Journal of Accounting Research

DOI: 10.1111/1475-679X.12568
Journal of Accounting Research
Vol. No. xxxx 2024
Printed in U.S.A.

CHICAGO BOOTH

Financial Transparency of Private Firms: Evidence from a Randomized Field Experiment

JOACHIM GASSEN* AND MAXIMILIAN MUHN†

Received 3 February 2023; accepted 9 January 2024

ABSTRACT

This paper examines why private firms choose to be financially transparent or opaque by conducting a field experiment with more than 25,000 firms in

*Humboldt University of Berlin; †University of Chicago, Booth School of Business

Accepted by Rodrigo Verdi. This paper is based on the first chapter of Maximilian Muhn's dissertation at Humboldt University of Berlin. We appreciate the helpful comments of two anonymous reviewers, John Barrios, Tobias Berg, Phil Berger, Matthias Breuer, Ulf Brüggemann, Rico Chaskel, Hans Christensen, Raphael Duguay, João Granja, Raji Jayaraman, Anya Kleymenova, Sarah Kröchert, Wayne Landsman, Laurence van Lent, Christian Leuz, Mike Minnis, Maximilian Mueller, Martin Nienhaus, Thomas Rauter, Cathy Shakespeare, Thorsten Sellhorn, Nemit Shroff, Christian Traxler, Steven Vanhaverbeke, Nastia Zakolyukina, and participants at the, BDPEMS Brown Bag, BERA Applied Economics 2018, BI Business School, Columbia University, COMPIE 2018, EAA Annual Congress 2019, EAA Doctoral Colloquium 2018, Emerging Scholars in Accounting Conference 2018 (Frankfurt School of Finance), TRR 266 Annual Conference 2023, Harvard Business School, IESE Business School, INSEAD, Lancaster University, Mannheim University, Stanford University, University of Amsterdam, University of Bern and Fribourg, University of Chicago, University of San Diego, UT Dallas, WHU International Accounting Research Workshop, WZB Field Days 2016, and Yale Recruiting Conference 2019. Tom Barry and Irene Tan provided excellent research assistance. We are grateful to Mike Minnis and Nemit Shroff as well as the German Business Panel (in particular Jannis Bischof, Davud Rostam-Afschar, and Samhitha Srinivas) for providing us with additional survey data. Joachim Gassen acknowledges financial support from the German Research Foundation (Deutsche Forschungsgemeinschaft – DFG): project ID 403041268 – TRR 266. Ethical approval for this project was granted at the School of Business and Economics, Humboldt University of Berlin. We have no conflicts of interest to declare. An online appendix to this paper can be downloaded at https://www.chicagobooth.edu/jar-online-supplements.

Germany. We inform a randomly chosen set of firms about a disclosure option that allows eligible firms to restrict access to their otherwise publicly available financial statements. We also vary the messaging in subtle ways to induce experimental variation in the probability that firms take transacting (capital providers or customers and suppliers) versus non-transacting stakeholders (competitors or general interest parties) into consideration when making their filing decision. Based on each firm's actual filing decision, we find that treated firms are 15% more likely to restrict access to their financial statements. This intention-to-treat effect is persistent and concentrated among firms that should derive lower net benefits from disclosure (smaller, more mature firms in less capital-intensive industries). These findings indicate that informational constraints affect firms' disclosure practice. Additionally, we show that the treatment effect is almost 40% larger for firms that have a higher, exogenously induced, probability of considering non-transacting stakeholders when making their disclosure decision. By analyzing subsequent firm activity and complementary survey evidence, we also provide suggestive evidence that disclosure requirements put an undue burden on very small private firms.

JEL codes: G30, G32, G38, K22, K23, M41, M48

Keywords: disclosure; financial transparency; field experiment; private firms; informational costs; transacting stakeholders; competition; privacy; capital providers; customers; suppliers; information processing; informational constraints

1. Introduction

Private firms are of central importance to most economies, contributing significantly to investment, employment, and market competition (e.g., Zetlin-Jones and Shourideh [2017], Bourveau, Breuer, and Muhn [2022], Kalemli-Ozcan et al. [2023]). Reflecting their collective importance, many regulatory bodies around the world require private firms with limited liability to prepare and disclose financial statements (Minnis and Shroff [2017]). Although size-based regulations reduce what and how much disclosure is mandated, the most basic disclosure (and sometimes audit) requirements typically still apply to the smallest private firms in the economy. In Europe, millions of so-called "micro entities" have been required to annually compile and file their financial statements (European Commission [2021]). These data, which are subject to fairly stringent regulatory oversight with significant penalties, are then disseminated to the public through business registers and information intermediaries. Yet, despite these far-reaching requirements, there is limited evidence on a number of important economic questions for these firms: To what extent do (small) private firms perceive required financial disclosures as beneficial or harmful? How do these firms make their disclosure decisions and which stakeholders—if any—do they take into consideration? Are there real effects of their disclosure decisions and do these requirements put an unfairly high burden on the smallest firms in the economy?

When studying these and related disclosure questions, prior literature often implicitly assumes that private firms optimally weigh benefits against costs when externally communicating with their stakeholders (e.g., Allee and Yohn [2009], Cassar [2009], Bernard [2016]). Voluntarily providing or withholding information is interpreted as (revealed preference) evidence that firms benefit or not from this disclosure. However, building on recent advances in the economic and finance literature (e.g., Bloom et al. [2013], Graham et al. [2017]), it seems plausible that a key factor in the decision-making process of private firms is that they often face significant informational frictions when making their disclosure decisions.[1] For instance, private firms might not be fully aware which stakeholders are using their disclosures, the impact of this usage on firm value or even which specific disclosure regulations apply to them. It seems particularly plausible that informational frictions play an important role for public disclosures of (small) private firms as they lack the continuous stock market feedback of public firms. Thus, to examine the research questions raised above, we adopt a firm-level information processing framework and study a deregulation setting in Germany. This setting allows us to causally estimate the relevance of informational frictions in small private firms' disclosure decisions and the role of stakeholders in the corresponding cost-benefit tradeoff.

We conduct our field experiment with more than 25,000 private firms in Germany. Since the end of 2012, small private firms in Germany have been allowed to restrict access to their otherwise publicly available financial statements.[2] We inform a randomly chosen set of eligible firms, which still publicly disclosed their 2014 financial statements about the possibility to exercise this disclosure option. We administer the information treatment indirectly by sending treated firms an email asking them to participate in a survey about their 2014 disclosure decision just before the 2015 annual filing season. In addition, we vary the messaging of these emails in subtle ways to induce experimental variation in the probability that firms think about *transacting* (capital providers or customers and suppliers) versus *non-transacting* stakeholders (competitors or general interest parties) when making their disclosure decision. We then study the outcome of both

[1] Throughout the paper, we are using "informational frictions" and "informational constraints" interchangeably. These types of frictions give rise to firm-level information processing costs, a framework which we more formally introduce in section 3.

[2] Between 2007 and 2012, all limited liability firms in Germany had been required to publicly file their financial statements. Starting with the 2012 financial statements, small private firms ("micro entities") were allowed to restrict access to their financial statements. There were three main consequences of exercising this option at the time of the experiment: (i) Financial statements are no longer publicly listed and published at the *Bundesanzeiger* [Federal Gazette]. Instead, interested users (ii) have to register at the *Unternehmensregister* [Business Register], and (iii) pay a small fee of about $5.50 for each requested financial statement. Ultimately, restricting access makes financial statements less visible (due to i) and accessing them more costly (due to ii and iii). We discuss the institutional setting in more detail in section 2.

treatment dimensions using administrative data uncovering each firm's actual disclosure decision weeks and months after the intervention.

Our first set of analyses focuses on the overall effect of the information treatment. Due to randomization, we can isolate the causal effect on the disclosure choice of firms. If private firms have negative net benefits from public disclosure *and* are unaware of the disclosure option, we expect that treated firms are more likely to restrict access to their financial statements. Indeed, we find that the causal effect of the information treatment is large, long lasting, and pervasive. Relative to the 26.2% baseline rate in the control group, treated firms are 15% more likely (3.94 percentage points) to restrict access to their 2015 financial statements. In additional tests with 2016 data, we show that this intention-to-treat (ITT) effect of the one-time intervention persists beyond the first year, which is substantially longer than most other one-time information interventions (e.g. Cavallo, Cruces, and Perez-Truglia [2017], Coibion, Gorodnichenko, and Kumar [2018]).[3] This large effect suggests that informational constraints are a meaningful factor in private firms' disclosure decisions.[4] Evidence on the role of information constraints in firms' decision making is important for regulators, firms, and academics because it indicates that firms might optimize their disclosure behavior under only imperfect information. We also indirectly show that private net benefits of public disclosure are negative (absent information processing costs) for a substantial fraction of private firms that seem to voluntarily disclose their financial statements.

Having established our main treatment effect, we next examine whether the effect is heterogeneous. Specifically, the effect of the information treatment should be larger for firms that have more negative net benefits of public disclosure. Consistent with this prediction, the treatment effect is more pronounced for smaller and more mature firms as well as for firms which are operating in less capital-intensive industries. These types of firms, potentially also because of existing relationship-based lending arrangements, rely on less external arms-length financing and, hence, should derive lower benefits from public disclosure (e.g., Cassar [2009], Cassar, Ittner, and Cavalluzzo [2015]). Furthermore, treated firms are more likely to restrict access if a higher proportion of firms in the same industry had already restricted access to their financial statements *before* the experiment. Assuming that net benefits of disclosure are positively correlated within an industry, this finding suggests that firms trade off potential costs and benefits of

[3] Similar to other field experiments (e.g., Bloom et al. [2013], Perez-Truglia and Cruces [2017]), our treatment effects correspond to ITT effects. The ITT approach, which measures the treatment effect conditional on treatment assignment, is conservative (as only a subset of the firms will read our email) but yields unbiased estimates. In section 4.1, we explore the TOT effect based on common rule-of-thumb rules of reading rates.

[4] This conclusion is supported by our supplementary survey evidence. We find that the treatment effect is concentrated among firms that indicate that they were unaware about the disclosure option before the intervention.

public disclosure and are not simply nudged toward a certain decision. Alternatively, this finding is consistent with firms learning from their peers' past disclosure choices after being informed about the availability of the restriction option.

Our second set of analyses directly addresses the relative importance of different stakeholders for the disclosure decision. Prior literature typically frames the related tradeoff in terms of capital market benefits vis-à-vis proprietary costs (e.g., Wagenhofer [1990], Hayes and Lundholm [1996], Ellis, Fee, and Thomas Shawn [2012]). We take a broader stance by considering stakeholders beyond capital providers (i.e., transacting stakeholders) and competitors (i.e., non-transacting stakeholders). More to the point, public disclosure of financial information reduces information asymmetry between firm insiders and all outsiders. Reduced information asymmetry results in lower agency costs in corporate transactions, ultimately benefiting both the firm and its transactional stakeholders (e.g., Verrecchia [1983], Diamond and Verrecchia [1991]). Therefore, benefits of public disclosure are increasing in the number of *transacting* stakeholders (Breuer, Hombach, and Müller [2023]). Transacting stakeholders not only include capital providers, but also other business partners such as customers or suppliers. On the other hand, *non-transacting* stakeholders should, if anything, negatively affect the firm when they could use publicly available financial statements. Therefore, we predict that firms will more strongly respond to the information treatment if they have a higher, exogenously induced, probability of taking non-transacting (instead of transacting) stakeholders into account when making the disclosure decision.[5]

We assign treated firms to one of four different subtreatment groups (two subtreatment groups per treatment arm). The non-transacting treatment arm consists of (I) competitors and (II) general interest parties and the transacting treatment arm comprises (III) capital providers and (IV) customer and suppliers. Each treated firm receives an almost identical email that only differs in terms of the referenced stakeholder group (I–IV). This specific stakeholder group is mentioned as a potential user of firms' publicly available financial statements three times throughout the email. Using this strategy, we document systematic variation in the impact of different stakeholders on the financial transparency decision of private firms. The treatment effect is 40% (1.29 percentage points) larger for firms in the non-transacting stakeholder treatment arm compared to firms in the transacting stakeholder treatment arm. This result confirms our expectation. It is also consistent with our supplemental survey evidence, which shows that transacting stakeholders are viewed as relatively more beneficial

[5] In that sense, this part of the paper is related to the field experiment by Heinrichs [2014]. She examines whether firms are more willing to respond to information requests by management consulting or investment firms, who both have different costs and benefits associated with them. We focus on a more diverse set of stakeholders and the effect on an actual public disclosure decision (compared to the willingness to engage in a private conversation).

financial statement users compared to non-transacting stakeholders. We find suggestive evidence that competition may well be the most important reason for firms' reluctance to publicly disclose financial statements in our setting. In addition, we document that general interest parties, a stakeholder group largely ignored by prior literature on voluntary disclosure, seems to be nearly as important as competitors. On the other hand, coupled with our survey evidence, we find that informing capital providers about the financial situation of the firm is arguably the main reason for the decision of some firms to publicly disclose financial statements.

Although our results suggest that private firms in our sample have negative net benefits of disclosure, it is not clear whether there are relevant real effects from this disclosure decision. To shed some light on this issue, we collect data on the activity status of websites of more than 22,000 firms with available website data using the *Wayback* machine from Archive.org (e.g., Bourveau, Boulland, and Breuer [2021], Bourveau, Breuer, and Muhn [2022]). We code firms as operational if they still have a valid website copy in the *Wayback* machine in 2022 or thereafter. Using these data, we provide evidence that treatment firms are significantly more likely to be active years after the experiment. As a key difference between treatment and control firms is their subsequent restriction decision, this evidence is consistent with mandatory public disclosure putting a burden on small private firms (e.g., via product market predation as in Bernard [2016] or other real effects explanations).

Finally, as we are also interested to understand the disclosure decisions of private firms in general, we run a series of analyses to assess the generalizability of our small firm sample results to the larger population of private firms (Al-Ubaydli and List [2015], Floyd and List [2016]). Specifically, we conduct a customized survey with professionals using the *Dynata* business panel in Germany. We find that microentities have somewhat more negative net benefits of disclosures, but the differences are less extreme than one might think. Additionally, we directly probe the differences in informational constraints with a series of questions. We only find limited evidence that somewhat larger private firms have significantly fewer informational constraints than our sample firms, supporting the evidence that the findings are likely to be at least locally generalizable to the wider population of private firms. We then corroborate these findings by analyzing survey data from Minnis and Shroff [2017] on firms' perceived net benefits as well as answers on private firms' perceived uncertainty about these net benefit assessments via the German Business Panel (Bischof et al. [2024]) conditional on firm size. Collectively, we interpret our findings to be locally generalizable to other small private firms and to be at least somewhat informative about medium-sized and larger private firms as well.

Our paper makes four contributions to the literature. First, our results highlight that informational constraints and processing costs can have a significant effect on the disclosure decision of private firms. In that sense, our study contributes to the recent economics and finance literature suggesting that firms might not follow best practices due to informational frictions (e.g., Bloom et al. [2013], Atkin et al. [2017], Graham et al. [2017], Bruhn, Karlan, and Schoar [2018], Zwick [2021], Bernstein et al. [2023]). For example, Bloom et al. [2013] use a randomized field experiment to show that Indian manufacturing firms substantially improve their operations by adopting certain managerial practices.[6] Connecting survey data with firms' financial data, Graham et al. [2017] show that public firms often use the wrong tax rate, which is correlated with suboptimal leverage and investment decisions. Zwick [2021] provides evidence that more than 60% of all eligible corporations fail to fully claim refunds for tax losses and that the proficiency level of tax preparers influences this phenomenon. Contemporaneously, Bernstein et al. [2023] show in a survey experiment that small private firms in the United States are underinformed about the bankruptcy code, which makes them less likely to consider Chapter 11 as a potential bankruptcy option. We complement the studies by demonstrating that informational frictions have an important effect on private firms' disclosure decisions. Although there is ample evidence in the accounting literature on information processing costs for financial statement *users* (e.g., Blankespoor, deHaan, and Marinovic [2020] for an overview), there is little work on informational processing costs of financial statement *preparers* as an economically relevant determinant for their disclosure decision.[7] We contribute to the disclosure literature by developing a framework for firms' disclosure processing costs and providing systematic (causal) evidence on behavioral factors in firms' decision making (e.g., Graham [2022]). Methodologically, our study adds to the growing literature on information experiments (Haaland, Roth, and Wohlfart [2023]).

[6] They conclude that companies do not adopt beneficial measures due to informational constraints and that competitive pressures are insufficient to force these firms out of the market. Atkin et al.'s [2017] field experiment shows that these information problems may arise because of different incentives within an organization. A firm might not adopt a more efficient technology if the information is not passed on to the person that would derive the net benefits from the technology. Their finding is consistent with our results. For example, based on our supplementary survey, we find that the effect of the information treatment is substantially larger for firms when only agents (accountants or tax advisors), but not the owner-manager, were involved in the filing decision. Some anecdotal evidence suggest that tax advisors did not inform firms about the disclosure option to avoid providing "unpaid" consulting services.

[7] In fact, it seems plausible that informational frictions are even more severe in the financial reporting setting. In contrast to operational decisions, disclosure choices are unlikely to have a first order impact on firm value (Zimmerman [2013]). In turn, managers should invest fewer resources to inform themselves about disclosure technology and about net benefits of different disclosure strategies. Although disclosure regulation is typically justified on the basis of externalities or market-wide effects (Leuz [2010], Leuz and Wysocki [2016], Breuer [2021]), one implication of our study is that regulatory intervention might be useful to overcome firm-level informational constraints. Thus, our paper provides a possible explanation why firms improve their operations following a regulatory change as documented by prior literature (e.g., Shroff [2017] for a series of GAAP changes).

Second, and directly related, our paper provides a microfoundation on how new disclosure equilibria arise. Across different settings, prior literature typically documents a time trend in the adoption of new accounting methods and a steady convergence toward a new equilibrium after a regulatory change or disclosure innovation (e.g., Kausar, Shroff, and White [2016], Berger, Choi, and Tomar [2023], Bourveau, Breuer, and Stoumbos [2023]).[8] Our paper argues that firm-level information constraints significantly explain the uptake of such new methods and that learning could explain why new disclosure practices steadily evolve over time.

Third, our paper contributes to the literature that examines why private firms disclose financial statement information and to what extent the relative importance of different stakeholders affects this decision (e.g., Ball and Shivakumar [2005], Burgstahler, Hail, and Leuz [2006], Allee and Yohn [2009], Dedman and Lennox [2009], Cassar, Ittner, and Cavalluzzo [2015], Bernard [2016], Minnis and Shroff [2017], Breuer, Hombach, and Müller [2022], Baik, Berfeld, and Verdi [2023], Breuer, Hombach, and Müller [2023]). For example, Allee and Yohn [2009] discuss and cross-sectionally test a variety of different motives for the use of financial statement information by private U.S. firms. Using survey data, Dedman and Lennox [2009] find that firm-level competition is negatively associated with firms' voluntary disclosure behavior. Breuer, Hombach, and Müller [2023] theoretically posit and then empirically show that larger demand for financial statement information translates into firms providing more voluntary information. Our paper complements and extends these studies. To the best of our knowledge, our paper is the first study that induces true random variation in the public disclosure behavior of private firms. Using a field experiment, we provide causal evidence that non-transacting stakeholders are arguably the main reason why private firms choose to be financially opaque. Due to our research design, we are also able to explore the role of nontraditional stakeholders (general interest parties) in a private firm's disclosure decision.

Fourth, we contribute to the accounting literature by examining an important but underexamined group of firms: very small ("micro") private firms. Although the economic and entrepreneurial literature has recognized the importance of this group of firms for investment and growth (e.g., Lawless [2014], McKenzie [2017], Bernstein et al. [2023]), there is scant evidence on them in the accounting literature (but see Beuselinck et al. [2023] for an overview). This lack of systematic evidence in the accounting literature is particularly notable as these small firms, due to their sheer numbers, are an important constituency. Our results tentatively suggest that current disclosure mandates in Europe have been excessive for microfirms.

[8] In particular, Bourveau et al. [2023] directly examine the steady unravelling in voluntary disclosures for the streetcar industry in the 1890s. They estimate a structural disclosure model featuring level-k thinking to explain the disclosure convergence (or "learning") over time.

2. Institutional Setting and Context

With the passage of the law *EHUG* and the related change in the enforcement system in late 2006, almost all private limited liability firms in Germany had been required to file their annual financial statements in the electronic Federal Gazette (e.g., Bernard [2016], Bernard, Burgstahler, and Kaya [2018]). At a minimum, these annual financial statement filings comprise a comparative balance sheet and abbreviated notes. As such, they convey standardized information about firms' solvency, capital structure, and profitability.[9] Notably, the capital structure and, to a slightly lesser degree, profitability are the most critical pieces of information that competitors can generally glean from private firms' financial statements (e.g., Bernard [2016], Minnis and Shroff [2017], Bernard, Kaya, and Wertz [2021]). As of 2012, financial statements of firms were accessed 42 million times in a given year, with over 80% of these views pertaining to small private firms (Bundesanzeiger [2012]).

Following initiatives to reduce the administrative burden on small firms in Europe, the EU Directive 2012/6/EU allowed member states to exempt small corporations ("micro entities") from certain disclosure provisions. Germany transposed this directive into law ("MicroBilG," *Kleinstkapitalgesellschaften-Bilanzrechtsänderungsgesetz*) in December 2012. Consequently, small private corporations in Germany face lower mandatory disclosure requirements for fiscal years ending on December 30, 2012, or thereafter: Besides reduced disclosure provisions (e.g., fewer notes and balance sheet items), eligible corporations are allowed to restrict access to their financial statements ("*hinterlegen*" [deposit]) instead of publicly disclosing them. These restricted financial statements are no longer listed in the easily accessible electronic Federal Gazette. Instead, they can only be requested after registering at the Company Register and by paying a small fee of about $5.50 for every report.

How economically meaningful are these restrictions? In essence, restricted financial statements are no longer readily and freely available for all market participants. Beyond the direct costs (registration and fee payment), less sophisticated users might also believe that restricted financial statements have not been published yet or that they are not available at all. Unlike unrestricted financial statements, restricted ones are not listed or even referenced in the Federal Gazette, which is the official and most commonly used publication outlet similar to the SEC's EDGAR website (Breuer, Hombach, and Müller [2018]).[10] The Company Register, which contains restricted financial statements, is a less prominent source to search

[9] Online appendix A describes the disclosure requirements for small firms in Germany in more detail (e.g., contains an example for a typical firm disclosure in figure A1).

[10] In the United States, even sophisticated investors were unaware of the existence of disclosed Form 144 filings because they were not listed in the SEC's EDGAR database (Lynch [2022]).

for financial statement information.[11] Even within the Company Register, the availability of restricted financial statements is not directly displayed on the main page after searching for a firm. Furthermore, more sophisticated users should find it more difficult to use restricted financial statement information even after ignoring the direct costs. Financial databases as well as other information intermediaries do no longer provide raw or preprocessed financial statement information for restricting firms. Therefore, other firms might be unable to use such standardized databases to scan for potentially vulnerable firms or for attractive markets to penetrate (e.g., Bernard [2016], Bernard, Kaya, and Wertz [2021], Breuer [2021]). Taken together, restricting access should impose economically meaningful barriers to financial statement access.[12]

Firms have to be eligible to restrict access to their financial statements. The MicroBilG exemption mainly relies on three criteria: Total assets less than or equal to €350,000 ($446,428 with 2014 exchange rates), total revenues less than or equal to €700,000 ($892,856), and an average number of up to 10 employees. Limited liability firms, which do not exceed two out of the three criteria in two consecutive years, are "micro-entities" and in principle eligible for the described disclosure exemptions (Article 267a HGB).[13] Eligible firms with a fiscal year-end of December 31 may use these exemptions for their 2012 financial statements or later.

Figure 1 shows the number of firms disclosing their financial statements between 2010 and 2014. Consistent with our argument that not all firms are immediately aware of the restriction option, we find that the restriction rate has steadily increased. Up until 2014, more than 70% of roughly 400,000 eligible firms have restricted access to their financial statements. Our experiment focuses on eligible firms that still publicly disclosed their 2014 financial statements.

As we run our experiment with firms that still disclose their 2014 financial statements, it raises the question how the findings are affected by the timing of the experiment. Although we discuss this topic in more detail in the

[11] For example, Google Trends data for the year 2016 show that "Bundesanzeiger" [Federal Gazette] has a four times higher search volume than "Unternehmensregister" [Business Register]. Three of the top five related search queries of the 'Federal Gazette -search refer to financial statement information ("annual accounts," "publication," and "balance sheet"), whereas none of the top five related search queries of the "Business Register"-search contain similar references.

[12] Bernard et al. [2018] find "size management" by European firms to avoid crossing various size thresholds that would otherwise lead to more extensive audit and disclosure requirements. This avoidance behavior is consistent with disclosure requirements imposing a significant cost on private firms in Europe. However, they do not study the threshold around the MicroBilG exemption (their sample ends in 2011) and, unfortunately, it is not feasible to conduct a similar bunching analysis for MicroBilG firms (as firms below the threshold are no longer observable in the Dafne database). Additionally, because firms in our sample are not required to publicly disclose employees and sales, firms do not need to report artificially low total assets to avoid more extensive disclosure requirements.

[13] The law includes some additional qualifiers. For example, holding companies are always ineligible for all MicroBilG exemptions.

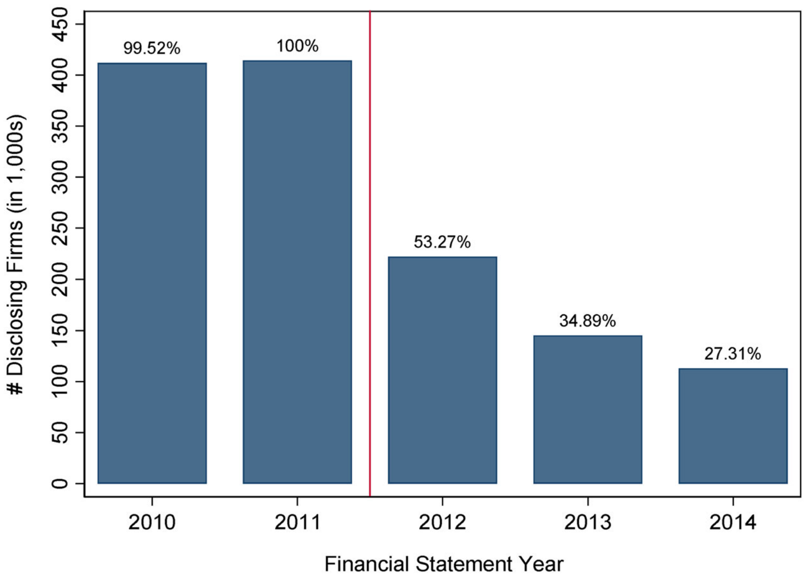


FIG. 1.—Adoption of restriction option over time. The figure shows the number of eligible limited liability firms that disclose their (unconsolidated) annual financial statements between 2010 and 2014 according to Dafne. For each year, we count the number of firms with available Dafne data that meet the size thresholds (as explained in footnote 20) and then exclude publicly listed firms as well as firms from ineligible industries (in particular, financial and holding companies). The vertical red line indicates that the restriction option was introduced for financial statements after 2011. Percentage values above the bars refer to the percentage of disclosed financial statements relative to the respective number in 2011 (i.e., the percentage in 2011 is by definition 100%). This percentage approximately equals the percentage of disclosing firms.

appendix (online appendix B), we argue that the effect would have been likely similar in magnitude or even larger if we had conducted the experiment in earlier years. The intuition is that more and more firms will become informed over time and, hence, the sample will increasingly comprise firms that knowingly disclose their financial statements. As these informed firms should be unaffected by our information treatment, the treatment effect will be naturally smaller in later years. However, the fraction of firms that become informed by other channels is also arguable larger in earlier years. As these firms should not react to our treatment, this countervailing effect implies that our estimate might be larger than the hypothetical treatment effect in the early adoption years. We calibrate a simple empirical model (table B1 and figure B1) to assess which of the two effects is likely to dominate in our setting. Based on the counterfactual treatment effects predicted by this model, we find that the experiment would have yielded similar or even stronger results before 2014.[14]

[14] To corroborate this finding, we conduct additional tests around firms' initial restriction decision from 2012 to 2014 (table B2 in the online appendix). We find that the type of firms

Our setting is appealing for several reasons. We can use a low-cost intervention with an unambiguous information treatment. The treatment also has a direct mapping into our outcome variable (restriction decision), which can be collected from a high-quality administrative database. Field experiments in economics often have to rely on self-reported outcomes from surveys (e.g., Cole, Giné, and Vickery [2017]) and prior literature in accounting typically has to rely on disclosure proxies that are rather indirectly related to the underlying cause (e.g., the characteristics of management disclosures as in Balakrishnan et al. [2014] or the length of the MD&A section as in Banerjee et al. [2023]). Furthermore, the only difference between the "disclosure" and the "restriction" regime is the accessibility of financial statement information. Under both regimes, firms still have to prepare the same set of disclosures and, thus, our results should be unrelated to other potential confounders. Finally, despite the relatively high adoption rate, we have a sufficiently large sample size of eligible firms that still publicly disclosed their 2014 financial statements.

3. *Research Design*

3.1 EXPERIMENTAL DESIGN

We conduct a randomized field experiment to address our research questions. Field experiments combine randomization inference with subjects making decisions in a realistic environment (e.g., Banerjee and Duflo [2009], Floyd and List [2016]). Panel A of figure 2 displays the timeline of our experiment. We conducted a small pilot study with around 500 treatment and control firms in December 2015. We used this setup as a feasibility test and to obtain ideas about potential effect sizes (for details on our power tests, see our online appendix C). The actual field experiment was carried out one year later just before the height of the annual filing season in December 2016.[15] We send emails to treated firms from December 13 to 15, 2016. We then collect data on firms' 2015 filing decision up until one year after the experiment (December 12, 2017). Whether a firm restricts access to their 2015 financial statements (or not) is the key data item and the outcome variable of our study.

Our experiment is related to the growing literature on information experiments, particularly those involving firms (for a recent overview, see

that were more likely to restrict access to their financial statements before 2014 are also the ones that reacted the strongest to the information treatment (e.g., smaller or more mature firms; see section 4.2). This analysis validates the calibrated model and alleviates concerns that this type of survivorship bias unduly influenced the sample or our results.

[15] Small firms with a fiscal year end of December 31 have to file their 2015 financial statements until December 31, 2016 (§325 HGB). However, fines must be only paid after the Federal Office of Justice detects the late filing violation and the firm does not react to it within six weeks. This procedure essentially gives firms more time beyond the original 12-month filing period. Most firms file their financial statements around the 'soft' deadline of December 31 (see figure 4).

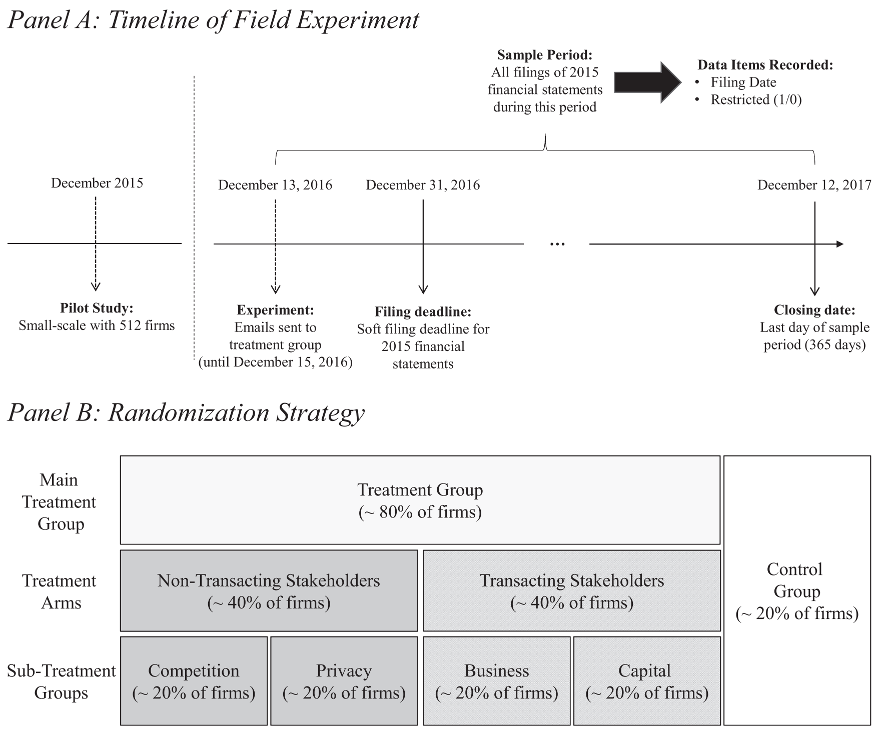


FIG. 2.—Overview of research design for field experiment. The figure illustrates the experimental design of the study and provides details on the timeline (panel A) and the randomization strategy (panel B). As indicated in panel A, we conducted a pilot study in December 2015. The fully fledged experiment was executed from December 13 to December 15, 2016. This period was chosen due to its close proximity to the soft filing deadline for the 2015 financial statements. Most firms file their 2015 financial statements shortly before or after December 31, 2016. For the empirical analyses, all 2015 financial statement filings between December 13, 2016 (beginning of experiment), and December 12, 2017 (365 days later), are considered. Two data items are recorded from the business register for each filing: the date of the filing and whether the firm restricted access to their 2015 financial statements (*Restricted* dummy). Panel B illustrates the randomization strategy based on the final sample as defined in table 1. The treatment group (20,556 firms) consist of two treatment arms. Each treatment arm contains two subtreatment groups.

Haaland, Roth, and Wohlfart [2023]). These information experiments vary the information set available to economic agents to understand how decisions are made (e.g., Kumar, Gorodnichenko, and Coibion [2023]). Such an information experiment is particularly appealing in our setting as informational constraints might be an important determinant of private firms' disclosure decisions. To illustrate this concept, we develop a framework of firm-level information processing costs based on Blankespoor et al. [2019] and Blankespoor, deHaan, and Marinovic [2020]. As shown in figure 3, firms make their disclosure decisions conditional on their awareness costs, acquisition costs, and integration costs. Our study primarily focuses on

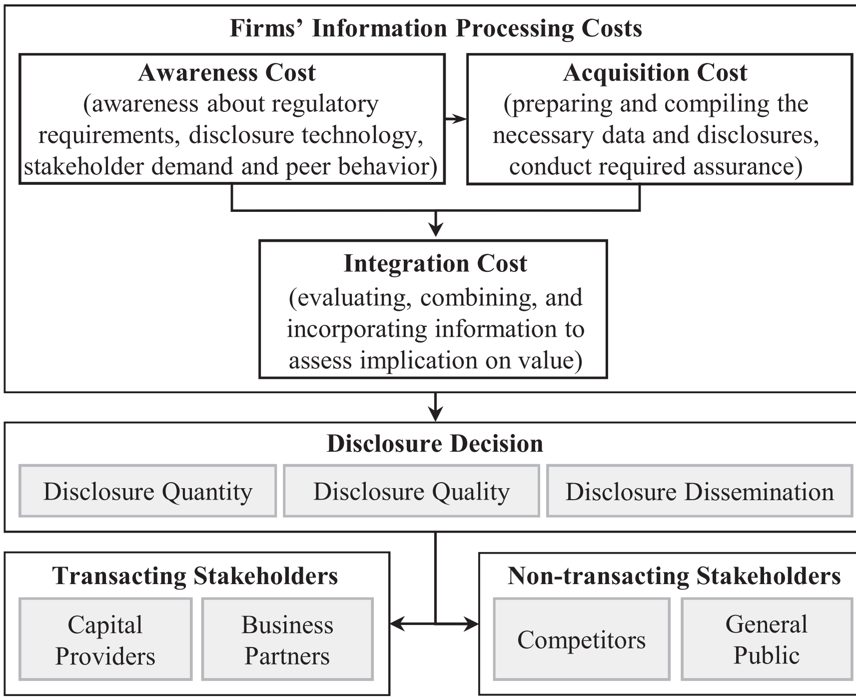


FIG. 3.—Firm's information processing costs. The figure provides a stylized overview of firms' information processing costs, adapted from users' disclosure processing costs by Blankespoor et al. [2019] and Blankespoor, deHaan, and Marinovic [2020]. Firms' information processing costs include awareness costs, acquisition costs, and integration costs.

awareness costs, which encompass the costs of being attentive to applicable disclosure requirements, tracking of stakeholder demand for information or observing the behavior of peer firms. As we explain below, our information experiment reduces awareness costs along two dimensions (regulatory requirements and potential stakeholders). On the other hand, acquisition costs include, for example, setting up the necessary measurement systems. These acquisition costs are largely fixed in our study as firms are always required to compile the same type of information irrespective of whether they choose the restriction option or not. Finally, depending on their awareness and information acquisition, firms then form expectations about the perceived net benefits of disclosure (integration costs) and ultimately decide whether or not to disclose financial statements. Overall, we believe that information processing costs might be particularly high for private firms' disclosure decisions as they lack the market feedback of public firms.

More specifically, the idea of our field experiment is twofold. First, we want to inform a random set of firms that publicly disclosed their 2014 financial statements about the restriction option. As displayed in panel B of figure 2, we assign about 80% of all firms to the treatment and the remaining firms to the control condition. If firms were unaware of the restriction option in 2014 and derive negative net benefits of public disclosure (absent information processing costs), then they should restrict access to their 2015 financial statements after receiving the information treatment. The difference in restriction rates between the treatment and the control group will be equal to the average ITT effect of our intervention.[16] We expect that the effect will be concentrated among firms that have lower net benefits of public disclosure (e.g., less financially healthy firms).

Second, we vary the framing of the information treatment to induce experimental variation in the probability that firms take certain stakeholders into consideration when making their disclosure decision. Therefore, we split the treatment group into two equally sized treatment arms (see panel B of figure 2). One treatment arm is related to potential costs, whereas the other treatment arm is related to potential benefits of disclosure. Prior literature mainly recognizes proprietary costs vis-à-vis capital market benefits as the driving force behind a firm's disclosure decision. However, we take a broader stance and consider stakeholders beyond competitors (costs) and capital providers (benefits). The first treatment arm consists of *non-transacting stakeholders*. These stakeholders do not have a direct business relationship with the firm and, thus, disclosing financial statement information should not influence transaction prices. On the contrary, these stakeholders, and in particular competitors, could use financial information to harm the firm (e.g., Verrecchia [1983], Bernard [2016]). The second treatment arm comprises stakeholders that potentially have a business relationship with the firm. We label them *transacting stakeholders* consistent with Breuer, Hombach, and Müller [2023]. Invoking a classic information asymmetry argument, these stakeholders will price-protect themselves in their transactions with the firm if they are unable to obtain financial statement information. In turn, firms will have an incentive to publicly disclose financial statement information (e.g., Milgrom [1981]). Based on these arguments, we predict that the restriction rate will be relatively higher for firms that receive the non-transacting relative to the transacting stakeholder treatment.

The concept of transacting and non-transacting stakeholders is abstract, and it is difficult to frame the treatment in terms of these high-level concepts. Therefore, we split each treatment arm into two equally sized subtreatment groups. These subtreatment groups act as concrete and representative stakeholders for the respective treatment arm. Specifically, we use (I) competitors and (II) general interest parties (e.g., neighbors) for the non-transacting stakeholder treatment. We choose competitors because they are often cited as the main reason for firms withholding information

[16] Control firms might also choose to restrict their 2015 financial statements for multiple reasons. Most importantly, they might become aware of the restriction option over the course of the year 2016 (i.e., between filing their 2014 and 2015 financial statements). A potential spillover effect from treatment to control firms (or across treatment groups) will bias our estimates downwards, which makes it more difficult to detect an effect.

(e.g., Dedman and Lennox [2009], Bernard [2016]). General interest parties, such as neighbors, are chosen as the second subtreatment group to capture nonmonetary considerations in a firm's disclosure decision. Although general interest parties do not directly influence the firm when using financial statement information, they might trigger more general privacy concerns that can be important in a private firm's decision making (and for researchers otherwise difficult to examine).

For the transacting stakeholder treatment, we use (III) capital providers (e.g., banks) as well as (IV) customers and suppliers. Capital providers seem to be the main users of private firms' financial statement information based on prior literature (e.g., Minnis [2011], Kausar, Shroff, and White [2016], Berger, Minnis, and Sutherland [2017], Breuer, Hombach, and Müller [2018]). Customer and suppliers are other business partners that might also rely on publicly available financial statement information when interacting with the firm (e.g., Minnis and Shroff [2017] or Samuels [2021] for public firms). Overall, the respective subtreatment groups should clearly and distinctly capture the higher level concepts of transacting and nontransacting financial statements. We choose two instead of a single group per treatment arm to rule out that the entire effect is driven by one particular group. We do not consider other potentially relevant stakeholders (e.g., tax authorities) or a neutral treatment arm due to power considerations.

The information treatment itself is a single email containing a survey participation request with a between-subject design. In the email, we disclose our identity, inform them that we are examining the disclosure behavior of private firms in Germany and that we have noticed that their firm has disclosed its 2014 financial statements, which are therefore publicly available without any restrictions. We then continue by referencing the firm's total assets, that these assets fall below the €350,000 threshold and, hence, that the firm was likely to be eligible for the restriction option (a link to a webpage containing further references and more information about the MicroBilG exemptions is provided as well). Next, we briefly explain the restriction option and its direct implications. We then ask the firm to participate in our online survey to understand why they disclosed their 2014 financial statements as well as the cost-benefit tradeoff that factored into this decision. The email ends with a link to an online survey and a brief explanation of it.

The email as well as the accompanying survey comes in four different versions; we label these four different versions (I) *Competition*, (II) *Privacy*, (III) *Business*, and (IV) *Capital*. The only difference is that the respective stakeholder group (I)–(IV) is mentioned three times throughout the email as an example for a potential financial statement user.[17] Below are the three statements with the bold part in square brackets indicating the different

[17] Using such subtle variation in the messaging has been frequently used in field experiments to illicit causal effects across various disciplines (e.g. Perez-Truglia and Cruces [2017], Guzman et al. [2020], Floyd et al. [2023], Kirgios et al. [2022]). Our empirical strategy is comparable even though our topic (public disclosure choice) and subject pool (private firms) is

subtreatment conditions (figure D1 in online appendix D contains the entire email for the *Capital* version):[18]

> "…Your 2014 financial statements are accessible online at the Federal Gazette without any restrictions; therefore, for example, [**(I) competitors; (II) people in your personal life (such as neighbors); (III) customers and suppliers; (IV) capital providers (such as banks)**] could access them at any time…"

> "…By only depositing the financial statements, you make it more difficult for third parties, e.g. for [**(I) competitors; (II) somebody in your private life; (III) customers and suppliers; (IV) capital providers**], to access them…"

> "…Specifically, we would like to understand your cost and benefit tradeoff and to which extent [**(I) competitors; (II) people in your personal life (e.g., neighbors); (III) customers and suppliers; (IV) capital providers (e.g., banks)**] have played a special role in this decision…"

The accompanying online survey itself consists of three sections and a total of 10 questions. We develop the survey by partially relying on prior literature (in particular, Brown et al. [2015], Minnis and Shroff [2017], Hail, Muhn, and Oesch [2021]) and revising it based on feedback from several academic colleagues. The first section contains four general questions about the 2014 disclosure decision. The second section comprises four questions, which address the relevance of a certain stakeholder group for a firm's disclosure decision. These questions only refer to the specific stakeholder group (I)–(IV) depending on the subtreatment condition. We only refer to one group to reinforce the treatment and to obtain inferences from a between-subject randomization design. The third section asks firms about their prospective 2015 decision and one demographic question. Our analyses do not, however, primarily focus on survey responses. Instead, the study mainly focuses on the 2015 restriction decision conditional on treatment assignment and irrespective of actual survey participation. But the survey responses are still useful. As explained in section 4.5, we use them as descriptive evidence to explore potential mechanisms and to complement our experimental evidence. We provide more details on the structure of the survey in online appendix D.

3.2 SAMPLE SELECTION AND DESCRIPTION

To execute our field experiment, we need to identify limited liability firms that were eligible for the MicroBilG provisions but still disclosed their 2014 financial statements. We identify these firms using the Dafne database

---

different. For an overview of these information experiments, see Haaland, Roth, and Wohlfart [2023].

[18] All emails were sent in German; these three sentences are rather literal than idiomatic translations.

by Bureau van Dijk (BvD), which is a Germany-specific and more detailed version of the Amadeus database. Among others, Dafne contains financial statements information for all German limited liability firms that have *not* restricted access to their financial statements as well as voluntary information provided by some firms. By focusing on firms that have 2014 financial statement information available in Dafne, we can restrict our sample to firms that publicly disclosed their 2014 financial statements.[19] We then identify eligible firms by applying the three size-related thresholds laid out in section 2. Unlike total assets, however, small firms often do not disclose sales and/or employee data. Thus, we need to use an approximation to identify eligible firms (e.g., see also Kausar, Shroff, and White [2016]).[20] This classification might result in some Type I or Type II errors. Type I errors are ineligible firms that still receive our information treatment. These errors will bias our treatment estimates downward, which makes it more challenging to find the stipulated effect. Type II errors are eligible firms that were not included in our study, which results in a smaller sample size. Such errors will not induce bias, but we might lose statistical power.

We apply several additional filters. For example, we remove industries that are excluded from the MicroBilG exemptions and, in particular, financial institutions. Additionally, we require that every firm has an available email address and does not belong to a larger corporate group. Because we want to limit our sample to firms that had not yet filed their 2015 financial statements at the date of the experiment, we also remove firms that had nonmissing 2015 data items in the most recent Dafne release as of December 2, 2016. We present all additional filters in panel A of table 1. After applying these filters, we obtain a provisional sample consisting of 36,923 firms.

The provisional sample is the basis of the randomization (see panel B of table 1); 7,386 firms were assigned to the control group and 29,537 firms were assigned to the treatment group. The treatment group further consists of the non-transacting (14,771 firms = 7,386 *Competition* + 7,385 *Privacy*) and the transacting treatment arm (14,766 firms = 7,382 *Business* + 7,384 *Capital*). The randomization was performed on the individual firm level,

[19] In some cases, Dafne has financial statement information of firms that restricted access to their financial statements (presumably because these firms voluntarily provided the data to BvD). As explained in the notes to table 1, we were able to identify almost all of these 'false public disclosers' (931) and discarded them before we assigned firms into treatment and control group. The remaining ones (64) were removed before conducting our analyses.

[20] First, we require that all firms have total assets below the asset threshold (€350,000). Second, if we have nonmissing data for sales or employees (but not both), we also require that the nonmissing variable does not cross the respective threshold. If we have both data items, only firms that cross at most one of the two thresholds are included in the sample. This strategy results in fewer Type I (potentially at the expense of Type II) errors than a strategy that only relies on the asset threshold. Type I error firms must (a) neither report employee nor sales data, (b) have total assets below the asset threshold, and (c) still exceed the employee *and* the sales threshold.

TABLE 1

Sample Selection and Treatment Assignment for Provisional and Final Sample

Panel A: Sample selection procedure

Data Requirements	Number of Firms
Initial sample based on BvD Dafne data in December 2016	40,402
– firms that restricted access to their 2014 financial statements based on manual checks	−931
– firms that did not pass additional filters	−2,548
Provisional Sample (used for randomization)	36,923
– firms with invalid e-mail addresses	−3,677
– firms that restricted access to their 2014 financial statements (erroneous data)	−64
– firms that published their 2015 data before the experiment (December 13, 2016)	−6,812
– firms that had not published their 2015 financial statements until December 12, 2017	−646
Final Sample (used for analyses)	25,724

Panel B: Information on treatment assignment for provisional and final sample

			Non-transacting Stakeholders		Transacting Stakeholders	
	N	*Control*	*Competition*	*Privacy*	*Business*	*Capital*
Provisional Sample (used for randomization)						
Number of obs.	36,923	7,386	7,386	7,385	7,382	7,384
			$\Sigma = 14{,}771$		$\Sigma = 14{,}766$	
Percentage (%)	100%	20.0%	40.0%		40.0%	
Final Sample (used for analyses)						
Number of obs.	25,724	5,158	5,109	5,187	5,143	5,127
			$\Sigma = 10{,}296$		$\Sigma = 10{,}270$	
Percentage (%)	100%	20.1%	40.0%		39.9%	

The table presents details on the sample selection (panel A) and statistics on the treatment assignment for the provisional and the final sample (panel B). The initial sample comprises all firms for which detailed Dafne data was initially downloaded. Specifically, the initial sample contains all limited liability firms in the Dafne database that (i) are likely to be eligible for the MicroBilG provisions, (ii) have available email addresses, (iii) based on Dafne data are assessed not to have restricted access to their 2014 financial statements, (iv) are not part of a corporate group, (v) had not yet published their 2015 financial statements, and (vi) were not mainly providing legal or accounting services. Based on the downloaded data for this initial sample, additional filters were applied. Notably, we identified 1,779 firms that had a high likelihood of being falsely recorded as disclosers in 2014. For these 1,779 firms, the 2014 filing status was manually checked in the business register and, ultimately, 931 firms were removed because these firms indeed restricted access to their 2014 financial statements. Other, more technical, filters were applied as well. For example, firms were removed when several firms shared the same email address or when they had a fiscal year-end other than December 31. We randomize firms using the provisional sample, but the analyses are carried out on the final sample (for a similar procedure see Duflo et al. [2013]). Three factors mostly explain the difference between the provisional and the final sample. First, firms with invalid email addresses are removed from the analyses to account for the fact that these firms cannot be (potentially) treated. To symmetrically remove firms from the control and treatment groups, a commercial email verification service (*NeverBounce*) was used one day after the end of the email delivery; 3,677 firms were flagged as having an invalid or disposable email address. Second, 6,812 firms had already published their 2015 financial statements before the beginning of the experiment (December 13, 2016). Third, we close our sample 365 days after the beginning of the experiment on December 12, 2017. Firms that had not filed their financial statements until then (e.g., because they became insolvent) were also removed from the analysis. Panel B contains information on the treatment assignment for the final sample. Although the final sample is 30.3% smaller, the relative proportions are comparable to the proportions in the provisional sample. A Pearson chi-squared test reveals no statistically significant differences in the likelihood of being included in the final sample across the different groups (p-value of 0.904 for the broader three treatment arms and a p-value of 0.684 for all five sub-treatment groups).

and we use stratification to ensure a minimum covariate balance along two potentially important dimensions. First, we form seven equally sized bins based on total assets (i.e., €0–€50,000; €50,001–€100,000, etc.). Stratifying on total assets is reasonable because larger firms might be generally more likely to publicly disclose financial statements (e.g., Allee and Yohn [2009]). Second, we rely on an indicator variable that captures the two most generic email addresses in our sample (info@… and kontakt@…). Emails sent to generic email addresses might be less likely to be read, which could moderate the effect of the information treatment. Taken together, we have 14 ($= 7 \times 2$) strata for our randomization.

The 29,537 randomized email messages were sent between December 13, 2016, and December 15, 2016. To study the outcome of our field experiment, we collect information on the 2015 restriction decision from the *Unternehmensregister* (Business Register). We use a web crawler to obtain two data items for each firm: (1) an indicator variable that captures whether a firm restricts access to their 2015 financial statements and (2) the filing date of the 2015 financial statements.[21] We collect these data for all firms in the provisional sample until December 12, 2017 (one year after the experiment).

Although the provisional sample is used for the randomization and for the email delivery, we use a smaller sample for our analyses (e.g., see Duflo et al. [2013]). This final sample differs in four aspects. First, we remove 3,677 firms that have invalid or disposable email addresses as these firms cannot receive our treatment. We identify these firms using a commercial email verification service one day after the experiment (December 16, 2016). By using an email verification service instead of actual bounce messages, we can symmetrically eliminate firms from both control and treatment group. Second, we remove 64 firms that actually restricted access to their 2014 financial statements and which we were only able to identify after the field experiment (but not with the Dafne data beforehand). Third, we remove 6,812 firms that filed their financial statements before the experiment. These firms are incidentally part of the provisional sample because we can only imperfectly observe firms that had already filed their 2015 financial statements at the date of the experiment via Dafne. Dafne typically lags behind firms' actual filings by two to four weeks, and it also does not flag firms that already filed but restricted access to their 2015 financial statements. Finally, we remove 646 firms that had not filed their financial statements until one year after the experiment. This group mostly comprises bankrupt or inactive firms. Taken together, the sample period covers all filings from December 13, 2016 (beginning of the experiment) to December 12, 2017 (one year later). Figure 4 displays the distribution of

[21] The web crawler uses a search strategy that relies on several firm characteristics and it perfectly matches firms from Dafne to the Business Register using a unique commercial register number. However, for 104 firms, we had to hand-collect these two data items because these firms changed their name and commercial register number in 2017.

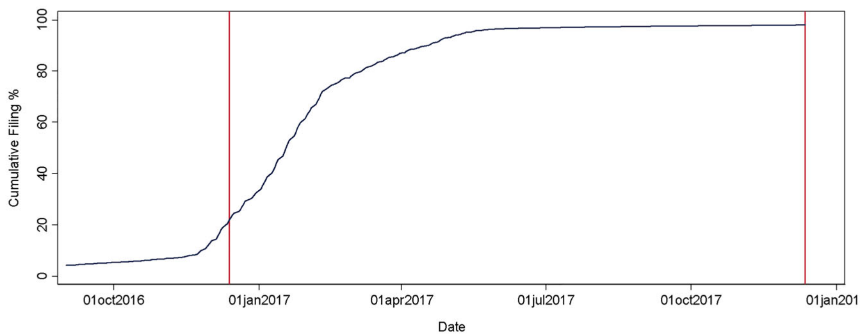


FIG. 4.—Cumulative filing percentage by date. The figure plots the cumulative percentage of firms that have filed their 2015 financial statements up until a certain date. The denominator of *Cumulative Filing %* equals the number of observations in the provisional sample of table 1 excluding firms that have invalid email addresses (3,677 firms) or erroneous data (64 firms). The first vertical red line indicates the beginning of the experiment (December 13, 2016) and the second red line the end of the sample period (December 12, 2017). At the beginning of the experiment, 20.52% of all firms (6,812) have already filed their 2015 financial statements and, hence, are excluded from subsequent analyses. At the end of the sample period, 1.95% of all firms (646) have not yet filed their 2015 financial statements and are excluded from the analyses as well.

filing dates for the 2015 financial statements and the two vertical red lines highlight our sample period. After applying all additional filters, we have 25,724 firms for our analyses.

The final sample is 30.3% smaller than the provisional sample.[22] For the validity of our inferences, it is important that we do not distort our sample composition. Conceptually, all additional filters affect control and treatment groups similarly and, thus, they should not induce any bias. Nevertheless, undesirable outcomes could potentially arise by chance (similar to randomization balancing observables and unobservables in expectation, but not necessarily in a single trial). As displayed in panel B of table 1, the relative proportions of treatment and control group for all subtreatment groups are insignificantly different between the provisional and the final sample. Due to the large sample size, these are high-powered tests and, hence, it is highly unlikely that any bias was introduced by this exercise.

Table 2 contains descriptive information on our final sample. Panel A of table 2 shows the distribution of our key variables. The unit of observation is the firm. The dependent variables are based on 2015 financial statements (i.e., measured *after* the experiment), while all control variables are based on 2014 financial statements (i.e., measured *before* the experiment). By definition, our sample contains only small firms. The median firm in our sample has total assets of €149,531 ($190,728 with 2014 exchange rates), which

[22] As a point of comparison, Duflo et al. [2013] eliminate 25.3% of all firms from their provisional sample.

TABLE 2

Descriptive Statistics and Covariate Balance

Panel A: Descriptive statistics for variables used in filing decision regressions

(N = 25,724)	Mean	Std. Dev.	P1	P25	Median	P75	P99
Dependent Variables:							
Restricted	0.294	0.455	0	0	0	1	1
Filing Lag (Days)	58.67	48.38	0	27	45	79	239
Control Variables:							
Total Assets	159,951	97,004	6,399	76,046	149,531	239,677	345,082
Firm Age (Years)	16.31	14.87	3	6	13	22	79
Tangibility	0.191	0.203	0	0.032	0.124	0.284	0.856
Equity Ratio	0.340	0.294	0	0.034	0.302	0.576	0.966

Panel B: Correlation matrix

	Restricted	*Log(Total Assets)*	*Log(Firm Age)*	*Tangibility*	*Equity Ratio*
Restricted	X				
Log(Total Assets)	–0.084***	X			
Log(Firm Age)	–0.016***	0.250***	X		
Tangibility	–0.025***	0.093***	–0.024***	X	
Equity Ratio	–0.003	–0.059***	0.016**	–0.044***	X

Panel C: Pretreatment differences in main control variables across groups

	(A) *Control* Mean [Std. Dev.]	(B) *Treatment* Mean [Std. Dev.]	(I) *Transacting* Mean [Std. Dev.]	(II) *Non-transacting* Mean [Std. Dev.]	*p*-value for differences in means [variances] (A) = (B)	*p*-value for differences in means [variances] (I) = (II)
Number of obs.	5,158	20,556	10,270	10,296		
Control Variables:						
Log(Total Assets)	11.70	11.70	11.70	11.70	0.870	0.897
	[0.903]	[0.912]	[0.914]	[0.909]	[0.399]	[0.558]
Log(Firm Age)	2.588	2.586	2.588	2.584	0.891	0.644
	[0.714]	[0.717]	[0.719]	[0.715]	[0.716]	[0.659]
Tangibility	0.192	0.191	0.193	0.190	0.874	0.337
	[0.202]	[0.203]	[0.204]	[0.201]	[0.923]	[0.134]
Equity Ratio	0.344	0.339	0.339	0.338	0.217	0.907
	[0.294]	[0.294]	[0.294]	[0.294]	[0.983]	[0.810]

The table presents descriptive statistics for the variables used in the cross-sectional filing decision regressions (panel A), Pearson correlation coefficients (panel B) and pretreatment covariate balances (panel C). *Restricted* is the proportion of firms that restricted access to their 2015 financial statements. *Filing Lag* is the difference between the filing date of the 2015 financial statements and the beginning of the experiment (December 13, 2016). All control variables are based on firms' 2014 financial statements. *Total Assets* is bounded between €0 and €350,000 due to the sample selection procedure. *Firm Age* is the difference between 2016 and the firm's founding year. For 174 firms with missing founding dates, the founding year is imputed using the date of the latest firm entry in the company register as a predictor in a quantile regression. *Tangibility* (*Equity Ratio*) is the ratio of noncurrent assets (equity) to total assets with missing values being replaced by zero. *Tangibility* and *Equity Ratio* are mildly winsorized at 0 and 1 (affects 32 firms in total). In panel B, we show pairwise correlations between the main variables and variables are logged (plus one) when indicated. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed). Panel C reports mean and standard deviations for the variables used in the regressions by treatment group and for each treatment arm. In the last two columns, the panel also reports p-values from tests for equal means and, in brackets, p-values from Bartlett's tests for equal variances. None of the tests are statistically significant at conventional levels.

is comparable to or even larger than the median firm in studies that focus on U.S. private firms using the Survey of Small Business Finances data.[23] Our median firm is 13 years old, has a tangibility ratio of 12.4%, and an equity ratio of about 30%. The average restriction rate for the 2015 financial statements is 29.4% and these financial statements are filed, on average, 59 days after the experiment. We also report a correlation matrix in panel B of table 2. Smaller, less mature and less tangible firms are more likely to restrict access to their 2015 financial statements (unconditionally). In online appendix A, we provide detailed information on the industry distribution of these firms and compare it to the U.S. industry distribution as reported in Minnis [2011]. We do not have any "finance and insurance" (NAICS code 52) or "management of companies and enterprises" (NAICS code 55) firms in our sample because these firms are not eligible for the restriction option. On the other hand, relative to the population of all German limited liability firms, our sample contains substantially more "construction" (NAICS code 23) and "professional, scientific, and technical services" (NAICS code 54) firms. The former (latter) group is less (more) dominant in our sample than in the U.S. private firm sample of Minnis [2011].

Due to our randomization strategy, treatment and control firms should have similar ex ante characteristics. In panel C of table 2, we show pretreatment mean and variance of all control variables conditional on treatment assignment. We do not find any significant differences between treatment and control group (column 5) or between both treatment arms (column 6). Specifically, mean and variance of all control variables are similar for treated and untreated firms. These tests provide comfort that the randomization was successful and that treatment estimates are unbiased.

4. *Analysis of Filing Decision*

4.1 EFFECT BY TREATMENT STATUS AND TREATMENT ARM

In this section, we report our main results. Since the treatment was randomly administered, we are able to draw causal inferences for the information treatment and for the two different treatment arms. Subsequently, we show cross-sectional analyses that explore the heterogeneity of the main treatment effect, but these additional analyses do not allow for a causal interpretation (e.g., Christensen, Hail, and Leuz [2016]). Correlated omitted variables can potentially explain these cross-sectional differences, although they cannot explain our average treatment effect or the average effects by treatment arm. It is also worth mentioning that our randomization was performed on the individual firm level and that we only have one

[23] For example, Cassar, Ittner, and Cavalluzzo [2015] report that the median firm in their sample has total assets of $90,000. According to table 4 of Allee and Yohn [2009], their median firm has total assets of about $150,000. Studies using *Sagework* data, such as Minnis [2011] or Badertscher, Shroff, and White [2013], are based on samples that include larger U.S. firms.

observation per firm. Thus, we follow the advice of Abadie et al. [2023] and use heteroscedasticity-consistent (robust) standard errors for statistical inferences in all tests.[24]

The main results are reported in table 3. Panel A of table 3 shows that 26.23% of all control firms restrict access to their financial statements. Relative to this baseline, treated firms are 15% (3.94 percentage points) more likely to restrict access to their financial statements. This difference is statistically significant and economically meaningful. The magnitude of the effect is even more remarkable when considering that we only measure an ITT effect. ITT estimates are unbiased but more conservative than the corresponding treatment-on-the-treated (TOT) effects. Specifically, it is plausible that most of the firms did not read our email or did not read it before filing their 2015 financial statements. Thus, to obtain the TOT effect, we need to scale up the ITT by the reading rate. For example, the ITT for the main treatment is 3.94 percentage points. Using a conservative scale-up factor of 4.6—based on the reading rate in Perez-Truglia and Cruces [2017] and consistent with average reading rates of business emails (e.g., Return Path [2017])—we estimate a TOT effect of 18.12 percentage points or 69.1%. This estimated TOT effect is of a similar magnitude as the 17.4 percentage points higher restriction rate of survey participants, who must have a 100% reading rate (see section 4.5).

Figure 5 explores the temporal dynamics of the main treatment effect. Specifically, panel A of figure 5 plots the cumulative difference in the restriction rate between treatment and control group by filing date. This panel shows that the difference between treatment and control group increases until the end of February 2017 and then slightly flattens out afterwards. In panel B, we examine the sharpness of the treatment effect. To do so, we include firms from the provisional sample, which were, somewhat accidentally, part of the experiment even though they had already filed their 2015 financial statements before our experiment (i.e., their filing date is before December 13, 2016). If we had accidentally induced covariate bias by our randomization, we would expect that these firms show a different filing behavior prior to the treatment. However, we find that treatment and control firms only meaningfully diverge for firms that file their 2015 financial statements after the field experiment.

Table 3 also contains information on the results by treatment arm. Consistent with our expectation, we find that firms which receive a transactional stakeholder treatment are *less* likely to restrict their financial statements relative to firms with a non-transactional stakeholder treatment. Therefore, we provide causal evidence on the determinants of firms' disclosures. This result is broadly consistent with Breuer, Hombach, and Müller [2023], who

[24] Neither our sampling process ("sampling design issue"), nor our treatment assignment ("experimental design issue") was clustered. Consequently, clustered standard errors are unnecessary and potentially biased (Abadie et al. [2023]).

TABLE 3

Filing Decision by Main Treatment and Treatment Arm Status

Panel A: Restriction rate by treatment status

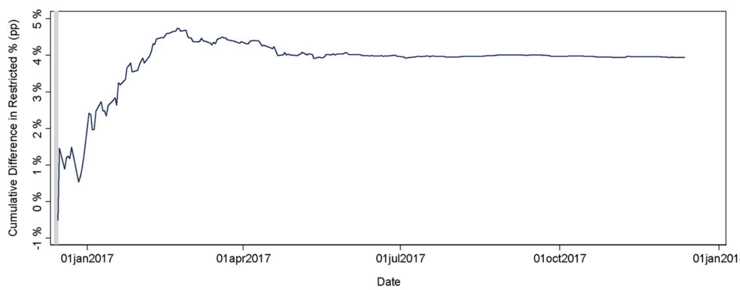
	Group	N	*Restricted*	Comparison	Difference	t-Value
Main Groups:						
(A)	*Control*	5,158	26.23%	(A) = (B)	3.94 pp***	5.69
(B)	*Treatment*	20,566	30.17%			
Treatment Arms:						
(I)	*Non-transacting*	10,296	30.81%	(A) = (I)	4.58 pp***	6.00
(II)	*Transacting*	10,270	29.52%	(A) = (II)	3.29 pp***	4.33
				(I) = (II)	1.29 pp**	2.01

Panel B: Differences in restriction rate by treatment status

	(1) *Restricted*	(2) *Restricted*	(3) *Restricted*	(4) *Restricted*	(5) *Log(Filing Lag)*
Constant (= No email)	0.262***	–	0.262***	–	3.726***
	(42.82)		(42.82)		(260.43)
Experimental Variables:					
Treatment	0.039***	0.039***	–	–	–
	(5.69)	(5.57)			
Non-transacting	–	–	0.046***	0.045***	-0.011
			(6.00)	(5.84)	(−0.66)
Transacting	–	–	0.033***	0.032***	−0.022
			(4.33)	(4.25)	(−1.24)
Control Variables:					
Log(Total Assets)	–	−0.036***	–	−0.036***	–
		(−11.44)		(−11.44)	
Log(Firm Age)	–	0.002	–	0.002	–
		(0.53)		(0.53)	
Tangibility	–	−0.006**	–	−0.006**	–
		(−2.08)		(−2.07)	
Equity Ratio	–	−0.005*	–	−0.005*	–
		(−1.66)		(−1.65)	
Fixed Effects					
Industry	No	Yes	No	Yes	No
District	No	Yes	No	Yes	No
P-value from F-test:					
Non-transacting = Transacting	–	–	0.045	0.057	0.468
Adjusted R^2	0.001	0.018	0.001	0.018	0.000
N (= Number of firms)	25,724	25,708	25,724	25,708	25,724

The sample in this analysis corresponds to the final sample of table 1 and comprises up to 25,724 firms. In panel A, we report the fraction of firms that choose to restrict access to their 2015 financial statements by treatment assignment. We also report t-statistics from tests for mean differences across groups. In panel B, we report OLS coefficient estimates and t-statistics from cross-sectional regressions of the 2015 filing decision on our experimental variables. In the first four columns, we use *Restricted* as the dependent variable and, in the last column, we use the natural logarithm of *Filing Lag* (plus one) as the dependent variable. All variables are defined as in the notes to table 2. For ease of interpretation, all control variables are standardized to mean of 0 and standard deviation of 1. Industry fixed effects are based on 3-digit WZ2008 (German equivalent for NAICS) industry codes, resulting in 74 different industries. District fixed effects refer to 398 administrative districts in Germany. Columns 2 and 4 have slightly fewer observations due to missing or unique industry and district values. t-statistics are based on robust standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Panel A: Cumulative Restriction Rate Difference Between Treatment and Control Group by Date



Panel B: Cumulative Restriction Rate Difference Around Experiment by Date

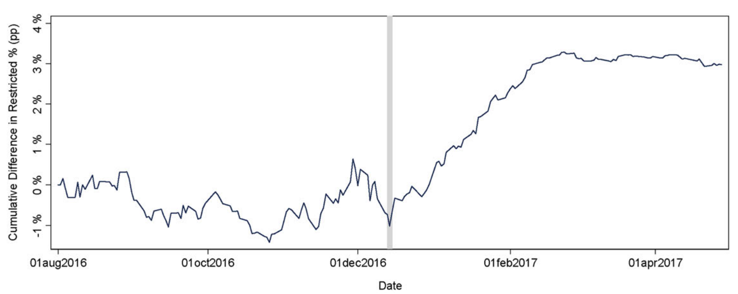
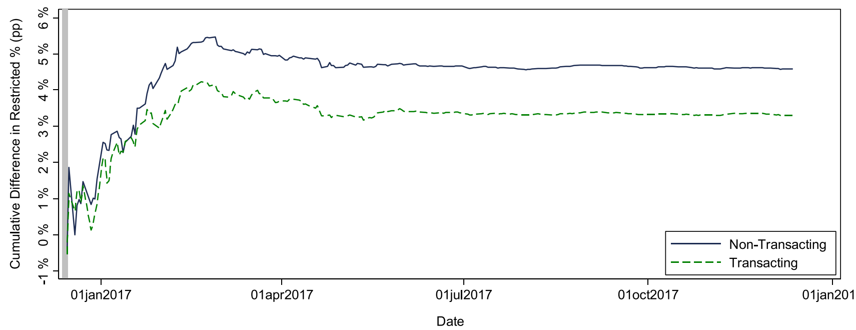


FIG. 5.—Main treatment effect by filing date. This figure shows the main treatment effect on the disclosure decision chronologically by filing date. Panel A plots the cumulative difference in the restriction rate in percentage points between treatment and control group starting at the last day of the experiment. The gray-shaded area highlights the experimentation period. The average difference in the cumulative restriction rate on the last date of the sample period (December 12, 2017) is equal to the mean difference (3.94%) between treatment and control group as reported in table 3. Panel B displays the cumulative restriction rate difference in percentage points between treatment and control group from August 1, 2016, to April 30, 2017. This panel includes the pre-experimentation period of the provisional sample and demonstrates that the treatment and control group only significantly diverge after the email delivery (gray-shaded area).

argue that transacting stakeholders are important in a firm's decision to publicly provide financial statement information. The difference between both groups is 1.29 percentage points (40%), which is statistically significant and economically meaningful. The temporal dynamics of both treatment arm effects, relative to the control group, are plotted in panel A of figure 6.

Panel A: Cumulative Restriction Rate Difference for Treatment Arm Groups by Date



Panel B: Cumulative Restriction Rate Difference for Sub-Treatment Groups by Date

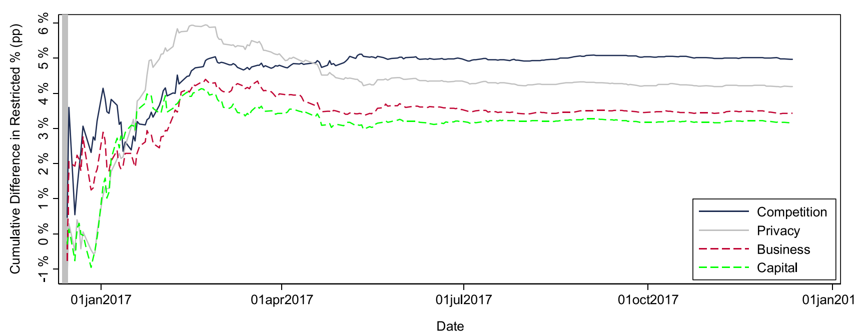


FIG. 6.—Treatment arm and subtreatment effects by filing date. The figure shows the treatment effect on the disclosure decision for the different treatment arms (panel A) and for the different subtreatment groups (panel B) by filing date. The gray-shaded area highlights the experimentation period. Panel A plots the cumulative difference in the restriction rate in percentage points (relative to the control group) for the *Non-transacting* and *Transacting* stakeholder treatment over time. The solid blue (dashed green) line corresponds to the *Non-transacting* stakeholder (*Transacting* stakeholder) treatment. Panel B shows the cumulative difference in the restriction rate in percentage points (relative to the control group) for the four subtreatment groups. The solid lines are assigned to non-transacting stakeholders with the dark blue (light gray) line representing the *Competition* (*Privacy*) treatment. The dashed lines are assigned to transacting stakeholders; the dark red (light green) line represents the *Business* (*Capital*) treatment. In both panels, the percentage on the last day of the sample period (December 12, 2017) is equal to the mean restriction rate of the respective group as reported in panel A of table 3 or panel A of table 4.

We also report the results using a conventional OLS regression framework in panel B of table 3. Column 1 shows the main treatment effect without any additional control variables. The coefficients directly correspond to the differences reported in panel A of table 3. In column 2, we add our control variables as well as industry and district fixed effects. The coefficient and *t*-statistic of the treatment variable remain remarkably stable, which further strengthens the interpretation that the treatment assignment is orthogonal to other potential confounders. In untabulated tests, we also add strata fixed effects and find virtually identical results. Column 3 reports the results for the two different treatment arms. Again, the coefficients directly correspond to the respective numbers in panel B of table 3. The non-transacting (transacting) stakeholder treatment causes a 4.6 percentage points (3.3 percentage points) higher restriction rate relative to the control group. The difference of 1.29 percentage points between both treatment arms is statistically significant. In column 4, we add our control variables and find similar results. Finally, we run a falsification test and examine whether the information treatment affects a firm's filing lag. One potential concern is that treated firms might delay their filing decision to process the new information and that late filers always have a higher restriction rate (e.g., because tax advisors have more time to advise their clients after the busy season). However, as evident from column 5, we do not find any significant differences between treatment and control group or between both treatment arms with respect to the filing lag. Finally, in online appendix C, we show that our results are robust to multiple hypothesis testing adjustments and provide information on the post study probability (PSP) for this effect based on different priors (Floyd and List [2016]).

4.2 HETEROGENEITY OF MAIN TREATMENT EFFECT

We next examine the heterogeneity of the main treatment effect. We do this by following two complementary empirical strategies. As a first step, we use theory to motivate a set of potential covariates that we expect to moderate the observed treatment effect. In a second step, we then use a machine learning approach to identify the most important covariates.

We expect that firms, which should derive lower net benefits of public disclosure react more strongly to the information treatment. Table 4 reports the results. In column 1, we interact all control variables with the treatment indicator. We find that the treatment effect is stronger for smaller, more mature, and less financially health firms. These results make intuitive sense. Smaller and more mature firms should rely on less external capital and, hence, the first two cross-sections are consistent with the idea that these firms are more likely to exercise the restriction option due to having low disclosure benefits. The last cross-section is also in line with our expectation. "Worse" firms (e.g., Verrecchia [1983]) or firms that are subject to predation risk (e.g., Bernard [2016]) should provide less voluntary disclosure.[25] In column 2, we gauge the robustness of the cross-sectional

[25] In online appendix B, we compare these cross-sectional results with firms' initial adoption decision between 2012 and 2014. In line with these firm-level interactions, we also find that smaller, more mature and less financially healthy firms are more likely to restrict access immediately after the regulatory change. These results are comforting as they indicate that our sample is not biased and that they are consistent with the notion that our treatment effect is rather a lower bound of the effect (as firms which would have reacted stronger to the treatment already left the sample).

TABLE 4
Heterogeneity of Main Treatment Effect

	(1) *Restricted*	(2) *Restricted*	(3) *Restricted*	(4) *Log(Filing Lag)*
Constant	0.262***	–	–	–
	(42.91)			
Firm-Level Interactions:				
Treatment × *Log(Total Assets)*	−0.018**	−0.022***	−0.018**	−0.001
	(−2.55)	(−2.89)	(−2.44)	(−0.03)
Treatment × *Log(Firm Age)*	0.016**	0.023***	0.016**	−0.014
	(2.32)	(3.09)	(2.22)	(−0.84)
Treatment × *Tangibility*	−0.007	−0.006	−0.004	0.005
	(−0.99)	(−0.83)	(−0.62)	(0.34)
Treatment × *Equity Ratio*	−0.012*	−0.012*	−0.011	0.008
	(−1.74)	(−1.65)	(−1.52)	(0.50)
Industry- Interactions:				
Treatment × *Peer Restriction*	–	–	0.014**	−0.008
			(2.09)	(−0.47)
Treatment × *Capital Intensity*	–	–	−0.015**	−0.011
			(−2.03)	(−0.60)
Main Effects:	Yes	Yes	Yes	Yes
Fixed Effects				
Industry	No	Implied	Yes	Yes
District	No	Implied	Yes	Yes
Treatment-Industry	No	Yes	No	No
Treatment-District	No	Yes	No	No
Adjusted R^2	0.009	0.017	0.019	0.024
N (= Number of firms)	25,724	25,692	25,707	25,707

This table shows the heterogeneity of the main treatment effect for up to 25,724 firms. We focus on interactions between our *Treatment* variable and various cross-sectional variables. *Peer Restriction* captures the percentage of eligible firms that have restricted access to their financial statements within the same industry as the focal firm. The ratio is calculated by dividing the number of noninsolvent firms that are no longer available in the Dafne database in the fiscal year 2014 (last year before the experiment) by the number of peer firms that were likely to be eligible for the filing exemption in 2011 (last year before the law change) for each industry. We determine eligibility as explained in footnote 20 and ignore the focal firm when calculating this ratio. *Capital Intensity* is the median total assets to sales ratio in an industry over a five-year period from 2010 to 2014 considering all firms in the Dafne database (winsorized at the 1% and 99% level). For both industry interactions, we use full-digit WZ2008 industry codes (877 different industries). *Treatment*-industry (*Treatment*-district) indicates that separate industry (district) fixed effects for treatment and control group are added to the regression. All necessary main effects are included in the regressions but not displayed for brevity. For the remaining variable definitions see the notes to table 2 and table 3. For ease of interpretation, all continuous independent variables and their values in the interaction terms are standardized to mean of 0 and standard deviation of 1. Column 2 contains fewer observation due to missing or unique *Treatment*-industry or *Treatment*-district values. Columns 3 and 4 contain fewer observations to missing or unique industry and district values. *t*-statistics are based on robust standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

findings by adding industry and district fixed effects to the regression. However, we are interested in the interaction terms and this fixed effects structure would only control for baseline differences between treatment and control group (e.g., Breuer and deHaan [2024]). Therefore, we also add the interactions between treatment indicator and all fixed effects to the regressions (i.e., twice as many fixed effects). Despite the tight fixed effects structure, all cross-sectional results are robust.

Next, in column 3, we examine how industry characteristics are associated with the restriction decision.[26] We focus on two additional interaction terms. First, *Peer Restriction* captures the percentage of peer firms that have restricted access to their financial statements before the experiment in the same industry as the focal firm. A higher peer restriction rate might lead to a stronger treatment effect as treated firms might learn from peers' past restriction decision or because public disclosures are less beneficial for firms in the same industry (unobserved net benefits should be positively correlated within an industry). Therefore, we expect and find that these firms are more likely to restrict access to their financial statements once they become aware about the disclosure option.[27] Second, *Capital Intensity* is the inverse of the median asset turnover in a given industry and higher values indicate that the industry is more capital intensive. Therefore, the significantly negative coefficient indicates that firms that are operating in capital-intensive industries are less likely to exercise the restriction option. In column 4, we repeat our falsification test of panel B of table 3. It is comforting to see that none of the interaction terms is significantly associated with the filing lag.[28]

We next adopt a machine learning approach to assess the heterogeneity of our main treatment effect. By estimating a generalized random forest model (Athey, Tibshirani, and Wager [2019]), we are able to predict the treatment effect for control and treatment firms based on a nonparametric function that includes all available covariates. This approach has a different objective compared to the traditional one discussed above. Rather than estimating theoretically predicted cross-sectional effects, it uses the available data to predict the observed effect. This prediction allows us to explore the heterogeneity of the treatment effect in more detail and to also verify whether our theoretically motivated covariates from above "survive" in a more theory-agnostic setup. We provide details on this approach in online appendix E. Figure 7 shows the conditional average treatment effect (CATE) predicted by our main covariates from table 4; more than 90% of all CATE values are above zero, and the most predictive variables are *Log(Total Assets)*, *Capital Intensity, Peer Restriction*, and *Equity Ratio*. Even

---

[26] Interaction effects between treatment indicator and fixed effects are not included in column 3. Otherwise, these fixed effects would absorb most of the variation that we aim to exploit in this specification.

[27] Inspired by Badertscher, Shroff, and White [2013] and Shroff, Verdi, and Yost [2017], we focus on industry spillovers between firms. In untabulated tests, we also find some evidence for geographical spillovers within local industries (e.g., Bernard, Kaya, and Wertz [2021]).

[28] In online appendix E, we conduct two additional cross-sectional tests related to variables that might be correlated with firms' disclosure incentives or awareness of the regulation. Specifically, we focus on firms which aggregated their financial statements before our experiment (i.e., they might have a higher likelihood of being aware about the restriction option as they already used another provision of MicroBilG) or which reported a loss in 2014 (i.e., they might have a stronger incentive to restrict access). Although we find directionally consistent results, the results are mostly statistically insignificant.

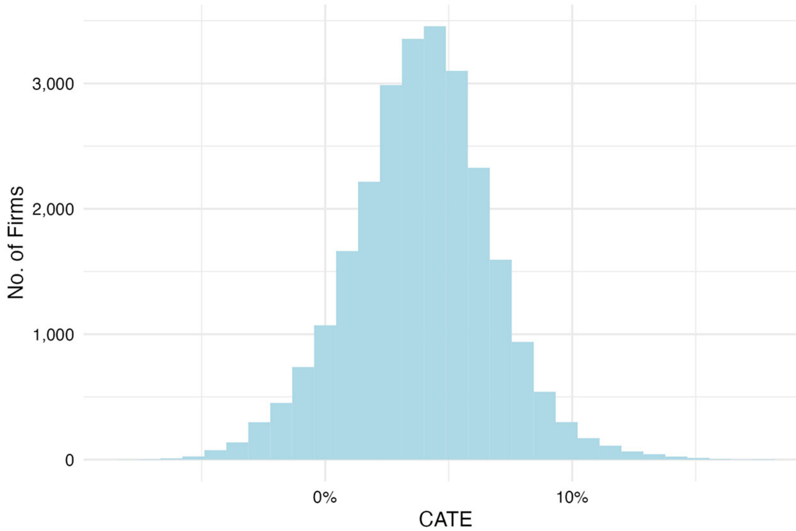


Fig. 7.—Histogram of conditional average treatment effect (CATE). The figure shows the CATE predicted using a generalized random forest model with the main covariates of table 4. It is measured as the percentage point difference in *Restricted* and is estimated for treatment as well as control firms. The variables included in the casual random forest model, ordered by their importance, are *Log(Total Assets)*, *Capital Intensity*, *Peer Restriction*, *Equity Ratio*, *Tangibility*, and *Log (Firm Age)*. All variables are as defined in the notes to tables 2 and 4. Variable importance is assessed by the weighted sum of how many times a variable was used to split the tree with the weights determined by the depth of the respective split. Up to four levels are considered.

when we include a substantially larger set of covariates, mainly the variables used in our traditional tests presented in table 4 possess predictive power for the CATE. Overall, we interpret the evidence from our causal random forest analysis as supporting the conclusions drawn by our traditional cross-sectional analysis.

Given the endogenous nature of our covariates and the limited power of the analyses however, we would like to stress that there are alternative explanations for our cross-sectional findings in table 4. For example, smaller firms might be less informed about the disclosure option before the intervention, or they might have a higher likelihood of reading our email. In turn, they might respond more strongly to the information treatment even if their net benefits are similar. Similar alternative explanations can be developed for our other covariates. Collectively, however, all cross-sectional findings are consistent with our "net benefit" interpretation. This interpretation is also consistent with our causal estimates and the differential reaction to the transacting and non-transacting stakeholder treatment. It is further supported by our supplementary survey evidence (see section 4.5).

TABLE 5
Persistence of Main Treatment Effect

	(1) *Restricted_2016*	(2) *Restricted_2016*	(3) *Restricted_2016*	(4) *Restricted_2016*
Constant (= No email)	0.363***	–	0.191***	0.196***
	(52.85)		(33.20)	(29.71)
Experimental Variables:				
Treatment	0.036***	0.034***	0.010	0.004
	(4.69)	(4.44)	(1.61)	(0.60)
Treatment × *Restricted_2015*	–	–	–	0.021
				(1.54)
Prior Decision:				
Restricted_2015	–	–	0.657***	0.640***
			(127.97)	(52.18)
Control Variables:				
Log(Total Assets)	–	−0.060***	–	–
		(−17.71)		
Log(Firm Age)	–	0.011***	–	–
		(3.43)		
Tangibility	–	0.001	–	–
		(0.23)		
Equity Ratio	–	−0.010***	–	–
		(−3.15)		
Fixed Effects				
Industry	No	Yes	No	No
District	No	Yes	No	No
Adjusted R^2	0.001	0.031	0.376	0.376
N (= Number of firms)	24,539	24,524	24,539	24,539

This table shows the persistence of the treatment effect by analyzing firms' 2016 filing decision conditional on treatment assignment. The sample contains all firms of our final sample which filed their 2016 financial statements until September 5, 2018. We remove 1,185 firms with unavailable 2016 data. We use the restriction decision in 2016 (*Restricted_2016*) as the binary dependent variable. *Restricted_2015* is an indicator variable that captures a firm's 2015 restriction decision. All other variables are defined as in the notes to table 2 and table 3. *t*-statistics are based on robust standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

4.3 PERSISTENCE OF TREATMENT EFFECTS

We next study the persistence of our treatment effects by examining the 2016 filing decision (i.e., one year after the experiment). We crawl the Business Register until September 5, 2018, and obtain information on the 2016 filing decision for 24,539 firms. Table 5 presents the results. In column 1, we run a linear regression with *Restricted_2016* as the outcome variable. First, we find that 36.27% of the control group chooses the restriction option, which is 10.1 percentage points higher compared to 2015 (table 3). This increase for untreated firms is in line with the general time trend that more and more firms are gravitating toward nondisclosure (see figure 1 or table B1). Second, the treatment coefficient is still significantly positive and only slightly smaller than in 2015 (0.036 vs. 0.039). The minor drop in the magnitude is not surprising as firms should become gradually aware of the restriction option via other sources (as suggested by the uptick in the restriction rate for the control group). In column 3, we check whether the treatment had an effect over and above the immediate 2015 filing decision. The insignificant coefficient for *Treatment* suggests that most firms adjusted their 2015 filing behavior and then made the same decision for their 2016 financial statements. In column 4, we examine whether the 2015 choice is more persistent for treatment or control firms. The insignificant positive coefficient on the interaction term suggests that, if anything, treated firms are less likely to switch back to public filings. This result is inconsistent with the explanation that our information treatment nudged firms into the "wrong" decision in 2015.

Collectively, our treatment effect shows a remarkable persistence compared to other one-time information interventions that typically fade more quickly (e.g., Cavallo, Cruces, and Perez-Truglia [2017], Coibion, Gorodnichenko, and Kumar [2018], Leonelli et al. [2023]). This stronger persistence is arguably a combination of the simple nature of the information ("restriction option available") and the fact that these firms had somewhat considerable negative net benefits of disclosure.

4.4 SUBTREATMENT GROUP EFFECTS

After establishing our main results, we now zero in on the effects by subtreatment group. Table 6 shows the corresponding regression results.[29] In column 1, we use the specification of column 3 in panel B of table 3 and split the non-transacting stakeholder treatment into both subtreatment groups *Competition* and *Privacy*. Relative to the control group, firms that received the *Competition* (*Privacy*) treatment have a 5.0 (4.2) percentage points higher restriction rate. Although only *Competition* is significantly different at conventional levels, both subtreatment groups have a higher restriction rate than firms which received the transacting stakeholder treatment (3.3 percentage points). In column 2, we split the transacting stakeholder treatment into both subtreatment groups (*Business* and *Capital*). The restriction rates for both groups are significantly higher than the restriction rate of the control group. Furthermore, the effect for the capital provider treatment is significantly smaller than for the average non-transacting stakeholder treatment effect (the "customers and suppliers" treatment has a two-sided *p*-value of 14%).

In column 3, we report the results for all four subtreatment groups. Two findings stand out. First, the ordering of all subtreatment groups is consistent with our expectations. Both non-transacting stakeholder groups have a higher restriction rate than each of the transacting stakeholder groups. Of 24 ($= 4\times3\times2\times1$) possible rankings, only 4 ($= 2\times1\times2\times1$) rankings would have produced a similar relative ordering by chance. This result provides comfort that the differences between both treatment arms are not solely driven by a single subtreatment group. Instead, the restriction decision

[29] The temporal dynamics by subtreatment group are plotted in panel B of figure 6.

TABLE 6

Filing Decision by Subtreatment Status

	(1) *Restricted*	(2) *Restricted*	(3) *Restricted*	(4) *Restricted*	(5) *Log(Filing Lag)*
Constant (= No email)	0.262***	0.262***	0.262***	–	3.726***
	(42.82)	(42.82)	(42.82)		(260.42)
Experimental Variables:					
Non-transacting	–	0.046***	–	–	–
		(6.00)			
Transacting	0.033***	–	–	–	–
	(4.33)				
Competition	0.050***	–	0.050***	0.049***	0.002
	(5.57)		(5.57)	(5.48)	(0.09)
Privacy	0.042***	–	0.042***	0.041***	−0.025
	(4.74)		(4.74)	(4.57)	(−1.22)
Business	–	0.034***	0.034***	0.034***	−0.022
		(3.87)	(3.87)	(3.78)	(−1.07)
Capital	–	0.032***	0.032***	0.031***	−0.022
		(3.58)	(3.58)	(3.54)	(−1.08)
Control Variables:	No	No	No	Yes	No
Fixed Effects					
Industry	No	No	No	Yes	No
District	No	No	No	Yes	No
P-values from *F*-Test:					
Transacting = Competition	0.034	–	–	–	–
Transacting = Privacy	0.250	–	–	–	–
Non-transacting = Business	–	0.140	–	–	–
Non-transacting = Capital	–	0.071	–	–	–
Competition = Business	–	–	0.089	0.090	0.243
Competition = Capital	–	–	0.047	0.054	0.239
Privacy = Business	–	–	0.393	0.438	0.893
Privacy = Capital	–	–	0.254	0.313	0.895
Adjusted R^2	0.001	0.001	0.001	0.018	0.000
N (= Number of firms)	25,724	25,724	25,724	25,708	25,724

This table reports OLS coefficient estimates and t-statistics from cross-sectional regressions of the 2015 filing decision on our experimental variables (as introduced in table 1). In the first four columns, we use *Restricted* as the dependent variable and, in the last column, we use the natural logarithm of *Filing Lag* (plus one) as the dependent variable. We use the same control variables and fixed effects as in table 3. *t*-statistics are based on robust standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

is systematically different between transacting and non-transacting stakeholders in line with our theoretical predictions. Second, *Competition* is the subtreatment group that has a significantly larger effect compared to both transacting subtreatment groups (which is a fairly low-powered test because the number of observations is substantially smaller when testing two subtreatment groups against each other).[30] This finding is consistent with Dedman and Lennox [2009] or Minnis and Shroff [2017]. They both show that

[30] Because we are testing eight different hypotheses, adjustments for multiple hypothesis testing might be warranted (Floyd and List [2016]). Unlike for our main tests reported in

competition seems to be the main reason why firms do not publicly disclose more information. Albeit not statistically significant at conventional levels, also the finding that the *Capital* subtreatment group has the lowest restriction rate is consistent with the survey evidence of Minnis and Shroff [2017].[31]

Next, we gauge the robustness of the subtreatment group results. In column 4, we add industry and district fixed effects to the regression. Both the relative rankings and the significance levels remain largely unchanged. Finally, in column 5, we replace our main dependent variable by *Log(Filing Lag)* to run our falsification test. There are no significant differences in terms of filing dates between all subtreatment groups.

4.5 SURVEY RESULTS

We use our survey to verify our field-based evidence and to explore potential mechanisms. Similar to the field experiment, we use between-subject randomization within the survey, thereby reducing social desirability bias and experimenter demand effects that typically affect traditional nonexperimental survey designs (e.g., Charness, Gneezy, and Kuhn [2012]). Nevertheless, in contrast to our field experiment, the survey-based effect sizes might still be biased for at least two reasons. First, responses stem from a highly selective sample. Second, the statements within a survey will not necessarily line up with actual firm decisions. Although we are able to address the second concern to some extent (by linking survey responses to actual filing decisions ex post), the first concern remains a valid caveat. However, we believe that the survey can offer valuable insights in combination with our causal estimates from the field experiment. For example, it allows us to explore whether the overall disclosure response can be explained by our proposed information channel rather than a simple nudging story.

The full survey results are reported in online appendix D. We receive 1,059 (774) valid survey responses based on our provisional sample (final sample), translating into a 3.58% (3.77%) response rate. Respondent firms are, on average, smaller, less mature and have a higher equity ratio than nonresponding firms. However, response rates only marginally differ between the two treatment arms (table D1).

Interestingly, most respondents indicate that tax advisors (71.47%), followed by managing directors (68.10%), are mainly involved in the preparation and filing of their financial statements [Q1 in table D2]. Furthermore,

---

section 4.1, using FWER corrections for multiple comparisons lead to p-values above 0.1 with two-sided tests in this table (except for column 1). See online appendix C.

[31] This result might be surprising if one believes that banks should have direct access to firms' financial statements via private channels (e.g., Minnis and Sutherland [2017]). Nevertheless, according to the survey by Minnis and Shroff [2017], mostly creditors and lenders are using publicly available financial statements of private firms. Additionally, Breuer, Hombach, and Müller [2018] show that banking relationships become transactional once more financial statement information is publicly available.

slightly more than 50% of the survey respondents state that they were unaware about the restriction option in 2014 [Q2 and Q4]. Only few firms (9.09%) indicate that they were ineligible for the MicroBilG exemptions in 2014 [Q3]. This observation begs the question why firms that were eligible and aware of the MicroBilG exemptions still publicly disclosed their 2014 financial statements. Most of these firms (46.78%) state that they obtain larger benefits than costs from public disclosure [Q4]. Only very few firms had a legal obligation, for example, via a memorandum of association, to do so (5.05%).

Table 7 contains the main results from the survey without linking the survey responses to our archival data set. Panel A of table 7 shows the responses to Question 7. There are two interesting results. First, firms generally view transacting stakeholders as more beneficial financial statement users than non-transacting stakeholders. The difference between both treatment arms is highly significant. Second, the relative ranking of all subtreatment groups is broadly consistent with the ordering reported in section 4.4 (with the only difference being the marginally higher relative importance of the *Privacy* treatment). Both results support our interpretation. While being overall skeptical about the net benefits of public disclosure, firms seem to be relatively more inclined to keep publicly disclosing their financial statements after receiving the transacting stakeholder treatment *because* they view transacting stakeholders as relatively more beneficial financial statement users.

In panel B of table 7, we then explore whether the main treatment effect is indeed due to the "information" channel. An alternative explanation would be that our email simply nudges all firms to reconsider their disclosure strategy ("nudging" channel). We find that the survey results are more consistent with the former explanation. Specifically, in Question 9, we asked respondents to indicate how likely it is that they will publicly disclose their 2015 financial statements. Responses are measured on a 1 to 5 Likert scale with higher values indicating a higher restriction probability for the next set of financial statements (we verify in panel A of table D4 that the self-reported score is strongly correlated with the actual decision for the 2015 financial statements). The unconditional mean is insignificantly different from the neutral response option.[32] We then split the sample based on firms' prior statements in the survey that allow us to gauge whether the firm was aware of the disclosure option in 2014.[33] We find that firms that were unaware about the disclosure option have a significantly higher

[32] The percentage of firms that prefer the restriction option (50.01%) is close to the untargeted empirical moment (48.80%) in our stylized restriction model of online appendix B (panel C of table B1). Interestingly, this calibrated model suggests that 86.74% of all firms in the entire population of small firms would prefer nondisclosure.

[33] We do this coding indirectly by asking survey participants whether they are *now* aware of the difference between "disclosing" and "depositing" financial statements without directly referring to their 2014 disclosure decision (Question 2). However, only relying on this question for the *Unaware of Option* coding would yield a downward biased estimate. Firms might have become informed between the 2014 financial statement filing date and the date of the survey

TABLE 7

Firm Survey—Assessment of Different Stakeholders and Future Filing Decision

Panel A: Overall assessment of financial statements for stakeholders

Question 7: How do you assess the fact that [stakeholder group] could potentially access the published financial statements of your firm? [7-point Likert Scale: 1 to 7]

						% of Respondents Who Answered	
Responses		*N*	Average Rating	Significantly Different Than [4 = Neutral]	Significantly Different Than	Very Positive [6 or 7]	Very Negative [1 or 2]
(I)	Transactional stakeholders	504	3.94		II	18.06	21.03
(II)	Nontransactional stakeholders	513	3.16	***	I	3.12	33.92
(1)	Capital providers	261	4.07		2–4	19.54	18.01
(2)	Customers and suppliers	243	3.80	*	1 and 3–4	16.46	24.28
(3)	Competitors	262	3.21	***	1–2	4.58	33.21
(4)	People in your private life	251	3.10	***	1–2	1.59	34.66

Panel B: Future filing decision conditional on knowledge about "Restrict Access" option

Question 9: Will you continue to disclose your financial statements in the future? [5-point Likert Scale: 1 to 5]

						% of Respondents Who Answered	
Responses		*N*	Average Rating	Significantly Different Than [3 = Neutral]	Significantly Different Than	Restrict Access [4 or 5]	Disclose [1 or 2]
	All survey participants	916	3.01		–	41.71	41.59
(i)	*With* prior knowledge about disclosure option	438	2.17	***	ii	21.69	67.81
(ii)	*Without* prior knowledge about disclosure option	478	3.78	***	i	60.04	17.57

(*Continued*)

TABLE 7—*(Continued)*

Panel C: Future disclosure decision conditional on treatment group assignment for firms without prior knowledge about "Restrict Access" option

Question 9: Will you continue to disclose your financial statements in the future? [5-point Likert Scale: 1 to 5]

Responses for Firms Without Prior Knowledge of Disclosure Option	*N*	Average Rating	Significantly Different Than [3 = Neutral]	Significantly Different Than	% of Respondents Who Answered: Restrict Access [4 or 5]	% of Respondents Who Answered: Disclose [1 or 2]
(I) Transactional stakeholders	231	3.64	***	II	53.68	20.35
(II) Nontransactional stakeholders	247	3.90	***	I	65.99	14.98
(1) Capital providers	112	3.58	***	3	49.11	18.75
(2) Customers and suppliers	119	3.70	***		57.98	21.85
(3) Competitors	124	3.99	***	1	67.74	13.71
(4) People in your private life	123	3.81	***		64.23	16.26

This table shows the answers to Question 7 (panel A) and Question 9 (panels B and C). The sample comprises up to 1,059 answers from participants of a survey sent to private firms (including 285 firms that are only part of the provisional sample). Each participant received one of four different versions (1–4) of the survey. Question 7 (panel A) is only referring to one specific stakeholder group depending on the version of the survey. For the analyses, we also aggregate (1) "capital providers" and (2) "customers and suppliers" to (I) "transactional stakeholders" as well as (3) "competitors" and (4) "people in your private life" to (II) "non-transactional stakeholders." Question 9 (panels B and C) was identical for all four different versions (1–4) and the question was not displayed to 107 participants that chose answer option "No" in Question 3 (see panel C of table D2). Panel B reports the responses to Question 9 conditional on the firm's prior knowledge about the "restrict access" option. Whether a firm was aware about the "restrict access" option is determined based on a firm's response to Question 2 and Question 4 (see the notes to table D2 for details). Panel C provides the responses for firms without prior knowledge about the disclosure option (477 participants as indicated in panel B) and conditional on receiving one of four different survey versions. Each panel provides the average ratings of the responses to the survey question indicated in the panel's header as well as the percentages of responses in various bins of the Likert scale. We report the results from *t*-tests comparing the average rating of an item to the midpoint (where ***, **, and * stand for statistical significance at the 1%, 5%, and 10% levels). We also compare the average rating of an item to the average ratings of all the other items, using Bonferroni-Holm adjusted *p*-values at the 10% level for the comparison. See the online appendix for more details and the full survey.

probability of restricting access to their next set of financial statements. Taking the survey results at face value, only 26.94% (= 21.69% + 0.5×10.50% [neutral option]) of all participants with prior knowledge about the disclosure option indicate that they will restrict their next financial statements. This restriction rate is almost identical to the 26.23% restriction rate of the control group (see table 3). This result strongly supports the notion that the email did not simply nudge all firms to change their disclosure behavior, but that the treatment mainly provided new information to firms that lacked knowledge about the disclosure option beforehand. Finally, in panel C of table 7, we examine how the response to Question 9 varies with the framing of the information treatment. In this regard, we focus on firms that were unaware about the disclosure option because the main treatment effect is concentrated among these firms. Firms that had no prior knowledge about the disclosure option *and* received the non-transacting stakeholder framing are even more likely to restrict access to their financial statements. We do not find a significant difference between both stakeholder framings for firms that were aware about the restriction option before the intervention (untabulated). This result is comforting as firms with prior knowledge should have a clearer sense of the costs and benefits associated with their (prior) disclosure decision.

In table 8, we combine our survey data with the archival data to conduct additional empirical analyses (e.g., Graham et al., [2014, 2017]). Column 1 shows that firms that participate in our survey have a 17.4 (= 3.4 + 14.0) percentage points higher restriction rate than control firms.[34] This large effect for survey respondents is not surprising because we know that these firms must have read our email. The treatment effect (3.4 percentage points) for nonparticipating firms is smaller, but still statistically significant. In all remaining columns, we only focus on firms that responded to the survey. Column 2 explores the "unawareness" theme in more detail. In line with the results of panel B of table 7, we find that the larger treatment effect for survey respondents is strongly concentrated among firms that were unaware about the option beforehand. Again, this result rather supports our information treatment–based argument. In column 3, we confirm that this finding is robust to adding fixed effects, control variables and a separate treatment arm indicator to the model. In the last two columns, we examine how the 2015 disclosure decision varies conditional on their answers

(in particular via our email). Therefore, we also asked firms why they publicly disclosed their 2014 financial statements (Question 4). An explicit answer choice "Unaware in 2014" was not provided for this question, but we code free text answers to this question accordingly. This approach, without an explicit answer option, is appealing as it should induce less response bias. For example, survey participants who are regretting their 2014 decision might otherwise chose the answer option "Unaware in 2014" as a scapegoat to not admit that they erred a year earlier. Accidentally coding "regretting" firms as "uninformed" firms would confound our estimates.

[34] In untabulated tests, we find that survey respondents also react more strongly to the non-transacting versus transacting treatment relative to all treatment firms (2.8 vs. 1.29 percentage points), although this incremental difference is not statistically significant.

TABLE 8

Filing Decision Conditional on Survey Participation and Responses

	(1) *Restricted*	(2) *Restricted*	(3) *Restricted*	(4) *Restricted*	(5) *Restricted*
Constant	0.262***	0.371***	–	0.501***	0.405***
	(42.82)	(15.06)		(9.21)	(18.81)
Treatment Variables:					
Treatment	0.034***	–	–	–	–
	(4.91)				
Non-transacting	–	–	0.034	–	–
			(0.72)		
Transacting	–	–	–	–	–
Survey Variables:					
Survey Participation	0.140***	–	–	–	–
	(7.74)				
Unaware of Option	–	0.130***	0.125***	0.102***	–
		(3.67)	(2.59)	(2.70)	
Q7 (Benefits / Costs)	–	–	–	−0.033***	–
				(−2.74)	
Q1 (No CEO Involved)	–	–	–	–	0.097**
					(2.54)
Controls:	No	No	Yes	No	No
Fixed Effects					
Industry	No	No	Yes	No	No
District	No	No	Yes	No	No
Adjusted R^2	0.004	0.016	0.042	0.026	0.007
N (= Number of firms)	25,724	774	667	742	772

This table presents the propensity of firms restricting their 2015 financial statements depending on survey participation and survey responses. Column 1 contains the final sample (see table 1). All remaining columns are restricted to survey participants. *Survey Participation* is an indicator variable that is coded as "1" if a firm replied to at least one survey question (and "0" otherwise). *Unaware of Option* is an indicator variable that is coded as "1" if a firm indicated in the survey that it was unaware of the restrict-access option while filing the 2014 financial statements (see the notes to table D2 for details). *Q7 (Benefits / Costs)* is the consolidated response to Question 7. Values range from 1 to 7 with higher values indicating that a specific stakeholder is viewed more positively for the filing decision. *Q1 (No CEO Involved)* is coded as "1" if the response to Question 1 indicates that the CEO is typically not involved in the filing of the financial statements and "0" otherwise. For the remaining variable definitions see the notes to table 2 and table 3. Column 3 contains fewer observations due to missing or unique industry and district values. Columns 4 and 5 only contain treatment firms that replied to Question 7 (see table 7) or Question 1 (see table D2), respectively. *t*-statistics are based on robust standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

to Question 7 and Question 1. Column 4 shows that firms are more likely to restrict access to their financial statement if they have a more negative view of a particular stakeholder. This effect is incremental to the *Unaware* indicator variable. Finally, in the last column, we show that firms for which the CEO (based on Question 1) was previously not involved in the 2014 filing decision react more strongly to the information treatment. This result is consistent with the arguments of Atkin et al. [2017], who argue that seemingly "wrong" decisions are made in an organization when the owner-manager is unaware about all options and their implications. For example, in our case, tax advisors might have little incentives to inform the CEO about the disclosure option even if the CEO would prefer exercising the option.

Taken together, our survey-based findings support the results from the field experiment by providing evidence that the TOT effect has a magnitude that resonates well with the ITT effect. Further, they support our interpretation that the main effect is predominantly driven by the informational component of the treatment and not by a general nudging effect triggered by our intervention. Finally, the survey response about the perceived cost and benefits associated with different stakeholder groups aligns with our conclusions from the field experiment as well as with the actual behavior of our survey respondents.

4.6 REAL EFFECTS OF (NON)DISCLOSURE

An obvious follow-up question is to what extent this disclosure decision has real effects (e.g., Shroff [2016]). Because restricted financial statements are unobservable to us, we cannot directly assess the impact of the restriction option on firms' financials. Instead, inspired by the entrepreneurship literature (e.g., Kerr, Lerner, and Schoar [2014], Bernstein et al. [2023]), we collect data on firms' website status as our primary proxy for firm activity or exit. We define a firm as operational if they still have an active website in the Wayback machine by *Archive.org* after 2021 (e.g., Bourveau, Boulland, and Breuer [2021], Bourveau, Breuer, and Muhn [2022]). Based on this measure, 74.9% of all 22,060 firms for which we have website address data are still active after 2021. In additional tests, we also enrich our website activity data with data from the credit agency *Verband der Vereine Creditreform e.V. (VVC)*. We provide details on the construction of the sample and the outcome variables in online appendix F.

Equipped with these measures, we regress firm activity indicators on treatment assignment in table 9. Column 1 shows that treated firms are 2.0 percentage points more likely to maintain an active website after 2021, compared to a baseline survival rate of 73.2%. In column 2, firms that received the non-transacting stakeholder treatment exhibit a larger, though insignificant, effect compared to those receiving the transacting stakeholder treatment. Although insignificant, this finding is directionally consistent with the higher restriction rate for the non-transacting stakeholder treatment. Column 3 introduces the *Survey Participation* variable from table 8. Consistent with the higher restriction rate for survey participants, these firms also show a higher likelihood of maintaining an active website years after the experiment.[35] In column 4 and column 5, we enrich our website activity data with data from VVC and continue to find a significant effect on firm activity. For example, according to column 4, treatment firms have a 1.8 percentage points higher likelihood of being operational according to both website activity data and VVC.

[35] However, because *Survey Participation* is a voluntary firm choice, interpreting this coefficient is more challenging compared to the coefficient of *Treatment*.

TABLE 9
Differences in Future Firm Activity by Treatment Status

	(1) *Active Website*	(2) *Active Website*	(3) *Active Website*	(4) *Both(Website, VVC)*	(5) *Any(Website, VVC)*
Constant (= No email)	0.732***	0.732***	0.732***	0.613***	0.900***
	(110.08)	(110.07)	(110.07)	(83.69)	(199.32)
Test Variables:					
Treatment	0.020***	–	0.019**	0.018**	0.009*
	(2.73)		(2.53)	(2.23)	(1.81)
Non-transacting	–	0.021***	–	–	–
		(2.61)			
Transacting	–	0.019**	–	–	–
		(2.39)			
Survey Participation	–	–	0.037**	–	–
			(2.31)		
Control Variables:	No	No	No	No	No
Fixed Effects					
Industry	No	No	No	No	No
District	No	No	No	No	No
P-value from *F*-Test:					
Non-transacting = Transacting	–	0.779	–	–	–
Adjusted R^2	0.000	0.000	0.000	0.000	0.000
N (= Number of firms)	22,060	22,060	22,060	22,060	22,060

This table analyzes future firm activity conditional on treatment assignment for 22,060 firms with available website data. Relative to our main analyses, 3,229 firms were removed due to missing website address data in Dafne (as of December 2016) and 435 firms since archive.org never crawled their website. We regress binary firm activity proxies on treatment group indicators. In column 1 to 3 we use the indicator variable *Active Website* that is coded as "1" if a firm has a valid website copy on archive.org after 2021 and "0" otherwise. In column 4, we code the outcome variable as "1" if the firm has both an Active Website and a status of "active" or "merged" according to the *Verband der Vereine Creditreform e.V.* (*VVC*) data item in Dafne (download date November 2023). In column 5, we code the outcome variable as "1" if the firm either has an *Active Website* or an "active" or "merged" *VVC* status. Column 3 also contains an indicator variable for treated firms that participated in our survey (*Survey Participation*, as defined in table 8). The remaining variables are defined in the notes to table 2. *t*-statistics are based on robust standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

We conduct various robustness tests in online appendix F. For example, we show that the results are robust to adding control variables and fixed effects. Additionally, we draw a random sample of 250 firms and manually verify the quality of our different firm activity proxies. Finally, we also show the importance of randomization-based inference by contrasting the results to an analysis that directly analyzes the correlation between firms' (endogenous) restriction decision and firm activity. Unlike our analysis that relies on true random variation, the resulting negative correlation from this alternative analysis is spurious because the decision to restrict access is correlated with various confounders (see also Brandon et al. [2024], who contrast observational with experimental evidence).

Taken together, we find that treated firms are significantly more likely to be active compared to control firms. As treatment firms have a higher restriction likelihood, this result supports the notion that mandatory public disclosure is potentially harmful for small private firms. Although there are multiple explanations that should be further explored (e.g., product market predation as in Bernard [2016], reputational cost concerns of credit rating agencies as in Vanhaverbeke, Balsmeier, and Doherr [2022], or increased demand for alternative information sources), these initial findings suggest that informational frictions have a meaningful impact on private firms. Additionally, they highlight that the decision of firms to restrict information is a critical disclosure choice for private firms in our sample.

4.7 COMPARABILITY OF FIRMS

In this study, we establish that the small private firms in our sample have negative net benefits of disclosure and high information processing costs. However, as our sample is limited to microfirms, we cannot directly test whether these tradeoffs are generalizable to larger private firms. To provide some evidence on the generalizability of our findings, we conduct a survey using the business panel by *Dynata*. Specifically, we survey 268 professionals working for German firms of various sizes about multiple disclosure topics. We then corroborate these results by utilizing survey data from Minnis and Shroff [2017] and the German Business Panel (Bischof et al. [2024]).

We provide the results in online appendix G. Collectively, these results suggest that smaller private firms (unsurprisingly) have lower perceived net benefits from disclosure, but these differences are less extreme as one might think.[36] More importantly, our survey results only provide limited evidence that larger private firms have substantially lower informational constraints than smaller private firms. These results tentatively suggest that also larger private firms should experience such informational frictions (albeit perhaps to a somewhat lower extent). Therefore, we believe that the economic forces studied in our study should be at least “locally generalizable” to somewhat larger private firms (Al-Ubaydli and List [2015]).

5. *Conclusion*

A significant literature examines the determinants of private firm disclosure choices. Doing so, it more or less implicitly assumes that private firm decision makers base their disclosure decisions on a reasonable complete set of information. However, using a large-scale field experiment in Germany, we are able to provide causal evidence that information frictions are a key factor in the disclosure choices of small German firms. Our information treatment has a large impact on a firm’s decision to restrict access to its financial statements as firms were previously unaware of the restriction

[36] If anything, these lower net benefits are consistent with mandatory disclosure requirements being more harmful for very small private firms. Considering that disclosures from smaller private firms should yield fewer positive externalities, it raises legitimate doubts about the welfare-enhancing effects of these disclosure requirements. This analysis therefore reinforces the findings from section 4.6.

option. Using out-of-sample survey data from various sources, we document that this finding can be expected to be locally generalizable to larger private firms. In that sense, our paper provides a possible microfoundation on how new disclosure equilibria arise via learning over time. It also suggests that loosening regulation might be relatively less effective than tightening regulation because only the latter is coupled with enforcement actions.

We also find that small private firms are relatively less likely to restrict access to their financial statements when they take transacting, relative to non-transacting, stakeholders into consideration when making their disclosure decision. This result provides causal evidence that balancing the potential net benefits of providing information to transacting stakeholders against the costs of releasing information to non-transacting stakeholders is the key tradeoff in a small firm's voluntary disclosure decision. This finding supports larger firm evidence that has documented this tradeoff. Given that small private firms are an important building block of economies worldwide, it seems comforting that prior results on the determinants of firm disclosure decisions also seem to apply to the small end of firms within economies. We also find suggestive evidence that competitors (capital providers) are the main reason why these firms choose to be financially opaque (remain transparent). Our findings are strengthened by triangulating our field-based evidence with survey results.

Finally, we also provide early causal evidence that financial disclosures and their restrictions by small private firms are consequential. Using firm-level website data to proxy whether firms remain active years after the experiment, we show that treated firms are more likely to maintain business activity, suggesting that direct and/or indirect costs of disclosures are potentially meaningful for these firms. Future research can directly explore the mechanism (e.g., product market predation) for these results.

REFERENCES

Abadie, A.; S. Athey; G. W. Imbens; and J. Wooldridge. "When Should You Adjust Standard Errors for Clustering?" *The Quarterly Journal of Economics* 138 (2023): 1–35.

Al-Ubaydli, O., and J. A. List. "On the Generalizability of Experimental Results in Economics." In: *Handbook of Experimental Economic Methodology*, edited by G. R. Fréchette and A. Schotter. New York: Oxford University Press, 2015.

Allee, K. D., and T. L. Yohn. "The Demand for Financial Statements in an Unregulated Environment: An Examination of the Production and Use of Financial Statements by Privately Held Small Businesses." *The Accounting Review* 84 (2009): 1–25.

Athey, S.; J. Tibshirani; and S. Wager. "Generalized Random Forests." *The Annals of Statistics* 47 (2019): 1148–78.

Atkin, D.; A. Chaudhry; S. Chaudry; A. K. Khandelwal; and E. Verhoogen. "Organizational Barriers to Technology Adoption: Evidence from Soccer-Ball Producers in Pakistan." *The Quarterly Journal of Economics* 132 (2017): 1101–64.

Badertscher, B.; N. Shroff; and H. D. White. "Externalities of Public Firm Presence: Evidence from Private Firms' Investment Decisions." *Journal of Financial Economics* 109 (2013): 682–706.

Baik, B. K.; N. Berfeld; and R. S. Verdi. "Do Public Financial Statements Influence Venture Capital and Private Equity Financing?" Working Paper, Harvard Business School, 2023.

Balakrishnan, K.; M. B. Billings; B. Kelly; and A. Ljungqvist. "Shaping Liquidity: On the Causal Effects of Voluntary Disclosure." *Journal of Finance* 69 (2014): 2237–78.

Ball, R., and L. Shivakumar. "Earnings Quality in UK Private Firms: Comparative Loss Recognition Timeliness." *Journal of Accounting and Economics* 39 (2005): 83–128.

Banerjee, A. V., and E. Duflo. "The Experimental Approach to Development Economics." *Annual Review of Economics* 1 (2009): 151–78.

Banerjee, S.; S. Dasgupta; R. U. I. Shi; and J. Yan. "Information Complementarities and the Dynamics of Transparency Shock Spillovers." *Journal of Accounting Research* 62 (2023): 55–99.

Berger, P. G.; J. H. Choi; and S. Tomar. "Breaking It Down: Economic Consequences of Disaggregated Cost Disclosures." Working Paper, University of Chicago, 2023.

Berger, P. G.; M. Minnis; and A. Sutherland. "Commercial Lending Concentration and Bank Expertise: Evidence from Borrower Financial Statements." *Journal of Accounting and Economics* 64 (2017): 253–77.

Bernard, D. "Is the Risk of Product Market Predation a Cost of Disclosure?" *Journal of Accounting and Economics* 62 (2016): 305–25.

Bernard, D.; D. Burgstahler; and D. Kaya. "Size Management by European Private Firms to Minimize Proprietary Costs of Disclosure." *Journal of Accounting and Economics* 66 (2018): 94–122.

Bernard, D.; D. Kaya; and J. Wertz. "Entry and Capital Structure Mimicking in Concentrated Markets: The Role of Incumbents' Financial Disclosures." *Journal of Accounting and Economics* 71 (2021): 101379.

Bernstein, S.; E. Colonnelli; M. Hoffman; and B. Iverson. "Life after Death: A Field Experiment with Small Businesses on Information Frictions, Stigma, and Bankruptcy." NBER Working Paper, 2023.

Beuselinck, C.; F. Elfers; J. Gassen; and J. Pierk. "Private Firm Accounting: The European Reporting Environment, Data and Research Perspectives." *Accounting and Business Research* 53 (2023): 38–82.

Bischof, J.; P. Doerrenberg; D. Rostam-Afschar; D. Simons; and J. Voget. "The German Business Panel: Firm-Level Data for Accounting and Taxation Research." *European Accounting Review* (forthcoming) 2024.

Blankespoor, E.; E. deHaan; and I. Marinovic. "Disclosure Processing Costs, Investors' Information Choice, and Equity Market Outcomes: A Review." *Journal of Accounting and Economics* 70 (2020): 101344.

Blankespoor, E.; E. D. deHaan; J. Wertz; and C. Zhu. "Why Do Individual Investors Disregard Accounting Information? The Roles of Information Awareness and Acquisition Costs." *Journal of Accounting Research* 57 (2019): 53–84.

Bloom, N.; B. Eifert; A. Mahajan; D. McKenzie; and J. Roberts. "Does Management Matter? Evidence from India." *The Quarterly Journal of Economics* 128 (2013): 1–51.

Bourveau, T.; R. Boulland; and M. Breuer. "A New Measure of Voluntary Disclosure: Evidence from Corporate Websites." Working Paper, Columbia Business School, 2021.

Bourveau, T.; M. Breuer; and M. Muhn. "How Private Companies Win the Market's Attention." Working Paper, University of Chicago, 2022.

Bourveau, T.; M. Breuer; and R. Stoumbos. "Learning to Disclose: Disclosure Dynamics in the 1890s Streetcar Industry." Working Paper, Columbia Business School, 2023.

Brandon, A.; C. Camerer; J. List; I. Muir; and J. Wang. "Evaluating the Evidence on Daily Income Targeting with Experimental and Observational Data." Working Paper, Johns Hopkins University, 2024.

Breuer, M. "How Does Financial-Reporting Regulation Affect Market-Wide Resource Allocation?" *Journal of Accounting Research* 59 (2021): 59–110.

Breuer, M., and E. deHaan. "Using and Interpreting Fixed Effects Models." *Journal of Accounting Research* 62 (2024): 1183–226.

Breuer, M.; K. Hombach; and M. A. Müller. "How Does Financial Reporting Regulation Affect Firms' Banking?" *The Review of Financial Studies* 31 (2018): 1265–97.

Breuer, M.; K. Hombach; and M. A. Müller. "When You Talk, I Remain Silent: Spillover Effects of Peers' Mandatory Disclosures on Firms' Voluntary Disclosures." *The Accounting Review* 97 (2022): 155–86.

Breuer, M.; K. Hombach; and M. A. Müller. "The Economics of Firms' Public Disclosure: Theory and Evidence." Working Paper, Columbia Business School, 2023.

Brown, L. D.; A. C. Call; M. B. Clement; and N. Y. Sharp. "Inside the 'Black Box' of Sell-Side Financial Analysts." *Journal of Accounting Research* 53 (2015): 1–47.

Bruhn, M.; D. Karlan; and A. Schoar. "The Impact of Consulting Services on Small and Medium Enterprises: Evidence from a Randomized Trial in Mexico." *Journal of Political Economy* 126 (2018): 635–87.

Bundesanzeiger. "Enormes Interesse Am Abruf Von Jahresabschlüssen [Enormous Interest in Accessing Financial Statements]," 2012: Available from https://www.bundesanzeiger.de/pub/Abrufe%20von%20Jahresabschluessen.pdf.

Burgstahler, D. C.; L. Hail; and C. Leuz "The Importance of Reporting Incentives: Earnings Management in European Private and Public Firms." *The Accounting Review* 81 (2006): 983–1016.

Cassar, G. "Financial Statement and Projection Preparation in Start-up Ventures." *The Accounting Review* 84 (2009): 27–51.

Cassar, G.; C. D. Ittner; and K. S. Cavalluzzo. "Alternative Information Sources and Information Asymmetry Reduction: Evidence from Small Business Debt." *Journal of Accounting and Economics* 59 (2015): 242–63.

Cavallo, A.; G. Cruces; and R. Perez-Truglia "Inflation Expectations, Learning, and Supermarket Prices: Evidence from Survey Experiments." *American Economic Journal: Macroeconomics* 9 (2017): 1–35.

Charness, G.; U. Gneezy; and M. A. Kuhn. "Experimental Methods: Between-Subject and Within-Subject Design." *Journal of Economic Behavior & Organization* 81 (2012): 1–8.

Christensen, H. B.; L. Hail; and C. Leuz. "Capital-Market Effects of Securities Regulation: Prior Conditions, Implementation, and Enforcement." *The Review of Financial Studies* 29 (2016): 2885–924.

Coibion, O.; Y. Gorodnichenko; and S. Kumar. "How Do Firms Form Their Expectations? New Survey Evidence." *American Economic Review* 108 (2018): 2671–713.

Cole, S.; X. Giné; and J. Vickery. "How Does Risk Management Influence Production Decisions? Evidence from a Field Experiment." *The Review of Financial Studies* 30 (2017): 1935–70.

Dedman, E.; and C. Lennox. "Perceived Competition, Profitability and the Withholding of Information About Sales and the Cost of Sales." *Journal of Accounting and Economics* 48 (2009): 210–30.

Diamond, D. W.; and R. E. Verrecchia. "Disclosure, Liquidity, and the Cost of Capital." *Journal of Finance* 46 (1991): 1325–59.

Duflo, E.; M. Greenstone; R. Pande; and N. Ryan. "Truth-Telling by Third-Party Auditors and the Response of Polluting Firms: Experimental Evidence from India." *The Quarterly Journal of Economics* 128 (2013): 1499–545.

Ellis, J.; C. E. Fee; and E. Thomas Shawn. "Proprietary Costs and the Disclosure of Information About Customers." *Journal of Accounting Research* 50 (2012): 685–727.

European Commission. "Fitness Check on the EU Framework for Public Reporting by Companies." Commission Staff Working Document, 2021.

Floyd, E.; M. Hallsworth; J. A. List; R. D. Metcalfe, K. Rotaru; and I. Vlaev. "The Role of Enforcement Action Uncertainty on Tax Compliance: Evidence from Three Experiments." Working Paper, University of California San Diego, 2023.

Floyd, E., and J. A. List. "Using Field Experiments in Accounting and Finance." *Journal of Accounting Research* 54 (2016): 437–75.

Graham, J. R. "Presidential Address: Corporate Finance and Reality." *The Journal of Finance* 77 (2022): 1975–2049.

Graham, J. R.; M. Hanlon; T. Shevlin; and N. Shroff. "Incentives for Tax Planning and Avoidance: Evidence from the Field." *The Accounting Review* 89 (2014): 991–1023.

Graham, J. R.; M. Hanlon; T. Shevlin; and N. Shroff. "Tax Rates and Corporate Decision-Making." *The Review of Financial Studies* 30 (2017): 3128–75.

Guzman, J.; J. J. Oh; and A. Sen. "What Motivates Innovative Entrepreneurs? Evidence from a Global Field Experiment." *Management Science* 66 (2020): 4808–19.

Haaland, I.; C. Roth; and J. Wohlfart. "Designing Information Provision Experiments." *Journal of Economic Literature* 61 (2023): 3–40.

Hail, L.; M. Muhn; and D. Oesch. "Do Risk Disclosures Matter When It Counts? Evidence from the Swiss Franc Shock." *Journal of Accounting Research* 59 (2021): 283–330.

Hayes, R. M.; and R. Lundholm. "Segment Reporting to the Capital Market in the Presence of a Competitor." *Journal of Accounting Research* 34 (1996): 261–79.

Heinrichs, A. "Investors' Access to Corporate Management: A Field Experiment About 1-on-1 Calls." Working Paper, University of Chicago, 2014.

Kalemli-Ozcan, S.; B. Sorensen; C. Villegas-Sanchez; V. Volosovych; and S. Yesiltas. "How to Construct Nationally Representative Firm Level Data from the Orbis Global Database: New Facts on Smes and Aggregate Implications for Industry Concentration." *American Economic Journal: Macroeconomics* 16 (2023): 353–74.

Kausar, A.; N. Shroff; and H. White. "Real Effects of the Audit Choice." *Journal of Accounting and Economics* 62 (2016): 157–81.

Kerr, W. R.; J. Lerner; and A. Schoar. "The Consequences of Entrepreneurial Finance: Evidence from Angel Financings." *The Review of Financial Studies* 27 (2014): 20–55.

Kirgios, E. L.; A. Rai; E. H. Chang; and K. L. Milkman. "When Seeking Help, Women and Racial/Ethnic Minorities Benefit from Explicitly Stating Their Identity." *Nature Human Behaviour* 6 (2022): 383–91.

Kumar, S.; Y. Gorodnichenko; and O. Coibion. "The Effect of Macroeconomic Uncertainty on Firm Decisions." *Econometrica* 91 (2023): 1297–332.

Lawless, M. "Age or Size? Contributions to Job Creation." *Small Business Economics* 42 (2014): 815–30.

Leonelli, S.; M. Muhn; T. Rauter; and G. Sran. "How Do Consumers Use Firm Disclosure? Evidence from a Randomized Field Experiment." Working Paper, University of Chicago, 2023.

Leuz, C. "Different Approaches to Corporate Reporting Regulation: How Jurisdictions Differ and Why." *Accounting and Business Research* 40 (2010): 229–56.

Leuz, C., and P. D. Wysocki. "The Economics of Disclosure and Financial Reporting Regulation: Evidence and Suggestions for Future Research." *Journal of Accounting Research* 54 (2016): 525–622.

Lynch, B. "Hidden Mandatory Disclosures." Working Paper, Wharton Business School, 2022.

McKenzie, D. "Identifying and Spurring High-Growth Entrepreneurship: Experimental Evidence from a Business Plan Competition." *American Economic Review* 107 (2017): 2278–307.

Milgrom, P. R. "Good News and Bad News: Representation Theorems and Applications." *The Bell Journal of Economics* 12 (1981): 380–91.

Minnis, M. "The Value of Financial Statement Verification in Debt Financing: Evidence from Private U.S. Firms." *Journal of Accounting Research* 49 (2011): 457–506.

Minnis, M., and N. Shroff. "Why Regulate Private Firm Disclosure and Auditing?" *Accounting and Business Research* 47 (2017): 473–502.

Minnis, M., and A. Sutherland. "Financial Statements as Monitoring Mechanisms: Evidence from Small Commercial Loans." *Journal of Accounting Research* 55 (2017): 197–233.

Perez-Truglia, R., and G. Cruces. "Partisan Interactions: Evidence from a Field Experiment in the United States." *Journal of Political Economy* 125 (2017): 1208–43.

Path, R. "The Hidden Metrics of Email Deliverability—2016 Industry Benchmarks for 7 Key Measurements." White Paper, 2017.

Samuels, D. "Government Procurement and Changes in Firm Transparency." *The Accounting Review* 96 (2021): 401–30.

Shroff, N. "Discussion of 'Is the Risk of Product Market Predation a Cost of Disclosure?'." *Journal of Accounting and Economics* 62 (2016): 326–32.

Shroff, N. "Corporate Investment and Changes in GAAP." *Review of Accounting Studies* 22 (2017): 1–63.

Shroff, N.; R. S. Verdi; and B. P. Yost. "When Does the Peer Information Environment Matter?" *Journal of Accounting and Economics* 64 (2017): 183–214.

Vanhaverbeke, S.; B. Balsmeier; and T. Doherr. "Mandatory Financial Information Disclosure and Credit Ratings." ZEW Working Paper, 2022.

Verrecchia, R. E. "Discretionary Disclosure." *Journal of Accounting and Economics* 5 (1983): 179–94.

Wagenhofer, A. "Voluntary Disclosure with a Strategic Opponent." *Journal of Accounting and Economics* 12 (1990): 341–63.

Zetlin-Jones, A., and A. Shourideh. "External Financing and the Role of Financial Frictions over the Business Cycle: Measurement and Theory." *Journal of Monetary Economics* 92: (2017) 1–15.

Zimmerman, J. L. "Myth: External Financial Reporting Quality Has a First-Order Effect on Firm Value." *Accounting Horizons* 27 (2013): 887–94.

Zwick, E. "The Costs of Corporate Tax Complexity." *American Economic Journal: Economic Policy* 13 (2021): 467–500.