273)	0.667*** (0.252)	0.689*** (0.247)	-0.294 (0.378)
<i>GDP</i>	0.035 (0.022)	-0.075* (0.040)	-0.050 (0.037)	-0.035 (0.038)	0.008 (0.053)
<i>Trade Openness</i>	0.030*** (0.007)	0.003 (0.011)	-0.006 (0.008)	0.032*** (0.009)	0.028** (0.014)
<i>Political Orientation</i>	-0.034 (0.216)	-0.061 (0.339)	0.079 (0.341)	-0.417 (0.352)	0.351 (0.483)
<i>Assurance Fee</i>	-2.480*** (0.961)	0.187 (1.175)	-1.748 (1.066)	-2.821** (1.285)	0.853 (1.504)
<i>Assurance Risk</i>	-0.010 (0.039)	-0.118* (0.065)	0.113** (0.046)	0.068 (0.052)	-0.155** (0.072)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Pseudo $R^2$	0.202	0.289	0.219	0.359	0.463
Mean VIF	2.27	2.42	2.49	2.49	2.46
N	5,628	2,099	2,099	2,099	2,099
Panel B: Controlling for additional cultural dimensions					
	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>UA Rule Orientation</i>	0.943*** (0.116)	0.874*** (0.188)	0.442*** (0.164)	0.594*** (0.175)	0.176 (0.207)
<i>Power Distance</i>	0.227 (0.155)	-0.119 (0.243)	0.446** (0.180)	-0.134 (0.210)	-0.412* (0.233)
<i>Collectivism</i>	-0.120 (0.103)	-0.216 (0.178)	0.265* (0.154)	-0.185 (0.186)	-0.322 (0.208)
<i>Gender Egalitarianism</i>	0.158** (0.065)	0.292*** (0.095)	0.046 (0.077)	0.366*** (0.091)	0.258** (0.105)
<i>Future Orientation</i>	0.055 (0.214)	0.831** (0.357)	0.425 (0.311)	-0.468 (0.316)	0.147 (0.375)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Pseudo $R^2$	0.194	0.239	0.187	0.372	0.324
Mean VIF	1.83	1.88	1.97	1.97	1.97
N	6,589	2,389	2,389	2,389	2,389

(continued on next page)

Panel C: Controlling for economic, geopolitical, and regulatory factor variables					
	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>UA Rule Orientation</i>	0.621*** (0.122)	0.606*** (0.190)	0.403*** (0.158)	0.382** (0.165)	0.064 (0.214)
<i>Economic Factor</i>	-0.357*** (0.082)	0.091 (0.119)	-0.060 (0.099)	-0.222** (0.102)	0.101 (0.139)
<i>Geopolitical Factor</i>	-0.304*** (0.051)	-0.411*** (0.077)	-0.099 (0.062)	-0.127** (0.063)	0.017 (0.082)
<i>Regulatory Factor</i>	0.190*** (0.044)	0.103 (0.076)	0.116** (0.057)	0.229*** (0.066)	0.281*** (0.078)
Control variables	Firm-level	Firm-level	Firm-level	Firm-level	Firm-level
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Pseudo $R^2$	0.156	0.231	0.133	0.360	0.319
Mean VIF	1.55	1.49	1.48	1.48	1.48
N	6,589	2,389	2,389	2,389	2,389

This table reports coefficients and standard errors (in parentheses) from weighted probit regressions. Weights are equal to the inverse of the number of firm-year observations in each country. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. In Panels A and B, the control variables are the same as in Table 4. In Panel C, the control variables only include firm-level variables. Standard errors are robust standard errors adjusted for clustering at the country level. VIFs are based on linear regressions. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 6**  
Hierarchical generalized linear modeling

	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>UA Rule Orientation</i>	0.835*** (0.059)	0.942*** (0.218)	0.566*** (0.103)	0.330*** (0.115)	-0.184 (0.207)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Variance of random effects	0.434***	0.231***	0.327***	0.366***	0.194***
N	6,589	2,389	2,389	2,389	2,389

This table reports coefficients and standard errors (in parentheses) from hierarchical generalized linear regressions. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. The control variables are the same as in Table 4. Standard errors are robust standard errors adjusted for clustering at the country level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 7**

Instrumental variable (IV) probit

Panel A: Reduced-form equation					
	(1) <i>UA Rule Orientation</i>	(2) <i>UA Rule Orientation</i>	(3) <i>UA Rule Orientation</i>	(4) <i>UA Rule Orientation</i>	(5) <i>UA Rule Orientation</i>
<i>Genetic Distance</i>	0.485*** (0.027)	0.586*** (0.038)	0.593*** (0.041)	0.510*** (0.036)	0.601*** (0.042)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
N	6,415	2,303	2,303	2,303	2,303
Panel B: Outcome equation					
	(1) <i>Assurance</i>	(2) <i>Assurance Provider</i>	(3) <i>Assurance Standard</i>	(4) <i>Assurance Scope</i>	(5) <i>Assurance Level</i>
<i>UA Rule Orientation</i>	1.994*** (0.244)	1.694*** (0.339)	1.220*** (0.343)	1.249*** (0.346)	0.097 (0.499)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
N	6,415	2,303	2,303	2,303	2,303
Wald test of exogeneity ( <i>p</i> -value)	0.000	0.000	0.009	0.002	0.013

This table reports coefficients and standard errors (in parentheses) from weighted LIML IV probit regressions. Weights are equal to the inverse of the number of firm-year observations in each country. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. The control variables are the same as in Table 4. Standard errors are robust standard errors adjusted for clustering at the country level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 8**  
Alternative cultural variables

Panel A: Big Data (BD) Rule Orientation					
	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>BD Rule Orientation</i>	0.011*** (0.002)	0.007*** (0.002)	0.015*** (0.003)	0.009*** (0.003)	0.005** (0.002)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Pseudo $R^2$	0.169	0.208	0.191	0.345	0.313
N	6,589	2,389	2,389	2,389	2,389
Panel B: Tightness					
	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>Tightness</i>	0.084*** (0.024)	0.025 (0.040)	0.148*** (0.034)	0.052 (0.038)	0.066 (0.050)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Pseudo $R^2$	0.171	0.254	0.236	0.291	0.453
N	4,700	1,688	1,688	1,688	1,688
Panel D: Hofstede Uncertainty Avoidance (Stress Orientation)					
	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>UA Stress Orientation</i>	-0.007 (0.005)	0.001 (0.004)	-0.003 (0.003)	-0.005 (0.003)	-0.002 (0.004)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Pseudo $R^2$	0.175	0.204	0.172	0.347	0.313
N	6,589	2,389	2,389	2,389	2,389

This table reports coefficients and standard errors (in parentheses) from weighted probit regressions. Weights are equal to the inverse of the number of firm-year observations in each country. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. The control variables are the same as in Table 4. Standard errors are robust standard errors adjusted for clustering at the country level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Table 9**  
Heckprobit self-selection model

	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>UA Rule Orientation</i>	0.787*** (0.151)	0.851*** (0.224)	0.477*** (0.155)	0.756*** (0.197)	0.334 (0.250)
Control variables	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
N	88,012	6,589	6,589	6,589	6,589
Censored N	81,423	4,200	4,200	4,200	4,200
Uncensored N	6,589	2,389	2,389	2,389	2,389
Selection equation	Table <a href="#">A4</a> (1)	Table <a href="#">A4</a> (2)	Table <a href="#">A4</a> (2)	Table <a href="#">A4</a> (2)	Table <a href="#">A4</a> (2)
Wald test of independent equations ( <i>p</i> -value)	0.071	0.031	0.023	0.025	0.033

This table reports coefficients and standard errors (in parentheses) of the outcome equation from weighted Heckprobit regressions. Weights are equal to the inverse of the number of firm-year observations in each country. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. The control variables are the same as in Table 4. Standard errors are robust standard errors adjusted for clustering at the country level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table [A1](#) for variable definitions.

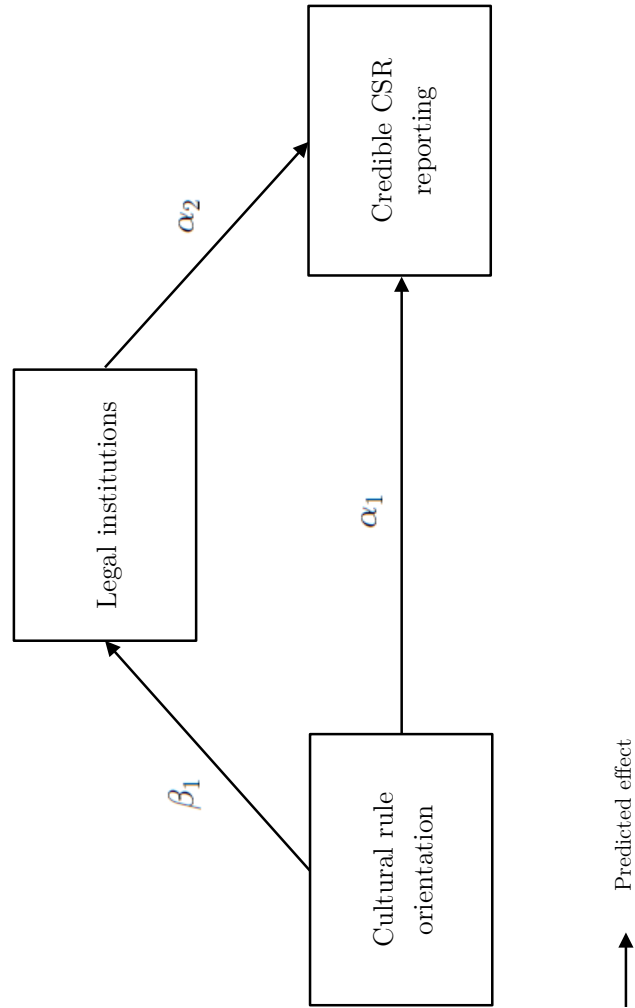
**Table 10**

Evidence from the Directive 2014/95/EU

	(1)	(2)	(3)	(4)	(5)
	<i>Assurance</i>	<i>Assurance Provider</i>	<i>Assurance Standard</i>	<i>Assurance Scope</i>	<i>Assurance Level</i>
<i>High UA Rule Orientation</i>	0.175***	0.215***	0.094**	0.116**	0.073**
× <i>EU CSR Directive</i>	(0.044)	(0.055)	(0.045)	(0.057)	(0.035)
<i>EU CSR Directive</i>	yes	yes	yes	yes	yes
<i>HighUA Rule Orientation</i>	no	no	no	no	no
Control variables	time-variant	time-variant	time-variant	time-variant	time-variant
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes	yes
$R^2$	0.277	0.309	0.201	0.414	0.290
N	6,589	2,389	2,389	2,389	2,389

This table reports coefficients and standard errors (in parentheses) from weighted OLS regressions. Weights are equal to the inverse of the number of firm-year observations in each country. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. The control variables include time-variant variables (i.e., all firm-level variables, *Financial System*, and, in Column (2), *Assurance Market*). Standard errors are robust standard errors adjusted for clustering at the country level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

**Figure 1**  
Path model



## Appendix A

**Table A1**  
Variable definitions

Variable name	Definition
CSR reporting variables:	
<i>Assurance<sub>ijct</sub></i>	Dummy variable equal to one if the firm receives assurance on its GRI report, and zero otherwise. Data source: GRI Sustainability Disclosure Database (GRI SDD)
<i>Assurance Level<sub>ijct</sub></i>	Dummy variable equal to one if the firm receives assurance on its GRI report on a high (reasonable) level, and zero otherwise. Data source: GRI SDD
<i>Assurance Provider<sub>ijct</sub></i>	Dummy variable equal to one if the firm receives assurance on its GRI report from an accounting firm, and zero otherwise. Data source: GRI SDD
<i>Assurance Scope<sub>ijct</sub></i>	Dummy variable equal to one if the firm receives assurance on its entire GRI report, and zero otherwise. Data source: GRI SDD
<i>Assurance Standard<sub>ijct</sub></i>	Dummy variable equal to one if the firm receives assurance on its GRI report in accordance with an international assurance standard (AA1000AS and/or ISAE 3000), and zero otherwise. Data source: GRI SDD
<i>GRI Report<sub>ijct</sub></i>	Dummy variable equal to one if the firm issues a GRI report, and zero otherwise.
<i>Peers(Assurance)<sub>ijct</sub></i>	The country-industry-year percentage of firms (excluding the focal firm) receiving assurance on their GRI report.
<i>Peers(GRI Report)<sub>ijct</sub></i>	The country-industry-year percentage of firms (excluding the focal firm) issuing a GRI report.
Cultural variables:	
<i>BD Rule Orientation<sub>c</sub></i>	The median of the Google search volume (between 2005 and 2015) of legal terms (e.g., law, regulation, constitution) in the country's official language(s) divided by the number of people regularly using the Internet. Data source: Google Trends, Global competitiveness reports of the World Economic Forum ( <a href="http://www.weforum.org/reports/">www.weforum.org/reports/</a> ), Population and vital statistics reports of the United Nations ( <a href="https://unstats.un.org/unsd/demographic/products/vitstats/">https://unstats.un.org/unsd/demographic/products/vitstats/</a> )
<i>Collectivism<sub>c</sub></i>	Institutional collectivism practices index from House et al. (2004). A higher index score indicates a higher degree of collectivism.
<i>Future Orientation<sub>c</sub></i>	Future orientation practices index from House et al. (2004). A higher index score indicates a higher degree of future orientation.
<i>Gender Egalitarianism<sub>c</sub></i>	Gender egalitarianism practices index from House et al. (2004). A higher index score indicates a higher degree of gender egalitarianism.
<i>High UA Rule Orientation<sub>c</sub></i>	Dummy variable equal to one if <i>UA Rule Orientation</i> is above the sample median, and zero otherwise.
<i>Power Distance<sub>c</sub></i>	Power distance practices index from House et al. (2004). A higher index score indicates a higher degree of power distance.
<i>Tightness<sub>c</sub></i>	Tightness index from Gelfand et al. (2011). A higher index score indicates a higher degree of tightness.
<i>UA Rule Orientation<sub>c</sub></i>	Uncertainty avoidance practices index from House et al. (2004). A higher index score indicates a higher degree of rule orientation.
<i>UA Stress Orientation<sub>c</sub></i>	Uncertainty avoidance index from Hofstede (1980). A higher index score indicates a higher degree of stress orientation.

(continued on next page)



Legal institutional variables:	
<i>Civil Law<sub>c</sub></i>	Dummy variable equal to one if the country has civil law origin, and zero otherwise. Data source: La Porta et al. (1997)
<i>CSR Laws<sub>c</sub></i>	The principal factor of the variables <i>Civil Law</i> , <i>CSR Reporting Regulations</i> , <i>Environment Laws</i> , and <i>Labor Laws</i> .
<i>CSR Reporting Regulations<sub>c</sub></i>	Number of voluntary and mandatory CSR reporting regulations for non-financial firms in 2016. Mandatory regulations are given a weight of two. Data source: Maniora and Ernstberger (2016), Dhaliwal et al. (2014), <a href="https://carrotsandsticks.net/">https://carrotsandsticks.net/</a> , <a href="http://reportingcsr.org">http://reportingcsr.org</a> , <a href="http://globalreporting.org">http://globalreporting.org</a> , <a href="http://iri.hks.harvard.edu/files/iri/files/corporate_social_responsibility_disclosure_3-27-15.pdf">http://iri.hks.harvard.edu/files/iri/files/corporate_social_responsibility_disclosure_3-27-15.pdf</a>
<i>Environment Laws<sub>c</sub></i>	Number of environmental laws in 2016. Data source: <a href="https://ecolex.org">https://ecolex.org</a> , <a href="https://www.lexadin.nl/wlg/legis/nofr/legis.php">https://www.lexadin.nl/wlg/legis/nofr/legis.php</a> <a href="https://en.wikipedia.org/wiki/List_of_environmental_laws_by_country">https://en.wikipedia.org/wiki/List_of_environmental_laws_by_country</a> , <a href="https://github.com/gchapron/LegalBoundaries/tree/master/Countries">https://github.com/gchapron/LegalBoundaries/tree/master/Countries</a> , and government websites
<i>Labor Laws<sub>c</sub></i>	Mean rank score of the following indexes: (1) human rights laws, (2) social security laws, (3) collective relations laws, and (4) employment laws. The first index is from Humana (1992). The remaining three indexes are from Botero et al. (2004). A higher variable score indicates greater protection of labor rights and benefits.
Other variables:	
<i>Age<sub>ijct</sub></i>	Number of years the firm has been covered by Compustat.
<i>Assurance Fee<sub>ct</sub></i>	Country median of assurance fees (paid for assurance on financial reports) divided by total assets. Data source: Thomson Reuters
<i>Assurance Litigation Risk<sub>c</sub></i>	Assurance providers' litigation risk index from Wingate (1997). A higher index indicates a higher degree of assurance providers' litigation risk.
<i>Assurance Market<sub>ct</sub></i>	Assurance provider concentration (for assurance on financial reports) in the country. Assurance provider concentration is measured by the number of clients of the market leader (assurance firm with the most clients) divided by the total number of clients of all assurance firms. Data source: Thomson Reuters
<i>Assurance Risk<sub>c</sub></i>	The principal factor of the variables <i>Assurance Litigation Risk</i> , <i>Assurance Work Environment</i> , and <i>Risk Aversion</i> .
<i>Assurance Work Environment<sub>c</sub></i>	The sum of the first seven items of the assurance providers' working environment index from Brown et al. (2014). A higher index score indicates a higher quality of assurance providers' working environment.
<i>British Rule<sub>c</sub></i>	Dummy variable equal to one if a country has been ruled by Britain in history, and zero otherwise. Data Source: Treisman (2000)
<i>Cape<sub>ijct</sub></i>	Capital expenditures divided by total sales. Data source: Compustat
<i>DJSI<sub>ijct</sub></i>	Dummy variable equal to one if the firm is included in the Dow Jones Sustainability World Index, and zero otherwise. Data source: RobecoSAM
<i>Earnings Volatility<sub>ijct</sub></i>	Natural logarithm of one plus the ex ante time-series standard deviation of earnings per share (EPS), using a rolling window of ten years. Data source: Compustat
<i>Economic Factor<sub>c</sub></i>	Standardized scores of the economic country factor (Factor 1) from Isidro et al. (2016).
<i>EU CSR Directive<sub>ct</sub></i>	Dummy variable equal to one if country <i>c</i> is a member of the EU and year <i>t</i> is 2015 or 2016, and zero otherwise. Data source: Compustat, <a href="https://europa.eu">https://europa.eu</a>

(continued on next page)

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<i>Financial Transparency<sub>c</sub></i>	Financial disclosure index developed by the Center for International Financial Analysis and Research (CIFAR) (1995). I obtain the CIFAR country scores from Bushman et al. (2004).
<i>Financial System<sub>ct</sub></i>	Country-median of firms' debt over total assets (based on Compustat universe). Data source: Compustat
<i>Genetic Distance<sub>c</sub></i>	Mahalanobis distance ( $M_{ST}$ ) of the frequency of blood types A and B between the dominant populations of two countries. A higher $M_{ST}$ score indicates greater blood distance from the United States. I use the natural logarithm of $M_{ST}$ , i.e., $\ln(1+M_{ST})$ . Data source: Online appendix of Gorodnichenko and Roland (2017)
<i>GDP<sub>ct</sub></i>	Natural logarithm of GDP per capita (constant 2011). Data source: The World Bank DataBank (World Development Indicators)
<i>Geopolitical Factor<sub>c</sub></i>	Standardized scores of the geopolitical country factor (Factor 2) from Isidro et al. (2016).
<i>ISA<sub>c</sub></i>	Dummy variable equal to one if the ISA are adopted in 2012, and zero otherwise. Data source: Boolaky and Soobaroyen (2017)
<i>Leverage<sub>ijct</sub></i>	One minus the ratio of equity over total assets. Data source: Compustat
<i>Market Share<sub>ijct</sub></i>	Total sales divided by the total sales of all firms in the same industry. Data source: Compustat
<i>Political Orientation<sub>c</sub></i>	Percentage of years (1928–1995) during which both the largest party in congress and the party of the chief executive had center or left political orientation. Data source: Botero et al. (2004)
<i>Profit<sub>ijct</sub></i>	Return on assets. Data source: Compustat
<i>R&amp;D<sub>ijct</sub></i>	Research and development expenditures over total assets. I assume that R&D is zero if data are missing. Data source: Compustat
<i>Regulatory Factor<sub>c</sub></i>	Standardized scores of the regulatory country factor (Factor 4) from Isidro et al. (2016).
<i>Risk Aversion<sub>c</sub></i>	Risk aversion index from Rieger et al. (2015). A higher index score indicates a higher degree of risk aversion.
<i>Rule of Law<sub>ct</sub></i>	Rule of law index. A higher index score indicates a higher quality of the legal environment. Data source: The World Bank DataBank (Worldwide Governance Indicators)
<i>Settler Mortality<sub>c</sub></i>	Settler mortality index from Acemoglu et al. (2001).
<i>Size<sub>ijct</sub></i>	Natural logarithm of total sales. Data source: Compustat
<i>Trade Openness<sub>c</sub></i>	Constructed trade share from Frankel and Romer (1990). A higher variable score indicates a higher degree of trade openness.

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$i$ ,  $j$ ,  $c$ , and  $t$  denote firm, industry, country, and year indexes, respectively. All firm-level variables with no natural lower and upper bounds are winsorized at the 1<sup>st</sup> and 99<sup>th</sup> percentiles.

**Table A2**  
Factor analysis

	<i>CSR Laws</i> (Principal factor)
Factor loadings:	
<i>Labor Laws</i>	0.924
<i>Civil Law</i>	0.790
<i>Environmental Laws</i>	0.697
<i>CSR Reporting Regulations</i>	0.630
Eigenvalue:	2.360
Proportion:	0.590

This table reports factor loadings, eigenvalue, and proportion for the principal factor. Please see Table [A1](#) for variable definitions.

**Table A3**  
Sample composition

	Reporting Sample	Assurance Sample
Australia	193	101
Austria	78	41
Brazil	269	79
Canada	310	92
Colombia	57	24
Denmark	66	34
Finland	223	105
France	233	133
Germany	298	119
Greece	57	12
Hong Kong	67	44
India	264	170
Ireland	40	19
Israel	29	0
Italy	158	102
Japan	930	168
Malaysia	89	37
Mexico	94	38
Netherlands	195	94
New Zealand	26	12
Philippines	39	19
Portugal	58	33
Singapore	107	42
South Africa	546	150
Spain	125	86
Sweden	209	106
Switzerland	207	83
Thailand	103	32
Turkey	65	13
United Kingdom	213	134
United States	1,241	267
Total	6,589	2,389
%	—	36.26%

**Table A4**  
Heckprobit: Selection equations

	(1) <i>GRI Report</i>	(2) <i>Assurance</i>
<i>Peers(GRI Report)</i>	<b>0.044***</b> (0.002)	—
<i>Peers(Assurance)</i>	—	<b>0.042***</b> (0.002)
<i>UA Rule Orientation</i>	0.788*** (0.078)	1.120*** (0.121)
<i>CSR Laws</i>	0.047*** (0.003)	0.128*** (0.045)
<i>Rule of Law</i>	−0.933*** (0.056)	−0.572*** (0.085)
<i>Financial Transparency</i>	0.026*** (0.004)	0.010 (0.008)
<i>Financial System</i>	2.063*** (0.545)	5.332*** (0.765)
<i>Size</i>	0.410*** (0.022)	0.118** (0.049)
<i>DJSI</i>	1.101*** (0.101)	0.653*** (0.109)
<i>Leverage</i>	−0.316*** (0.119)	−0.451* (0.258)
<i>Profit</i>	0.122 (0.283)	−0.849 (0.592)
<i>R&amp;D</i>	1.205 (0.757)	−0.558 (1.602)
<i>Capex</i>	0.498*** (0.172)	0.584 (0.360)
<i>Market Share</i>	67.982*** (5.545)	43.652*** (11.166)
<i>Age</i>	0.015*** (0.003)	−0.008 (0.006)
<i>Earnings Volatility</i>	−0.010 (0.017)	0.022 (0.033)
Industry fixed effects	yes	yes
Year fixed effects	yes	yes
N	88,012	6,589

This table reports coefficients, standard errors (in parentheses) of the selection equations from weighted heckprobit regressions. Weights are equal to the inverse of the number of firm-year observations in each country. The sample comprises firm-year observations from 2012 to 2016. Fixed effects are included as indicated. Industry fixed effects are based on one-digit SIC codes. Standard errors are robust standard errors adjusted for clustering at the country level. Statistical significance at the 1%, 5%, and 10% levels is denoted by \*\*\*, \*\*, and \*, respectively. Please see Table A1 for variable definitions.

## Figure A1

Correlations of Google web search activities

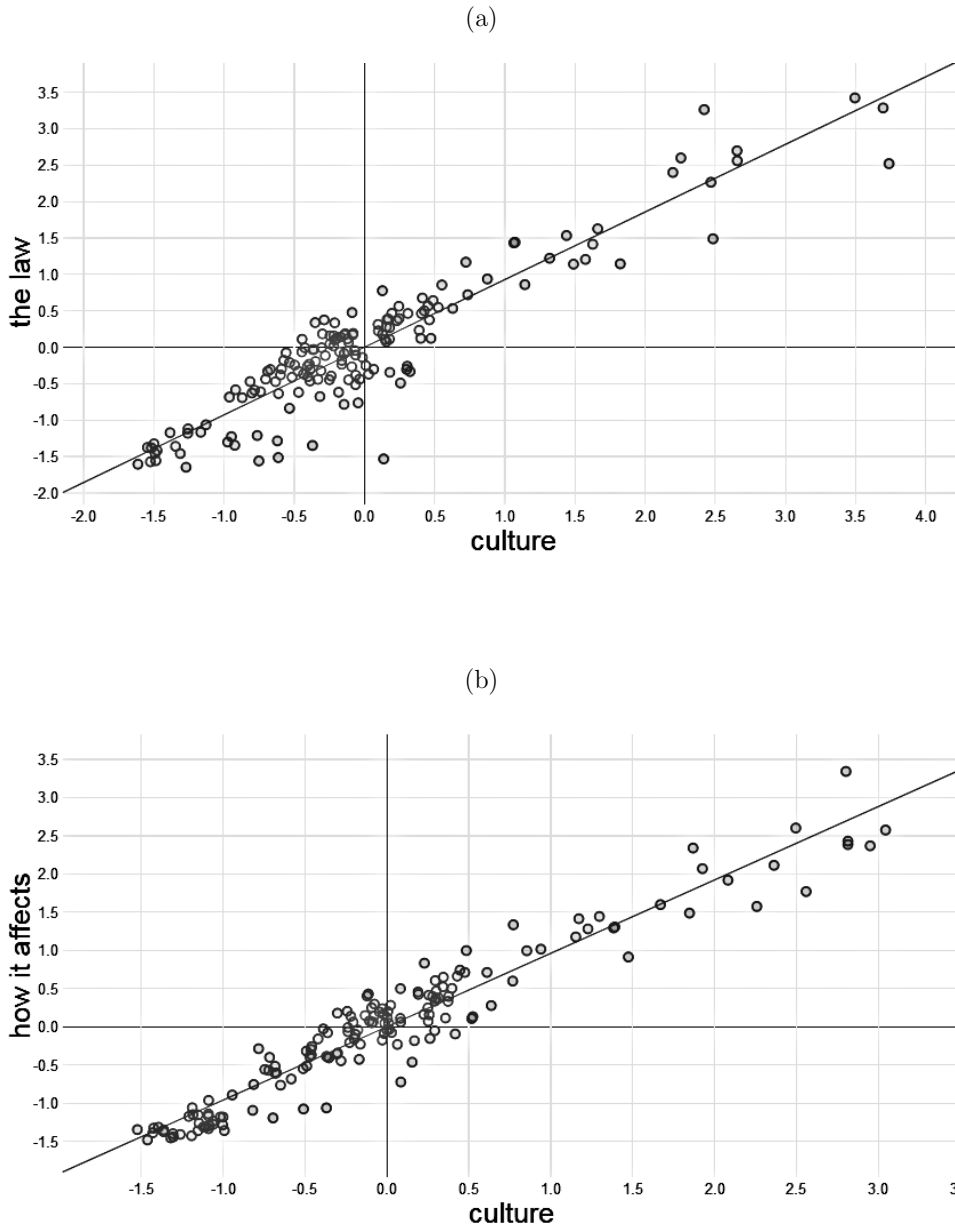
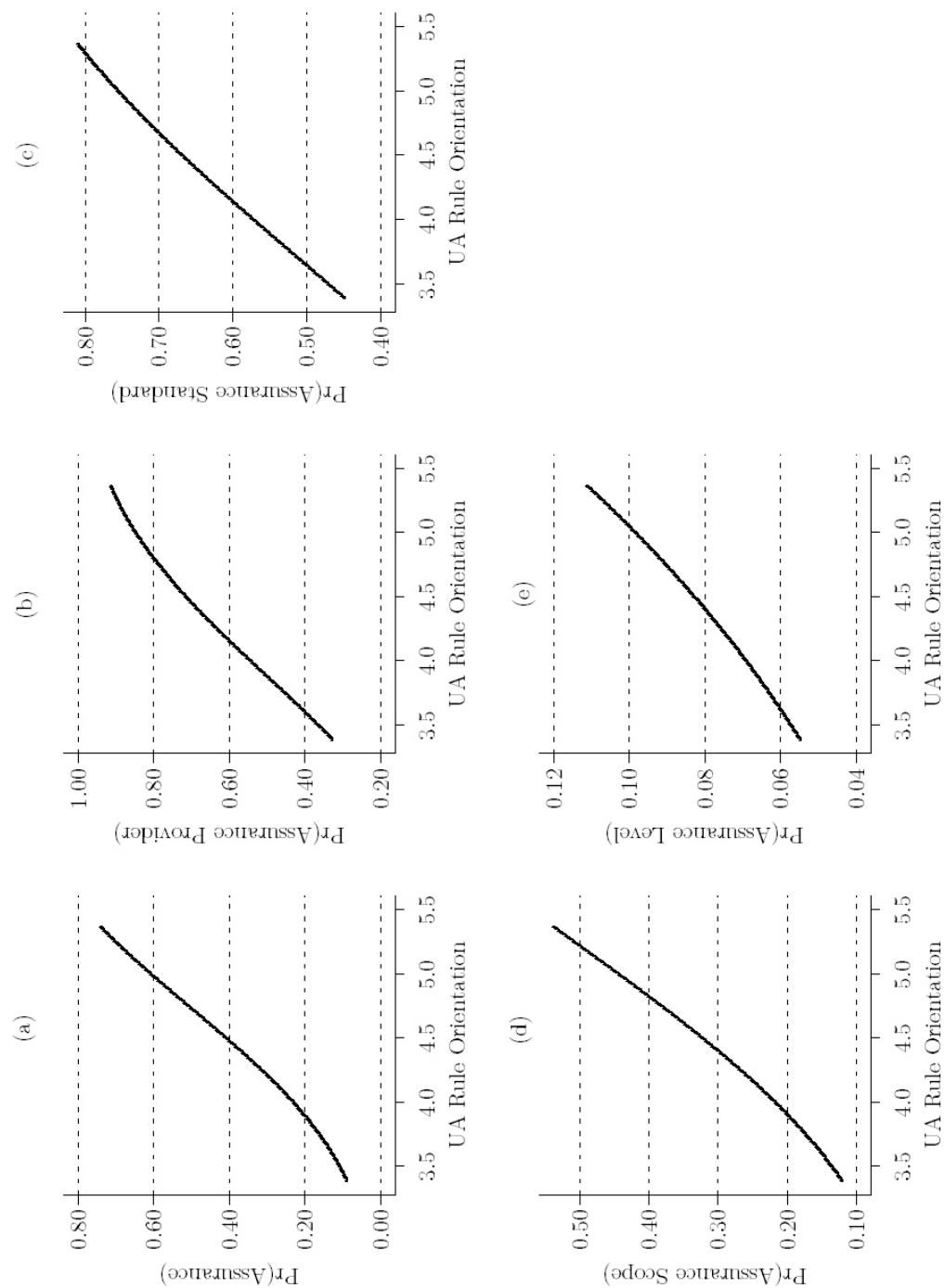


Figure A1 (a) shows the correlation of the web search activities for the terms 'culture' and 'the law' in Australia. The Pearson correlation coefficient is 0.928. Figure A1 (b) shows the correlation of the web search activities for the terms 'culture' and 'how it affects' in the United States. The Pearson correlation coefficient is 0.961. Source: Google Correlate

**Figure A2**  
Predicted probabilities



This figure shows predicted probabilities based on the probit regressions reported in Table 4. Please see Table A1 for variable definitions.

## Appendix B

Guiso et al. (2006) define culture as the societal beliefs and social norms that are transmitted fairly unchanged from generation to generation. By focusing on beliefs and norms, i.e., non-standard preferences, the idea of culture fits into an economic agent's utility function (Ahern et al., 2015). Based on Benjamin et al. (2010), I provide a parsimonious model on how the strength of a specific dimension of culture *directly* shapes corporate decision-making.

Let  $d$  be a corporate decision. A manager belongs to a culture, with the strength of a specific cultural dimension  $s > 0$ . Let  $d_0$  be the preferred decision in the absence of culture, and let  $d_C$  be the preferred decision in the presence of culture  $C$ . The manager chooses  $d$  to maximize

$$U = -(1 - w(s))(d - d_0)^2 - w(s)(d - d_C)^2, \quad (\text{B1})$$

where  $0 \leq w(s) \leq 1$  is the weight placed on  $C$ . I assume that  $w(0) = 0$  and  $w' > 0$ . Deviating from  $C$  causes disutility that is increasing in  $s$ . The first-order condition of Equation (B1) yields the optimal decision,

$$d^*(s) = (1 - w(s))d_0 + w(s)d_C, \quad (\text{B2})$$

and proposes that the higher  $s$ , the closer  $d^*$  is to  $d_C$ .



# V. Conclusion

## 1. Summary

The aim of this thesis is to provide novel and holistic evidence on why managers pursue CSR. To achieve this aim, this thesis has three objectives. The first objective is to provide more nuanced and truly causal evidence on managers' agency motives (i.e., managerial preferences) in terms of CSR investments. The second objective is to provide more nuanced evidence on managers' financial motives (i.e., value relevance) in terms of CSR reporting. The third objective is to provide novel evidence on managers' institutional motives in terms of credible CSR reporting (i.e., CSR assurance).

The first study of this thesis (Chapter II) addresses the first objective. Prior studies investigate whether CSR *per se* reflects an agency problem (Surroca and Tribo, 2008; Masulis and Reza, 2015; Ferrell et al., 2016). I provide more nuanced evidence by decomposing firms' *overall* CSR investments into CSR over-investments, CSR under-investments, CSR mis-investments, and risky CSR investments. Furthermore, prior studies use traditional agency proxies to measure agency problems (e.g., ownership structures), which do not allow to provide causal evidence (i.e., solve the endogeneity problem) (Surroca and Tribo, 2008; Barnea and Rubin, 2010; Borghesi et al., 2014). To provide truly causal evidence, I use the staggered adoption of universal demand (UD) laws across US states as a natural experiment. UD laws exogenously increase agency problems by insulating managers from shareholder litigation. Using 23,190 firm-year observations, I find that, after managers are insulated by the adoption of a UD law, they both mis-invest in immaterial CSR and over-invest in material CSR. This finding suggests that managers have an underlying preference for *building a social empire*. At the same time, it rules out alternative managerial preferences (i.e., *supporting CSR pet projects, enjoying the quiet life, and playing it safe*). Further tests corroborate

the empire building interpretation: only managers who have greater ex ante incentives to empire build—for instance, when free cash flow is abundant—react to a UD law being adopted. Auxiliary analysis shows that, after a UD law is adopted, managers issue CSR press releases more frequently, and these press releases have a more positive tone. This finding suggest that managers want the general public to notice their social empire. Collectively, this study provides evidence that US managers build social empires to satisfy their hunger for power, status, fame, and prestige.

The second study (Chapter III) addresses the second objective. Prior empirical studies show that the value relevance of CSR reporting is unclear, concluding that this is likely due to the opposing value relevance of CSR reports for different types of firms (Haddock-Fraser and Fraser, 2008; Margolis et al., 2011; Gietl et al., 2013). I provide nuanced evidence by examining whether customer profile differences explain the opposing value relevance effects of CSR reporting. Using a sample of US listed firms, I show that the value relevance of CSR reporting is affected by customer profile differences. In particular, only firms that address end-consumers (so called B2C firms) and have a lower profitability level experience an increase in financial performance after the issuance of a CSR report (i.e., the value relevance is positive). For firms that address other businesses (so called B2B firms), issuing a CSR report is detrimental to the financial performance (i.e., the value relevance is negative). This finding suggests that only managers of less profitable B2C firms have a financial motive to issue a CSR report.

The third study (Chapter IV) addresses the third objective. Prior studies show that legal institutions (e.g., legal origin) well explain managers' decisions regarding the credibility of CSR reports (Simnett et al., 2009; De Beelde and Tuybens, 2015; Zhou et al., 2016). I provide novel evidence by exploring the role of culture—in particular, cultural rule orientation. Cultural rule orientation can be defined as people's proclivity for adhering to laws, rules, and regulations (Venaik and Brewer, 2010). Using an international sample, I show that managers of firms located in countries with a stronger rule orientation culture decide to issue more credible CSR reports. In particular, they are more likely to decide to receive assurance on the firm's CSR report, to receive assurance from an accounting firm, to receive assurance in accordance with an assurance standard, and to receive assurance on the firm's *entire* CSR report. Path analysis shows that the *direct* effect of cultural rule orientation is much stronger than its *indirect* effect via legal institutions. Specifically, the indirect effect, at most, accounts for 23% of the total effect, while the direct effect,

at least, accounts for 77% of the total effect. The dominance of the direct effect is confirmed in a qualitative comparative analysis. In essence, the third study shows that cultural rule orientation pushes managers to issue more credible CSR reports, much more than legal institutions do.

Overall, this thesis provides novel and holistic evidence on managers' motives to pursue CSR by focusing on agency motives, financial motives, institutional motives, CSR investments, CSR reporting, and CSR assurance.

## 2. Implications

The first study (Chapter II) maps out managerial preferences. I find that managers have an underlying preference for *building a social empire*—that is, mis-investing in immaterial CSR and over-investing in material CSR. This finding has important practical implications for designing effective firm-level corporate governance mechanisms, such as CSR contracts.<sup>1</sup> An effectively designed CSR contract either discourages empire-building managers from excessively investing in CSR, prevents self-serving managers from investing in their own CSR pet projects, motivates lazy managers to invest in fundamental CSR projects, or encourages risk-averse managers to invest in risky CSR. While extremely valuable if well designed, CSR contracts that fail to antagonize managers' underlying preferences can exacerbate, rather than mitigate, agency problems. By knowing that managers have an underlying preference for building a social empire, CSR contracts can be effectively designed to countervail such behavior.

The second study (Chapter III) examines whether customer profile differences affect the value relevance of CSR reporting. I find that issuing a CSR report is financially beneficial for less profitable B2C firms, but financially detrimental for (profitable) B2B firms. This finding has important practical implications. Above all, by showing that CSR reporting is not beneficial *per se*, I call attention to the inherent traps and pitfalls of CSR reporting. When firms do not address end-consumers, my findings show that issuing a CSR report is actually financially harmful. Hence, it is essential for managers to consider customer profile differences when determining their CSR reporting strategy. In particular, managers of B2B firms may rethink their strategic decisions of issuing a CSR report, especially in cases of lower levels of profitability. In contrast, my findings show

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<sup>1</sup> CSR contracts link managers' compensation to certain CSR criteria. By 2013, almost 40% of the S&P 500 firms had adopted some sort of CSR contracting (Flammer et al., 2016).

that managers of less profitable B2C firms are financially motivated to commit to CSR reporting. Taken together, I offer managers fine-grained guidance for value relevant CSR reporting and stresses that CSR departments and chief sustainability officers play an important role in helping managers to pursue a CSR strategy that maximizes financial performance.

The third study (Chapter IV) investigates whether and how cultural rule orientation affects the credibility of CSR reports. I find that cultural rule orientation has a weak indirect effect (via legal institutions) and a strong direct effect on the credibility of CSR reports. This finding has important policy implications. It encourages (European) supra-national and national regulators to consider the rule-oriented culture of a country when making decisions, because how corporate decisions are made and how regulations are perceived—such as the EU CSR reporting directive—appear to be driven by cultural rule orientation. Specifically, regulators of countries with a weak cultural rule orientation might implement stricter (mandatory) regulations in order to ensure compliance, while regulators of countries with strong cultural rule orientation might opt for voluntary guidelines or less strict regulations with detailed instructions. Overall, this study suggests that supra-national policy makers and regulators should not only implement strict (mandatory) regulations in order to force countries with weak cultural rule orientation to comply rigorously with the preferred behavior—such as receiving assurance on a CSR report—but also spell out regulations in great detail to satisfy the demand for guidance required by countries with strong rule orientation culture.

### 3. Limitations

The major limitation of the first study (Chapter II) is the measurement of CSR investments. To measure CSR investments, I use the most comprehensive CSR investment data available to date: MSCI ESG STATS (previously known as KLD). Since these data are based on a rating process, it is possible that they are subjectively influenced by rating analysts or the rating process itself (Dilly and Mählmann, 2015; Krüger, 2015). Hence, measurement error is a concern. Although, I confirm the robustness of the MSCI ESG STATS data by using corporate donations and environmental innovation data, I am not able to *fully* rule out a measurement error.

The major limitation of the second study (Chapter III) is causality. The issuance of a CSR report is a corporate decision and, therefore, not random. On this account, it might be possible

that not the issuance of a CSR report causes an increase in financial performance, but rather a high financial performance causes managers' to issue a CSR report. Although I control for firm fixed effects, use lagged explanatory variables, apply a Heckman-type correction, and employ regression discontinuities, I think that I am not able to *fully* rule out reverse causality concerns—rather, I would say that I am able to mitigate severe reverse causality concerns.

The major limitation of the third study (Chapter IV) is omitted variables. It is possible that countries that differ in terms of cultural rule orientation also differ on other unobservable factors. Therefore, my results might simply capture the effect of those other factors, rather than the cultural effect itself. As with reverse causality in the second study, I employ several approaches (e.g., instrumental variable approach) to confidently mitigate this concern. However, I cannot *fully* rule out an omitted variable bias.

## 4. Avenues for future research

The findings of my thesis open avenues for future research. The first study (Chapter II) exploits the staggered adoption of UD laws across the US to identify exogenous variation in agency problems. Future studies might use the staggered adoption of these laws to identify the effect of agency problems on other corporate actions, such as earnings management, cash holdings, or compensation. Furthermore, I decompose firms' *overall* CSR investments into material, immaterial, risky, and non-risky. Future research might use these decompositions to answer other questions apart from managerial preferences (i.e., managers' agency motives).

The second study (Chapter II) shows that customer profile difference—that is, the difference between B2C and B2B firms—affects the value relevance of CSR reporting practices. As the number of CSR reports increases, future studies could investigate more differentiated customer profiles (e.g., goods and services in addition to B2C and B2B). Furthermore, the political debate about mandatory CSR reporting in the US may allow future studies to better address endogeneity issues by exploiting the (staggered) adoption of laws (that mandate CSR reporting) as a natural experiment.

In the third study (Chapter IV), I show that cultural rule orientation—a novel cultural construct—has a weak indirect effect (via legal institutions) and a strong direct effect on credible CSR reporting. I measure cultural rule orientation with a traditional survey-based measure but also with a big-data-

based measure. Future studies might push the literature forward by using these novel measures and apply it to other research questions and fields (e.g., earnings management). Moreover, I use several corporate decisions to determine the credibility of CSR reports (e.g., whether firms seek assurance in accordance with an assurance standard). Future research might use this broader array of corporate decisions to answer research questions related to the credibility of CSR reports. Eventually, I exploit the adoption of the EU CSR reporting directive as a natural experiment. Future research might use this directive to examine its real effects (e.g., pollution reduction) or financial effects (e.g., liquidity).

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# Declaration

This thesis is submitted in fulfillment of the requirements outlined in the co-tutelle agreement between Macquarie University (Australia) and Katholische Universität Eichstätt-Ingolstadt (Catholic University Eichstaett-Ingolstadt, Germany). The work presented in this thesis is my own original work and has not been submitted for a higher degree to any university or institution other than Macquarie University and Katholische Universität Eichstätt-Ingolstadt. The sources of information used and the extent to which the work of others has been utilized is acknowledged according to appropriate academic conventions. I am the main author of all co-authored chapters (studies) included in this thesis. Undertaking this thesis has not involved any human subjects.

Tobias Steindl