How Does Financial-Reporting Regulation Affect Industry-wide Resource Allocation?

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Abstract

This paper examines the impact of mandatory reporting and auditing of firms' financial statements on industry-wide resource allocation. Using threshold-induced variation in the share of mandated firms in a given industry, I document that reporting mandates facilitate ownership dispersion in capital markets and spur competition in product markets. I, however, do not find that reporting mandates unambiguously improve the efficiency of industry-wide resource allocation. With respect to auditing mandates, I find only that they impose a fixed cost on firms, deterring smaller entrants.

Keywords: Financial-Reporting Regulation; Disclosure; Auditing; Competition; Resource Allocation

JEL classification: K22, L51, M41, M42, M48, O43, O47

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1. Introduction

Regulations mandating the reporting and auditing of firms' financial statements are ubiquitous. In the United States, reporting and auditing mandates are a centerpiece of securities regulation. They require public firms to disclose audited financial statements to instill investor confidence in public capital markets. In the European Union (EU) and other parts of the world, similar mandates aimed at protecting firms' various stakeholders are part of corporate law, applying even to private firms. Yet, although reporting and auditing mandates are imposed on several million firms across the globe to aid resource allocation, their desirability remains an open question.

The literature provides evidence on firm-level costs (e.g., audit fees) and benefits (e.g., liquidity) of reporting and auditing mandates incurred by mandated firms (for a review, see Leuz and Wysocki [2016]). This evidence, however, cannot directly speak to the overall desirability of the mandates, as it provides little guidance on aggregate costs and benefits. It also tends to neglect externalities of mandated firms' reporting and auditing, even as such externalities are offered up to justify the mandates.

In this paper, I examine the effects of reporting and auditing mandates on *aggregate* resource allocation. I specifically investigate how subjecting more firms in an industry to reporting or auditing mandates affects the resource allocation in the entire industry. My focus on aggregate effects provides two benefits. First, aggregate effects naturally weigh and combine various firm-level costs and benefits of the mandates. Second, aggregate effects capture not only the direct effect on mandated firms, but also externalities affecting other firms. As a relevant aggregation level, I focus on the industry level because information and competitive (e.g., business stealing) externalities should be most pronounced among firms in the same industry (e.g., Foster [1981], Aghion and Howitt [1992]). While clearly not perfect (e.g., cross-industry spillovers are neglected), my focus on the industry level offers a useful first step away from the firm level toward an aggregate assessment of the mandates.[1]

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[1] To define industries, I use the four-digit NACE classification, which is the finest classification consistently coded across European countries. While this fine classification aids the measurement of outcome variables (e.g., productivities) and

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Reporting and auditing mandates can affect industry-wide resource allocation in various ways. The literature argues the mandates, by forcing reliable public reporting, can promote a transactional *type* of resource allocation, deemphasizing the importance of close relationships for allocating resources (e.g., Rajan and Zingales [2003]). In capital markets, for example, the mandates can reduce information asymmetries between firms and prospective investors, allowing firms to disperse their ownership more widely (e.g., La Porta et al. [2006]). In product markets, the mandates can help prospective entrants spot profitable niches and prospective customers identify low-cost producers, increasing competition. To capture the mandates' impact on the type of allocation, I examine market-structure measures such as the concentration of ownership, the concentration of market share, and business dynamism (entry and exit rates).

The mandates' impact on the *efficiency* of resource allocation is a priori unclear (Zingales [2009]). They may improve allocative efficiency if their benefits offset their costs. By revealed preference, the mandated firms' benefits of expanded reporting and auditing fall short of the additional costs. Accordingly, positive externalities enjoyed by others are crucial for the mandates to help aggregate resource allocation. The literature provides several such examples for reporting (e.g., Badertscher et al. [2013], Shroff et al. [2017]). A firm's mandatory reporting, for example, could help its competitors invest more efficiently and avoid duplicate market-intelligence efforts. If this helps competitors more than the loss of proprietary information hurts the reporting firm, this mandate improves the aggregate allocation of resources (see Roychowdhury et al. [2019] for discussion). The literature provides fewer examples of positive externalities for auditing (e.g., Donovan et al. [2014], Minnis and Shroff [2017]). Nevertheless, it is an empirical question whether reporting and auditing mandates help or hurt aggregate allocative efficiency. To capture the mandates' impact on allocative efficiency, I examine market-performance measures such as the within-industry dispersion of productivities (Hsieh and Klenow [2009]), the covariation of market shares and productivities (Bartelsman et al. [2013]),

increases power, it may miss important externalities spilling over the boundaries of four-digit industries. In untabulated results, I find that my inferences remain largely unchanged when I use coarser three-digit or two-digit classifications.

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andaggregate productivity levels and growth (Basu et al. [2010], Fuchs et al. [2016]). Following the literature, I view a narrow productivity dispersion (e.g., due to competitor learning), a positive size-productivity covariance (e.g., due to customers reallocating business to more efficient firms), and, above all, high aggregate productivity levels and growth (e.g., due to a faster reallocation of resources) as indicators of an efficient resource allocation.

To test the industry-wide effects of reporting and auditing mandates, the EU financial-reporting regulation implemented by members of the European Economic Area (EEA) provides a suitable setting. It stipulates that limited-liability firms—private and public ones—must prepare and publish a full set of audited financial statements. Exemptions from the reporting and auditing requirements are granted to private firms below size thresholds related to firms' total assets, sales, and number of employees. Typically, firms exempted from reporting requirements are allowed to publish highly abbreviated financial statements, and those exempted from auditing requirements can forgo auditing. The extent of reporting and auditing exemptions and, in particular, the exemption thresholds vary by country. The exemption thresholds also differ between reporting and auditing in multiple countries. Some countries exempt more firms from auditing requirements than from full reporting requirements, and vice versa. [2]

In my empirical design, I exploit variation in the extent or *scope* of reporting and auditing mandates arising from the size-based regulation. Following the EU's measurement of regulatory scope (European Commission [2009]), I use the share of firms exceeding a given country's exemption thresholds in a given industry as my measure of scope. An important feature of this measure is that it not only varies at the country level, as a result of variation in thresholds across countries, but also at the industry level, as a result of variation in firm-size distributions across industries. A threshold exempting firms below 50 employees from auditing mandates, for example, has a different scope in labor-intensive industries, where more firms will tend to have 50-plus employees, than in capital-

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[2] Countries may require audits even absent an expanded public reporting mandate, for example, to ensure that outsiders obtain credible abridged information publicly, shareholders obtain credible information privately, and firms obtain external expert advice. Countries may also mandate auditing to fight money laundering or tax evasion.

intensive industries. This feature allows accounting for regulators' endogenous choice of country-level thresholds, an important concern in empirical studies of regulatory effects, by exploiting variation in scope within a country at a point in time.

Using a cross-sectional difference-in-differences design, I focus on variation in regulatory scope arising from the differential impact of countries' thresholds on distinct industries (e.g., labor-versus capital-intensive industries). I control for systematic differences across countries (e.g., legal origins) and industries (e.g., capital intensity) using country-year and industry-year fixed effects. To ensure that the remaining country-industry-specific variation in scope is not unduly confounded by endogenous differences and changes in countries' industry structure (e.g., industrial specialization), I purge my scope measure of variation arising from countries' endogenous differences and changes in firm-size distributions. Following the simulated instruments approach (Currie and Gruber [1996], Mahoney [2015]), I use one *standardized* firm-size distribution per industry across all countries and years to calculate the regulatory scope. This standardized scope acts as an instrument for a country's actual share of mandated firms in a given industry. It is a relevant instrument because it varies with both the given country's actual thresholds and typical firm-size distribution differences across the industries operating in that country. Unlike a country's actual share of mandated firms, however, the standardized scope does *not* vary with the country's endogenous differences (e.g., industrial specialization) and changes (e.g., firm growth) in its industries' firm-size distributions.[3] (For details on the design and the standardized scope, refer to sections 4 and 6.1.)

I find reporting mandates support the dispersion of ownership, as shown by an increased share of publicly listed firms, a larger number of shareholders, and a greater dispersion of control rights. In product markets, I find they promote competition, as shown by increased entry and exit rates, reduced market-share concentration, and a reduced dispersion of profit margins. My instrumented estimates suggest mandating an additional 10% of firms in an industry to publicly disclose full financial

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[3] Bernard et al. [2018] show that a country's threshold-based regulation impacts its firm-size distribution as firms attempt to manage their size downward to avoid the regulation. By using a standardized distribution, instead of the country's actual distribution, my scope measure is purged of such endogenous variation in the share of firms above the thresholds.

statements, for instance, increases the product-market entry rate by 6% relative to its average. Taken together, the capital- and product-market results suggest reporting mandates facilitate a transactional type of resource allocation.

Regarding the efficiency of resource allocation, I find mixed evidence. While some evidence points towards a positive effect of reporting mandates (e.g., a reduced dispersion of revenue productivities), I do not find clear evidence that the mandates help or hurt aggregate productivity and its growth. Regarding productivity levels, for example, I find some evidence of a positive effect. This effect, however, is of limited economic magnitude and not robust to using alternative productivity measures. Regarding productivity growth, by contrast, I find some evidence of a negative effect. While the sign of this effect is widely robust to using alternative growth measures, its statistical significance is not. Accordingly, I cannot reject the null that reporting mandates do not significantly impact aggregate productivity levels and growth, the key measures of allocative efficiency.

With respect to auditing mandates, I find they deter entry, especially by smaller firms. Similarly, I find they raise the minimum required level of productivity to operate. I do not find any other effects. With a view to the economic magnitudes, my instrumented estimates suggest mandating an additional 10% of firms in an industry to obtain a financial-statement audit reduces, for instance, the product-market entry rate by 11% relative to its average.

Collectively, I interpret my results as suggesting that reporting mandates primarily change the way resources are allocated. The mandates appear to substitute a transactional type of resource allocation, based on public information, for a relational one, based on private information. This substitution, however, does not appear to unambiguously improve the allocation of resources (e.g., productivity growth), consistent with the literature on relational versus transactional economic systems (e.g., Dewatripont and Maskin [1995], Rajan and Zingales [1998b], La Porta et al. [2008]). One possible explanation for these findings is that, while others benefit from mandated firms' reporting (e.g., entrants), mandated firms themselves lose proprietary information, which deters their incentives to innovate and improve productivity (e.g., Arrow [1962]). These potential reallocative effects of

reporting mandates and their implications for innovation incentives warrant further investigation. Regarding auditing mandates, I interpret my results as suggesting that they impose fixed costs on mandated firms. These costs do not appear to be offset by significant externalities enjoyed by others in the same industry. While this does not immediately imply that audit mandates are undesirable, it at least suggests that future research may have to turn to different outcomes (e.g., tax collection or fraud) and aggregation levels (e.g., country level) to detect the benefits of audit *mandates*.

My paper contributes to the literature in several ways. It is a first attempt at assessing the net effects of reporting and auditing mandates on resource allocation at the industry level (see, e.g., Donovan et al. [2014], Leuz and Wysocki [2016], ICAEW [2016], Minnis and Shroff [2017], Roychowdhury et al. [2019] for calls for such research). Empirical evidence on these effects is key to understanding which among the many potential effects of financial-reporting mandates dominate at the industry level. It aids the development of descriptive theories and the design of efficient regulations. While my evidence does not provide immediate regulatory implications, as regulators may pursue objectives other than allocative efficiency (e.g., equality), it informs the debate about reporting and auditing mandates by documenting their impact on outcomes of first-order concern from the viewpoint of economic theory.

My paper adds to the vast literature on the proprietary cost of financial reporting. Several studies document that firms' concerns about proprietary costs affect their voluntary reporting choices (e.g., Berger [2011], Lang and Sul [2014]). My evidence extends this literature by document that firms' mandatory reporting invites competition in product markets, validating firms' concerns about proprietary costs of reporting. In this vein, my paper complements recent work by Bernard [2016] and Granja [2018].

More broadly, my paper contributes to the literature concerned with the effects of institutions and regulation on competition, resource allocation, and growth. Numerous studies investigate the effects of business regulation (e.g., labor protection or entry regulation) on competition and resource allocation (e.g., Loayza and Serven [2010]). I add to these studies by documenting that reporting

regulation, unlike most other business regulation, can foster competition and resource reallocation (as conjectured by Leuz and Wysocki [2016]). In this sense, my findings provide direct evidence for the conjecture of Rajan and Zingales [2003, 2003b] that transparency-enhancing financial-reporting regulation supports the functioning of competitive and dispersed capital and product markets. My findings, however, also echo prior evidence that institutions, such as financial-reporting regulation, determine the type of private contracting (e.g., relational versus transactional), but not necessarily the long-run growth of economies (e.g., Acemoglu and Johnson [2005], La Porta et al. [2008]).

2. Conceptual Underpinnings

Financial statements portray a firm's economic position and activities. A firm's balance sheet, for example, provides information on the mix of its operating assets and financing sources, while its income statement shows its operating profitability (e.g., cost structure, profit margin) and financing costs (e.g., interest expense). The cash flow statement, notes, and management report add further information on past and future financing and investing activities.

The information in these statements is useful to a firm's stakeholders. Investors, bankers, and suppliers, for example, can use it to determine financing terms (e.g., equity prices, interest rates, and trade credit terms). Customers can use it to find cost-efficient and financially stable suppliers. And competitors can use the information to spot profitable investment opportunities or financially constrained peers to prey on (e.g., Bernard [2016]).

The usefulness of financial statements to capital providers tends to motivate a firm's voluntary reporting (e.g., Beyer et al. [2010]). By contrast, the usefulness to competitors tends to discourage voluntary reporting, especially public reporting (e.g., Verrecchia [1983]). To minimize proprietary costs from information leakage to competitors, while still benefiting from reduced adverse selection concerns of capital providers, a firm can opt to share its statements privately with its capital providers. Private sharing is particularly viable if a firm relies on close relationships with few capital providers instead of dispersed and competitive capital markets. Consistent with this argument, the literature documents that private firms, in particular, prefer private sharing (e.g., Minnis and Shroff [2017]).

To benefit from reporting to capital providers, a firm's reported statements must be credible. Credibility can be enhanced by a third-party auditor, who verifies the firm's reported numbers. While this service is costly (consuming audit fees and management attention), it benefits the firm through better financing terms (e.g., Minnis [2011]). The literature documents that many firms, even private ones abstaining from public reporting, obtain audits or similar means of verification voluntarily (e.g., Watts and Zimmerman [1983], Minnis and Shroff [2017]).

Given firms' voluntary reporting and auditing incentives, it is a priori unclear whether mandates are called for. Mandates expand the reporting and auditing of firms that do *not* publicly report or buy audits voluntarily. These firms, by revealed preference, expect the costs of expanded reporting and auditing to exceed their benefits. Therefore, mandates need to provide compensating benefits to parties other than the mandated firms to aid aggregate resource allocation.

Reporting mandates can benefit relationship outsiders who, absent the mandates, would not obtain firms' financial statements. In capital markets, prospective capital providers, for example, could use a firm's mandatory report not only to compete with the firm's existing capital providers (Breuer et al. [2018]), but also to assess and finance other firms in the industry (Garmaise and Natividad [2016]). In product markets, competitors could use the report to spot profitable investment opportunities (Badertscher et al. [2013]). Similarly, customers could use the reports to identify the most efficient or stable firm in a given industry and could reallocate their business to this firm (Crawford et al. [2019]). By helping relationship outsiders, reporting mandates can be expected to shift the way resources are allocated from the relational toward the transactional type (e.g., characterized by dispersed ownership and dynamic product markets) (Rajan and Zingales [2003]). Whether such a shift increases the efficiency of aggregate resource allocation is unclear. It can help if the benefits enjoyed by competitors, for example, exceed the costs incurred by the mandated firms. It can hurt if instead the mandate damps the mandated firms' investment incentives more than it helps competitors make better investments (Zingales [2009]).

Unlike reporting mandates, auditing mandates per se do not grant relationship outsiders the benefit of information access. Accordingly, it is unclear which party, other than auditors, would benefit from these mandates. Survey evidence from Minnis and Shroff [2017] suggests there are few positive externalities of auditing, rendering voluntary auditing incentives sufficient and mandatory auditing superfluous. Still auditing mandates may improve resource allocation by enhancing the credibility of firms' mandatory reporting, thereby contributing to the externality of firms' reporting (e.g., Lennox and Pittman [2011], DeFond and Zhang [2014]). They may also help resource allocation if firms underestimate the value of audits for capital raising and their own decision making (e.g., Bae et al. [2017], Shroff [2017], Barrios et al. [2019]). Given these contradictory arguments, it is an empirical question whether and how reporting and auditing mandates affect industry-wide resource allocation.

3. Institutional Background

To establish a uniform regulatory framework for the common European economic market, the EU and its predecessors introduced the Fourth and Seventh Directives, called "Accounting Directives," in 1978 and 1983. These directives prescribe acceptable accounting practices and formats as well as reporting (comprising preparation and public disclosure) and auditing requirements for limited-liability firms to ensure the availability of comparable information across European countries (in particular, members of the EEA). Under the directives, firms must prepare and publicly disclose a full set of audited financial statements (European Parliament [2013]).

To reduce the regulatory burden for smaller firms, the regulation allows substantial exemptions from reporting and auditing requirements for private firms below thresholds related to firms' total assets, sales, and number of employees. The regulation sets maximum exemption thresholds to prevent a race to the bottom among member states in terms of mandated firms. EEA member states can choose to deviate from the maximum thresholds by exempting fewer firms from reporting and auditing requirements. The country-specific implementation has resulted in notable variation in the extent of exemptions (especially exemption thresholds) across countries, despite the common regulatory framework (e.g., Cna Interpreta [2011], Minnis and Shroff [2017]).

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Typical reporting exemptions allow smaller firms to publicly disclose highly aggregated balance-sheet and income-statement information (e.g., only showing major asset and liability classes instead of individual accounts), abbreviate notes to the financial statements, omit management reports (e.g., on the competitive position, investment and financing activities, and business risks and opportunities), and file their public disclosures within an extended period (ranging up to 13 months).[4] Smaller firms also are typically allowed to omit cash-flow statements in countries otherwise requiring firms to prepare and publicly disclose cash-flow-statement information. In a few countries (e.g., Germany), smaller firms are further allowed to omit income statements from their public disclosures. Irrespective of the reporting exemption, smaller firms must typically prepare a full set of financial statements and share these statements privately with their shareholders and tax authorities (e.g., Bernard et al. [2018]). Typical auditing exemptions allow smaller firms to forgo an audit.

Smaller firms are typically those not exceeding any two of three size thresholds, where the typical thresholds are about €4 million in total assets, €8 million in sales, and 50 employees. Although the thresholds for reporting and auditing exemptions often coincide, in several countries, the thresholds differ for reporting and auditing exemptions (e.g., Croatia, Denmark, France, Finland, Norway, and Sweden).

In this paper, I use the reporting- and auditing-exemption thresholds as a comparable summary measure of countries' extent of reporting and auditing regulation for three reasons. First, the exemption thresholds represent a key provision in countries' financial-reporting framework that is at the core of academic and practitioners' debates and regulators' reforms in Europe (e.g., European Commission [2008], ICAEW [2016], Minnis and Shroff [2017], Bernard et al. [2018]). Second, the thresholds affect many firms, typically around 90% of limited-liability firms, allowing them to markedly reduce their reporting and auditing. Third, the exemption thresholds strongly shape firm-

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[4] The literature suggests the disaggregation of financial-statement disclosures is an important dimension of disclosure quality (e.g., Hope and Thomas [2008], Chen et al. [2015]). For examples of exempted and non-exempted firms' reporting, click on the corresponding links (referring to the official publication platform (*Companies House*) of the United Kingdom) or refer to Table A1 in the Online Appendix.

level reporting and auditing according to prior studies (e.g., Lennox and Pittman [2011], Kausar et al. [2016], Breuer et al. [2018]).

4. Empirical Strategy

4.1. Scopes of reporting and auditing mandates

To identify the impact of reporting and auditing mandates on industry-wide resource allocation, I exploit variation in the extent or *scope* of these mandates. I measure the reporting and auditing scope as the share of non-exempted ("mandated") firms in a given country, industry, and year.[5] The reporting scope captures the share of firms in a given country, industry, and year that must publicly disclose a full set of financial statements, including extensive notes and a management report. The auditing scope captures the share of firms in a given country, industry, and year that must obtain a financial-statement audit.[6]

A key benefit of this regulatory scope measure is that it varies not only at the country level, as a result of differences in exemption thresholds, but also at the industry level, as a result of differences in firm-size distributions (Figure 1). For example, a threshold exempting firms below 50 employees from auditing requirements has a markedly different scope in labor-intensive industries, where more firms will have 50-plus employees, than in capital-intensive industries.

This country-industry variation in the regulatory scope permits accounting for concerns about the endogeneity of exemption thresholds at the country level and over time. Using country-year fixed effects, I can focus on regulatory variation in the same country at the same point in time that is not directly chosen by regulators. Regulators choose thresholds at the country level, whereas I focus on differential implications of these thresholds at the country-industry level. This contrasts with studies

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[5] I classify those firms exceeding two out of three size thresholds in a given year as "mandated." This classification represents the typical size-class determination rule of the countries in my sample. I do not account for additional variation in the precise determination rule across countries, for example, related to the number of years to look back in making the size determination. If a country only prescribes one or two thresholds, I require that all of these (i.e., one or two) be exceeded to be considered "mandated." I expect that, if anything, using my simplified size-determination rule introduces uncorrelated measurement error in my treatment, resulting in the attenuation bias.
[6] These scopes are similar in spirit to the public firm share used in Badertscher et al. [2013] as an industry-level measure of corporate transparency.

using cross-country differences in regulations or the timing of regulatory reforms. Cross-country differences and the timing of reforms are typically at the discretion of regulators.

While the country-industry variation in scope reduces concerns about threshold endogeneity, it arises from endogenous differences and changes in firm-size distributions (e.g., due to industrial specialization). This endogenous variation, reflected in the scope, raises important correlated variable and reverse causality issues. A country's policies to protect its most important industry, for example, can lead to larger firms and concentrated market shares in that industry, resulting in a positive correlation between the scope and concentration. Similarly, firm-size growth in a given industry can cause an increase in the share of firms exceeding a country's exemption thresholds, yielding a positive correlation between the scope and growth due to reverse causality.

To address concerns about the endogeneity of firm-size distributions, I purge my regulatory scope measure of endogenous country-industry-specific differences and changes in firm-size distributions, following the simulated instruments approach (Currie and Gruber [1996], Mahoney [2015]). In particular, I use one typical or *standardized* firm-size distribution per industry across all countries and years to calculate the regulatory scope. I obtain the standardized firm-size distributions by pooling all firm-level observations in a given industry across countries and years.[7] For each industry, I then calculate the key moments characterizing its pooled firm-size distribution: the averages, standard deviations, and pairwise correlations of (log) total assets, sales, and employees. Equipped with these industry-specific moments, I simulate 100,000 firms characterized by (log) values for total assets, sales, and employees using a multivariate normal distribution for each industry.[8]

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[7] I impose two sample restrictions to obtain the pooled cross-country sample. First, I restrict the sample to countries without a reporting exemption related to income statements. This restriction ensures sales information is available for all firms, not just for non-exempted ones, alleviating concerns over the truncation of the observable firm-size distribution. Second, I restrict the sample to fiscal years 2007 and later to ensure near-complete coverage of firms in my database. Starting from 2007, coverage in Amadeus is substantially more comprehensive for the majority of countries due to a coverage expansion in the years leading up to 2007 and increased electronic dissemination of firms' financial statements as a result of EU Directive 2003/58/EC.

[8] Size distributions in general and firm-size distributions in particular tend to be well approximated by Pareto or log-normal distributions (e.g., Axtell [2001], Fazio and Modica [2015]). However, my results do not depend on the log-normality assumption. Using bootstrapped firm-size distributions by industry based on draws from actual firm-level observations (similar to Currie and Gruber [1996], Mahoney [2015]) yields virtually identical regulatory scopes.

Finally, I use these industry-specific simulated firm-size distributions, instead of the actual country-industry-specific distributions, to calculate the standardized scope. Specifically, I calculate the standardized scope as the share of simulated firms in a given standardized industry exceeding the regulatory thresholds of a given country in a given year. (For more detail, refer to section "Standardized Scope" in the Online Appendix.)

The standardized scope essentially captures the typical share of firms in a given industry that a country's mandates would affect. This then serves as an instrument for a country's actual share of mandated firms in a given industry. It drives variation in the country's actual share because it varies with the country's thresholds and typical differences across industries. Importantly, however, it does *not* vary with endogenous differences and changes in firm-size distributions across countries and over time. This feature alleviates concerns about endogenous country-industry-specific differences and changes in firm-size distributions polluting my regulatory scope measures.

4.2. Research Design

I use a within-country-year and within-industry-year design to isolate variation in the standardized scope due to the *interaction* of country-level thresholds and industry-level firm-size distributions, while accounting for any country-level threshold endogeneity and industry-level firm-size differences. I implement this design by estimating the following specification:

$$Y_{c,i,t} = \beta_1 Reporting_{c,i,t-1} + \beta_2 Auditing_{c,i,t-1} + \alpha_{c,t} + \delta_{i,t} + \varepsilon_{c,i,t},$$

where $Y_{c,i,t}$ is the outcome variable of interest (e.g., market-share concentration) in country c, industry i (four-digit NACE industry classification), and year t; $Reporting_{c,i,t-1}$ is the standardized scope of reporting regulation (i.e., the share of firms exceeding reporting-exemption thresholds) in country c, industry i, and year $t-1$; $Auditing_{c,i,t-1}$ is the standardized scope of auditing regulation (i.e., the share

of firms exceeding auditing-exemption thresholds) in country c, industry i, and year $t-1$; $\alpha_{c,t}$ denotes country-year fixed effects; and $\delta_{i,t}$ denotes industry-year fixed effects.[9]

My design essentially asks by how much an increase of the standardized scope of reporting and auditing mandates—from mandating no firms (0%) to all firms (100%) in an industry—affects industry-wide outcomes in the average country, industry, and year.[10] Notably, this design does not compare outcomes of mandated versus non-mandated firms, unlike most prior firm-level studies. Rather, it compares industry-wide outcomes of industries with a high versus low share of mandated firms. This feature allows accounting for externalities and industry-wide effects of reporting and auditing mandates that not only directly affect mandated firms but also indirectly affect other firms (e.g., Crépon et al. [2013]).

My design is akin to a cross-sectional difference-in-differences design. It exhibits three notable advantages, compared to a standard time-series difference-in-differences design. First, it allows focusing on the same country at the same point in time, addressing concerns about the endogenous timing of regulatory reforms (e.g., Ball [1980]). Second, it captures long-run effects. These effects are more informative than short-run changes around regulatory reforms if aggregate effects take a while to play out in the data. Lastly, my design allows exploiting the rich cross-sectional variation in mandates induced by the distinct reporting and auditing thresholds. Time-series variation in the mandates, by contrast, is limited and polluted as changes in the thresholds are infrequent, minimal, and usually bundled (e.g., reporting and auditing thresholds change at the same time). (For more detail on the design choice, see section "Cross-Sectional Design" in the Online Appendix.)

My design treats the country-industry-year panel data as a repeated cross-section. To account for the repeated cross-section in the estimation of standard errors, I cluster standard errors at the country-industry level (where the industry is defined as the one-digit NACE industry classification)

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[9] I lag the reporting and auditing scope by one year because up to a 13-month lag exists between the fiscal year end and the publication date in several countries.

[10] In the results section, I consider a 10% change (which is closer to the within-country and within-industry standard deviation in regulatory scope observed in my sample) in interpreting the coefficient magnitudes.

and the country-year level.[11] This approach accommodates arbitrary dependence within a given country in a given year and within coarse country-industry blocks across the entire sample period.

My approach relies on two necessary conditions. First, for the within-country-year specification, I require that significant differences exist across industries in terms of relative total assets, sales, and employees' distributions such that the same exemption thresholds at the country level indeed matter differentially across industries. Second, for the standardization of the scope, I require that the significant cross-industry differences persist across countries. These two conditions are a priori innocuous. For one, the literature documents significant and systematic differences in firm-size distributions (e.g., consider labor-intensive service vs. capital-intensive manufacturing industries; Rajan and Zingales [1998a], Haltiwanger et al. [2014]). For another, the empirical validity of these conditions is testable (refer to section 6.1.2) and any violation of these conditions works against finding a regulatory effect.

For a causal interpretation, my approach relies on the identifying assumption that the reporting and auditing scope measures are uncorrelated with other unobserved factors determining the industry-level resource allocation within a given country-year and industry-year. My approach would be invalid, for example, if countries have other economic policies that differentially affect industry-level outcomes *and* systematically line up with the relative (within-country-year and within-industry-year) scope of reporting and auditing mandates.

One obvious candidate for such a factor would be product- or labor-market regulations with similar regulatory thresholds at the country level. To the best of my knowledge, no other threshold-based regulations overlap with both reporting and auditing requirements in the majority of my sample countries. The most prominent alternative size-based regulations pertain to labor protection and representation, for example, in France, Germany, and Italy. These regulations tend to share the 50-

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[11] The industry-classification level of my observations and fixed effects is substantially finer (four-digit NACE) than the level used for the clustering of standard errors (one-digit NACE). The finer observations and fixed effects enhance precision and reduce bias in my coefficient estimates, whereas the broader clustering (more conservatively) allows for broader cross-sectional and time-series dependence in calculating standard errors.

employees size threshold with the reporting and auditing mandates. Notably, however, they do not share the other size-based thresholds (related to total assets and sales) and cannot explain both reporting and auditing mandates simultaneously. They should also exhibit a chilling effect on competition and resource reallocation (unlike the potential effects of the mandates) (e.g., Haltiwanger et al. [2014]) and do not exist or overlap in several countries (e.g., Garicano et al. [2016]).

More generally, my approach would be invalid if countries endogenously chose their thresholds to achieve country-industry-specific objectives. Salient institutional features suggest that this is unlikely to be true. Most notably, countries' exemption thresholds are uniformly set at the country level instead of differentially chosen industry-by-industry in a given country. The exemptions are explicitly tied to uniform firm-size thresholds because they are motivated by concerns about regulatory fixed costs levied on smaller firms, irrespective of the firms' industry membership. Clearly, a given country may tailor its country-level thresholds to a specific industry (e.g., its most important industry). This sort of choice, however, would render the thresholds plausibly exogenous for all industries other than the specifically targeted one (i.e., the vast majority of my regulatory variation).[12] (For an assessment of factors correlated with the scope, refer to section 7.1.)

5. Data

I collect information on reporting and auditing requirements and thresholds for 26 European countries for the years 2000 to 2014 (note: one-year lag relative to sample years in accordance with research design) through research of official legislative documents, consulting and research reports (e.g., Cna Interpreta [2011], Bernard et al. [2018]), and a questionnaire administered to knowledgeable parties in the respective countries (e.g., ministries of law and commerce, official publication platforms, associations of accountants, audit firms, and academics).[13]

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[12] Controlling for the relative within-country importance of industries (e.g., through the inclusion of various industry-size measures) does not significantly affect my estimates and inferences, suggesting economic policies tailored to country-specific industry specializations cannot explain my results.
[13] I only include country-years for which I found at least one reliable source describing the official thresholds.

I construct a firm-level panel of ownership and financial-statement information of limited-liability firms combining information from Amadeus discs for years 2005 to 2015 with information downloaded from Amadeus through WRDS in 2016 (following Kalemli-Ozcan et al. [2015]).[14] For financial information, I merge historical information from discs 2005, 2008, 2012, and the WRDS download in 2016 to construct a firm-year panel of financial information covering the years 2001 to 2015. This approach reduces survivorship issues associated with Bureau van Dijk's practice of dropping firms from its database after several years of inaction. My approach increases the underlying sample from about 80 million firm-year observations available with the 2016 WRDS download to about 115 million firm-year observations. For other static information items (e.g., auditor, ownership, and legal-form information), I construct a firm-year panel using all discs from 2005 to 2015 and the 2016 WRDS download. This panel construction allows me to investigate non-financial information (e.g., ownership information) in the years 2004 to 2015, instead of only in the last available year.[15] Using currency exchange rates and GDP deflators from the World Bank, I translate all monetary values to real U.S. dollars as of 2015.

6. Results

6.1. Validation of standardized scopes

6.1.1. Variation in standardized scopes

Turning to the data, I first descriptively investigate the variation in the standardized scope of reporting and auditing mandates.[16] Table 2 documents that the reporting and auditing scopes provide substantial variation and span nearly the entire range from 0% to 100%. The majority of the variation (25th to 75th percentile) in reporting and auditing scopes ranges from 5% to 28% and 8% to 38%, respectively. This highlights that the scopes primarily capture variation in mandates levied onto the

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[14] I thank Kalemli-Ozcan et al. [2015] for sharing their NACE correspondence table with me.
[15] I lag all static items by one year relative to the year of the Amadeus disc (Kalemli-Ozcan et al. [2015]). Hence, the sample period for (most) static items ranges from 2004 to 2015. For further details on the data construction, limitations, and corresponding robustness tests, refer to sections "Data Limitations" and "Supplemental Results" in the Online Appendix.
[16] For variable definitions, refer to Table 1. For average scopes of reporting and auditing mandates by country-year and the exemption thresholds, refer to Table A2, Table A3, and Table A4 in the Online Appendix.

largest firms in a given industry (e.g., the top 5 to 28% of firms for reporting mandates). These firms can be expected to be of substantial importance for industry-level outcomes.

Figure 2 plots the country-industry variation in the scopes by year. Notable variation exists in each year for both the reporting scope and the auditing scope. By contrast, only limited variation exists in the average reporting and auditing scopes over time. These patterns highlight the need for a cross-sectional research design.

Figure 3 plots countries' reporting scopes against their auditing scopes. The figure documents that my sample contains both industries where the reporting scope is higher than the auditing scope and industries where the reporting scope is lower than reporting scope (as evidenced by circles on the off-diagonal). This feature allows disentangling the effects of reporting and auditing mandates.

6.1.2. Standardized scopes and actual scopes

The variation in standardized scopes should drive variation in the actual share of mandated firms in a given country, industry, and year to be a relevant instrument for the regulatory scope. To examine the relevance of the standardized scopes, Table 3 presents estimates of regressions of the actual shares of mandated firms ("Actual Reporting Scope" and "Actual Auditing Scope") and firms' actual auditing behavior ("Audit") on the standardized scopes.[17] "Actual Reporting Scope" and "Actual Auditing Scope" are calculated as the fraction of firms exceeding reporting and auditing thresholds, using countries' actual firm-size distributions, and "Audit" is calculated as the fraction of firms obtaining a financial-statement audit within a given country, industry, and year.

Column 1 documents that the standardized reporting scope is strongly positively associated with the actual fraction of firms subject to reporting requirements. The coefficient of 0.476 (standard error: 0.085) suggests that a 10-percentage-point increase in the reporting scope is associated with about a 4.8-percentage-point increase in the actual fraction of mandated firms. By contrast, the

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[17] I truncate the within-country-year and within-industry-year distribution of each variable (using regression-specific samples) at the first and 99th percentiles in all regressions to account for extreme values, due to potential data errors.

standardized auditing scope is slightly negatively associated with the fraction of firms subject to reporting requirements.

Column 2 documents the reverse relation for the actual fraction of firms affected by the auditing requirements. The standardized reporting scope is not significantly associated with the actual fraction of firms subject to auditing requirements, whereas the standardized auditing scope is strongly positively associated with it. Column 3 further documents that this relation even holds for firms' actual auditing behavior. The standardized reporting scope is not significantly associated with firms' actual auditing behavior, whereas the standardized auditing scope is strongly positively associated with firms' auditing.

Collectively, these estimates document the first-stage relevance of the standardized scopes for countries' actual reporting and auditing scopes and firms' actual financial reporting (F-statistic for "Actual Reporting (Auditing) Scope": 28.13*** (44.63***) following Sanderson and Windmeijer [2016]). In addition, the estimates make three more subtle points. First, they imply that the standardized industries indeed capture differences in industries' firm-size distributions which are systematic across countries, validating the necessary conditions underlying the standardization approach. Second, the estimates suggest that the scopes capture separate reporting- and auditing-specific variation in countries' mandates, enabling the separate investigation of reporting and auditing mandates. Third, the estimates document that the scopes (the auditing scope, in particular) affect firms' observed reporting and auditing choices, suggesting the mandates are actually enforced.

6.2. Standardized scopes and the type of resource allocation

After validating the standardized scopes, I examine the impact of reporting and auditing scopes on the type of resource allocation in capital and product markets.

6.2.1. Capital-market structure

I first investigate the impact of reporting and auditing scopes on the allocation of resources in capital markets, one of the most important and extensively studied input markets. In line with the

literature, I focus on the dispersion of equity ownership as a central aspect of the structure of capital markets (e.g., La Porta et al. [2006]).

Table 4 presents estimates of regressions of measures of equity ownership dispersion on the standardized scopes. I use the fraction of publicly listed firms ("Publicly Listed"), the number of shareholders ("Shareholders"), and control-rights dispersion ("Independence") as measures of ownership dispersion. Columns 1, 3, and 5 present estimates using equally weighted outcomes ("Average"), whereas columns 2, 4, and 6 present estimates using market-share-weighted outcomes ("Aggregate").

Table 4 documents that the reporting scope is significantly positively associated with the fraction of publicly listed firms, the average number of shareholders, and control-rights dispersion, whereas the auditing scope is not. The coefficients on the reporting scope are larger for market-share-weighted than for equally weighted capital-market outcomes, suggesting reporting mandates allow especially larger firms to spread their ownership more widely.[18]

Consistent with the literature (e.g., La Porta et al. [2006]), the results suggest reporting mandates reduce ownership concentration. They indicate a shift from a relational toward a transactional type of financing. Absent reporting mandates, many firms obtain financing via concentrated relationships with a few capital providers. An important benefit of this relational financing is that it facilitates the private exchange of information, allowing firms to economize on the costs of public reporting (e.g., proprietary costs). By forcing firms to incur these costs anyhow, reporting mandates diminish the value of relational financing. At the same time, the mandates enhance the appeal of transactional financing in competitive and liquid capital markets (e.g., public markets). To access these markets, firms typically have to report publicly to bridge information asymmetries between relationship insiders (e.g., managers and existing investors) and prospective investors. By

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[18] In the following, I discuss reduced-form estimates. The corresponding second-stage estimates can be found in Table A5 in the Online Appendix.

forcing (private) firms to incur these costs anyhow, reporting mandates lower the incremental cost of a public listing.[19]

Unlike reporting mandates, auditing mandates per se do not appear to significantly contribute to ownership dispersion. My evidence suggests forcing firms that would *not* voluntarily buy audits to be audited does not come with significant capital-market benefits for the mandated firms or other firms in the same industry.

6.2.2. Product-market structure

I next investigate the impact of reporting and auditing scopes on the allocation of resources in product markets, firms' main output market. The product market is of particular interest because proprietary costs arising from the leakage of information to competitors and customers feature prominently in firms' voluntary financial-reporting decisions and the rationale for mandates.

I focus on three market-structure measures capturing the competitiveness of product markets: entry and exit rates, market-share concentration, and profit-margin dispersion. I define "Entry" as the fraction of firms founded within the last two years (e.g., Klapper et al. [2006], Messina and Vallanti [2007]) and "Exit" as the fraction of firms that became inactive for bankruptcy or illiquidity reasons within a given country, industry, and year. I measure market-share concentration ("HHI") as the sum of squared market shares within a given country, industry, and year. And I measure profit-margin dispersion as the standard deviation ("Dispersion") and the distance between the 80th and the 20th percentile ("Distance") of the "Gross Margin" and "EBITDA/Sales" distributions within a given country, industry, and year.[20] The within-industry dispersion of profit margins is commonly viewed as a measure of informational barriers to competition manifesting in the violation the law of one price (Stigler [1961], Jensen [2007]).

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[19] This evidence suggests levelling the regulatory playing field in terms of reporting requirements between private and public firms could increase the attractiveness of a public listing by reducing its incremental cost, consistent with recent proposals (Michaely [2017]) and evidence in a concurrent working paper (Aghamolla and Thakor [2019]).
[20] To account for differences in scale, the distance and dispersion measures (of profit margins and revenue productivities in later tests) are scaled by the mean of the respective distribution (e.g., Syverson [2004]).

Columns 1 and 2 of Table 5 document that reporting scope is positively associated with entry rates, whereas auditing scope is negatively associated with them. In particular, a 10-percentage-point increase in reporting scope is associated with a 0.75 percentage-point ($0.75/18.3 \approx 4.1\%$) increase in average entry rates and a 0.80 percentage-point increase in aggregate entry rates. The slight difference between average and aggregate entry-rate coefficients suggests that, if anything, reporting mandates appear to facilitate entry marginally more for larger firms. By contrast, a 10 percentage-point increase in auditing scope is associated with a 1.30 percentage-point ($1.30/18.3 \approx 7.1\%$) decrease in average entry rates and a 0.69 percentage-point decrease in aggregate entry rates. The larger coefficient in the average than the aggregate entry-rate specification suggests auditing mandates especially deter entry of smaller firms. Columns 3 and 4 document no significant evidence of associations between the reporting and auditing scopes and aggregate exit rates, and only weak evidence that the reporting scope is associated with greater average exit rates. These weak results are likely due to the poor measurement of firm exit in my data.[21]

Column 5 documents that the reporting scope is significantly negatively associated with product-market concentration, whereas the auditing scope is not significantly associated with product-market concentration. Columns 6 to 9 further document that the reporting scope is significantly negatively associated with all four measures of profit-margin dispersion, whereas the auditing scope is not significantly associated with any of these measures.

Taken together, the results suggest reporting mandates foster competition in product markets. The results are consistent with firms' mandatory reporting leading to competitive costs due to leakage of proprietary information. They are further consistent with reporting mandates helping relationship outsiders such as entrants, competitors, and customers to learn about profitable business opportunities. This learning reduces entrants' uncertainty, accelerates competitors' imitation, and increases customers' outside options, translating into increased dynamism, reduced market-share

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[21] Firm exits are not systematically recorded in the database, rendering this measure comparably noisy (Klapper et al. [2006]). Using official entry and exit statistics in Germany, I document a positive impact of reporting mandates on entry and exit rates in supplemental results reported in section 7.2.

concentration, and reduced profit-margin dispersion. Competitors, for example, can use mandated firms' public financial statements to benchmark their cost structures and spot profitable markets. Absent mandatory reporting, firms with valuable proprietary information on their cost structures, profitability, or investment opportunities tend to abstain from public reporting, hampering their competitors' learning. Similarly, customers can reallocate their business toward low-cost producers identified based on comparable public financial-statement information among a potentially large pool of producers. Absent mandatory reporting, customers instead tend to gravitate toward a few incumbent firms, known as a result of previous relationships or their reputation.

With respect to auditing mandates, the results suggest the mandates impose a fixed cost of operating on firms. As the auditing mandates extend to smaller firms, fewer potential entrants appear to find it worthwhile to enter and start operating due to the audit cost.

6.3. Standardized scopes and the efficiency of resource allocation

To investigate the mandates' impact on the efficiency of resource allocation, I examine the effects of reporting and auditing scopes on measures of industry-wide allocative efficiency established in the literature. Clearly, the measurement of allocative efficiency is challenging and there is no single measure that perfectly captures the concept. Accordingly, I employ several measures and base my inferences on the collective results.[22]

6.3.1. Distribution of productivities

I begin my investigation by examining the across-firm efficiency of resource allocation. The across-firm efficiency is an important dimension to examine because externalities (e.g., competitor and customer learning) should lead to a reallocation of resources across firms. I focus on two common

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[22] Although the measurement of resource-allocation efficiency is generally challenging, this measurement issue is likely less severe in my study. Notably, I do not compare levels of resource-allocation efficiency proxies across countries or industries or over time. Instead, I am interested in the co-movement of allocation efficiency measures with financial-reporting regulation. Any noise in my efficiency measures ends up in the error term, increasing the standard errors rather than attenuating the coefficients of interest.

measures of the across-firm efficiency in a given industry: revenue-productivity dispersion and size-productivity covariance.

Revenue-productivity dispersion is a popular measure of resource *mis*allocation (Hsieh and Klenow [2009]). The idea underlying this measure is that frictions in input and output markets sustain dispersions in prices and technical efficiency. Market power, for example, allows local monopolists to charge higher prices than other firms in the industry and to continue operating, even if they are technically inefficient. The resulting dispersion in prices and technical efficiencies in the industry shows up in the dispersion of revenue productivities, because revenue productivity captures variation in both prices and technical efficiency (Foster et al. [2008]).

Columns 1 to 4 of Table 6 present estimates of regressions of measures of revenue-productivity dispersion on the standardized scopes. As measures of the cross-firm productivity dispersion, I use the standard deviation ("Dispersion") and difference between the 80th and 20th percentiles ("Distance") of total factor productivities ("TFP") in a given country, industry, and year. [23] Panel A presents estimates for employees-based productivity measures, whereas Panel B presents estimates for wage-expense-based productivity measure.

Columns 1 and 2 document that the reporting scope is significantly negatively associated with the dispersion and distance of the revenue-productivity distribution for both measures (Panels A and B), whereas the auditing scope is not. To illuminate which tail of the productivity distribution contributes to the reduced dispersion, I investigate the impact on the 20th percentile ("Lower Tail") and the 80th percentile ("Upper Tail") of the productivity distributions separately. Column 3 documents that the reporting scope is not significantly associated with the lower tail of the revenue-productivity distribution (Panel A and Panel B), whereas the auditing scope is significantly positively associated with the lower tail. Column 4, by contrast, documents that the reporting scope is

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[23] I follow the index approach to calculating total factor productivity (e.g., Syverson [2011]). I use typical labor and capital expenditure shares (labor: 0.7, capital: 0.3) uniformly across countries and industries. This simplified approach provides a basic comparison of firms' input-output ratios across countries and industries, circumventing the difficulties associated with the measurement of productivity. I use multiple alternative productivity measures (e.g., labor productivity) to ensure my results do not depend on one approach to measuring productivity.

significantly negatively associated with the upper tail of the revenue-productivity distribution (Panel A and Panel B), whereas the auditing scope is not significantly associated with the upper tail. In terms of magnitude, a 10 percentage-point increase in the reporting scope is associated with a decrease of the upper tail of the productivity distribution of about 9% (relative to the mean of the upper tail).

These results are consistent with reporting mandates improving the allocation of resources across firms, as shown by the reduced revenue-productivity dispersion. This improvement results from a reduction of "extreme" revenue productivities. These extremes likely represent firms with high markups, not high technical efficiency. Thus, reporting mandates appear to shrink the dispersion in revenue productivities by reducing market power and corresponding markups. With respect to auditing mandates, the results again suggest the mandates impose a cost of operating on firms. By forcing audit costs on firms, mandates increase the minimum level of productivity required for firms to profitably operate, as reflected by the increased lower tail of the productivity distribution (e.g., Syverson [2004], Syverson [2011]).

Besides the productivity dispersion, the size-productivity covariance is a popular measure of the across-firm allocative efficiency (e.g., Olley and Pakes [1996], Bartelsman et al. [2013]). The idea underlying this measure is that more productive firms should command more inputs and be more successful in output markets, resulting in a positive covariance between firm size and productivity.

Columns 5 and 6 of Table 6 present estimates of regressions of the size-productivity covariance on the standardized scopes. I calculate the covariance between market shares and productivities ("Size-Productivity Covariance") as the difference between the market-share-weighted productivity and the average productivity of a given country, industry, and year. I use labor productivity ("Y/L") and total factor productivity ("TFP") as the relevant productivity measures in columns 5 and 6 respectively. Panel A presents estimates using employees-based productivity measures, whereas Panel B presents estimates using wage-expense-based productivity measure.

Columns 5 and 6 document weak evidence that the reporting scope is positively associated with the size-productivity covariance. For the wage-expense-based productivity measures (Panel B),

a 10 percentage-point increase in reporting scope is statistically significantly associated with an increase in the size-productivity covariance of about 3.4-3.8% [4.9-5.6%] (relative to the [conditional] standard deviation of the covariance). By contrast, the auditing scope is not significantly associated with the size-productivity covariance in any of the specifications.

These estimates suggest that, if at all, reporting—but not auditing—mandates contribute significantly to an improved across-firm resource allocation.[24] The positive impact of the former on the size-productivity is consistent with, for example, customers reallocating their business toward more efficient producers.

6.3.2. Aggregate productivity

I conclude my investigation by examining the impact of reporting and auditing scopes on productivity levels and growth. I investigate both the average and aggregate impact of the scopes. Average productivity levels and growth can be impacted, for example, if the average firm benefits from an improved ability to spot investment opportunities. Aggregate levels and growth can additionally be impacted by a reallocation of resources *among* firms. A reallocation of resources from unproductive toward productive firms, for example, increases aggregate productivity levels, even if the average firm's productivity remains unchanged. Likewise, a speedy reallocation of resources from firms with declining investment opportunities toward firms with thriving opportunities can lift aggregate productivity growth (e.g., Basu et al. [2010], Fuchs et al. [2016]).

Columns 1 to 4 of Table 7 present estimates of regressions of productivity levels on the standardized scopes. Panel A presents estimates using equally weighted productivities, whereas Panel B presents estimates using market-share-weighted productivities. Columns 1 to 4 document only weak evidence that the reporting scope is positively associated with average productivity, and slightly stronger evidence that the reporting scope is positively associated with aggregate productivity. The

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[24] I caution that the size-productivity and aggregate productivity level results (see next subsection) are susceptible to important biases. For corresponding robustness tests, refer to section "Supplemental Results: Robustness to research-design choices" in the Online Appendix.

statistically and economically significant associations are concentrated in the specifications using wage-expense rather than employees-based productivity measures (columns 2 and 4 in Panel B). A 10 percentage-point increase in the reporting scope is associated with a 2.1% increase in wage-based aggregate total factor productivity (relative to its mean). The auditing scope, by contrast, is neither significantly associated with average nor aggregate productivity in any of the specifications.

These estimates suggest that, *if at all*, reporting—but not auditing—mandates appear to significantly improve aggregate revenue productivity. The impact appears to primarily result from cross-firm reallocation of resources rather than within-firm improvements, as average productivity is widely unaffected. Compared to the impact on the type of resource allocation (e.g., ownership dispersion in capital markets and competition in product markets), the estimates imply a limited impact of reporting mandates on aggregate productivity levels though. Moreover, in supplemental tests (7.3), I find that the estimated impact is not robust to using alternative productivity measures. Accordingly, it remains unclear whether reporting mandates actually help aggregate productivity.

Columns 5 to 8 of Table 7 present estimates of regressions of productivity growth on the standardized scopes. Panel A presents estimates using equally weighted year-over-year productivity changes, whereas Panel B presents estimates using year-over-year changes of market-share-weighted productivities. Columns 5 to 8 document some weak evidence that the reporting scope is *negatively* associated with average and (partially) aggregate productivity growth. A 10 percentage-point increase in reporting scope, for example, is associated with a 0.8% [1.5%] decrease in employee-based aggregate labor productivity growth (relative to the [conditional] standard deviation). By contrast, I find no evidence that the auditing scope is significantly associated with productivity growth.[25]

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[25] I validate my measures of productivity growth by regressing them on the number of firms (and its squared term) as a measure of industry-level competition. Aghion et al. [2005] argue that aggregate innovation and the associated aggregate productivity growth exhibit an increasing (concave) relationship with respect to industry-level competition. Consistent with their argument, I find strong evidence of such relationship between my aggregate productivity growth and the number of firms (Table A6 in the Online Appendix). This evidence allays concerns that the negative association between reporting scope and productivity growth is due to mismeasurement or a negative effect of competition on price changes.

These results suggest reporting mandates, while spurring competition by disseminating proprietary information to competitors and customers, do not significantly spur productivity growth. If anything, reporting mandates appear to deter productivity growth. A potential reason for the absence of a positive growth effect is that reporting mandates, by dissipating mandated firms' proprietary information, stifle these firms incentives to innovate (e.g., Arrow [1962], Bhattacharya and Chiesa [1995], Zingales [2009]). Recent evidence by Breuer et al. [2019] supports this conjecture.

7. Robustness

In supplemental tests, I assess the robustness and clarify the interpretation of my main results. I summarize the supplemental tests and findings below. (For more detail, refer to section "Supplemental Results" in the Online Appendix.)

7.1. Firm size and other confounding factors

My main tests rely on the identifying assumption that other factors such as firm-size differences across country-industries do not confound my reporting and auditing scopes. While this assumption is ultimately untestable, I probe its plausibility by assessing the associations of potentially confounding factors (e.g., firm size) with my regulatory scopes.

In column 1 of Table 8, I find that country-industry-level factors such as the average firm size in terms of sales and employees in a given country-industry are *not* significantly associated with the standardized reporting scope, after accounting for country-year and industry-year fixed effects. Notably, these factors only explain a negligible fraction of the residual variation in the standardized scope (within-R-squared: 0.1%). These results suggest the within-country-year and within-industry-year variation in standardized scopes is not confounded by firm-size factors and appears plausibly exogenous, supporting my identifying assumption.

By contrast, the actual reporting scope is strongly positively associated with several firm-size and other country-industry-level factors (column 2). Even after controlling for country-year and industry-year fixed effects, firm size and other factors explain a substantial fraction of the residual

variation in the actual scope (within-R-squared: 29.3%). These results stress the importance of using standardized firm-size distributions in calculating the scopes to avoid confounding firm-size factors.

7.2. Enforcement reform in Germany

In my main tests, I rely on a cross-sectional research design and focus on a sample of limited-liability firms with available data. These design choices raise two important concerns. First, the cross-sectional design may be susceptible to confounding time-invariant differences across country-industries (e.g., other size-based regulations). Second, the focus on limited-liability firms with available data may result in important biases due to a potential impact of financial-reporting mandates on sample inclusion. The mandates, for example, can directly impact sample inclusion by increasing the availability of firm information. Similarly, they can indirect impact sample inclusion by altering firms' legal form choice (e.g., some firms may drop out of the sample by switching toward legal forms that do not grant limited liability to avoid the mandates).

To assess the importance of these concerns and the robustness of my results to alternative design and sample choices, I reinvestigate the impact of reporting mandates on product-market competition exploiting a major enforcement reform in Germany. This alternative single-country setting complements my main setting in two important respects. First, it permits a familiar time-series difference-in-differences design around the enforcement change, allowing me to corroborate my main results obtained using a cross-sectional design. Second, in the German setting, I observe limited-liability *and* unlimited-liability firms, irrespective of their financial-reporting mandate, by virtue of confidential census data access. This feature circumvents concerns about a confounding impact of mandates on sample inclusion and legal form choice.

In accordance with the EU Accounting directives, Germany prescribes public reporting mandates for limited-liability firms. It, however, had virtually not enforced these mandates until a sweeping enforcement reform in 2007 (e.g., Bernard [2016]), triggered by mounting pressure from the EU. I exploit the enforcement reform as a shock to limited-liability firms' reporting mandates. The strength of this shock varies across local industries depending on the share of limited-liability firms

among all firms in the respective local industry (in the pre-period). To measure this share of limited-liability firms and the local product-market competition, I use comprehensive census data from the German Federal Statistical Office on limited-liability and unlimited-liability firms' sales and business notifications (on entry and exit) for the years 2003 to 2012.

I test for the local product-market consequences of the enforcement reform using the following difference-in-differences specification with a continuous treatment variable:[26]

$$Y_{c,i,t} = \sum_{\tau \neq 2006} \beta_\tau Regulated_{c,i} \times 1(t = \tau) + \alpha_{c,t} + \delta_{i,t} + \gamma_{c,i} + \varepsilon_{c,i,t} \ ,$$

where $Y_{c,i,t}$ is the outcome variable of interest (e.g., market-share concentration) in county c, industry i (two-digit NACE industry classification), and year t; $Regulated_{c,i}$ is the share of limited-liability firms (among all firms) in county c and industry i in the pre-enforcement period (in particular, in the base year: 2006); $1(t = \tau)$ represents a separate year indicator for each year (except for the base year: 2006); $\alpha_{c,t}$ denotes county-year fixed effects; $\delta_{i,t}$ denotes industry-year fixed effects; and $\gamma_{c,i}$ denotes county-industry fixed effects.

Figure 4 plots the difference-in-differences coefficients for firm entry, exit, and product-market concentration around the enforcement reform, respectively. The figure documents that entries and exits increase after the reform for local industries with a greater share of limited-liability firms, whereas product-market concentration decreases. These findings are consistent with fiercer product-market competition as a result of increased enforcement of reporting mandates. They confirm the results obtained in my main tests, allaying concerns that time-invariant confounders (e.g., other size-based regulations) or sample selection (e.g., related to legal form choice or database coverage) unduly confound my cross-sectional design and results.

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[26] Source: RDC of the Federal Statistical Office and Statistical Offices of the Länder in Germany, Unternehmensregister and Gewerbeanzeigenstatistik, survey years 2003 - 2012, own calculations.

7.3. Alternative productivity measures

My main tests produce conflicting evidence regarding the impact of reporting mandates on aggregate productivity and its growth. This conflicting evidence is plausibly due to the difficulty of measuring productivity (Syverson [2011]). The measurement issue is particularly acute for the aggregate revenue-productivity *levels* calculated within my dataset. These measures not only capture technical efficiency, but also firms' markups. As such, reporting mandates, which foster competition and decrease markups, can exhibit a downward biased relation with productivity, if measured via revenue productivity (Foster et al. [2008]).[27] Moreover, reporting mandates can mechanically be related to my aggregate productivity measures by affecting the sample of firms with available data (e.g., wage expense disclosed in firms' income statements) necessary for the calculation of aggregate productivity.[28] Notably, these issues are less severe for my measures of aggregate productivity *growth* as they difference out any level differences related to the impact of reporting mandates on markups and sample inclusion. Accordingly, I regard the productivity-growth results as more credible than the productivity-level ones.

To clarify the impact of reporting mandates on productivity levels and growth, I re-examine the association between reporting scope and productivity measures using official industry-level statistics (EU KLEMS, OECD, and WIOD) (Timmer et al. [2015], Stehrer et al. [2019], Adarov and Stehrer [2019]). The key benefit of these statistics is that their measurement is independent of the reporting mandates (i.e., they rely on administrative data, not financial-reporting information). The key drawback is their high-level of aggregation (coarse two digit NACE industries), which reduces power and the ability to effectively differentiate between reporting and auditing mandates.

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[27] Consistent with such a downward bias, I find the relation between the reporting scope and revenue-based productivity measures turns negative when additionally accounting for intermediate inputs (i.e., when more closely approximating profits).

[28] In supplemental placebo tests, I find that a hypothetical coverage effect (i.e., greater reporting scope leads to more firms with available data included in the productivity calculation) would mechanically result in a positive association between reporting scope and aggregate productivity levels. (For details, refer to section "Supplemental Results: Robustness to research-design choices" in the Online Appendix.) This finding suggests caution in attributing the positive association between reporting scope and aggregate productivity levels in my main results to a positive economic impact of reporting regulation.

Accordingly, I regress the official productivity measures on reporting scope or a combined reporting and auditing scope measure. Across all but two specifications, in Table 9, I find a negative, though widely insignificant association between the reporting (and auditing) scope and both productivity-level *and* productivity-growth measures.

These supplemental results highlight that the productivity-level results in my main tests are not robust to using alternative productivity-level measures. They suggest that reporting mandates, if anything, appear to have a negative impact on aggregate allocative efficiency, consistent with the productivity-growth results in my main tests. Overall, however, I caution that my collective results primarily suggests that there is no clear evidence that reporting mandates help or hurt aggregate allocative efficiency, despite their significant impact on the type of resource allocation.

7.4. Interaction of reporting and auditing mandates

In my main tests, I estimate separate effects for reporting mandates and auditing mandates. Reporting mandates, however, may actually only matter in industries with corresponding auditing mandates. Similarly, auditing mandates may substantially contribute to the documented effects of expanded reporting mandates. To clarify the interpretation of my estimates, I examine how reporting and auditing mandates interact.

I find similar effects of reporting mandates in industries with and without a corresponding auditing mandate (Table A7 in the Online Appendix). Likewise, I find similar effects of auditing mandates in industries with and without a corresponding expanded reporting mandate. Although these results do not rule out that auditing mandates may strengthen the effects of reporting mandates, they at least document that auditing mandates are not a prerequisite for the effects of reporting mandates in my setting (e.g., due to alternative mechanisms ensuring regulatory compliance and

credibility of firms' financial statements). [29]   This finding supports the separate assessment and interpretation of the effects of reporting and auditing mandates in my main tests.

7.5. Other tests

My main findings are robust to a variety of further sample-composition and research-design choices. My inferences, for example, remain unchanged when broadening the industry definition to allow for more (cross-four-digit-industry) externalities. The robustness to broader industry definitions reduces concerns that my results miss important cross-industry externalities or are unduly affected by inconsistencies in industry classifications across countries. My inferences are also robust to excluding countries one-by-one. This robustness alleviates concerns that a particular country (e.g., a country with particularly high or low thresholds) drives my results (e.g., Goldsmith-Pinkham et al. [2020]).

8. Discussion

8.1. Evidence on reporting mandates

My empirical evidence is consistent with reporting mandates shifting the industry-wide allocation of resources from a relational towards a more transactional type. This shift is consistent with reporting mandates benefiting relationship outsiders such as prospective investors, competitors, and customers who, absent the mandates, would not have access to firms' financial statements.

Interestingly, the shift to a more transactional type of resource allocation does not appear to translate into productivity growth. Primarily, the shift seems to reallocate rents from relationship insiders to outsiders (e.g., increasing customers' bargaining power) rather than enhance productivity growth. A potential explanation for the "missing" growth may lie in the adverse effects of competitors' free-riding on firms' incentives to discover profitable markets, products, or processes. More generally, the absence of a positive growth effect echoes earlier work on differences in

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[29] Consistent with this finding, EEA members are required to ensure credible financial reporting through appropriate penalties if they allow auditing exemptions (European Commission [1996]). Moreover, McLeay [1999] and Bernard [2016] argue that the credibility of firms' financial reporting is largely not contingent on financial-statement audits in my setting, due to the alignment of book and tax reporting and the corresponding enforcement of tax authorities (Beck et al. [2014]). Supporting these arguments, respondents to the survey of Minnis and Shroff [2017] state that expanded reporting mandates rather than auditing mandates increase the benefits derived from competitors' financial reporting.

investment horizons and innovative activity between market- and relationship-based systems (e.g., Dewatripont and Maskin [1995], Rajan and Zingales [1998b]).

Overall, the mixed evidence on the effects of reporting mandates on the efficiency of resource allocation is consistent with these mandates primarily crowding out alternative information sources (e.g., private information sharing) and contracting approaches (e.g., concentrated relationships) instead of unambiguously improving economic efficiency (e.g., Gonedes [1980], Kurlat and Veldkamp [2015], Goldstein and Yang [2017]). This interpretation comports with the conclusions of Winston [2006], who argues that regulations addressing information frictions frequently fail to enhance economic efficiency, because market solutions already limit the adverse impact of information frictions on allocative efficiency.

8.2. Evidence on auditing mandates

My empirical evidence on auditing mandates is consistent with them imposing fixed costs of operating on firms.[30] Reduced entry rates, especially among smaller firms, and elevated minimum levels of productivity required for firms to operate in a given industry indicate that mandatory auditing imposes a fixed and recurring cost (e.g., Syverson [2004]). I do not find any other impact of mandatory auditing on industry-wide resource allocation. Notably, the absence of significant other effects does *not* mean auditing has no value. Prior work clearly documents firms frequently obtain voluntary audits because they expect to benefit from external third-party certification (e.g., Jamal and Sunder [2008], Lennox and Pittman [2011], Dedman et al. [2014]). In supplemental tests, I also find voluntary auditing is strongly positively associated with external financing and growth at the industry level.[31]

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[30] Although the annual fee for an audit may be relatively low (around €7 thousand according to Bernard et al. [2018]), the present value of all future audit fees and indirect audit costs (e.g., management attention) can be quite sizeable. This present value is the relevant cost estimate for potential entrants' deciding whether to start operating or not. This estimate will be particularly prohibitive for small firms with owner-managers (whose management attention is costly) and in cases when the audit mandate is unavoidable (i.e., when even the smallest firms have to obtain audits). Consistent with these cross-sectional predictions, Table A7 documents that the audit mandate deters entry more for smaller firms and Table A7 documents that the deterrence is strongest in industries where the mandate is pushed down to even the smallest firms.
[31] In untabulated tests, I find associations between voluntary auditing and resource-allocation outcomes consistent with auditing being an efficient private contracting institution demanded in growing industries and supporting resource-allocation efficiency (e.g., Watts and Zimmerman [1983], Hope et al. [2011]).

Therefore, my evidence should be interpreted as suggesting that, in my setting, uniform auditing mandates do not lead to improvements beyond firms' voluntary audit choices.

8.3. Institutional caveats

A number of institutional features contribute to the specific findings of my paper. These features are to be considered in interpreting my findings. First, my paper focuses on reporting and auditing mandates pertaining to private firms. Public firms' reporting and auditing requirements are not affected by the exemption thresholds. Accordingly, my evidence first and foremost speaks to the effects of mandates for private firms on industry-wide outcomes.

Second, the EU regulation typically requires firms to prepare a full set of financial statements and provide it to their shareholders, irrespective of any public reporting exemptions. As a result, the effects of reporting regulation in this paper abstract from any costs or benefits of mandating firms to internally produce financial statements (e.g., Cheng et al. [2013]) and to disclose these statements to their existing shareholders (e.g., Greenstone et al. [2006]). [32]

Third, in the EU setting, the largest private firms are subject to full reporting requirements in all sample countries. Accordingly, the marginal firms affected by the cross-country differences in exemption thresholds are mid-sized firms. These firms may not be expected to provide substantial positive information externalities. Besides this institutional feature, however, there are also important economic reasons for why one would expect reporting mandates to primarily yield reallocative effects (e.g., mandated firms lose, entrants gain) rather than an aggregate improvement (e.g., due to substantial information externalities) (for a related discussion, see Roychowdhury et al. [2019]).

Uniform reporting mandates mainly increase the reporting of two types of firms: firms with high costs of public reporting (e.g., high proprietary-information cost firms; Verrecchia [1983]) and firms with low benefits of public reporting (e.g., small firms with few stakeholders; Breuer et al.

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[32] Unlike information externalities, (manager-shareholder) agency conflicts do not constitute an obvious argument for reporting regulation if the ultimate goal is allocative efficiency (e.g., Beyer et al. [2010], Leuz [2010]).

[2020]). Increasing the reporting of high-cost firms can provide valuable information externalities to outsiders (e.g., competitors). The benefits to outsiders, however, come at a cost to the mandated firm. Accordingly, mandating high-cost firms' reporting should be expected to primarily result in a reallocation of resources rather than an aggregate improvement. Increasing the reporting of low-benefit firms, by contrast, is unlikely to generate substantial information externalities to begin with, because these firms tend to be small and/or transact with few stakeholders. Accordingly, mandating low-benefit firms' reporting should not be expected to lead to notable aggregate improvements either.

This insight about the population of firms which is *effectively* treated by reporting mandates is important because it suggests that externalities arising from financial reports of economically important firms (e.g., publicly listed firms) do not per se justify reporting mandates (e.g., Badertscher et al. [2013]). These firms tend to commit to transparency (e.g., via a public listing or voluntary reporting) even absent a mandate, limiting the extent to which mandates can generate industry-wide externalities and aggregate improvements (e.g, Gonedes [1980], Dye [1990]).

9. Conclusion

I investigate the industry-wide effects of reporting and auditing mandates on resource allocation. I exploit the fact that European countries prescribe size-based financial-reporting regulations, exempting smaller private firms from reporting and auditing requirements. These exemptions generate useful within-country variation in the scopes of reporting and auditing mandates as a result of natural firm-size differences across industries, allowing me to estimate the industry-wide effects of reporting and auditing mandates for almost the entire population of limited-liability firms in a large sample of countries, controlling for confounding country and industry factors.

I find reporting mandates foster ownership dispersion in capital markets and competition in product markets. Yet, I do not find they unambiguously improve the efficiency of industry-wide resource allocation. With respect to auditing mandates, I find they impose a fixed cost of operating

on firms, deterring smaller entrants. I do not find any other effects of auditing mandates on industry-wide resource allocation in my setting.

I interpret my findings as consistent with the idea that reporting mandates substitute a transactional type of resource allocation, based on public information, for a relational one, based on private information. This substitution, however, does not appear to unambiguously improve the allocation of resources (e.g., productivity growth). One possible explanation for these findings is that, while others benefit from mandated firms' reporting (e.g., entrants), mandated firms themselves lose proprietary information, which deters their incentives to innovate and improve productivity (e.g., Arrow [1962]). These potential reallocative effects of reporting mandates and their implications for innovation incentives warrant further investigation. Regarding auditing mandates, I interpret my findings as consistent with mandates imposing fixed costs on mandated firms without providing significant externalities manifesting in my measures of industry-wide resource allocation. My findings suggest future research may have to investigate other outcomes (e.g., tax evasion or money laundering) or aggregation levels (e.g., the country level) to detect the benefits of auditing mandates.

My paper's findings are subject to several caveats. Most notably, my paper cannot directly speak to country-level effects of reporting and auditing mandates. My research design explicitly purges my estimation of any country-level effects due to concerns about correlated omitted variables, strengthening my identification but also preventing me from learning about more aggregate effects. Moreover, my paper does not speak to the optimal scope of reporting and auditing mandates, and, in particular, does not suggest more financial-reporting regulation is always better. Rather, my paper supports the existence of a trade-off between ex post informational efficiency/competitiveness of markets and ex ante investment incentives (e.g., Kanodia and Sapra [2016]). I leave the investigation of country-level effects and the optimal scope of financial-reporting regulation to future research.

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Figures & Tables

Figure 1

DISTRIBUTION OF FIRMS

All Firms	
Mandated Firms	

VARIATION IN REGULATORY THRESHOLD

All Firms	
Mandated Firms (1)	
Mandated Firms (2)	

VARIATION IN FIRM-SIZE DISTRIBUTION

Labor Intensive Industry (L)	
Capital Intensive Industry (K)	
Mandated Firms in L	
Mandated Firms in K	
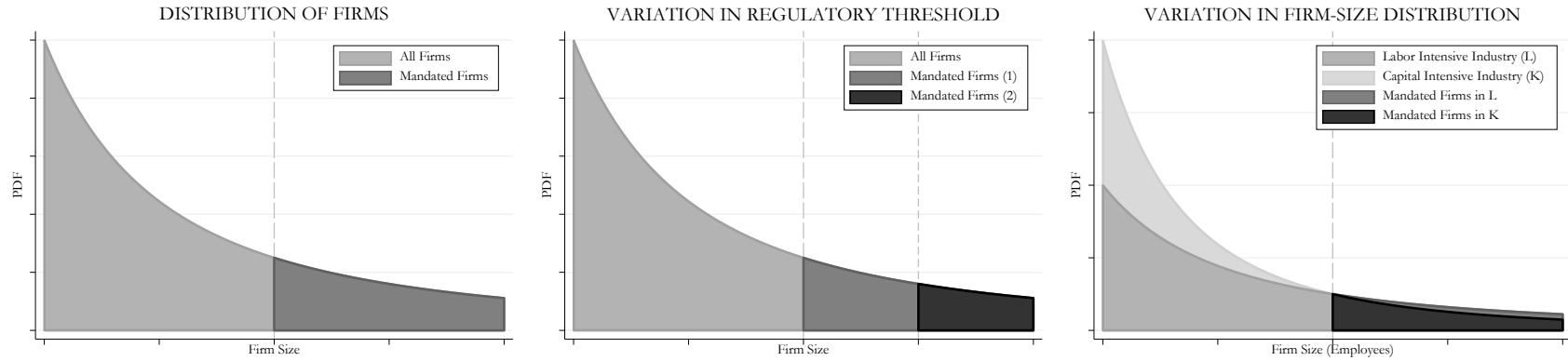
Notes: The figure provides an exemplary depiction of my regulatory scope measure and its variation. The left graph illustrates my measure of the regulatory scope and its cross-country and cross-industry variation. It plots (part of) a (Pareto) probability density function (PDF) of a univariate firm-size dimension. The area to the right of the exemption threshold (dashed vertical line) represents the share of mandated (or non-exempted) firms. The center graph illustrates the within-industry variation in the scope arising from cross-country differences in exemption thresholds. The greater the exemption threshold (dashed vertical line), the lower the scope. The right graph illustrates the cross-industry variation in the scope arising from cross-industry differences in firm-size distributions. The same (employees-related) exemption threshold (dashed vertical line) has different implications for labor- versus capital-intensive industries. The share of mandated firms is larger for labor- than capital-intensive industries, because the (employees) firm-size distribution for the labor-intensive industry exhibits a thicker right tail than for the capital-intensive industry.

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Figure 2

DISTRIBUTION & TIME TREND
## OF REPORTING AND AUDITING SCOPES

Notes: The figure depicts the distribution and time trend of standardized reporting and auditing scopes. The left graph summarizes the distribution of standardized reporting and auditing scopes for each sample year (pooled across countries and industries) using box plots. The box plots provide the median (horizontal line within the boxes), the 25th and 75th percentile (lower and upper bound of the boxes), and adjacent values (end points of vertical lines/whiskers). Adjacent values are defined as the lowest and highest observations that are still inside the region spanned by the following limits: 25th (75th) percentile – (+) 1.5 × (75th – 25th percentile). The right graph depicts the average standardized reporting and auditing scopes (pooled across countries and industries) for each sample year.

Figure 3

REPORTING VERSUS AUDITING SCOPE

Notes: The figure depicts the variation in standardized reporting and auditing scopes before (left graph) and after (right graph) accounting for country-year and industry-year effects. The variation in reporting and auditing scope is collapsed into a coarse grid, reducing the number of observations for the purpose of clarity. Each circle represents observations within a grid point (quadratic area) of size 0.05 × 0.05. The size of the circles represents the number of observations within each grid point.

Figure 4

Panel A   **Panel B**   **Panel C**

Notes: The figure depicts estimates of regressions of entry, exit, and product-market concentration on the share of affected firms in panels A, B, and C, respectively. The annual estimates represent difference-in-differences coefficients relative to the base year 2006. "Entry" is defined as the log number of firms newly registering at the local commercial register/court in a given county, industry, and year in Germany. "Exit" is defined as the log number of firms deregistering at the local commercial register/court in a given county, industry, and year in Germany. "Product Market Concentration" is defined as the sum of squared market shares in a given county, industry, and year in Germany. The share of affected firms is defined as the fraction of limited-liability firms in a given county and industry in the base year 2006. The gray shading represents the point-wise 95% confidence interval.

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Table 1

VARIABLE DEFINITIONS

Variable	Aggregation	Definition
Financial Reporting		
Standardized Reporting Scope	Average	Fraction of firms exceeding reporting thresholds using standardized firm-size distributions
Standardized Auditing Scope	Average	Fraction of firms exceeding auditing thresholds using standardized firm-size distributions
Actual Reporting Scope	Average	Fraction of firms exceeding reporting thresholds using countries' actual firm-size distributions
Actual Auditing Scope	Average	Fraction of firms exceeding auditing thresholds using countries' actual firm-size distributions
Audit	Average	Fraction of firms obtaining a financial-statement audit
Type of Resource Allocation		
Publicly Listed	Average	Fraction of publicly listed firms
Publicly Listed	Aggregate	Market-share-weighted sum of publicly listed firms
Shareholders	Average	Average number of (log) shareholders
Shareholders	Aggregate	Market-share-weighted sum of number of (log) shareholders
Independence	Average	Average independence score based on numeric transformation of Bureau van Dijk's alphanumeric independence score (1: most independent, 0: most dependent)
Independence	Aggregate	Market-share-weighted sum of independence score based on numeric transformation of Bureau van Dijk's alphanumeric independence score (1: most independent, 0: most dependent)
Entry	Average	Fraction of firms founded within the last two years
Entry	Aggregate	Market-share-weighted sum of firms founded within the last two years
Exit	Average	Fraction of firms that turned inactive for bankruptcy/illiquidity reasons
Exit	Aggregate	Market-share-weighted sum of firms that turned inactive for bankruptcy/illiquidity reasons
HHI	Sum	Sum of squared market shares
Dispersion (Gross Margin)	Standard deviation	Standard deviation of sales less wage and labor expense (or cost of goods sold) scaled by sales (normalized by the average gross margin)
Distance (Gross Margin)	p80-p20	Difference between 80th and 20th percentile of sales less wage and labor expense (or cost of goods sold) scaled by sales (normalized by the average gross margin)
Dispersion (EBITDA/Sales)	Standard deviation	Standard deviation of EBITDA scaled by sales (normalized by the average EBITDA/Sales ratio)
Distance (EBITDA/Sales)	p80-p20	Difference between the 80th and 20th percentile of EBITDA scaled by sales (normalized by the average EBITDA/Sales ratio)
Efficiency of Resource Allocation		
Dispersion (TFP (Employees))	Standard deviation	Standard deviation of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log employees (normalized by average TFP (Employees))

Distance (TFP (Employees))	p80-p20	Difference between 80th and 20th percentile of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log employees (normalized by average TFP (Employees))
Lower Tail (TFP (Employees))	p20	20th percentile of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log employees
Upper Tail (TFP (Employees))	p80	80th percentile of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log employees
Dispersion (TFP (Wage))	Standard deviation	Standard deviation of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log wage expense (normalized by average TFP (Wage))
Distance (TFP (Wage))	p80-p20	Difference between 80th and 20th percentile of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log wage expense (normalized by average TFP (Wage))
Lower Tail (TFP ((Wage))	p20	20th percentile of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log wage expense
Upper Tail (TFP (Wage))	p80	80th percentile of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log wage expense
Covariance Y/L and Y (Employees)	Aggregate-Average	Market-share-weighted sum less equally weighted average of labor productivity defined as log sales less log employees
Covariance TFP and Y (Employees)	Aggregate-Average	Market-share-weighted sum less equally weighted average of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log employees
Covariance Y/L and Y (Wage)	Aggregate-Average	Market-share-weighted sum less equally weighted average of labor productivity defined as log sales less log wage expense
Covariance TFP and Y (Wage)	Aggregate-Average	Market-share-weighted sum less equally weighted average of total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log wage expense
Y/L (Employees)	Average	Average labor productivity defined as log sales less log employees
Y/L (Wage)	Average	Average labor productivity defined as log sales less log wage expense
TFP (Employees)	Average	Average labor productivity defined as log sales less 0.3*log tangible capital and 0.7*log employees
TFP (Wage)	Average	Average labor productivity defined as log sales less 0.3*log tangible capital and 0.7*log wage expense
Y/L (Employees)	Aggregate	Market-share-weighted sum of labor productivity defined as log sales less log employees
Y/L (Wage)	Aggregate	Market-share-weighted sum of labor productivity defined as log sales less log wage expense
TFP (Employees)	Aggregate	Market-share-weighted sum of labor productivity defined as log sales less 0.3*log tangible capital and 0.7*log employees
TFP (Wage)	Aggregate	Market-share-weighted sum of labor productivity defined as log sales less 0.3*log tangible capital and 0.7*log wage expense

ΔY/L (Employees)	Average	Average first difference in labor productivity defined as log sales less log employees
ΔY/L (Wage)	Average	Average first difference in labor productivity defined as log sales less log wage expense
ΔTFP (Employees)	Average	Average first difference in total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log employees
ΔTFP (Wage)	Average	Average first difference in total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log wage expense
ΔY/L (Employees)	Aggregate	First difference of market-share-weighted labor productivity defined as log sales less log employees
ΔY/L (Wage)	Aggregate	First difference of market-share-weighted labor productivity defined as log sales less log wage expense
ΔTFP (Employees)	Aggregate	First difference of market-share-weighted total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log employees
ΔTFP (Wage)	Aggregate	First difference of market-share-weighted total factor productivity defined as log sales less 0.3*log tangible assets and 0.7*log wage expense

Notes: "Average" outcomes represent equally-weighted country-industry-year averages, whereas "Aggregate" outcomes represent sales-share-weighted country-industry-year totals.

Table 2

DESCRIPTIVE STATISTICS									
Variable	Aggregation	N	Mean	SD	p10	p25	p50	p75	p90
Standardized Reporting Scope	Average	223,924	0.226	0.268	0.014	0.051	0.134	0.277	0.530
Standardized Auditing Scope	Average	223,924	0.308	0.321	0.034	0.080	0.184	0.380	0.999
Actual Reporting Scope	Average	223,924	0.203	0.287	0.000	0.006	0.070	0.258	0.742
Actual Auditing Scope	Average	223,924	0.253	0.324	0.000	0.011	0.096	0.375	0.895
Audit	Average	223,924	0.162	0.265	0.000	0.000	0.010	0.218	0.579
Type of Resource Allocation									
Variable	Aggregation	N	Mean	SD	p10	p25	p50	p75	p90
Publicly Listed	Average	180,154	0.005	0.042	0.000	0.000	0.000	0.000	0.002
Publicly Listed	Aggregate	171,685	0.031	0.134	0.000	0.000	0.000	0.000	0.016
Shareholders	Average	171,315	0.836	0.343	0.322	0.693	0.890	1.055	1.194
Shareholders	Aggregate	162,568	0.993	0.471	0.617	0.719	0.957	1.143	1.402
Independence	Average	167,375	0.195	0.149	0.000	0.106	0.184	0.261	0.375
Independence	Aggregate	158,767	0.170	0.194	0.000	0.016	0.120	0.242	0.444
Entry	Average	221,894	0.183	0.173	0.000	0.063	0.148	0.253	0.393
Entry	Aggregate	211,700	0.087	0.199	0.000	0.000	0.022	0.085	0.230
Exit	Average	177,665	0.024	0.068	0.000	0.000	0.000	0.023	0.060
Exit	Aggregate	169,210	0.008	0.054	0.000	0.000	0.000	0.001	0.009
HHI	Sum	214,262	0.377	18.751	0.028	0.075	0.205	0.493	0.914
Dispersion (Gross Margin)	Standard deviation	197,555	0.106	0.162	0.005	0.014	0.045	0.126	0.281
Distance (Gross Margin)	p80-p20	197,341	0.120	0.253	0.001	0.003	0.016	0.096	0.387
Dispersion (EBITDA/Sales)	Standard deviation	178,711	0.125	0.190	0.006	0.017	0.052	0.146	0.332
Distance (EBITDA/Sales)	p80-p20	178,370	0.145	0.292	0.001	0.004	0.023	0.126	0.463
Efficiency of Resource Allocation									
Variable	Aggregation	N	Mean	SD	p10	p25	p50	p75	p90
Dispersion (TFP (Employees))	Standard deviation	183,660	0.140	0.183	0.010	0.028	0.079	0.187	0.358
Distance (TFP (Employees))	p80-p20	183,660	0.166	0.295	0.002	0.007	0.036	0.182	0.574
Lower Tail (TFP (Employees))	p20	201,507	0.972	2.565	0.001	0.003	0.020	0.179	3.229
Upper Tail (TFP (Employees))	p80	201,507	2.291	3.235	0.018	0.075	0.474	3.679	8.273
Dispersion (TFP (Wage))	Standard deviation	175,317	0.162	7.370	0.009	0.025	0.075	0.194	0.404
Distance (TFP (Wage))	p80-p20	175,317	0.181	11.139	0.001	0.005	0.030	0.162	0.574
Lower Tail (TFP ((Wage))	p20	190,366	0.150	0.563	0.000	0.000	0.002	0.017	0.278
Upper Tail (TFP (Wage))	p80	190,366	0.414	0.756	0.003	0.011	0.070	0.487	1.409
Covariance Y/L and Y (Employees)	Aggregate-Average	188,295	0.854	0.826	0.055	0.318	0.688	1.191	1.829
Covariance TFP and Y (Employees)	Aggregate-Average	183,648	0.568	0.682	-0.065	0.142	0.433	0.839	1.378
Covariance Y/L and Y (Wage)	Aggregate-Average	176,748	0.365	0.645	-0.205	0.008	0.245	0.583	1.071
Covariance TFP and Y (Wage)	Aggregate-Average	175,300	0.280	0.598	-0.266	-0.045	0.169	0.482	0.953
Y/L (Employees)	Average	204,837	11.703	1.344	10.092	10.905	11.720	12.371	13.127
Y/L (Wage)	Average	191,504	1.686	0.773	0.898	1.242	1.637	2.082	2.576
TFP (Employees)	Average	201,507	8.762	1.053	7.520	8.135	8.771	9.305	9.922
TFP (Wage)	Average	190,366	1.700	0.717	0.981	1.336	1.663	2.061	2.522
Y/L (Employees)	Aggregate	204,837	12.488	1.464	10.878	11.631	12.374	13.135	14.173
Y/L (Wage)	Aggregate	191,504	2.023	0.985	1.003	1.412	1.891	2.497	3.214
TFP (Employees)	Aggregate	201,507	9.280	1.172	8.002	8.584	9.191	9.812	10.689
TFP (Wage)	Aggregate	190,366	1.958	0.889	1.075	1.428	1.827	2.369	3.051
ΔY/L (Employees)	Average	198,797	-0.050	0.560	-0.251	-0.095	-0.006	0.077	0.213
ΔY/L (Wage)	Average	187,184	-0.026	0.320	-0.226	-0.099	-0.021	0.048	0.162
ΔTFP (Employees)	Average	194,397	-0.032	0.441	-0.237	-0.088	0.000	0.080	0.206
ΔTFP (Wage)	Average	185,859	-0.016	0.300	-0.210	-0.087	-0.010	0.059	0.169
ΔY/L (Employees)	Aggregate	184,790	-0.055	0.804	-0.485	-0.140	0.004	0.133	0.387
ΔY/L (Wage)	Aggregate	172,938	-0.017	0.545	-0.337	-0.112	-0.005	0.097	0.297
ΔTFP (Employees)	Aggregate	181,505	-0.035	0.649	-0.429	-0.132	0.005	0.131	0.365
ΔTFP (Wage)	Aggregate	171,846	-0.012	0.507	-0.329	-0.112	0.000	0.105	0.302

Table 3

	(1)	(2)	(3)
	Actual Reporting Scope	Actual Auditing Scope	Audit
Standardized Reporting Scope	0.476***	-0.095	-0.114
	(0.085)	(0.077)	(0.080)
Standardized Auditing Scope	-0.104**	0.525***	0.201***
	(0.052)	(0.060)	(0.045)
Industry-Year FE (4-Digit)	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes
Observations	211,573	211,608	211,571
Clusters (Country-Industry (1-Digit))	260	260	260
Clusters (Country-Year)	387	387	387
Adjusted R-Squared	0.835	0.871	0.877

STANDARDIZED SCOPE AND ACTUAL SCOPE

Notes: The table presents estimates from regressions of actual reporting and auditing scopes and actual auditing on standardized reporting and auditing scopes. "Actual Reporting Scope" is the share of firms exceeding reporting-related exemption thresholds in a given country, industry, and year. "Actual Auditing Scope" is the share of firms exceeding auditing-related exemption thresholds in a given country, industry, and year. "Audit" is the share of firms providing audited financial statements in a given country, industry, and year. "Standardized Reporting Scope" is the share of (simulated) firms exceeding reporting-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). "Standardized Auditing Scope" is the share of (simulated) firms exceeding auditing-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). The regressions include industry-year fixed effects (where the industries are defined using four-digit NACE classifications) and country-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

Table 4

STANDARDIZED SCOPE AND OWNERSHIP CONCENTRATION						
	(1)	(2)	(3)	(4)	(5)	(6)
	Publicly Listed		Shareholders		Independence	
	Average	Aggregate	Average	Aggregate	Average	Aggregate
Standardized Reporting Scope	0.008***	0.056***	0.273***	0.442***	0.089*	0.122**
	(0.003)	(0.019)	(0.071)	(0.090)	(0.052)	(0.055)
Standardized Auditing Scope	-0.001	-0.010	0.012	-0.095	-0.010	-0.035
	(0.003)	(0.019)	(0.047)	(0.084)	(0.037)	(0.041)
Industry-Year FE (4-Digit)	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	169,845	161,720	161,385	153,030	157,788	149,502
Clusters (Country-Industry (1-Digit))	260	260	260	260	260	260
Clusters (Country-Year)	311	311	311	311	311	311
Adjusted R-Squared	0.239	0.208	0.819	0.390	0.475	0.212

Notes: The table presents estimates from regressions of ownership concentration measures on standardized reporting and auditing scopes. "Publicly Listed" is the equally (market-share) weighted fraction (sum) of publicly listed firms in a given country, industry, and year in column 1 (2). "Shareholders" is the equally (market-share) weighted average (sum) of firms' logarithmic number of shareholders in a given country, industry, and year in column 3 (4). "Independence" is the equally (market-share) weighted average (sum) of Bureau van Dijk's independence score encoded to range from 0 to 1 in a given country, industry, and year in column 5 (6). "Standardized Reporting Scope" is the share of (simulated) firms exceeding reporting-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). "Standardized Auditing Scope" is the share of (simulated) firms exceeding auditing-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). The regressions include industry-year fixed effects (where the industries are defined using four-digit NACE classifications) and country-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

Table 5

	STANDARDIZED SCOPE AND PRODUCT-MARKET COMPETITION								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Entry		Exit		HHI	Gross Margin		EBITDA/Sales	
	Average	Aggregate	Average	Aggregate		Dispersion	Distance	Dispersion	Distance
Standardized Reporting Scope	0.075**	0.080***	0.011*	0.001	-0.216**	-0.093**	-0.148**	-0.132***	-0.201***
	(0.032)	(0.028)	(0.006)	(0.003)	(0.098)	(0.040)	(0.063)	(0.041)	(0.075)
Standardized Auditing Scope	-0.130***	-0.069***	0.009	0.001	0.056	0.029	0.019	0.011	0.025
	(0.022)	(0.020)	(0.006)	(0.002)	(0.079)	(0.034)	(0.049)	(0.034)	(0.056)
Industry-Year FE (4-Digit)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	209,377	199,717	167,263	159,397	202,124	186,362	186,157	168,374	168,073
Clusters (Country-Industry (1-Digit))	260	260	260	260	260	260	260	250	250
Clusters (Country-Year)	387	387	307	307	385	387	387	372	372
Adjusted R-Squared	0.442	0.219	0.602	0.118	0.503	0.402	0.362	0.389	0.364

Notes: The table presents estimates from regressions of measures of product-market competition on standardized reporting and auditing scopes. "Entry" is the equally (market-share) weighted fraction (sum) of firms founded within the least two years in a given country, industry, and year in column 1 (2). "Exit" is the equally (market-share) weighted fraction (sum) of firms that turned inactive for bankruptcy/illiquidity reasons in a given country, industry, and year in column 3 (4). "HHI" is the sum of squared market shares in a given country, industry, and year. "Gross Margin" is the standard deviation (difference between the 80th and 20th percentile) of the distribution of gross margins defined as sales less wage and material expense or cost of goods sold scaled by sales in a given country, industry, and year in column 6 (7), normalized by the average margin. "EBITDA/Sales" is the standard deviation (difference between the 80th and 20th percentile) of the distribution of EBITDA scaled by sales in a given country, industry, and year in column 8 (9), normalized by the average margin. "Standardized Reporting Scope" is the share of (simulated) firms exceeding reporting-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). "Standardized Auditing Scope" is the share of (simulated) firms exceeding auditing-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). The regressions include industry-year fixed effects (where the industries are defined using four-digit NACE classifications) and country-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

Table 6

STANDARDIZED SCOPE, REVENUE-PRODUCTIVITY DISPERSION,
AND SIZE-PRODUCTIVITY COVARIANCE

	(1) Dispersion	(2) Distance (p80-p20)	(3) Lower Tail (p20)	(4) Upper Tail (p80)	(5) Size-Productivity Covariance	(6)
PANEL A: EMPLOYEES						
	TFP	TFP	TFP	TFP	Y/L	TFP
Standardized Reporting Scope	-0.091*	-0.148*	-0.769	-2.155**	0.063	0.118
	(0.049)	(0.078)	(0.536)	(1.085)	(0.133)	(0.097)
Standardized Auditing Scope	-0.028	-0.030	0.861**	0.330	0.014	0.066
	(0.039)	(0.060)	(0.372)	(0.773)	(0.106)	(0.073)
Industry-Year FE (4-Digit)	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	173,083	173,076	190,097	190,146	177,451	172,978
Clusters (Country-Industry)	260	260	260	260	260	260
Clusters (Country-Year)	384	384	387	387	384	384
Adjusted R-Squared	0.491	0.433	0.356	0.558	0.421	0.379
PANEL B: WAGE						
	(1) Dispersion	(2) Distance (p80-p20)	(3) Lower Tail (p20)	(4) Upper Tail (p80)	(5) Size-Productivity Covariance	(6)
	TFP	TFP	TFP	TFP	Y/L	TFP
Standardized Reporting Scope	-0.145**	-0.260***	-0.119	-0.398**	0.242**	0.202**
	(0.065)	(0.099)	(0.094)	(0.201)	(0.119)	(0.088)
Standardized Auditing Scope	0.010	0.007	0.155**	0.105	0.085	0.122
	(0.054)	(0.080)	(0.064)	(0.150)	(0.111)	(0.080)
Industry-Year FE (4-Digit)	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	165,135	165,119	179,452	179,484	166,505	165,097
Clusters (Country-Industry)	240	240	240	240	240	240
Clusters (Country-Year)	353	354	356	356	354	354
Adjusted R-Squared	0.288	0.263	0.272	0.445	0.336	0.339

Notes: The table presents estimates from regressions of measures of allocative efficiency (revenue-productivity dispersion and size-productivity covariance) on standardized reporting and auditing scopes. "Dispersion" is the standard deviation of the distribution of total factor revenue productivities calculated using employees (wage expense) in a given country, industry, and year in Panel A (B), normalized by the average productivity. "Distance" is the difference between the 80th and the 20th percentile of the distribution of total factor revenue productivities calculated using employees (wage expense) in a given country, industry, and year in Panel A (B), normalized by the average productivity. "Lower Tail (p20)" is the 20th percentile of the distribution of total factor revenue productivities calculated using employees (wage expense) in a given country, industry, and year in Panel A (B). "Upper Tail (p80)" is the 80th percentile of the distribution of total factor revenue productivities calculated using employees (wage expense) in a given country, industry, and year in Panel A (B). "Size-Productivity Covariance" is the difference between the market-share weighted sum and the equally weighted average of labor revenue productivity calculated using employees (wage expense) in a given country, industry, and year in column 5 of Panel A (B). In column 6, "Size-Productivity Covariance" is the difference between the market-share weighted sum and the equally weighted average of factor revenue productivity calculated using employees (wage expense) in a given country, industry, and year in Panel A (B). "Standardized Reporting Scope" is the share of (simulated) firms exceeding reporting-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). "Standardized Auditing Scope" is the share of (simulated) firms exceeding auditing-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). The regressions include industry-year fixed effects (where the industries are defined using four-digit NACE classifications) and country-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

Table 7

	STANDARDIZED SCOPE AND REVENUE PRODUCTIVITY							
	PANEL A: AVERAGE							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Y/L	Y/L	TFP	TFP	ΔY/L	ΔY/L	ΔTFP	ΔTFP
	(Employees)	(Wage)	(Employees)	(Wage)	(Employees)	(Wage)	(Employees)	(Wage)
Standardized Reporting Scope	-0.001	0.123	0.103	0.174*	-0.046**	-0.024	-0.037*	-0.016
	(0.149)	(0.119)	(0.112)	(0.096)	(0.022)	(0.019)	(0.019)	(0.018)
Standardized Auditing Scope	-0.151	-0.126	-0.013	0.008	0.024	-0.004	0.022	0.008
	(0.114)	(0.108)	(0.094)	(0.085)	(0.018)	(0.015)	(0.017)	(0.014)
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	193,245	180,585	190,059	179,505	187,519	176,496	183,326	175,245
Clusters (Country-Industry)	260	240	260	240	260	240	260	240
Clusters (Country-Year)	387	356	387	356	387	354	387	354
Adjusted R-Squared	0.860	0.615	0.801	0.598	0.840	0.119	0.748	0.125
	PANEL B: AGGREGATE							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Y/L	Y/L	TFP	TFP	ΔY/L	ΔY/L	ΔTFP	ΔTFP
	(Employees)	(Wage)	(Employees)	(Wage)	(Employees)	(Wage)	(Employees)	(Wage)
Standardized Reporting Scope	0.076	0.393***	0.198	0.402***	-0.066*	-0.029	-0.029	-0.010
	(0.142)	(0.143)	(0.123)	(0.128)	(0.036)	(0.034)	(0.028)	(0.025)
Standardized Auditing Scope	-0.143	-0.071	0.046	0.105	0.032	-0.003	-0.007	-0.015
	(0.123)	(0.125)	(0.104)	(0.108)	(0.030)	(0.025)	(0.026)	(0.022)
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	193,232	180,572	190,031	179,473	174,154	162,928	171,003	161,871
Clusters (Country-Industry)	260	240	260	240	260	240	260	240
Clusters (Country-Year)	387	356	387	356	387	354	387	354
Adjusted R-Squared	0.757	0.603	0.703	0.562	0.525	0.068	0.420	0.065

Notes: The table presents estimates from regressions of measures of revenue-productivity levels and growth on standardized reporting and auditing scopes. "Y/L (Employees)" is the equally (market-share) weighted average (sum) of labor revenue productivity calculated using employees in a given country, industry, and year in Panel A (B). "Y/L (Wage)" is the equally (market-share) weighted average (sum) of labor revenue productivity calculated using wage expense in a given country, industry, and year in Panel A (B). "TFP (Employees)" is the equally (market-share) weighted average (sum) of total factor revenue productivity calculated using employees in a given country, industry, and year in Panel A (B). "TFP (Wage)" is the equally (market-share) weighted average (sum) of total factor revenue productivity calculated using wage expense in a given country, industry, and year in Panel A (B). "ΔY/L (Employees)" is the equally weighted average of labor revenue productivity growth calculated using employees in a given country, industry, and year in Panel A. "ΔY/L (Wage)" is the equally weighted average of labor revenue productivity calculated using wage expense in a given country, industry, and year in Panel A. "ΔTFP (Employees)" is the equally weighted average of total factor revenue productivity calculated using employees in a given country, industry, and year in Panel A. "ΔTFP (Wage)" is the equally weighted average of total factor revenue productivity calculated using wage

expense in a given country, industry, and year in Panel A. The aggregate productivity growth measures in Panel B are calculated as the first difference in market-share weighted productivities in a given country, industry, and year. "Standardized Reporting Scope" is the share of (simulated) firms exceeding reporting-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). "Standardized Auditing Scope" is the share of (simulated) firms exceeding auditing-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). The regressions include industry-year fixed effects (where the industries are defined using four-digit NACE classifications) and country-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

Table 8

	(1) Standardized Reporting Scope	(2) Actual Reporting Scope
CORRELATED FACTORS		
Number of firms	0.000	-0.001
	(0.001)	(0.002)
Average sales	0.001	0.020***
	(0.001)	(0.002)
Average employees	0.000	0.045***
	(0.001)	(0.003)
Average tangible assets	-0.002**	0.029***
	(0.001)	(0.002)
Concentration (HHI)	-0.000*	0.000***
	(0.000)	(0.000)
Industry-Year FE (4-Digit)	Yes	Yes
Country-Year FE	Yes	Yes
Observations	205,660	205,660
Clusters (Country-Industry)	260	260
Clusters (Country-Year)	387	387
R-Squared (Within)	0.001	0.293

Notes: The table presents estimates of regressions of standardized and actual reporting scopes on potential country-industry-level confounders. Columns (1) and (2) include country-year and industry-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

Table 9

STANDARDIZED SCOPE AND ALTERNATIVE PRODUCTIVITY MEASURES								

PANEL A: EU KLEMS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Value Added		Value Added (Y/L)		Value Added (TFP)		ΔValue Added	
Standardized Reporting Scope	-1.015**		-0.242		-0.285		-0.053**	
	(0.436)		(0.184)		(0.201)		(0.027)	
Standardized Reporting and Auditing Scope		-1.307**		-0.263		-0.096		-0.048
		(0.614)		(0.220)		(0.275)		(0.038)
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,680	12,682	12,659	12,666	8,350	8,352	12,556	12,554
Clusters (Country-Industry)	878	880	883	885	580	580	873	876
Adjusted R-Squared	0.941	0.941	0.776	0.777	0.618	0.617	0.276	0.278

PANEL B: OECD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Value Added		Value Added (Y/L)		Value Added (TFP)		ΔValue Added	
Standardized Reporting Scope	-0.402		-0.521**		-0.332		-0.041	
	(0.647)		(0.205)		(0.217)		(0.034)	
Standardized Reporting and Auditing Scope		-1.872**		-0.593**		-0.440*		-0.038
		(0.840)		(0.259)		(0.245)		(0.042)
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,124	10,129	10,050	10,051	9,607	9,608	10,091	10,088
Clusters (Country-Industry)	719	721	716	717	688	686	725	726
Adjusted R-Squared	0.934	0.934	0.802	0.804	0.604	0.610	0.407	0.412

PANEL C: WIOD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Value Added		Value Added (Y/L)		Value Added (TFP)		ΔValue Added	
Standardized Reporting Scope	-0.438		0.006		-0.083		-0.003	
	(0.397)		(0.150)		(0.065)		(0.004)	
Standardized Reporting and Auditing Scope		-0.897*		-0.161		-0.146*		0.002
		(0.543)		(0.193)		(0.082)		(0.005)
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,429	18,424	18,410	18,419	17,591	17,594	18,425	18,405
Clusters (Country-Industry)	1,366	1,371	1,374	1,378	1,354	1,357	1,379	1,383
Adjusted R-Squared	0.931	0.931	0.698	0.695	0.715	0.711	0.328	0.328

Notes: The table presents estimates from regressions of measures of productivity levels and growth on standardized reporting (and auditing) scopes. The productivity measures are based on official industry-level statistics calculated and provided by EU KLEMS (Panel A), OECD (Panel B), and WIOD (Panel C). The official statistics are provided for two-digit (or coarser) NACE industries for the period 2001 to 2015. The data sources (EU KLEMS, OECD, and WIOD) vary slightly in the granularity of the industry classification, the covered countries, and the covered years. "Value Added" is the logarithm of value added. "Value Added (Y/L)" is the logarithm of value added less the logarithm of labor compensation. "Value Added (TFP)" is the logarithm of value added less 0.7 times the logarithm of labor compensation and 0.3 times the logarithm of capital stock (or capital compensation). "ΔValue Added" is the logarithm of value added less the logarithm of previous period's value added. "Standardized Reporting Scope" is the share of (simulated) firms exceeding reporting-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). "Standardized Reporting and Auditing Scope" is the minimum of the "Standardized Reporting Scope" and the share of (simulated) firms exceeding auditing-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). The scopes are calculated for two-digit NACE industries. They are averaged across two-digit NACE industries if the official statistics are only available at the level of a group of two-digit NACE industries. The regressions include industry-year fixed effects (where the industries are defined using (groups of) two-digit NACE classifications) and country-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using (groups of) two-digit NACE classifications). *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

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Online Appendix

How Does Financial-Reporting Regulation Affect

Industry-wide Resource Allocation?

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Table of Contents

A. Standardized Scope
a. Standardized firm-size distributions

I construct standardized firm-size distributions per industry using the following step-by-step approach:

<u>(1) Moments of the empirical firm-size distributions</u>

I estimate the means, standard deviations, and pairwise correlations of (the logarithm of) total assets, sales, and employees for each industry using observations from all countries and years in which the smallest firms are not exempted from the requirement to publish their income statements. I include the latter restriction to obtain moments of firm-size distributions that are not unduly truncated from below (e.g., the sales distribution) due to the observability of certain size variables.

<u>(2) Multivariate normal draws</u>

I draw 100,000 random observations for each industry from a multivariate normal distribution parameterized by the industry-specific moments (means, standard deviations, and pairwise correlations). Each observation represents a "simulated" firm characterized by three values. These values mimic the firm-size dimensions (logarithmic) total assets, sales, and employees, because they are generated using the moments of the empirically observed joint distribution of firm sizes across firms in a given industry.

<u>(3) Alternative bootstrap approach</u>

As an alternative approach to drawing from a multivariate normal distribution, I draw 100,000 random (firm-year) observations for each industry from the empirically observed firm-year data with replacement. The benefit of this bootstrap approach is that it provides industry-specific firm-size distributions without assuming any particular parametric structure. Its drawback is that it provides industry-specific samples that contain firms with missing data for some of the firm-size dimensions, potentially introducing noise or bias in the estimation of standardized scope. Empirically, using the bootstrap approach yields measures of reporting and auditing scope highly correlated with those obtained using the multivariate normal approach.

b. Formal representation

The standardized scope captures the typical share of firms in a given industry affected by a given country's threshold-based mandate. It is distinct from the actual share of mandated firms in a given country's industry in that it does not use the actual country-industry-specific firm-size distribution to calculate the mandated share. The standardized scope rather uses a typical or standardized distribution per industry to calculate the mandated share. As such, the standardized scope, by construction, does not vary with endogenous differences and changes in industry-level firm-size distributions across countries or over time.

The standardized scope can be represented as follows:

$$StandardizedScope_{ict} = \frac{1}{N_i} \sum_{j=1}^{N_i} 1(s_{ij} > \bar{s}_{ct}),$$

2

where N is the number of firms in industry i, s is the size of firm j, and $\bar{s}$ is the exemption-threshold in a given country (c) at a given point in time (t). (Note that the firm sizes (s) and number of firms (N) do not vary across countries (c) and over time (t).) After accounting for country-year (ct) and industry-year effects (it), the standardized scope only varies due to the *interaction* of country-level thresholds and standardized industry-level firm-size distributions.

The actual scope, by contrast, varies with endogenous differences and changes in country-industry-specific firm-size distributions (e.g., in the number of firms, N_{cit}, and firm sizes, s_{cijt}), even after accounting for the country-year and industry-year fixed effects:

$$Scope_{ict} = \frac{1}{N_{cit}} \sum_{j=1}^{N_{cit}} 1(s_{cijt} > \bar{s}_{ct}).$$

c. Illustrative example
The standardized scope, in combination with the country-year and industry-year fixed effects, resembles a difference-in-differences design which isolates plausibly exogenous variation in regulatory scope driven by the *interaction* of country-level thresholds and industry-level firm-size distributions. The following stylized example illustrates my approach with two countries, A and B, and two industries, 1 and 2.

The table below shows the actual scopes in industry 1 for countries A and B; that is, the shares of firms exceeding the country-level exemption thresholds in the respective country-industries.
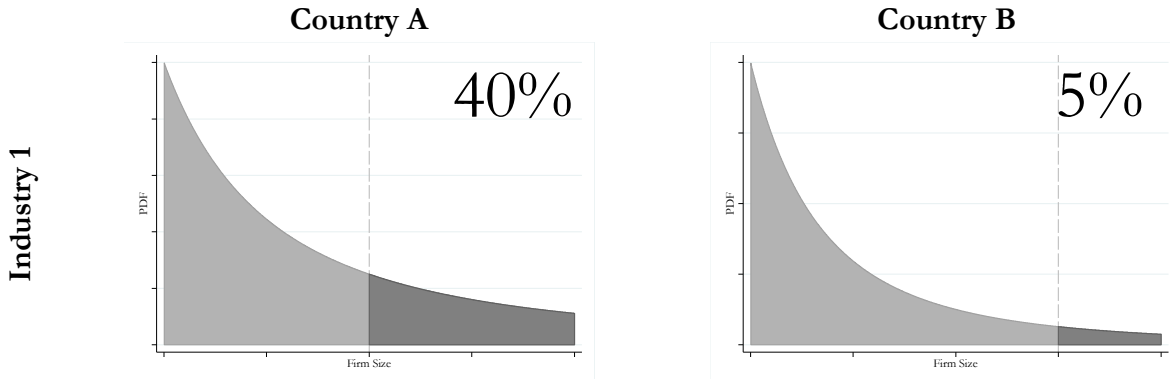
The scope is higher in country A (40%) than in country B (5%). This within-industry difference (35%) is driven by two factors. For one, country A sets a lower exemption threshold than country B. For another, country A's firm-size distribution exhibits a higher share of large firms (thick right tail) than country B's firm-size distribution (thin right tail). The difference in firm-size distributions could be due to different industrial specialization and policies across the country-industries. These policies would confound the scope variation, even when comparing the same industry across countries. To avoid such confounders, I use a standardized firm-size distribution per industry to calculate the standardized scopes.

In this stylized example, I simply use the firm-size distribution of country A as the reference or standardized distribution. Using the same distribution for industry 1 across countries A and B to calculate the scopes, I obtain the following standardized scopes (see below):

Country A   **Country B**
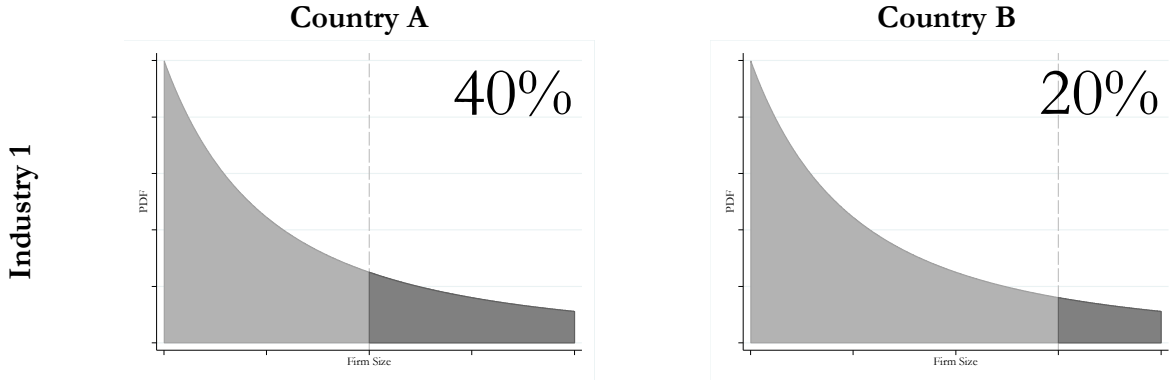
Compared to the difference in actual scopes, the difference in standardized scopes across countries shrinks from 35% to 20% (40% - 20%). The remaining difference purely reflects the regulatory threshold difference across the two countries. While this regulatory variation is precisely the variation I am interested in, it, clearly, could be confounded by other cross-country differences correlated with the threshold choice. Country A, for example, may not just set lower exemption thresholds, but may also exhibit better institutions (e.g., creditor protection) than country B. These country-level differences would correlate with the country-level threshold variation.

To account for the cross-country endogeneity of the thresholds, I focus on within-country variation. This within-country variation arises because a given country's threshold has differential implications for the regulatory scope across industries. In the example below, the standardized firm-size distribution for industry 2 exhibits a lower share of large firms than industry 1, resulting in a lower scope for industry 2 (15%) than industry 1 (40%) in country A.
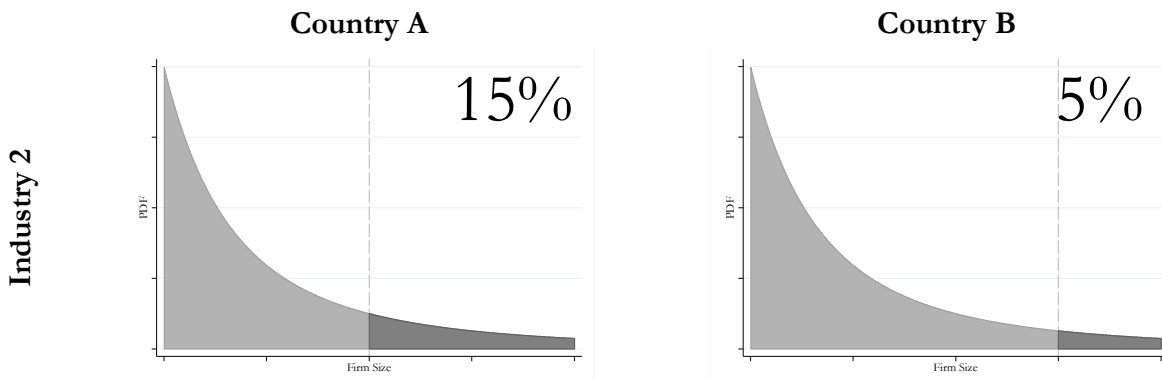
Country A   **Country B**

The within-country difference between industry 1 and 2 is 25% (40% - 15%) for country A and 15% (20% - 5%) for country B. These within-country differences purge any country-level factors from the regulatory scope variation, alleviating concerns about the endogenous choice of the country-level thresholds.

		Difference-in-Differences Variation							
		Actual Scope					Standardized Scope		
		Country					Country		
		A	**B**	**(A)-(B)**			**A**	**B**	**(A)-(B)**
Industry	**1**	40%	5%	35%	Industry	**1**	40%	20%	20%
	2	15%	5%	10%		**2**	15%	5%	10%
	(1)-(2)	25%	0%	**25%**		**(1)-(2)**	25%	15%	**10%**

The ultimate within-country and within-industry variation in scope compares the differential scopes between industry 1 and industry 2 across the two countries A and B:

$$(40\% - 15\%) - (20\% - 5\%) = 10\%$$

This difference-in-differences variation in standardized scopes is purged of any systematic country and industry factors. Moreover, it is free of variation due to country-industry-level firm-size differences. By contrast, the difference-in-differences variation in actual scopes would contain endogenous firm-size variation:

$$(40\% - 15\%) - (5\% - 5\%) = 25\%$$

In essence, the standardization reduces the amount of variation used in my tests. It hones in on variation driven by regulatory threshold variation and its differential impact across industries. It purges any differential variation in scopes across country-industries unrelated to threshold variation (e.g., merely related to firm-size differences). If both country A and B had the same threshold, for example, the difference-in-differences variation in standardized scopes would be zero. This feature assures that my treatment variation is chiefly driven by threshold variation and, as such, captures regulatory instead of other confounding variation. By contrast, the difference-in-differences variation in actual scopes would be non-zero, despite both countries setting the same threshold, due to the endogenous difference in firm-size distributions across country-industries.

Consistent with the above conceptual arguments, Table 8 documents that the within-country-year and within-industry-year variation in standardized scopes is widely uncorrelated with confounding firm-size factors (column 1), whereas the within-country-year and within-industry-year variation in actual scopes is strongly correlated with these factors (column 2).

B. Cross-Sectional Design

There are at least three important reasons for choosing my cross-sectional research approach over alternative approaches relying on time-series variation. First, in contrast to cross-sectional variation in reporting and auditing regulations, there is only limited variation in the regulations within countries over time (e.g., Greenstone et al. [2006]). In particular, the time-series variation in regulations is either limited to a few extreme cases where exemption thresholds were introduced for the first time, or pertains to slight threshold changes as a result of periodic inflation adjustments. The former changes are problematic as several other institutions/regulations tend to change around the time of the extreme reforms (e.g., Leuz [2007], Hail et al. [2017]). The latter changes are problematic, as inflation adjustments tend to change reporting and audit regulations in concert, preventing their separate identification (e.g., Christensen et al. [2013]). Moreover, there is a secular trend toward less extensive regulation over time for nearly all countries in my sample. This trend would not only threaten to confound regulatory effects with general time trends, but would also result in less useful variation: an increase in exemption thresholds reduces current reporting requirements, but does not erase previously reported information. Hence, reductions in reporting regulation provide less powerful regulatory variation than increases in reporting regulation (which are only infrequently observed in the time series of my sample) owing to the continued existence of historical reporting information (e.g., Drake et al. [2016]).

Second, the use of time-series variation in regulation requires a reasonably precise dating of the effective regulation change and the timing of the regulatory incidence. As both the temporal distance between law changes and effective dates and the maximum lags between fiscal year-ends and publication dates vary across countries, it is difficult to assure the correct treatment timing, favoring attenuation bias (e.g., Cochrane [2012]). This issue is compounded by the fact that the use of time-series variation in regulation requires a timely incidence of any regulatory effects and essentially estimates short-run regulatory effects. By contrast, cross-sectional estimates can be interpreted as long-run/steady-state effects (especially given limited time-series changes in regulation in my sample). These long-run effects are arguably of greater interest, especially when considering aggregate (or general-equilibrium-type) effects.

Lastly, the coverage of firms in my data varies by country over time as a result of changes in the data provider's coverage decisions and countries' enforcement actions (e.g., Bernard [2016], Breuer et al. [2018]). These within-country time-series changes are accounted for in my above specification through the inclusion of country-year fixed effects. A specification predicated on the use of within-country time-series variation would have a harder time dealing with these database changes.

C. Data Limitations

Although my construction of the firm-level sample alleviates crucial issues of the Amadeus database, a number of notable limitations remain. The key limitation is that the coverage of firms in Amadeus is generally contingent on countries' reporting regulation. Hence, Amadeus mainly covers the mandatorily reported financial information of limited-liability firms. This has at least three important implications for my study.

First, I cannot observe all firms in a given country and industry, but rather all limited-liability firms subject to at least some financial-reporting requirements. To account for this fact, I explicitly restrict my analysis to limited-liability firms. Although this restriction does not allow me to speak to the mandates' impact on the entire economic activity in an industry, I still capture a substantial portion of economic activity carried out by limited-liability firms (e.g., Kalemli-Ozcan et al. [2015]). This restriction also entails a benefit: by defining my regulatory scopes and outcome measures for the subset of limited-liability firms, I purge my analysis of endogenous cross-country differences in the fraction of limited-liability firms among all operating firms (e.g., due to legal and tax-code differences; Bergner and Heckemeyer [2016]). A drawback of this restriction is that I implicitly assume that firms avoiding mandates through their legal form choice (i.e., by choosing unlimited liability) do not operate rather than operate using another legal form. As this may confound the measurement of my outcomes and my estimation, I assess the robustness of my inferences to this potential legal form choice issue in section "Supplemental Results: Enforcement reform in Germany" using an alternative empirical setting in which I can observe all, not just limited-liability firms.

Second, I cannot observe income statement information (e.g., sales and wage expense) for limited-liability firms that are exempted from the requirements to publicly disclose their income statement and do not choose to voluntarily provide this information. Hence, an increase in financial-reporting regulation in the form of fewer exempted firms would mechanically lead to, for example, a greater number of observed firms (and output), confounding my estimation. Fortunately, there are only nine countries (Austria, Croatia, Germany, Ireland, Luxembourg, Netherlands, Poland, Slovakia, and the United Kingdom) allowing exempted firms to withhold their income statement information, comprising less than a third of all country-industry-year observations. I gauge the robustness of my inferences to this mechanical coverage effect in section "Supplemental Results: Robustness to research-design choices" by excluding the subset of observations potentially affected by this issue and comparing my results with placebo estimates (which, by construction, are merely due to a mechanical coverage effect).

Third, there are some cross-country differences in the availability of data items (e.g., wage expense, employees), resulting in changing samples depending on the definition of outcome measures. For example, income-statement formats used by firms are either prepared classifying expenses by nature (e.g., wage expense; primarily used in continental Europe) or by function (e.g., cost of goods sold; primarily used in the United Kingdom). Accordingly, wage expense is available for most countries, but not all. Similarly, the number of employees is provided for firms in most countries, but not all. I address issues arising due to cross-country differences in collected data items by calculating

multiple versions of key outcome measures (e.g., productivity) using different items (e.g., wage expense versus number of employees) and assess the robustness of my inferences to the exclusion of individual countries by re-estimating my specifications dropping one country at a time.

D. Supplemental Results

My main findings are robust to a variety of different sample-composition and research-design choices and the pro-competitive effect of reporting regulation replicates in an alternative single-country setting, exploiting a substantial enforcement reform pertaining to firms' reporting requirements in Germany.

a. Robustness to research-design choices

I re-estimate my specifications including the standardized reporting scope and standardized auditing scope separately. Without conditioning on the other (reporting or auditing) scope, I find results generally consistent with their jointly estimated results. Accordingly, the distinct associations of reporting and auditing scopes in my main specifications are not merely due to multicollinearity.

Even more so, I find that the effects of reporting and auditing mandates are broadly independent (Table A7). In particular, I find similar effects of reporting mandates in country-industry combinations with and without a corresponding auditing mandate. Likewise, I find similar effects of auditing mandates in country-industry combinations with and without a corresponding (full) reporting mandate. These findings support the separate assessment of the average effects of reporting and auditing mandates in my main tests.

I further re-estimate my specifications excluding all countries exempting smaller firms from the requirement to publish their income statement and excluding one country at a time. The (untabulated) estimates are generally consistent with my main results. Accordingly, my findings do not appear to be unduly driven or affected by individual countries or a mechanical coverage effect associated with the income statement publication exemption.

Additionally, I re-estimate my specifications controlling for country-industry-specific dimensions of firm-size distributions, such as the average, aggregate, dispersion, and correlation of total assets, sales, and employees. My inferences remain unchanged. Notably, controlling for aggregate sales of an industry, for example, amounts to accounting for country-specific industrial specializations in my within-country and within-industry design. My results do not appear to be confounded by such country-industry-specific factors.

I further assess the sensitivity of my results to my choice of the specific industry classification. In untabulated tests, I find using broader classifications (two and three digit NACE codes) does not alter my inferences, but appears to worsen the measurement of some outcomes (e.g., within-industry productivity dispersion). I also assess the robustness of my results to using historical thresholds determined before my sample period instead of the actual thresholds in place in a given year. In untabulated tests, I find broadly consistent results using the pre-sample period thresholds. In particular, I find using the historical thresholds appears to mainly weaken the ability to differentiate between reporting and auditing mandates (due to reduced variation). It does not, however, hint at any endogeneity issues associated with using the actual thresholds "chosen" for a given year. This evidence is consistent with my design successfully alleviating endogeneity issues related to changes in the

thresholds by focusing within a given country at a given point in time instead of using endogenous threshold changes.

Lastly, I explicitly gauge the impact of a hypothetical coverage effect on my results. I calculate "placebo" outcomes (e.g., average and aggregate labor productivity) for a given industry in a given country and year using the previously simulated firms (of the standardized firm-size distributions). To mechanically induce a hypothetical coverage effect, I calculate the placebo outcomes using only those simulated firms exceeding a country's reporting thresholds in a given year. As a result, I obtain placebo outcomes that vary within industries and across countries not because firms and firm-size distributions are different, but merely because more firms are "observable" and thus included in the placebo outcome calculation for countries exempting fewer firms (i.e., with lower thresholds).

Using the placebo outcomes as dependent variables in my specifications, I find (in untabulated tests) that the hypothetical coverage effect produces dispersion results opposite to my empirical findings, but also generates aggregate productivity and size-productivity covariance overlapping with my main results. Consistent with these placebo results, I find that my main dispersion results are, if anything, strengthened when controlling for the placebo effects (using the placebo outcomes as controls), whereas the aggregate productivity and size-productivity results attenuate and are no longer statistically significant. Accordingly, the placebo results suggest that the dispersion results are unlikely to be driven by a mechanical coverage effect. By contrast, I cannot discern an economic effect of reporting regulation on aggregate productivity and size-productivity covariance from a hypothetical coverage effect. In sum, these results support the pro-competitive effect of reporting mandates and suggest caution in interpreting the resource allocation results related to aggregate productivity and the size-productivity covariance.

b. Enforcement reform in Germany: Design & Results

To corroborate the pro-competitive effect of reporting mandates on product markets in an alternative setting, I exploit a major shift in enforcement of reporting requirements in Germany. Despite prescribing size-based reporting requirements in accordance with EU directives, Germany had virtually not enforced these requirements until a sweeping enforcement reform in 2007 (e.g., Bernard [2016]). Before 2007, limited-liability firms were required to file their financial statements with local courts and publish their statements in local newspapers. As local courts were not allowed to engage in pro-active enforcement and legal/monetary sanctions for non-disclosing firms were low, the share of limited-liability firms complying with reporting requirements was as low as 5%. Only in response to mounting pressure from the EU commission and the transposition deadline for EU Directive 2003/58/EC did the German legislator reform its disclosure enforcement via the Bill on the Electronic Registers for Commerce, Companies and Associations (EHUG) in 2007 (effective for financial statements covering fiscal years ending December 2006 or later), switching to a central electronic publication register, pro-active enforcement by the ministry of justice, and escalating fines.

Using comprehensive census data from the German Federal Statistical Office on firm sales and business notifications (on entry and exit) for the years 2003 to 2012, I investigate the effect of the

enforcement reform on product-market competition using a flexible difference-in-differences design with a continuous treatment variable:[1]

$$Y_{c,i,t} = \sum_{\tau \neq 2006} \beta_\tau Regulated_{c,i} \times 1(t = \tau) + \alpha_{c,t} + \delta_{i,t} + \gamma_{c,i} + \varepsilon_{c,i,t} ,$$

where $Y_{c,i,t}$ is the outcome variable of interest (e.g., market-share concentration) in county c, industry i (two-digit NACE industry classification), and year t; $Regulated_{c,i}$ is the share of limited-liability firms (among all firms) in county c and industry i in the pre-enforcement period (in particular, in the base year: 2006); $1(t = \tau)$ represents a separate year indicator for each year (except for the base year: 2006); $\alpha_{c,t}$ denotes county-year fixed effects; $\delta_{i,t}$ denotes industry-year fixed effects; and $\gamma_{c,i}$ denotes county-industry fixed effects.

This specification generates nine difference-in-differences coefficients (each relative to the base year: 2006). These coefficients capture, for each year separately, differences in sensitivities (i.e., regression slopes) of the outcome variable with respect to the share of limited-liability firms relative to the respective sensitivity in the base year 2006.[2] As the enforcement reform increases the pressure on all limited-liability firms to publicly disclose their financial information, I use the share of limited-liability firms among all firms as my continuous treatment variable ($Regulated_{c,i}$), assuming that county-industry combinations with a greater (pre-enforcement) share of limited-liability firms will be more strongly affected by the enforcement reform.

Figure A1 documents that the enforcement reform is associated with a steep increase in the share of disclosing firms (approximated by the number of limited-liability firms covered in Amadeus relative to all firms covered in the census data for a given county, industry, and year), consistent with prior evidence (e.g., Bernard [2016], Breuer et al. [2018]).[3] Figure 4 documents that firm entry ("Entry") and exit ("Exit") increase, whereas product-market concentration ("HHI") decreases after 2006/7 for county-industries with a greater (pre-enforcement) share of limited-liability firms. These findings are consistent with fiercer product-market competition as a result of increased enforcement of reporting regulation.[4]

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[1] Source: RDC of the Federal Statistical Office and Statistical Offices of the Länder in Germany, Unternehmensregister and Gewerbeanzeigenstatistik, survey years 2003 - 2012, own calculations.

[2] The interaction between the share of limited-liability firms ("Regulated") and year indicators constitute the difference-in-differences coefficients of interest. As my treatment variable (share of limited-liability firms) is continuous, the difference-in-differences coefficients do not capture the differential levels across treatment and control and the pre- and post-period, but rather differential slopes (e.g., Carpenter and Dobkin [2011]). The main effects ("Regulated" and the year indicators) are subsumed by the county-industry, county-year, and industry-year fixed effects. I cluster standard errors at the county level.

[3] The significant pre-trend before 2006 is due to the database expansion of Amadeus which resulted in increased coverage of limited-liability firms even before the enforcement reform. The sharp increase in 2007, however, is clearly due to the enforcement reform as documented in prior literature and shown by more than 300,000 non-compliance notices sent by the Federal Ministry of Justice under threat of punishment to non-disclosing firms in 2007 (Schlauss [2008]).

[4] As most public disclosures were made in and after December 2007, the informational (in contrast to the avoidance) effect of the enforcement reform should be expected to mostly occur after 2007.

Figure A2, Figure A3, and Figure A4 further document that increases in entry by subsidiaries and exit due to unprofitability as well as decreases in product-market concentration after 2006/7 are concentrated in county-industries composed of few firms in the pre-enforcement period. Consistent with reduced informational entry barriers due to public disclosure, these findings suggest that the enforcement of reporting regulation can spur competition and reallocation of market shares especially in previously opaque and concentrated markets.

This alternative single-country setting complements my prior analysis in three important respects. First, it permits a more familiar temporal difference-in-differences approach that compares more and less affected county-industries across several years before and after the enforcement reform.[5] Second, the alternative setting allows me to observe all firms independent of their legal form choice and reporting requirements. Third, the alternative setting provides me with official entry and especially exit information including the type of and reason for entry or exit. Using the temporal difference-in-differences approach on a comprehensive firm sample with detailed entry and exit information, I find results consistent with my main analysis. Thus, the findings of the alternative setting corroborate the cross-sectional difference-in-differences approach employed in my main analysis, allay concerns that time-invariant confounders (e.g., other size-based regulations) and sample selection/truncation (related to legal form choice, Amadeus coverage, etc.) unduly confound my main results, and contribute an improved measurement of business dynamism (i.e., entry and especially exit).

c. Enforcement reform in Germany: Data

I obtain access to confidential data on firm sales from the AFiD-Panel Unternehmensregister and to data on firm entry and exit from the Gewerbeanzeigenstatistik for the years 2003 to 2012, provided by the Research Data Centers of the Federal Statistical Office and the statistical offices of the States in Germany. I harmonize the county codes across years using the official county correspondence table provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) (with 2014 as the reference year). I harmonize the NACE industry codes across years using the official industry code correspondence table provided by the Federal Statistical Office (with 2008 as the reference year).

I code limited-liability firms (GmbH, GmbH & Co. KG, AG, KGaA) as affected and unlimited-liability firms (sole proprietorship, OHG, KG, cooperative) as unaffected by the enforcement change.

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[5] This setting exhibits a number of drawbacks relative to my main setting. First, I have to worry about concurrent events confounding the single-shock temporal difference-in-differences design (e.g., a reduction of minimum legal capital requirements for limited-liability firms (Becht et al. [2008], Braun et al. [2011], Braun et al. [2013]), or a corporate tax reform (Dobbins and Jacob [2016])). Second, the reformed enforcement of reporting mandates does not allow studying the separate effect of auditing mandates. Third, the census databases provide only few potential outcome variables and exhibits structural breaks in industry classifications that can only imperfectly be harmonized. Lastly, estimates from the single-country setting are arguably less generalizable than those obtained using a broader sample of countries.

d. Alternative productivity measures

To clarify the net impact of reporting mandates on aggregate productivity and its growth, I re-examine the relationship using official industry-level productivity statistics provided by EU KLEMS, the OECD, and WIOD (Timmer et al. [2015], Stehrer et al. [2019], Adarov and Stehrer [2019]). The three data sources provide a comparable set of productivity measures for (groups of) two-digit NACE industries for the period of 2001 to 2015. The sources vary slightly in the granularity of the industry classification and the coverage of my main sample countries and years.

I investigate the association between the standardized reporting scope and four aggregate productivity measures. The productivity measures are a raw productivity or output measure ("Value Added"), a labor productivity measure ("Value Added (Y/L)"), a total factor productivity measure ("Value Added (TFP)"), and a productivity or output growth measure ("ΔValue Added").

Across all three data sources, I find that reporting scope is either insignificantly or significantly negatively associated with the productivity level and growth measures in Table 9. Similar results obtain when using a combined reporting and auditing scope measure (i.e., the minimum of the reporting and auditing scopes).

These supplemental results highlight that the productivity level results in my main tests are not robust to using alternative productivity level measures. They suggest that reporting mandates, if anything, appear to have a negative impact on aggregate allocative efficiency, consistent with the productivity growth results in my main tests. Overall, however, I caution that my collective results primarily suggests that there is no clear evidence that reporting mandates help or hurt aggregate allocative efficiency, despite their significant impact on the type of resource allocation (e.g., ownership dispersion in capital markets and competition in product markets).

E. References

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Online Appendix Figures & Tables

Figure A1

PUBLIC DISCLOSURE ENFORCEMENT
AND DISCLOSURE RATE
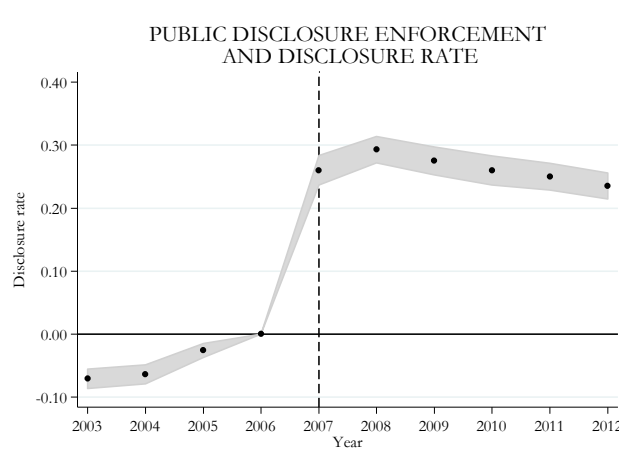
Notes: The figure depicts estimates of a regression of disclosure rate on the share of affected firms. The annual estimates represent difference-in-differences coefficients relative to the base year 2006. "Disclosure Rate" is defined as the fraction of limited-liability firms observable in Bureau van Dijk's Amadeus database relative to all firms in a given county, industry, and year in Germany. The share of affected firms is defined as the fraction of limited-liability firms in a given county and industry in the base year 2006. The gray shading represents the point-wise 95% confidence interval.

Figure A2

PUBLIC DISCLOSURE ENFORCEMENT
AND ENTRY OF SUBSIDIARIES
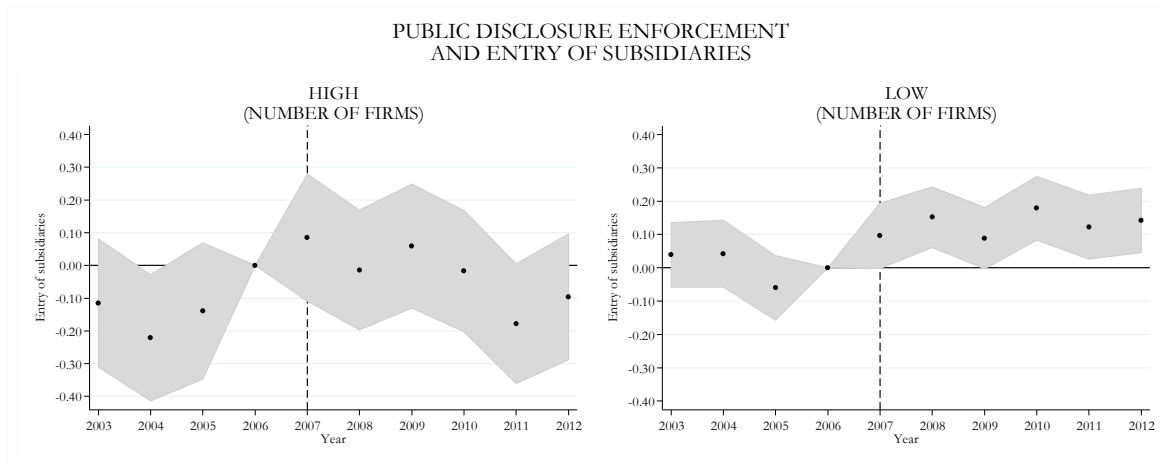
Notes: The figure depicts estimates of regressions of entry of subsidiaries on the share of affected firms split by the number of operating firms in the pre-enforcement period. The left (right) graph shows estimates for county-industries with an above (below) median number of operating firms in the pre-enforcement period. The annual estimates represent difference-in-differences coefficients relative to the base year 2006. "Entry of Subsidiaries" is defined as the log number of subsidiaries newly registering at the local commercial register/court in a given county, industry, and year in Germany. The share of affected firms is defined as the fraction of limited-liability firms in a given county and industry in the base year 2006. The gray shading represents the point-wise 95% confidence interval.

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Figure A3

PUBLIC DISCLOSURE ENFORCEMENT
AND EXIT DUE TO UNPROFITABILITY
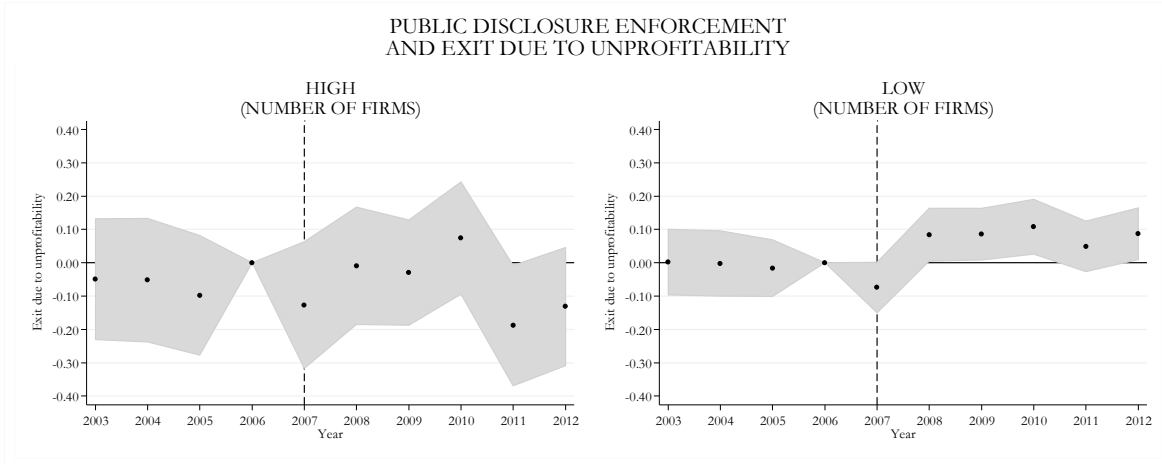
Notes: The figure depicts estimates of regressions of exit due to unprofitability on the share of affected firms split by the number of operating firms in the pre-enforcement period. The left (right) graph shows estimates for county-industries with an above (below) median number of operating firms in the pre-enforcement period. The annual estimates represent difference-in-differences coefficients relative to the base year 2006. "Exit due to Unprofitability" is defined as the log number of firms deregistering at the local commercial register/court due to unprofitability in a given county, industry, and year in Germany. The share of affected firms is defined as the fraction of limited-liability firms in a given county and industry in the base year 2006. The gray shading represents the point-wise 95% confidence interval.

Figure A4

PUBLIC DISCLOSURE ENFORCEMENT
AND PRODUCT MARKET CONCENTRATION
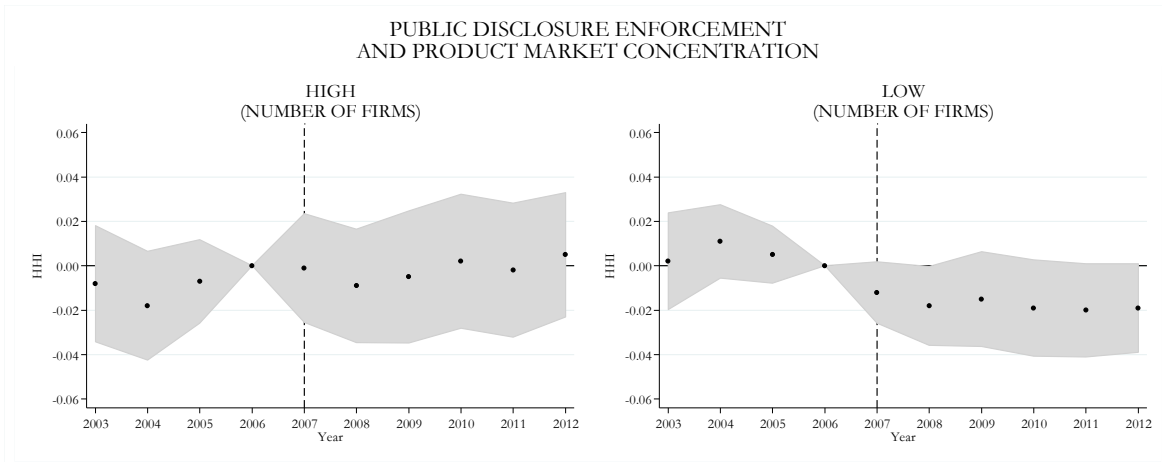
Notes: The figure depicts estimates of regressions of product-market concentration on the share of affected firms split by the number of operating firms in the pre-enforcement period. The left (right) graph shows estimates for county-industries with an above (below) median number of operating firms in the pre-enforcement period. The annual estimates represent difference-in-differences coefficients relative to the base year 2006. "Product Market Concentration" is defined as the sum of squared market shares in a given county, industry, and year in Germany. The share of affected firms is defined as the fraction of limited-liability firms in a given county and industry in the base year 2006. The gray shading represents the point-wise 95% confidence interval.

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Table A1

REPORTING-REGULATION EXAMPLE

Excerpt of title page and table of contents	Individual pages

Exempted firm

NMC Surfacing Limited

Abbreviated Accounts

for the Year Ended 31 December 2014
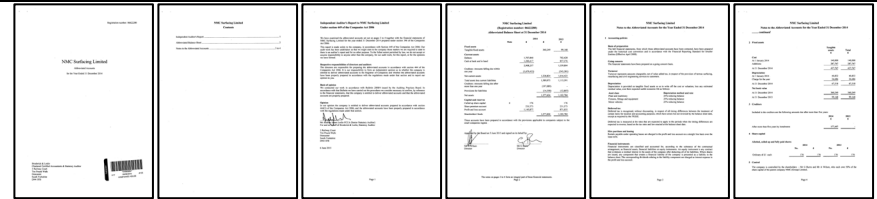
Non-exempted firm

NMC Surfacing Limited

Annual Report and Financial Statements

for the Year Ended 31 December 2015

Table A2

	STANDARDIZED REPORTING AND AUDITING SCOPES BY COUNTRY AND YEAR													
	2001		2002		2003		2004		2005		2006		2007	
Country	Reporting	Auditing	Reporting	Auditing	Reporting	Auditing	Reporting	Auditing	Reporting	Auditing	Reporting	Auditing	Reporting	Auditing
Austria	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.16	0.16	0.15	0.15	0.15	0.15
Belgium	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.15	0.15	0.15	0.15
Bulgaria	0.31	0.41	0.31	0.41	0.31	0.35	0.29	0.33	0.28	0.32	0.20	0.24	0.20	0.24
Croatia	0.25	1.00	0.25	1.00	0.25	1.00	0.26	1.00	0.26	1.00	0.25	1.00	0.06	0.93
Czech Republic	1.00	0.17	1.00	0.17	0.06	0.06	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05
Denmark	1.00	1.00	1.00	1.00	0.08	1.00	0.07	1.00	0.06	1.00	0.06	1.00	0.06	0.27
Estonia	1.00	0.26	1.00	0.25	1.00	0.26	1.00	0.26	1.00	0.25	1.00	0.18	1.00	0.18
Finland	0.21	1.00	0.21	1.00	0.16	1.00	0.16	1.00	0.16	1.00	0.15	1.00	0.15	1.00
France	0.47	0.22	0.47	0.22	0.47	0.22	0.47	0.22	0.47	0.22	0.47	0.22	0.47	0.22
Germany	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.15	0.15	0.15	0.15	0.15	0.15
Greece	0.24	0.24	0.23	0.23	0.22	0.22	0.22	0.22	0.23	0.23	0.23	0.23	0.23	0.23
Hungary	0.03	0.13	0.03	0.13	0.03	0.12	0.03	0.12	0.03	0.12	0.03	0.12	0.02	0.12
Ireland	0.20	0.27	0.20	0.27	0.20	0.28	0.21	0.28	0.20	0.28	0.20	0.23	0.21	0.24
Italy	0.18	0.18	0.18	0.18	0.17	0.17	0.17	0.17	0.16	0.16	0.17	0.17	0.17	0.17
Lithuania	1.00	1.00	1.00	1.00	1.00	1.00	0.11	1.00	0.20	0.12	0.20	0.12	0.20	0.12
Luxembourg	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.14	0.14	0.14	0.14
Netherlands	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.14	0.14
Norway	0.08	1.00	0.08	1.00	0.08	1.00	0.07	1.00	0.06	1.00	0.06	1.00	0.06	1.00
Poland	0.27	0.19	0.21	0.19	0.21	0.19	0.21	0.19	0.20	0.19	0.20	0.19	0.20	0.18
Portugal	1.00	0.31	1.00	0.31	1.00	0.22	1.00	0.22	1.00	0.22	1.00	0.22	1.00	0.22
Romania	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	0.16	0.16	0.16	0.16
Slovakia	0.11	0.11	0.11	0.11	0.11	0.11	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10
Slovenia					0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.01	0.16	0.16
Spain	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Sweden	0.17	1.00	0.17	1.00	0.17	1.00	0.16	1.00	0.16	1.00	0.16	1.00	0.16	1.00
United Kingdom	0.23	0.27	0.24	0.28	0.23	0.27	0.22	0.25	0.16	0.16	0.16	0.16	0.16	0.16
Total	0.37	0.43	0.37	0.43	0.29	0.41	0.26	0.40	0.26	0.36	0.22	0.32	0.22	0.29

Country	2008 Reporting	2008 Auditing	2009 Reporting	2009 Auditing	2010 Reporting	2010 Auditing	2011 Reporting	2011 Auditing	2012 Reporting	2012 Auditing	2013 Reporting	2013 Auditing	2014 Reporting	2014 Auditing	2015 Reporting	2015 Auditing
Austria	0.15	0.15	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.13	0.13
Belgium	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Bulgaria	0.20	0.20	0.20	0.20	0.20	0.20	0.21	0.21	0.20	0.20	0.21	0.21	0.21	0.21	0.21	0.21
Croatia	0.06	0.93	0.05	0.93	0.05	0.93	0.06	0.93	0.05	0.93	0.06	0.93	0.06	0.93	0.06	0.93
Czech Republic	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05
Denmark	0.06	0.26	0.05	0.26	0.05	0.26	0.05	0.26	0.05	0.19	0.05	0.19	0.05	0.19	0.05	0.19
Estonia	1.00	0.18	1.00	0.17	1.00	0.17	1.00	0.26	1.00	0.26	1.00	0.26	1.00	0.26	1.00	0.26
Finland	0.15	1.00	0.15	0.66	0.15	0.66	0.15	0.66	0.15	0.66	0.15	0.66	0.15	0.66	0.15	0.66
France	0.47	0.22	0.47	0.22	0.47	0.22	0.47	0.22	0.29	0.22	0.29	0.22	0.30	0.22	0.15	0.22
Germany	0.15	0.15	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.13	0.13
Greece	0.23	0.23	0.19	0.19	0.18	0.18	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19
Hungary	0.02	0.12	0.02	0.10	0.02	0.10	0.02	0.10	0.02	0.10	0.02	0.10	0.02	0.10	0.02	0.09
Ireland	0.21	0.16	0.21	0.16	0.21	0.16	0.21	0.16	0.21	0.16	0.20	0.16	0.15	0.15	0.14	0.14
Italy	0.16	0.16	0.15	0.15	0.14	0.15	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14
Lithuania	0.19	0.12	0.15	0.11	0.15	0.11	0.16	0.11	0.15	0.11	0.16	0.11	0.15	0.11	0.15	0.10
Luxembourg	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.13	0.13	0.13	0.13	0.13	0.13	0.09	0.09
Netherlands	0.15	0.15	0.15	0.15	0.14	0.14	0.14	0.14	0.15	0.15	0.14	0.14	0.15	0.15	0.14	0.14
Norway	0.06	1.00	0.05	1.00	0.06	1.00	0.06	1.00	0.05	0.34	0.05	0.34	0.05	0.34	0.05	0.34
Poland	0.20	0.19	0.20	0.19	0.20	0.19	0.20	0.18	0.20	0.18	0.20	0.18	0.20	0.19	0.20	0.18
Portugal	1.00	0.22	0.11	0.22	0.11	0.22	0.36	0.22	0.22	0.22	0.22	0.22	0.22	0.22	0.20	0.20
Romania	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16		
Slovakia	0.09	0.09	0.09	0.09	0.28	0.28	0.28	0.28	0.28	0.28	0.28	0.28	0.28	0.28	0.28	0.28
Slovenia	0.16	0.16	0.16	0.16	0.15	0.15	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14
Spain	0.19	0.19	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.14	0.16
Sweden	0.06	1.00	0.06	1.00	0.06	1.00	0.06	1.00	0.05	0.34	0.05	0.35	0.05	0.35	0.05	0.35
United Kingdom	0.15	0.15	0.14	0.14	0.14	0.14	0.15	0.15	0.14	0.14	0.15	0.15	0.15	0.15	0.15	0.15
Total	0.21	0.29	0.17	0.27	0.18	0.27	0.19	0.28	0.17	0.22	0.18	0.23	0.17	0.22	0.16	0.23

Table A3

REGULATORY THRESHOLDS

PANEL A: REPORTING THRESHOLDS

Country	Period	Currency	Total Assets	Sales*	Employees
Austria	2000-2004	EUR	3,125,000	6,250,000	50
	2005-2007	EUR	3,650,000	7,300,000	50
	2008-2014	EUR	4,840,000	9,680,000	50
Belgium	2000-2004	EUR	3,125,000	6,250,000	50
	2005-2014	EUR	3,650,000	7,300,000	50
Bulgaria	2000-2004	BGN	800,000	1,000,000	50
	2005-2006	BGN	1,000,000	5,000,000	50
	2007-2014	BGN	1,500,000	2,500,000	50
Croatia	2000-2005	DEM	2,000,000	4,000,000	50
	2006-2007	HRK	27,000,000	54,000,000	50
	2008-2014	HRK	32,500,000	65,000,000	50
Czech Republic	2000-2001	CZK	0	0	0
	2002-2014	CZK	40,000,000	80,000,000	50
Denmark	2000-2001	DKK	0	0	0
	2002-2003	DKK	20,000,000	40,000,000	50
	2004-2008	DKK	29,000,000	58,000,000	50
	2009-2014	DKK	36,000,000	72,000,000	50
Estonia	2000-2014	EUR	0	0	0
Finland	2000-2001	FIM	10,000,000	20,000,000	50
	2002-2004	EUR	3,125,000	6,250,000	50
	2005-2014	EUR	3,650,000	7,300,000	50
France	2000-2001	FRF	1,750,000	3,500,000	10
	2002-2010	EUR	267,000	534,000	10
	2011-2013	EUR	1,000,000	2,000,000	20
	2014-2014	EUR	4,000,000	8,000,000	50
Germany	2000-2001	DEM	6,720,000	13,440,000	50
	2002-2003	EUR	3,438,000	6,875,000	50
	2004-2007	EUR	4,015,000	8,030,000	50
	2008-2013	EUR	4,840,000	9,680,000	50
	2014-2014	EUR	6,000,000	12,000,000	50
Greece	2000-2001	GRD	500,000,000	1,000,000,000	50
	2002-2006	EUR	1,500,000	3,000,000	50
	2007-2014	EUR	2,500,000	5,000,000	50
Hungary	2000-2000	HUF	150,000,000	300,000,000	100
	2001-2005	HUF	150,000,000	300,000,000	50
	2006-2014	HUF	500,000,000	1,000,000,000	50
Ireland	2000-2012	IEP	1,500,000	3,000,000	50
	2013-2014	EUR	4,400,000	8,800,000	50
Italy	2000-2001	ITL	4,700,000,000	9,500,000,000	50
	2002-2006	EUR	3,125,000	6,250,000	50
	2007-2008	EUR	3,650,000	7,300,000	50
	2009-2014	EUR	4,400,000	8,800,000	50
Lithuania	2000-2002	LTL	0	0	0
	2003-2003	LTL	5,000,000	10,000,000	250
	2004-2007	LTL	5,000,000	7,000,000	10
	2008-2014	LTL	6,000,000	10,000,000	15

Luxembourg	2000-2010	EUR	3,125,000	6,250,000	50
	2011-2014	EUR	4,400,000	8,800,000	50
Netherlands	2000-2001	NLG	7,500,000	15,000,000	50
	2002-2003	EUR	3,500,000	7,000,000	50
	2004-2005	EUR	3,650,000	7,300,000	50
	2006-2014	EUR	4,400,000	8,800,000	50
Norway	2000-2003	NOK	20,000,000	40,000,000	50
	2004-2009	NOK	30,000,000	60,000,000	50
	2010-2014	NOK	35,000,000	70,000,000	50
Poland	2000-2000	EUR	1,000,000	2,000,000	50
	2001-2014	EUR	2,000,000	4,000,000	50
Portugal	2000-2007	EUR	0	0	0
	2008-2009	EUR	10,000,000	10,000,000	50
	2010-2010	EUR	500,000	1,000,000	20
	2011-2014	EUR	1,500,000	3,000,000	50
Romania	2000-2004	EUR	0	0	0
	2005-2014	EUR	3,650,000	7,300,000	50
Slovakia	2000-2008	SKK	20,000,000	40,000,000	20
	2009-2014	EUR	1,000,000	2,000,000	30
Slovenia	2002-2004	SIT	500,000,000	1,000,000,000	50
	2005-2005	SIT	850,000,000	1,700,000,000	50
	2006-2008	EUR	3,650,000	7,300,000	50
	2009-2014	EUR	4,400,000	8,800,000	50
Spain	2000-2007	ESP	395,000,000	790,000,000	50
	2008-2013	EUR	2,850,000	5,700,000	50
	2014-2014	EUR	4,000,000	8,000,000	50
Sweden	2000-2006	SEK	23,000,000		10
	2007-2010	SEK	25,000,000	50,000,000	50
	2011-2014	SEK	40,000,000	80,000,000	50
United Kingdom	2000-2003	GBP	1,400,000	2,800,000	50
	2004-2007	GBP	2,800,000	5,600,000	50
	2008-2014	GBP	3,260,000	6,500,000	50

PANEL B: AUDITING THRESHOLDS

Country	Period	Currency	Total Assets	Sales*	Employees
Austria	2000-2004	EUR	3,125,000	6,250,000	50
	2005-2007	EUR	3,650,000	7,300,000	50
	2008-2014	EUR	4,840,000	9,680,000	50
Belgium	2000-2004	EUR	3,125,000	6,250,000	50
	2005-2014	EUR	3,650,000	7,300,000	50
Bulgaria	2000-2001	BGN	300,000	600,000	30
	2002-2004	BGN	5,000,000	1,000,000	30
	2005-2006	BGN	10,000,000	2,000,000	50
	2007-2014	BGN	15,000,000	2,500,000	50
Croatia	2000-2005	DEM	0	0	0
	2006-2014	HRK		30,000,000	
Czech Republic	2000-2001	CZK	20,000,000	40,000,000	
	2002-2014	CZK	40,000,000	80,000,000	50
Denmark	2000-2005	DKK	0	0	0
	2006-2010	DKK	1,500,000	3,000,000	12
	2011-2014	DKK	4,000,000	8,000,000	12
Estonia	2000-2002	EEK		1,000,000	
	2003-2004	EEK	3,000,000	6,000,000	5
	2005-2009	EEK	5,000,000	10,000,000	10
	2010-2014	EEK	1,000,000	2,000,000	30
Finland	2000-2006	EUR	0	0	0
	2007-2014	EUR	100,000	200,000	3
France	2000-2001	FRF	10,000,000	20,000,000	50
	2001-2014	EUR	1,550,000	3,100,000	50
Germany	2000-2001	DEM	6,720,000	13,440,000	50
	2002-2003	EUR	3,438,000	6,875,000	50
	2004-2007	EUR	4,015,000	8,030,000	50
	2008-2013	EUR	4,840,000	9,680,000	50
	2014-2014	EUR	6,000,000	12,000,000	50
Greece	2000-2002	GRD	500,000,000	1,000,000,000	50
	2002-2006	EUR	1,500,000	3,000,000	50
	2007-2014	EUR	2,500,000	5,000,000	50
Hungary	2000-2007	HUF		50,000,000	
	2008-2011	HUF		100,000,000	50
	2012-2013	HUF		200,000,000	50
	2014-2014	HUF		300,000,000	50
Ireland	2000-2004	IEP	1,500,000	250,000	50
	2005-2007	EUR	1,904,607	1,500,000	50
	2008-2012	EUR	3,650,000	7,300,000	50
	2013-2014	EUR	4,400,000	8,800,000	50
Italy	2000-2001	ITL	4,700,000,000	9,500,000,000	50
	2002-2006	EUR	3,125,000	6,250,000	50
	2007-2009	EUR	3,650,000	7,300,000	50
	2010-2014	EUR	4,400,000	8,800,000	50
Lithuania	2000-2003	LTL	0	0	0
	2004-2007	LTL	5,000,000	10,000,000	50
	2008-2014	LTL	6,000,000	12,000,000	50
Luxembourg	2000-2010	EUR	3,125,000	6,250,000	50
	2011-2014	EUR	4,400,000	8,800,000	50

Netherlands	2000-2001	NLG	7,500,000	15,000,000	50
	2002-2003	EUR	3,500,000	7,000,000	50
	2004-2005	EUR	3,650,000	7,300,000	50
	2006-2014	EUR	4,400,000	8,800,000	50
Norway	2000-2010	NOK	0	0	0
	2011-2014	NOK	20,000,000		10
Poland	2000-2014	EUR	2,500,000	5,000,000	50
Portugal	2000-2001	PTE	140,000,000	280,000,000	50
	2002-2014	EUR	1,500,000	3,000,000	50
Romania	2000-2004	EUR	0	0	0
	2005-2014	EUR	3,650,000	7,300,000	50
Slovakia	2000-2008	SKK	20,000,000	40,000,000	20
	2009-2014	SKK	1,000,000	2,000,000	30
Slovenia	2002-2004	SIT	500,000,000	1,000,000,000	50
	2005-2005	SIT	850,000,000	1,700,000,000	50
	2006-2008	EUR	3,650,000	7,300,000	50
	2009-2014	EUR	4,400,000	8,800,000	50
Spain	2000-2007	ESP	395,000,000	790,000,000	50
	2008-2014	EUR	2,850,000	5,700,000	50
Sweden	2000-2010	SEK	0	0	0
	2011-2014	SEK	1,500,000	3,000,000	3
United Kingdom	2000-2003	GBP	1,400,000	1,000,000	50
	2004-2007	GBP	2,800,000	5,600,000	50
	2008-2014	GBP	3,260,000	6,500,000	50

Notes: The table lists threshold values for reporting and auditing exemptions gathered via researching legal sources and surveying knowledgeable parties in the respective countries. * The sales dimension is defined as operating income in a number of countries.

	LEGAL SOURCES FOR REPORTING AND AUDITING REQUIREMENTS	
Country	Reporting	Auditing
Austria	§906 Abs. 2 UGB §221 Abs. 1 UGB §906 Abs. 11 UGB & ReLÄG 2004 §906 Abs. 18 UGB & URÄG 2008 §906 Abs. 28 UGB & RÄG 2014	§ 268 Abs. 1 UGB
Belgium	Art.4 Arrêté royal modifiant Art. 15 Art. 2 Arrêté royal modifiant Art. 15 Art. 3 Loi 2005 modifiant Art. 15 Art. 15 Code des sociétés & Art. 3 Modifications du Code de sociétés 2015	Art. 141 (2) Code des sociétés 1999 Art. 141 (2) Code des sociétés as amended by Art. 10 Loi 2006 Art. 141 (2) Code des sociétés as amended by Art. 27 Modifications du Code de sociétés 2015
Bulgaria	Art. 22b Accountancy Act as amended by SG 105-2006 Accountancy Act 2006 § 1 No. 15 Art. 19 Accountancy Act 2016	Art. 37 (1) Accountancy Act 2016
Croatia	Art. 16 (2) Accounting Act 1992 (Official Gazette No. 90/92) Art. 17 (1) Accounting Act 2005 (Official Gazette No. 146/05) Art. 3 (2) Accounting Act 2007 (Official Gazette No. 109/07) Art. 5 (3) Accounting Act 2015 (Official Gazette No. 78/15)	Art. 6 Audit Act 2005 Art. 6a Audit Act 2008 & 2012 Art. 20 (3) Accounting Act 2015 (Official Gazette No. 78/15)
Czech Republic	§ 18 Accounting law § 18 Accounting law as amended by Accounting Act 2001 §§ 1b, 18 & 20 Accounting law 2016	§ 20 Accounting law § 20 Accounting law as amended by Accounting Act 2001 § 20 Accounting law 2016
Denmark	§ 7 stk. 2 No. 1 Danish Financial Statements Act 2001 § 7 stk. 2 No. 1 Danish Financial Statements Act as amended by Sec. 5 of the Law on the amendment of the Danish Financial Statements Act 2004 § 7 stk. 2 No. 1 Danish Financial Statements Act as amended by Sec. 5 of the Law on the amendment of the Danish Financial Statements Act 2008 § 7 stk. 2 No. 1 Danish Financial Statements Act as amended by Sec. 13 of the Law on the amendment of the Danish Financial Statements Act 2015	§ 135 Danish Financial Statements Act 2001 § 135 Danish Financial Statements Act as amended by Sec. 50 of the Audit Act 2006 § 135 Danish Financial Statements Act as amended by Sec. 1 of the Audit Act 2011
Estonia	§ 3 (15) Accounting Act	§ 14 (3) Accounting Act 2003 § 14 (3) Accounting Act 2005 § 91 (1) & (2) Auditors Activities Act 2010 § 91 (1) & (2) Auditors Activities Act

Finland	Ch. 3 § 9 Accounting Act 1997 Ch. 3 § 9 Accounting Act as amended by Amendment 2001 Ch. 3 § 9 Accounting Act as amended by Amendment 2004 Ch. 1 § 4a Accounting Act 2016	Ch. 3 § 9 Audit Act 1994 Ch. 2 § 4 Audit Act 2007 Ch. 2 § 2 Audit Act 2016
France	Art. 17 of Decree No. 83-1020 of November 29, 1983 as amended by Decree 1994 Art. 17 of Decree No. 83-1020 of November 29, 1983 as amended by Decree 2001 Art. 17 of Decree No. 83-1020 of November 29, 1983 as amended by Decree 2005 Article R 123-200 Code de Commerce Decree of 28 December 2010 concerning approval of Regulation No. 2010-10 of the Accounting Standards Authority of 7 October 2010 Decree n° 2014-136 of February 17, 2014 & Article D 123-200 Code de Commerce	Art. 12 of Decree No. 67-236 of March 23, 1967 as amended by Decree 1985 Art. 12 of Decree No. 67-236 of March 23, 1967 as amended by Decree 2001 Article R 223-27 & Article R 221-5 Code de Commerce
Germany	§ 267 (1) HGB amended through Art. 1 Nr. 6 KapCoRiLiG § 267 (1) HGB amended through Art. 1 Nr. 1 EuroBilG § 267 (1) HGB amended through Art. 1 Nr. 3 BilReG § 267 (1) HGB amended through Art. 1 Nr. 19 BilMoG § 267 (1) HGB amended through Art. 1 Nr. 10 BilRUG	§ 316 (1) HGB in conjunction with § 267 (1) HGB
Greece	Art. 43a (2) & Art. 43b (1) Law 2190/1920 refer to Art. 42a (6) Law 2190/1920 Art. 2 (4) Law 4308/2014	Art. 42a (6) Law 2190/1920 as amended by Art. 2 Law 325/1994 Art. 42a (6) Law 2190/1920 as amended by Art. 16 (4) Law 2919/2001 Art. 42a (6) Law 2190/1920 as amended by Art. 52 Law 3604/2007 Art. 2 (A) Subparagraph (A1) Nr. 1a Law 4336/2015
Hungary	Sec. 7 Act XVIII of 1991 Sec. 9 (2) Act C of 2000 on Accounting Sec. 9 (2) Act C of 2000 on Accounting as amended by Sec. 49 of Act XXVI of 2005 Sec. 9 (2) Act C of 2000 on Accounting as amended by Sec. 2 (2) Act CI of 2015	Sec. 73 (7) Act XVIII 1991 as amended by Sec. 20 (2) Act CXXX of 1997 Sec. 155 (3) Act C of 2000 on Accounting Sec. 155 (3) Act C of 2000 on Accounting as amended by Sec. 213 of Act LXXV of 2007 Sec. 155 (3) Act C of 2000 on Accounting as amended by Sec. 25 (i) of Act XCVI of 2011 Sec. 155 (3) Act C of 2000 on Accounting as amended by Sec. 25 (j) of Act XCVI of 2011
Ireland	Sec. 8 (2) Companies (Amendment) Act 1986 as amended by S.I. No. 396 of 1993 Sec. 8 (2) Companies (Amendment) Act 1986 as amended by S.I. No. 304 of 2012 Ch. 14 Sec. 350 (5) Companies Act 2014	Sec. 32 (3) Companies Act 1999 Sec. 32 (3) Companies Act 1999 as amended by Sec. 53 (b) Companies Act 2003 Sec. 32 (3) Companies Act 1999 as amended by Sec. 9 (1b) Companies Act 2006 Sec. 32 (3) Companies Act 1999 as amended by S.I. No. 308 of 2012

Italy	Art. 2435 bis Code Civil as amended by Art. 19 Law 1996 No. 52 Art. 2435 bis Code Civil as amended by Art.1 Decree 2001 No. 203 Art. 2435 bis Code Civil as amended by Art. 1 Decree 2003 No. 6 Art. 2435 bis Code Civil as amended by Art. 2 Decree 2003 No. 394 Art. 2435 bis Code Civil as amended by Art. 1 Decree 2006 No. 285 Art. 2435 bis Code Civil as amended by Art. 1 (4) Decree 2008 No. 173 Art. 2435 bis Code Civil as amended by Art. 6 Decree 2015 No. 139	Art. 2477 Code Civil Art. 2477 Code Civil as amended by Art. 37 Decree 2010 No. 39 Art. 2477 Code Civil as amended by Art. 14 Decree 2011 No. 183 Art. 2477 Code Civil as amended by Art. 35 Decree 2012 No. 5 Art. 2477 Code Civil as amended by Art. 20 Decree 2014 No. 91
Lithuania	Art. 24 (4) Law on Financial Statements of Entities Art. 24 (6) Law on Financial Statements of Entities as amended by Art. 11 Amendment 2003 No. IX-1915 Art. 24 (6) Law on Financial Statements of Entities as amended by Art. 11 Amendment 2006 No. X-731 Art. 24 (1) Law on Financial Statements of Entities as amended by Amendment 2008 No. X-1633 Art. 24 (1) Law on Financial Statements of Entities as amended by Art. 1 Amendment 2012 No. XI-2164 Art. 24 (1) Law on Financial Statements of Entities as amended by Art. 4 Amendment 2014 No. XII-1124 Art. 4 (2) Law on Financial Statements of Entities as amended by Amendment 2015 No. XII-1696	Art. 58 (4) Joint-Stock Company Law as amended by Amendment 2003 No. IX-1889 Art. 19 (2) Law on Financial Statements of Entities as amended by Art. 8 Amendment 2006 No. X-731 Art. 20 (2) Law on Financial Statements of Entities as amended by Amendment 2008 No. X-1633 Art. 20 (2) Law on Financial Statements of Entities as amended by Art. 3 Amendment 2011 No. IX-1799 Art. 20 (2) Law on Financial Statements of Entities as amended by Art. 2 Amendment 2014 No. XII-1124 Art. 24 (2) Law on Financial Statements of Entities as amended by Amendment 2015 No. XII-1696
Luxembourg	Art. 215 of the amended Law of December 1915 as amended by Art. 1 Law of 29 December 2000 Art. 35 Law of 19 December 2002 Art. 35 Law of 19 December 2002 as amended by Law of 10 December 2010 Art. 35 Law of 19 December 2002 as amended by Art. 2 Law of 18 December 2015	Art. 256 of the amended Law of December 1915 as amended by Art. 1 Law of 10 May 1984 Art. 69 (2) Law of 19 December 2002 Art. 69 (2) Law of 19 December 2002 as amended by Law of 10 December 2010 Art. 69 (2) Law of 19 December 2002 as amended by Art. 2 Law of 18 December 2015

Netherlands	Art. 396 (1) Civil Code Book 2 as amended by Decision 1999-515 Art. 396 (1) Civil Code Book 2 as amended by Law 2001-664 Art. 396 (1) Civil Code Book 2 as amended by Law 2002-225 Art. 396 (1) Civil Code Book 2 as amended by Decision 2004-54 Art. 396 (1) Civil Code Book 2 as amended by Law 2005-377 Art. 396 (1) Civil Code Book 2 as amended by Decision 2006-474 Art. 396 (1) Civil Code Book 2 as amended by Law 2008-217 Art. 396 (1) Civil Code Book 2 as amended by Law 2008-243 Art. 396 (1) Civil Code Book 2 as amended by Law 2008-550 Art. 396 (1) Civil Code Book 2 as amended by Law 2012-300 Art. 396 (1) Civil Code Book 2 as amended by Law 2015-349	Art. 396 Civil Code Book 2 as amended by Decision 1999-515 Art. 396 Civil Code Book 2 as amended by Law 2001-664 Art. 396 Civil Code Book 2 as amended by Law 2002-225 Art. 396 Civil Code Book 2 as amended by Decision 2004-54 Art. 396 Civil Code Book 2 as amended by Law 2005-377 Art. 396 Civil Code Book 2 as amended by Decision 2006-474 Art. 396 Civil Code Book 2 as amended by Law 2008-217 Art. 396 Civil Code Book 2 as amended by Law 2008-243 Art. 396 Civil Code Book 2 as amended by Law 2008-550 Art. 396 Civil Code Book 2 as amended by Law 2012-300 Art. 396 Civil Code Book 2 as amended by Law 2015-349
Norway	Ch. 1 § 1-6 Law on Financial Statements Ch. 1 § 1-6 Law on Financial Statements as amended by Law of 10 December 2004 No. 81 Ch. 1 § 1-6 Law on Financial Statements as amended by Law of 10 June 2005 No. 46 Ch. 1 § 1-6 Law on Financial Statements as amended by Law of 25 June 2010 No. 33	§ 7-6 Law on Private Limited Liability Companies as amended by Law of 15 April 2011 No. 10 § 7-6 Law on Private Limited Liability Companies as amended by Law of 14 June 2014 No. 40
Poland	Art. 50 Accounting Act 1994 Art. 50 Accounting Act 1994 as amended by Amendment Act 2000 Art. 50 Accounting Act 1994 as amended by Amendment Act 2004 Art. 50 Accounting Act 1994 as amended by Amendment Act 2009 Art. 50 Accounting Act 1994 as amended by Amendment Act 2012 Art. 28b Accounting Act 1994 as amended by Amendment Act 2015	Art. 64 Accounting Act 1994 as amended by Amendment Act 2000 Art. 64 Accounting Act 1994 as amended by Amendment Act 2003 Art. 64 Accounting Act 1994 as amended by Amendment Act 2004 Art. 64 Accounting Act 1994 as amended by Amendment Act 2005 Art. 64 Accounting Act 1994 as amended by Amendment Act 2008 Art. 64 Accounting Act 1994 as amended by Amendment Act 2009 Art. 64 Accounting Act 1994 as amended by Amendment Act 2011 Art. 64 Accounting Act 1994 as amended by Amendment Act 2015
Portugal	Art. 2 Annex Decree Law No. 372-2007 Art. 9 (1) Decree Law No. 158-2009 Art. 9 (1) Decree Law No. 158-2009 as amended by Law No. 20-2010 Art. 9 (2) Decree Law No. 158-2009 as amended by Decree-Law No. 98-2015	Art. 262 (2) Commercial Company Code as amended by Decree Law No. 262-86 Art. 262 (2) Commercial Company Code as amended by Decree Law No. 343-98

Romania	Art. 3 Order No. 1752-2005 Art. 3 Order No. 3055-2009 Annex 1 Sec. 1.3 No. 9.(3) of Order No. 1802-2014 Annex 1 Sec. 1.3 No. 9.(3) of Order No. 1802-2014 as amended by Art. 8 of Order No. 773-2015	Art. 5 Order No. 1752-2005 Art. 5 Order No. 3055-2009 Annex 1 Sec. 10.1 No. 563.(2) of Order No. 1802-2014
Slovakia	§ 2 (7) Accounting Law 431-2002 as amended by Law 333-2014 § 2 (7) Accounting Law 431-2002 as amended by Law 130-2105 & 423-2015	§ 20 Accounting Law 563/1991 as amended by Law 336/1999 § 19 Accounting Law 431-2002 § 19 Accounting Law 431-2002 as amended by Law 561-2004 § 19 Accounting Law 431-2002 as amended by Law 540-2007 & 198-2007 § 19 Accounting Law 431-2002 as amended by Law 61-2009 § 19 Accounting Law 431-2002 as amended by Law 504-2009 § 19 Accounting Law 431-2002 as amended by Law 352-2013 § 19 Accounting Law 431-2002 as amended by Law 333-2014
Slovenia	Art. 52 (2) Companies Act (ZGD) as amended by Art. 12 ZGD-F Art. 52 (2) Companies Act (ZGD) as amended by Art. 4 ZGD-H Art. 55 (3) Companies Act (ZGD-1) 2006 Art. 55 (3) Companies Act (ZGD-1) 2006 as amended by Art. 3 ZGD-1B Art. 55 (3) Companies Act (ZGD-1) 2006 as amended by Art. 12 ZGD-1I	Art. 54 (1) Companies Act (ZGD) as amended by Art. 12 ZGD-F Art. 54 (1) Companies Act (ZGD) as amended by Art. 6 ZGD-H Art. 57 (1) Companies Act (ZGD-1) 2006 Art. 57 (1) Companies Act (ZGD-1) 2006 as amended by Art. 5 ZGD-1B Art. 57 (1) Companies Act (ZGD-1) 2006 as amended by Art. 14 ZGD-1I
Spain	Art. 181 Legislative Decree 1564-1989 as amended by Decree 572-1997 Art. 175 Legislative Decree 1564-1989 as amended by Law 16-2007 Art. 257 (1) Legislative Decree 1-2010 Art. 257 (1) Legislative Decree 1-2010 as amended by Art. 49 Law 14-2013 Art. 3 (9) Legislative Decree 1-2010 as amended by Law 22-2015	Art. 203 (2) in conjunction with Art. 181 Legislative Decree 1564-1989 Art. 203 (2) in conjunction with Art. 175 Legislative Decree 1564-1989 Art. 263 (2) in conjunction with Art. 257 (1) Legislative Decree 1-2010 Art. 263 (2) Legislative Decree 1-2010 as amended by Art. 49 Law 14-2013
Sweden	Ch. 1 § 3 Annual Accounts Act 1995:1554 as amended by Amendment 2006:871 Ch. 1 § 3 Annual Accounts Act 1995:1554 as amended by Amendment 2007:541 Ch. 1 § 3 Annual Accounts Act 1995:1554 as amended by Amendment 2009:34 Ch. 1 § 3 Annual Accounts Act 1995:1554 as amended by Amendment 2010:848 Ch. 1 § 3 Annual Accounts Act 1995:1554 as amended by Amendment 2015:813	§ 2 Audit Act 1999:1079 as amended by Amendment 2010:837

United Kingdom	Sec. 247 Companies Act 1985 as amended by Art. 5 SI 1992-2452	Sec. 249A Companies Act 1985 as amended by Art. 2 SI 1997-936
	Sec. 247 Companies Act 1985 as amended by Art. 2 SI 2004-16	Sec. 249A Companies Act 1985 as amended by Art. 2 SI 2000-1430
	Sec. 382 Companies Act 2006 as amended by Art. 3 SI 2008-393	Sec. 249A Companies Act 1985 as amended by Art. 4 SI 2004-16
	SI 2015-980	Sec. 477 (2) Companies Act 2006 as amended by SI 2008-393
		SI 2015-980

Notes: The table provides a selected list of official legal sources for country-specific financial reporting regulations and reporting- and auditing-exemption thresholds, in particular.

Table A5

	SECOND STAGE ESTIMATES (IV)				
		Instrumented Reporting Scope		Instrumented Auditing Scope	
Variable	Aggregation	Coefficient	Standard Error	Coefficient	Standard Error
	Financial Reporting				
Audit	Average	-0.167	(0.168)	0.319***	(0.084)
	Type of Resource Allocation				
Publicly Listed	Average	0.018**	(0.008)	0.001	(0.007)
Publicly Listed	Aggregate	0.112**	(0.049)	-0.002	(0.043)
Shareholders	Average	0.630**	(0.255)	0.179	(0.199)
Shareholders	Aggregate	0.904***	(0.307)	0.017	(0.245)
Independence	Average	0.186	(0.135)	0.031	(0.088)
Independence	Aggregate	0.241*	(0.143)	-0.013	(0.092)
Entry	Average	0.112	(0.071)	-0.207***	(0.043)
Entry	Aggregate	0.138**	(0.059)	-0.094**	(0.036)
Exit	Average	0.025**	(0.012)	0.024**	(0.011)
Exit	Aggregate	0.001	(0.005)	0.002	(0.004)
HHI	Sum	-0.399**	(0.178)	0.032	(0.120)
Dispersion (Gross Margin)	Standard deviation	-0.170**	(0.072)	0.020	(0.053)
Distance (Gross Margin)	p80-p20	-0.280**	(0.112)	-0.017	(0.077)
Dispersion (EBITDA/Sales)	Standard deviation	-0.249***	(0.080)	-0.027	(0.052)
Distance (EBITDA/Sales)	p80-p20	-0.374***	(0.133)	-0.028	(0.086)
	Efficiency of Resource Allocation				
Dispersion (TFP (Employees))	Standard deviation	-0.174**	(0.085)	-0.078	(0.055)
Distance (TFP (Employees))	p80-p20	-0.280**	(0.137)	-0.102	(0.089)
Lower Tail (TFP (Employees))	p20	-1.282	(0.983)	1.194*	(0.634)
Upper Tail (TFP (Employees))	p80	-3.847**	(1.878)	-0.194	(1.140)
Dispersion (TFP (Wage))	Standard deviation	-0.264**	(0.116)	-0.028	(0.076)
Distance (TFP (Wage))	p80-p20	-0.471***	(0.173)	-0.064	(0.115)
Lower Tail (TFP ((Wage))	p20	-0.186	(0.171)	0.221**	(0.105)
Upper Tail (TFP (Wage))	p80	-0.699**	(0.344)	0.050	(0.221)
Covariance Y/L and Y (Employees)	Aggregate-Average	0.124	(0.256)	0.045	(0.191)
Covariance TFP and Y (Employees)	Aggregate-Average	0.232	(0.199)	0.150	(0.142)
Covariance Y/L and Y (Wage)	Aggregate-Average	0.465**	(0.220)	0.217	(0.195)
Covariance TFP and Y (Wage)	Aggregate-Average	0.399**	(0.191)	0.263*	(0.157)
Y/L (Employees)	Average	-0.031	(0.277)	-0.255	(0.191)
Y/L (Wage)	Average	0.200	(0.220)	-0.173	(0.183)
TFP (Employees)	Average	0.185	(0.201)	0.012	(0.153)
TFP (Wage)	Average	0.319*	(0.180)	0.067	(0.153)
Y/L (Employees)	Aggregate	0.117	(0.259)	-0.216	(0.199)
Y/L (Wage)	Aggregate	0.703**	(0.276)	0.001	(0.221)
TFP (Employees)	Aggregate	0.366	(0.237)	0.145	(0.187)
TFP (Wage)	Aggregate	0.750***	(0.277)	0.299	(0.225)
ΔY/L (Employees)	Average	-0.081*	(0.042)	0.024	(0.032)
ΔY/L (Wage)	Average	-0.045	(0.037)	-0.013	(0.027)
ΔTFP (Employees)	Average	-0.062*	(0.034)	0.025	(0.029)
ΔTFP (Wage)	Average	-0.027	(0.034)	0.009	(0.024)
ΔY/L (Employees)	Aggregate	-0.116*	(0.068)	0.033	(0.049)
ΔY/L (Wage)	Aggregate	-0.052	(0.062)	-0.012	(0.043)
ΔTFP (Employees)	Aggregate	-0.054	(0.052)	-0.021	(0.043)
ΔTFP (Wage)	Aggregate	-0.021	(0.045)	-0.028	(0.035)

Notes: The table summarizes the second-stage estimates of a two-stage least squares estimation using "Standardized Reporting Scope" and "Standardized Auditing Scope" as instruments for "Actual Reporting Scope" and "Actual Auditing Scope". The

"Sign" columns provide the signs of my main results. "Actual Reporting Scope" is the share of firms exceeding reporting-related exemption thresholds in a given country, industry, and year. "Actual Auditing Scope" is the share of firms exceeding auditing-related exemption thresholds in a given country, industry, and year. "Standardized Reporting Scope" is the share of (simulated) firms exceeding reporting-related exemption thresholds in a given country, industry, and year a standardized firm-size distribution per industry (across countries). "Standardized Auditing Scope" is the share of (simulated) firms exceeding auditing-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). The regressions include industry-year fixed effects (where the industries are defined using four-digit NACE classifications) and country-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

		Number of firms		Number of firms (squared)	
FIRM DENSITY AND RESOURCE ALLOCATION					
Variable	Aggregation	Coefficient	Standard Error	Coefficient	Standard Error
Financial Reporting					
Actual Reporting Scope	Average	0.009***	(0.003)	-0.001***	(0.000)
Actual Auditing Scope	Average	0.008**	(0.003)	-0.001***	(0.000)
Audit	Average	0.011***	(0.003)	-0.002***	(0.000)
Type of Resource Allocation					
Publicly Listed	Average	0.002***	(0.000)	-0.000***	(0.000)
Publicly Listed	Aggregate	0.010***	(0.002)	-0.000*	(0.000)
Shareholders	Average	0.011***	(0.003)	-0.002***	(0.000)
Shareholders	Aggregate	0.013*	(0.007)	0.000	(0.001)
Independence	Average	0.015***	(0.003)	-0.001***	(0.000)
Independence	Aggregate	-0.002	(0.003)	0.001***	(0.000)
Entry	Average	0.027***	(0.003)	-0.003***	(0.000)
Entry	Aggregate	0.005***	(0.002)	-0.001***	(0.000)
Exit	Average	0.007***	(0.001)	-0.001***	(0.000)
Exit	Aggregate	0.001***	(0.000)	-0.000	(0.000)
HHI	Sum	-0.257***	(0.004)	0.017***	(0.001)
Dispersion (Gross Margin)	Standard deviation	-0.130***	(0.003)	0.009***	(0.000)
Distance (Gross Margin)	p80-p20	-0.238***	(0.006)	0.019***	(0.001)
Dispersion (EBITDA/Sales)	Standard deviation	-0.140***	(0.004)	0.010***	(0.000)
Distance (EBITDA/Sales)	p80-p20	-0.254***	(0.007)	0.020***	(0.001)
Efficiency of Resource Allocation					
Dispersion (TFP (Employees))	Standard deviation	-0.145***	(0.004)	0.010***	(0.000)
Distance (TFP (Employees))	p80-p20	-0.275***	(0.008)	0.021***	(0.001)
Lower Tail (TFP (Employees))	p20	-2.091***	(0.072)	0.179***	(0.008)
Upper Tail (TFP (Employees))	p80	-2.924***	(0.076)	0.216***	(0.010)
Dispersion (TFP (Wage))	Standard deviation	-0.173***	(0.005)	0.012***	(0.001)
Distance (TFP (Wage))	p80-p20	-0.325***	(0.011)	0.025***	(0.001)
Lower Tail (TFP ((Wage))	p20	-0.332***	(0.014)	0.028***	(0.002)
Upper Tail (TFP (Wage))	p80	-0.536***	(0.015)	0.039***	(0.002)
Covariance Y/L and Y (Employees)	Aggregate-Average	0.202***	(0.013)	-0.013***	(0.002)
Covariance TFP and Y (Employees)	Aggregate-Average	0.137***	(0.010)	-0.009***	(0.001)
Covariance Y/L and Y (Wage)	Aggregate-Average	0.104***	(0.010)	-0.008***	(0.001)
Covariance TFP and Y (Wage)	Aggregate-Average	0.078***	(0.010)	-0.006***	(0.001)
Y/L (Employees)	Average	-0.036***	(0.008)	-0.000	(0.001)
Y/L (Wage)	Average	0.003	(0.011)	0.001	(0.001)
TFP (Employees)	Average	-0.012	(0.008)	-0.002**	(0.001)
TFP (Wage)	Average	0.014	(0.009)	-0.001	(0.001)
Y/L (Employees)	Aggregate	0.176***	(0.013)	-0.013***	(0.002)
Y/L (Wage)	Aggregate	0.111***	(0.011)	-0.008***	(0.001)
TFP (Employees)	Aggregate	0.127***	(0.011)	-0.011***	(0.001)
TFP (Wage)	Aggregate	0.091***	(0.011)	-0.007***	(0.001)
ΔY/L (Employees)	Average	-0.001	(0.002)	-0.000	(0.000)
ΔY/L (Wage)	Average	-0.002	(0.002)	0.000	(0.000)
ΔTFP (Employees)	Average	-0.002	(0.002)	0.000	(0.000)
ΔTFP (Wage)	Average	-0.002*	(0.001)	0.000*	(0.000)
ΔY/L (Employees)	Aggregate	0.014***	(0.003)	-0.001***	(0.000)
ΔY/L (Wage)	Aggregate	0.010***	(0.003)	-0.001***	(0.000)
ΔTFP (Employees)	Aggregate	0.009***	(0.002)	-0.001***	(0.000)
ΔTFP (Wage)	Aggregate	0.007***	(0.002)	-0.001***	(0.000)

Notes: The table summarizes estimates from regressions of financial reporting and resource allocation measures on the number of firms and its squared term (as a measure of endogenous competition). The estimates provide a benchmark for the association of financial reporting and

resource allocation measures with competition as measured by firm density. "Number of firms" is the log number of firms in a given country, industry, and year. "Number of firms (squared)" is the squared log number of firms in a given country, industry, and year. The regressions include industry-year fixed effects (where the industries are defined using four-digit NACE classifications) and country-year fixed effects. Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.

Table A7

		INTERACTION OF REPORTING AND AUDITING MANDATES			
		(1)	(2)	(3)	(4)
		Standardized Reporting Scope		Standardized Auditing Scope	
Variable	Aggregation	> Auditing Scope	≤ Auditing Scope	> Reporting Scope	≤ Reporting Scope
		Financial Reporting			
Actual Reporting Scope	Average	0.670***	0.429***	-0.084	-0.129***
		(0.119)	(0.064)	(0.061)	(0.045)
Actual Auditing Scope	Average	0.069	-0.176**	0.534***	0.534***
		(0.120)	(0.084)	(0.067)	(0.060)
Audit	Average	0.124	-0.222***	0.247***	0.134***
		(0.106)	(0.059)	(0.049)	(0.051)
		Type of Resource Allocation			
Publicly Listed	Average	0.006*	0.010***	-0.001	-0.002
		(0.003)	(0.003)	(0.003)	(0.003)
Publicly Listed	Aggregate	0.025	0.067***	-0.016	-0.015
		(0.023)	(0.022)	(0.022)	(0.022)
Shareholders	Average	0.131	0.244***	0.016	0.017
		(0.117)	(0.061)	(0.048)	(0.049)
Shareholders	Aggregate	0.379**	0.483***	-0.124	-0.157*
		(0.157)	(0.087)	(0.081)	(0.080)
Independence	Average	-0.025	0.109***	-0.071*	0.009
		(0.043)	(0.038)	(0.039)	(0.038)
Independence	Aggregate	0.062	0.152***	-0.100**	-0.032
		(0.055)	(0.045)	(0.043)	(0.046)
Entry	Average	0.037	0.081**	-0.164***	-0.079***
		(0.034)	(0.033)	(0.025)	(0.025)
Entry	Aggregate	0.059**	0.062*	-0.089***	-0.042*
		(0.028)	(0.032)	(0.022)	(0.023)
Exit	Average	0.005	0.010*	0.007	0.009
		(0.008)	(0.006)	(0.006)	(0.007)
Exit	Aggregate	0.001	-0.000	-0.001	0.003
		(0.003)	(0.003)	(0.002)	(0.003)
HHI	Sum	-0.271	-0.164	0.116*	-0.046
		(0.173)	(0.132)	(0.065)	(0.106)
Dispersion (Gross Margin)	Standard deviation	-0.118**	-0.036	0.057**	-0.035
		(0.054)	(0.050)	(0.028)	(0.042)
Distance (Gross Margin)	p80-p20	-0.144*	-0.076	0.044	-0.069
		(0.086)	(0.076)	(0.052)	(0.065)

Dispersion (EBITDA/Sales)	Standard deviation	-0.155***	-0.088*	0.026	-0.042
		(0.051)	(0.050)	(0.030)	(0.043)
Distance (EBITDA/Sales)	p80-p20	-0.236***	-0.136	0.045	-0.028
		(0.088)	(0.085)	(0.057)	(0.070)

<p style="text-align:center">Efficiency of Resource Allocation</p>

Dispersion (TFP (Employees))	Standard deviation	-0.147**	-0.056	-0.020	-0.071
		(0.071)	(0.063)	(0.033)	(0.051)
Distance (TFP (Employees))	p80-p20	-0.183	-0.099	-0.023	-0.086
		(0.116)	(0.105)	(0.055)	(0.083)
Lower Tail (TFP (Employees))	p20	-0.774	-0.646	1.471***	0.489
		(1.006)	(0.612)	(0.543)	(0.459)
Upper Tail (TFP (Employees))	p80	-2.564	-1.577	0.583	-0.498
		(1.643)	(1.387)	(0.734)	(1.054)
Dispersion (TFP (Wage))	Standard deviation	-0.175**	-0.085	0.046	-0.048
		(0.086)	(0.083)	(0.041)	(0.070)
Distance (TFP (Wage))	p80-p20	-0.272**	-0.138	0.062	-0.086
		(0.133)	(0.126)	(0.067)	(0.103)
Lower Tail (TFP ((Wage))	p20	-0.163	-0.089	0.207**	0.128
		(0.139)	(0.108)	(0.092)	(0.081)
Upper Tail (TFP (Wage))	p80	-0.561*	-0.275	0.195	0.015
		(0.291)	(0.271)	(0.134)	(0.207)
Covariance Y/L and Y (Employees)	Aggregate-Average	-0.073	0.057	-0.134	0.127
		(0.210)	(0.152)	(0.148)	(0.129)
Covariance TFP and Y (Employees)	Aggregate-Average	0.028	0.117	-0.057	0.155*
		(0.161)	(0.106)	(0.106)	(0.087)
Covariance Y/L and Y (Wage)	Aggregate-Average	0.249*	0.208	0.015	0.135
		(0.147)	(0.134)	(0.135)	(0.132)
Covariance TFP and Y (Wage)	Aggregate-Average	0.147	0.200**	0.037	0.185**
		(0.143)	(0.101)	(0.096)	(0.093)
Y/L (Employees)	Average	0.294	-0.030	0.021	-0.220
		(0.192)	(0.160)	(0.128)	(0.139)
Y/L (Wage)	Average	0.372**	0.076	-0.098	-0.152
		(0.173)	(0.134)	(0.121)	(0.134)
TFP (Employees)	Average	0.147	0.070	0.021	-0.012
		(0.152)	(0.119)	(0.098)	(0.113)
TFP (Wage)	Average	0.278*	0.113	-0.019	0.023
		(0.162)	(0.106)	(0.103)	(0.098)
Y/L (Employees)	Aggregate	0.191	0.079	-0.143	-0.093
		(0.205)	(0.170)	(0.151)	(0.164)
Y/L (Wage)	Aggregate	0.588***	0.314*	-0.188	-0.039
		(0.216)	(0.171)	(0.165)	(0.151)

TFP (Employees)	Aggregate	0.205	0.179	-0.019	0.119
		(0.180)	(0.142)	(0.132)	(0.132)
TFP (Wage)	Aggregate	0.434**	0.340**	-0.005	0.170
		(0.175)	(0.151)	(0.144)	(0.129)
ΔY/L (Employees)	Average	-0.057**	-0.034	0.012	0.016
		(0.029)	(0.022)	(0.026)	(0.018)
ΔY/L (Wage)	Average	-0.037	-0.042*	-0.013	-0.008
		(0.024)	(0.021)	(0.022)	(0.017)
ΔTFP (Employees)	Average	-0.061**	-0.026	0.001	0.024
		(0.031)	(0.022)	(0.022)	(0.018)
ΔTFP (Wage)	Average	-0.027	-0.027	-0.005	0.007
		(0.024)	(0.020)	(0.020)	(0.017)
ΔY/L (Employees)	Aggregate	-0.095**	-0.042	0.054	0.041
		(0.047)	(0.039)	(0.041)	(0.029)
ΔY/L (Wage)	Aggregate	-0.083**	-0.032	0.030	0.014
		(0.035)	(0.036)	(0.033)	(0.024)
ΔTFP (Employees)	Aggregate	-0.028	-0.016	0.016	0.002
		(0.034)	(0.029)	(0.034)	(0.024)
ΔTFP (Wage)	Aggregate	-0.041	-0.022	0.023	-0.006
		(0.030)	(0.027)	(0.025)	(0.020)

Notes: The table summarizes the estimates from regressions of financial-reporting and resource-allocation measures on the scopes of reporting regulation and auditing mandates and their interactions. "Standardized Reporting Scope" is the share of (simulated) firms exceeding reporting-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). "Standardized Auditing Scope" is the share of (simulated) firms exceeding auditing-related exemption thresholds in a given country, industry, and year using a standardized firm-size distribution per industry (across countries). The first column of reporting scope (subtitled: "> Auditing Scope") captures variation in reporting scope if the auditing scope in the same country, industry, and year is lower; otherwise the reporting scope is set to zero. This column captures the effects of reporting mandates without a corresponding auditing mandate. The second column of reporting scope (subtitled: "≤ Auditing Scope") captures variation in reporting scope if the auditing scope in the same country, industry, and year is the same or higher; otherwise the reporting scope is set to zero. This column captures the effects of reporting mandates with a corresponding auditing mandate. The first column of auditing scope (subtitled: "> Reporting Scope") captures variation in auditing scope if the reporting scope in the same country, industry, and year is lower; otherwise the reporting scope is set to zero. This column captures the effects of auditing mandates without a corresponding (expanded) reporting mandate. The second column of reporting scope (subtitled: "≤ Auditing Scope") captures variation in auditing scope if the reporting scope in the same country, industry, and year is the same or higher; otherwise the reporting scope is set to zero. This column captures the effects of auditing mandates with a corresponding (expanded) reporting mandate. Differences between the reporting scope columns (with and without auditing mandate) and the auditing scope columns (with and without reporting mandate) may arise not only due to a potential interaction of reporting and auditing mandates, but also because of heterogeneity in treatment effects related to the level of the regulatory scope (e.g., variation among higher vs. lower scopes can matter differentially). The regressions include industry-year fixed effects (where the industries are defined using four-digit NACE classifications), country-year fixed effects, and fixed effects for each partition (i.e., (a) reporting scope higher than auditing scope, (b) reporting scope lower than auditing scope, and (c) reporting scope equal to auditing scope). Standard errors (in parentheses) are clustered at the country-industry level (where the industries are defined using one-digit NACE classifications) and the country-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level (two-tailed), respectively.