The Effect of Auditing on Promoting Exports:
Evidence from Private Firms in Emerging Markets

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Abstract
We investigate the effect of auditing on promoting exports for private firms in emerging markets. Using a sample of private firms from 125 countries between 2006 and 2015, we show that firms that have their financial statements audited have more exports than firms that do not have their financial statements audited. To infer causality, we employ a regression discontinuity design (RDD). Using the discontinuity around the mandatory financial audit threshold, we find that firms slightly above the threshold have more exports than do firms that are slightly below the threshold. We also exploit the countries with exogenous regulation shocks to the mandatory audits. Using the difference-in-differences (DiD) design, we find that firms that are exempted from mandatory audits have less exports subsequent to the regulation change. Further analyses reveal that the effect of auditing is more pronounced in countries with higher audit quality and for firms with limited alter-native information. Our findings suggest that the auditing function promotes exports—an important economic consequence for the global economic development.

Keywords: audit, international trade, private firms, emerging markets, regression discontinuity design

1. Introduction

International trade gives rise to today's global economy. By exchanging services, goods, and capital among various countries and regions, international trade accounts for a significant amount of a country gross domestic product[1]. History has witnessed many economies (e.g., Singapore, South Korea, and Taiwan) grow rapidly through successful exporting. An important factor that influences the extent of international trade for emerging markets is the credibility of the exporters because this credibility directly affects the timely delivery of quality products. An effective way to assess the exporter's credibility is to examine its financial statements. Credible

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[1] The World Bank reports that international trade represents ap-proximately 56% of the 2016 global GDP.

financial information enhances the perceived trustworthiness of the exporter, thereby alleviating the information asymmetry between the exporter and the importer. In this study, we investigate whether having financial audits leads to more exports. To the extent that foreign trade represents an important source of global economic outputs, financial statement verification can add great value to the economy if it can promote exports[2].

Our research is motivated by the lack of evidence on the impact of auditing in the product market, despite increasing interest in its consequences in the capital market (e.g., Kim et al. 2011, Minnis 2011, Kausaretal. 2016). The credibility of financial information is particularly important in the cross-border product market for at least two reasons. First, contract completeness and enforceability are much lower in a cross-border trading scenario than in the domestic context (Rodrik 2000, Macchiavello and Morjaria 2015), partially due to differences in the legal systems and country-specific institutions[3]. Second, due to the absence of local knowledge, a lack of trust, or the inconvenience of inspection, information asymmetry between the buyer and the supplier generally worsens as geographic distance increases (Hortaçsu et al. 2009, Costello 2013, Göttner and Limbach 2013). Unlike information gained via other channels (e.g., on-site visits), accounting information is superior in that its accessibility is not sensitive to the geographical distance. To avoid high information acquisition costs, an importer can obtain credible information about the exporter from a third party that provides the audit and assurance services.

Our investigation is also motivated by a fundamental question in economics: why and how do (some) firms export? As Bernard and Jensen (2004) argue, this question appeals not only to academics, but also to practitioners and regulators because the state government devotes an enormous amount of resources to promoting foreign trades every year. Although studies by trade economists demonstrate that firm-level characteristics collectively shape the export propensity, less is known about how a firm's financial statement verification affects cross-border trading activities. This perspective is important because importers can use financial information to assess the credibility and survivability of potential exporters. An article posted on Trade Ready, a popular blog for international trade experts, identifies seven sources that importers and exporters should use to assess their trade risks in foreign markets (Hyatt 2015). In particular, the author lists accountants as a major source that firms can use to learn about the potential exporter[4]. To the extent that an external audit helps ensure the creditworthiness of the exporter's financial information and thereby mitigates the information problem in trades, we investigate whether those private firms in the emerging markets that have their financial statements audited have more export sales in large-sample archival data.

We develop our hypothesis based on the role of financial information in the buyer-supplier relationship. To the extent that long-run financial conditions affect the supplier's willingness and ability to fulfill its implicit commitments, credible information about the supplier's financial condition is crucial to buyers. This information problem is more severe in international trade, when the buyer and the supplier are located in different countries. A financial audit con-strains the exporter's opportunistic reporting and enhances the creditworthiness of the exporter's financial information. As such, the financial audit alleviates the information risk in international trades. We hypothesize that private firms with audited financial statements have higher export sales.

We choose to study a sample of private firms because, in general, private firms are not required to have their financial statements audited. As Minnis (2011) argues, one hurdle to empirically examining the value of financial statement verification is the lack of variation in financial statement verification in public firms because they are all subject to mandatory audits. We test our hypothesis by employing a novel data set from the World Bank Enterprise Survey (WBES). The World Bank collects the survey data from face-to-face interviews with top managers and business owners. Unlike common commercial databases such as Compustat Global or Worldscope, the WBES collects data from field surveys and covers a large number of private firms that are not listed on stock exchanges, providing sufficient variation in financial statement verification (i.e., the presence of an auditor). Using

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[2] Throughout this paper, we use "financial statement verification" and "having the financial statements audited" interchangeably.

[3] Rodrik (2000) regards contract enforcement as the most obvious source of transaction costs to cross-border trades, because (1) a written contract that involves residents of different jurisdictions is unlikely to be enforced by either local courts (due to unwillingness) (2) informal enforcement mechanisms that provide sanctions against breaches of contract in a local sphere rest on social networks and are thus weakened in a cross-border context.

[4] Interestingly, the supply chain management company Achilles lists five reasons why suppliers should undergo an audit. Specifically, the company suggests that suppliers "improve their business" by "undergoing an audit" because "buyers are paying attention" (Achilles 2017).

a sample of 58,373 firm-year observations covering 125 countries from 2006 to 2015, we show that financial statement verification is both positively and statistically significantly associated with export intensity. Our results are robust to the inclusion of various firm characteristics as well as industry, country, and year fixed effects. On average, moving from a firm without an auditor to an otherwise similar firm with an auditor is associated with an approximately 13% increase in export intensity. This finding is consistent with our prediction that financial statement verification promotes exports.

Inferring causality is a challenging task in this setting. It is possible that exports and auditing are endogenously determined and the positive correlation simply captures association, rather than causality. To provide a more causal link, we employ several methodologies. First, we employ a fuzzy regression discontinuity design (RDD). In particular, we manually collect information on the mandatory financial audit thresholds around the world. For private firms, some countries impose mandatory audit requirements based on number of employees. Firms whose number of employees is slightly above the threshold are selected into the "treatment" group, whereas similar firms whose number of employees is slightly below the threshold serve as the "control" group. As such, the exogenous mandatory audit regulation triggers a forced increase in the probability of receiving financial audits for the treatment group, and the difference in export between the treatment group and the control group can be ascribed to the forced audit effect. Using various subsamples of firms around this threshold (i.e., the discontinuity), we find that firms slightly above the threshold have more ex-ports than do those that are slightly below the threshold. We also conduct several falsification tests to show that the sharp increase in exports is not due to the parallel trends in the number of employees and export intensity. Second, to further shed light on the causality, we employ a difference-in-differences (DiD) design. We explore the setting where a country changes its threshold for mandatory audits. We show that firms that are exempted from mandatory audits have less exports subsequent to the regulation change. Taken together, our test procedures help mitigate the risk that our result simply documents the association between having an external auditor and high export intensity, but we cannot completely rule out that possibility.

To further infer causality, we next perform several cross-sectional variation tests by varying the effectiveness of external audits. We employ three proxies for audit effectiveness: the audit profession development index (Michas 2011), the perceived court fairness, and the auditor litigation risk index. We expect that when audits are more effective, the effect of auditing on promoting exports is stronger. Consistent with our prediction, we find that the effect of auditing on ex-ports is more pronounced in countries with higher audit profession development scores, fairer courts, and higher auditor litigation risks.

We also perform two cross-sectional variation tests to examine how the demand or financial information moderates the effect of auditing on promoting exports. These tests rest on the prediction that the availability of other information in the exporter–importer relationship may "crowd out" the effect of financial statement verification. We consider two measures of additional information availability about the exporter: firm age and whether the exporter has a corporate website. The rationale is that exporters with a longer traceable history and exporters with additional information available to importers from their corporate website are less likely to rely on financial statement verification to alleviate information asymmetry. Consistent with this prediction, we find that the effect of auditing on exports is more pronounced for younger firms and for firms with no website available.

We make several important contributions to the literature. First, to the best of our knowledge, our study is the first to show the impact of auditing on the product markets in an international setting. The extant literature on the role of financial reporting credibility primarily focuses on the capital market. For example, Kausaretal (2016) show that the choice to obtain an audit provides incremental information to creditors, resulting in reduced financing friction. Using the cross-border product market setting, we complement this line of research by showing the bene-fits of having audited financial statements in the international product market—helping firms export[5].

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[5] We acknowledge that there is also cost for firms to have external audits. The costs not only include the fees paid to the auditors but also include considerable management time/efforts required to prepare for, communicate in, and oversee, the audit process (Kausar et al. 2016). For example, Kausar et al. (2016) estimate that the fee paid to auditors represents approximately 6% of the average firm's earnings using a set of UK firms. This cost alone does not include nonmonetary costs incurred during an audit such as the significant management time and efforts devoted to the audit process. In addition, firms may incur additional cost for regulatory compliance. To the extent that auditors bear litigation risks and regulatory costs in case of audit failures, they would request the client firms to comply with the laws, regulations, and guidelines in many dimensions. These compliance costs are large burdens to small firms. Even when the benefits of hiring an auditor exceed the

Second, our study contributes to the fundamental economic question of how developing countries can grow their economies. Whereas the audit engagement must be "purchased," thus imposing a cost to private firms, we show that there are also economic values attached to the audit engagement. To the extent that on average a country's exports represent a significant proportion of the total gross domestic product (GDP), our study highlights the microeconomic foundation for the benefits of financial audits in promoting economic development[6].

Third, we also contribute to the literature in international trade. Economists have been investigating the key determinants driving the significant differences in export intensity among firms/countries (see Chabowski et al. 2018 for a comprehensive review). One implication of this line of literature is that if a factor can potentially alleviate a specific export barrier for a firm, it has the potential to increase the firm's export intensity. A major export barrier is the great information asymmetry between the exporter and the importer. As informal screening (based on soft information) and formal screening (based on hard information) are often substitutive in alleviating information problems (Berger and Udell 1995), in the case of international trade when personal networks are absent, formal screening, such as financial statements verification, is valuable to the establishment of a cross-border supplier-customer relation. To this end, we also contribute to the literature in development economics and economic growth.

Fourth, we provide a methodological contribution to tackle the endogeneity of auditing. The extant literature lacks evidence on the economic benefits of having financial audits because a firm's decision to have an auditor and the economic outcomes are often endogenously determined. Our study offers relatively new methodologies (i.e., RDD and DiD) by exploiting firms around the thresholds for mandatory financial audits.

The rest of the paper proceeds as follows. Section 2 discusses the prior literature and develops the hypothesis. Section 3 outlines the variable construction and sample selection process. Section 4 discusses the main empirical results. Section 5 discusses our strategies to infer causality. Section 6 provides the cross-sectional variation tests. Section 7 provides additional analyses and Section 8 concludes.

2. Literature and Hypothesis Development

2.1. Role of Financial Information in the Customer–Supplier Relationship

We develop our hypothesis by first investigating the customer–supplier relationship. Bowen et al. (1995) show that contracting between the buyer and the supplier is a long-term arrangement rather than a one-time exchange of a product or service, and it often takes the form of "implicit contracts" (i.e., the implied promises of delivering inputs that customers require in a continuous and timely manner), where reputation is the disciplining force[7]. Prior studies show that suppliers have incentives to alter their accounting practices to influence customers. For example, Bowen et al. (1995) find that management tends to choose income-increasing inventory and depreciation methods to present a better financial image to its customers. Raman and Shahrur (2008) find that suppliers/customers use earnings management to opportunistically influence their counterpart's perception of the firm's prospect. Burgstahler and Dichev (1997) document that firms are incentivized to manipulate earnings to attract customers to pay a higher price, as better earnings are a sign of honoring implicit commitments. More directly, Graham et al. (2005) surveyed 401 financial executives and report that "assuring customers/suppliers that business is stable" is the third most important reason for income smoothing.

To the extent that financial information reveals the supplier's ability to fulfill its commitment, and that the supplier has incentives to manipulate earnings to attract customers, the credibility of financial information shapes the information risk faced by customers and the extent to which the trade will be executed.

2.2. Role of Financial Statement Verification in Promoting Exports

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costs, the existence of transaction costs may impede the auditor engagement. These transaction costs include, but are not limited to (1) the searching and contracting costs associated with the auditor engagement; (2) the rent-seeking incentives of managers, which may hinder a firm from employing an external auditor; and (3) in a relation-based society, reluctance of private firms to expose sensitive and proprietary financial information to an external professional (i.e., the auditor). In the presence of these transaction costs, a firm may select a suboptimal decision of not engaging an auditor.

[6] We also evaluate the economic importance of exports for our sample countries by calculating exports as a percentage of GDP. On average, exports represent 35% (untabulated) of total GDP for the country years in our sample.

[7] The breach of such implicit claims may cause disruptions in the supply chain, which can result in customers' operational disturbance, loss in market shares, and even an additional cost of switching to a new supplier, thus negatively affecting customers' operating performance and stock value (Hendricks and Singhal 2003, 2005a, b; Hertzel et al. 2008).

Information asymmetry between the buyer and the supplier is more critical if the two parties are from different countries. Theoretically, there are at least two possible mechanisms through which geographic distance affects information asymmetry between the buyer and supplier, and the cross-border scenario can add another layer to these mechanisms. First, information supply about the exporter is scarcer as the distance grows. Difficulty in cross-border communications increases the information acquisition cost and decreases information supply even further. Second, long distances render monitoring increasingly difficult. Contrary to the case where a customer can efficiently monitor a local supplier via frequent spot inspections, the high travel cost makes it hard for an importer to monitor its exporter. Moreover, differences in cultural characteristics and legal systems further increase the difficulty in monitoring. Taken together, both soft information gathering and direct monitoring become less available between the exporter and the importer than between domestic suppliers and customers.

Because information asymmetry is much higher when the two parties come from different countries, the credibility of financial information is particularly crucial for cross-border trades than for domestic trades. One way for the exporter to alleviate the information asymmetry is to have the financial statements audited. By providing an independent, objective evaluation of the exporter's financial reports and the financial reporting process, the financial audit constrains the exporter's opportunistic reporting and increases the importer's confidence in the exporter's creditworthiness and honoring implicit claims. In addition, relative to other sophisticated monitoring techniques, auditing serves as a less expertise-dependent device for importers to assess the creditworthiness of exporters. To the extent that audited financial statements reduce the information risk faced by importers, we hypothesize that importers have greater faith in international trades when the exporters' financial statements are audited.

In addition to enhancing the credibility of the exporter's financial reports, auditors provide an "advisor" role for the exporter[8]. As Bae et al. (2017) argue, auditors have extensive knowledge about their client firms' operations. Audit firms also invest substantial resources in analyzing the industries in the domestic economy as well as in the international markets, since this knowledge helps auditors assess the client's going concern risk. As such, auditors may have in-formational advantages over the management about the firm's competitiveness internationally. Via frequent interactions with the exporter's management throughout the year, auditors may provide an "advisor" role for the client firm by communicating the information about the international markets to the management. To the extent that the information on the global product market is conducive to the firm's export decisions, we also hypothesize that firms with external auditors have higher export sales due to the advisor role of auditing. Both the assurance role and the advisor role predict a positive effect of auditing on exports and they are not mutually exclusive. We state our hypothesis in the alternative form, as follows:

Hypothesis 1. Ceteris paribus, firms with audited financial statements have higher export sales.

We next investigate the moderating role of audit quality. Not all auditors provide the same quality of services. Instead, there are significant degrees of variations in audit quality, especially across countries. Specifically, the audits of private firms may be less reliable in some emerging markets where the overall auditing quality is low. In particular, in the 2005 Global Public Policy Symposium in London, the former CEO of Deloitte, William Parrett, made the following statement: "The varying environments create enormous challenges to the audit function, including the differing accounting and auditing standards, the quality of regulation and degree of oversight and enforcement, and auditor qualifications" (Deloitte 2005). In other words, the (cross-country) variations in regulations lead to great difference in the effectiveness of auditing. In-deed, Ke et al. (2015) show that even international Big-4 auditors provide low-quality auditing service in a weak institutional environment. As such, we expect that the effect of auditing on promoting exports is contingent on the overall audit quality embedded in a country's institutional context.

Hypothesis 2a. The effect of auditing on promoting exports is stronger in countries with more effective audits.
We also probe the moderating role of the availability of alternative information. Other information in the supplier-customer relationship may crowd out the effect of financial statement verification. As in the scenario of bank financing where relationship lending (based on soft information) and formal screening (based on hard information) are often substitutive (Berger and Udell 1995), alternative information about the exporter may mitigate the effect

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of auditing. As such, we expect that the effect of auditing on promoting exports is contingent on the availability of alternative information about the exporter.

Hypothesis 2b. The effect of auditing on promoting exports is stronger when alternative information about the exporter is less available.

3.   Sample Selection and Research Design

3.1. Sample Selection

We use the survey data from the WBES conducted from 2006 to 2015 using a globally standardized methodology[9]. The main objective of the WBES is to provide comparable indicators of the business environment across the world, thus enabling researchers and policymakers to assess the important constraints to enterprise performance and the growth of the private sector. As such, the WBES data set contains both firm performance measures (sales, historical sales, costs of production) and rich information about business environment features, such as access to finance, infrastructure, market competition, and labor. Some variables reflect objective facts (e.g., whether the firm has its financial report audited), and many others are based on survey respondents' opinions of current situations.

To mitigate the concerns of inadequate credibility stemming from sample bias or untruthful responses, the World Bank has undertaken a series of procedures. Specifically, the World Bank sends its private contractors to conduct face-to-face interviews with the owners, top managers, and other relevant staff members of the target firms. To ensure the best dis-closure of firm-level data, the World Bank keeps the identities of the respondent firms anonymous to secure integrity. This is important given that the questionnaire includes some sensitive questions (e.g., the informal payment to the public officials). As such, the WBES data used in our study should be of high quality.

The raw data from the WBES are the managers' responses to the survey questions. We obtain business description data from the qualitative responses and restore basic financial data from the quantitative responses. To construct our sample, we remove all firms with missing listing status information and firms that are publicly traded. This is because publicly traded firms are required to have their financial statements audited in almost all jurisdictions around the world and thereby exhibit no variation in financial audits. To ensure data quality, we remove observations where the interviewer indicates that he or she perceives the survey responses as being untruthful or unreliable[10]. After requiring that the necessary data are available for all test variables, we have 58,373 firm-year observations covering 125 countries. Table 1 describes the sample selection procedure.

3.2. Research Design

To test our main hypothesis, we estimate the following equation:

$$Export = \beta_0 + \beta_1 Audit + Firm\,characteristics$$
$$+ Country\,characteristics$$
$$+ Industry,\,Country,\,Year\,fixed\,effects. \quad (1)$$

[9] There are two large-scale surveys conducted by the World Bank, the first from 2002 to 2005, and the second from 2006 onward, respectively. The first WBES covers fewer countries and a relatively shorter span of time. Since 2006, the World Bank has substantially expanded the list of countries for the second survey; and continues to update its data as more observations become available from the survey. We use the second large-scale survey because it covers more countries and years and is more up-to-date.

[10] Specifically, at the end of each interview, the interviewer must answer two questions: Q1: It is my perception that the questions regarding opinions and perceptions are: (1) truthful, (2) some-what truthful, or (3) not truthful. In the raw sample, approximately 57.55%, 28.91%, 2.15%, and 11.39% of the interviews were indicated by the interviewer as being truthful, somewhat truthful, not truthful, and missing values in this question, respectively. Q2: The questions regarding specific figures (productivity and employment numbers) are: (1) taken directly from establishment records, (2) estimates computed with some precision, or (3) arbitrary and unreliable numbers. In the raw sample, approximately 33.19%, 56.28%, 5.90%, and 4.63% of the interviews were indicated by the interviewer as being taken directly from establishment records, computed with some precision, arbitrary and unreliable numbers, and missing values in this question, respectively. To ensure that the test sample remains as large as possible without sacrificing the reliability of the survey responses, when we construct our sample, we remove those interviews where the interviewer responded "not truthful" in Q1 and/or "arbitrary and unreliable numbers" in Q2. Our inferences are similar if we also remove interviews where the interviewer responded "somewhat truthful" in Q1 and "estimates computed with some precision" in Q2 and interviews returned with missing values in either Q1 or Q2. We provide the robustness check in section 7.

Our key dependent variable is constructed from the response to this question:
In fiscal year [insert last complete fiscal year], what percent of this establishment's sales were exports?

We define Export as the export sales scaled by total sales. Export is bounded between zero and one and a higher value indicates higher export intensity[11]. Our key independent variable is constructed from the response to this question:
In fiscal year [insert last complete fiscal year], did this establishment have its annual financial statement checked and certified by an external auditor?

We define Audit as an indicator variable that takes a value of one if the firm has an auditor and zero otherwise. We obtain firm characteristic controls from the qualitative survey responses, as well as from the restored financial data.

Table 1. Sample Selection Procedures

Procedure	No. in sample
All firm years in the WBES database as of 2016	118,206
Less firms with missing listing status information	(1,011)
Less publicly listed firms	(5,622)
Less observations with arbitrary numbers or untruthful opinions	(7,710)
Private firms with trustable answers	103,863
Less firms with missing data on exporting	(1,035)
Less firms with missing data on financial report audits	(1,737)
Less firms with missing data on firm-level control variables	(40,620)
Less firms with missing data on country-level control variables	(2,098)
Final sample	58,373

We include many firm characteristic variables that may affect export activities in our baseline equation. We measure firm size using the natural logarithm of total sales (Size). We employ net profit margin (ROS) to control for the firm's profitability. Profit-ability can be viewed as resource endowments, and this resource-based view suggests that firms with richer resources are more likely to be exporters (Bernard and Jensen 2004). We calculate ROS as the net income as a percentage of sales[12]. We impute net income by subtracting the production costs (including the total annual cost of labor, electricity, rental of land/buildings, equipment, cost of finished goods bought to resell, etc.) from the total annual sales. We include an indicator variable to capture whether the firm is part of a larger group (Part) because business group affiliation may affect export intensity[13]. We include the continuous variables to capture the proportion of firm shares held by state/government (State), foreign owners (Foreign), and the largest owners (Block). We include these ownership structure variables because a firm's ownership structure shapes many aspects of its operations, including its export behavior (Fern´andez and Nieto 2006).

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[11] The WBES provides data on direct exports and indirect exports (through a third party). We use the sum of both types of exports to calculate the total exports for a given firm for three reasons. First, foreign importers should care about the financial credibility of the indirect exporters as well as the direct exporters because ultimately, it is the producer's (whether an indirect exporter or direct exporter) role to deliver quality products in a timely manner. In other words, the presence of the third party does not completely eliminate the information asymmetry between the ultimate buyer and the producer in the supply chain. Having an external audit increases the chance that an indirect exporter would be selected by the foreign importer, among the list of exporters provided by the third party. Second, there is also information asymmetry between the indirect exporter and the third party. The third-party cares about its reputation and therefore wants to minimize the potential liability arising from failing to supply quality products on time to foreign customers. As such, having an external audit also increases the chance that the third-party contracts with an indirect exporter domestically. Third, the advising service provided by external audits contributes to both indirect ex-ports and direct exports (Bae et al. 2017). Nevertheless, in untabulated tests, we perform an additional analysis to estimate the effect of auditing on indirect exports and direct exports separately. Consistent with the argument that the third-party reduces but does not completely eliminate the information problem between the ultimate importer and the exporter, the impact of auditing on indirect exports is also positive and statistically significant, although to a lesser extent.

[12] The WBES provides limited data to recover the balance sheet and income statement. We use return on sales (i.e., ROS) rather than return on assets because the WBES does not ask about total assets.

[13] The results and inferences are similar if we do not include Part.

The next set of controls captures a firm's product market incentives and competitiveness. We include the natural logarithm of firm age (log(Age)), the natural logarithm of the number of years that the top manager has been in the industry sector (log(MEXP)), the product concentration measured by the proportion of total sales derived from the main product (Main), whether the firm has a government contract (Govcont). We expect that older firms, firms with a manager that has more specialized industry experience, and firms with more concentrated products have more exports. Government contracts assure a firm's local competence and provide the firm with good domestic opportunities, making the firm less incentivized to compete in foreign markets (Lee and Weng 2013). As such, we expect that government contracts and export sales serve as substitutes—that is, firms with a government contract have fewer exports. We also control for product quality using the indicator variable that captures whether the firm has obtained an internationally recognized quality certification (Quality), and the indicator variable that captures whether the firm uses technology licensed from a foreign company (Foreign Tech). We expect that firms with an internationally recognized quality certification and firms that use foreign technology have more exports. Given our focus on the relationship between distant buyers and suppliers, and the importance of telecommunications in fostering it (Cannon and Homburg 2001), we employ two indicator variables to capture whether the firms use email/websites to communicate with their business partners (i.e., Email and Website).

We also control for financing because firms that have better access to finance have the capacity to export (Manova 2013). Because private firms borrow almost exclusively from banks (Brav 2009), we use an indicator variable (Finance) that takes a value of one if the firm has a loan or a line of credit from a bank and zero otherwise. We also employ Access debt (defined as an indicator variable that takes a value of one if a firm finances its fixed assets with new debt and zero otherwise). To further control for the effect of fi-financial constraints, we include FinCon, an indicator variable that takes a value of one if the degree of obstacle is three or greater for access to finance, and zero otherwise[14].

Finally, we control for country-year-specific social and economic characteristics: GDP per capita, inflation, population, and GDP growth. Specifically, GDP is the natural logarithm of GDP per capita measured using 2016 U.S. dollars. Inflation is the inflation rate. Population is the natural logarithm of population. GDP growth is the growth rate of GDP per capita. We obtain these variables from the World Bank. We also control for country-specific institutions. We employ data developed by the World Bank in its World Development Indicators (WDI) project. WDI measures institutions in six dimensions: government effectiveness, regulatory quality, corruption, political stability, rule of law, and voice and accountability. To the extent that the six indicators are highly correlated, we calculate the first principal component of the six indicators as Institution. Lastly, for all regressions, we include country, industry, and year fixed effects. These fixed effects control for any sample-wide systematic differences across countries, industries, and years. We define industry classifications using the industry code (i.e., sector) provided in the WBES. As most firms are interviewed only once, we do not include firm fixed effects[15]. We present the detailed definitions for our key variables in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles to mitigate the effect of outliers. We cluster standard errors by country[16].

4. Empirical Results

4.1. Summary Statistics

Our final sample consists of 58,373 firm-year observations from 125 countries. Table 2 describes the sample composition. Specifically, panel (a) shows the sample breakdown by industry. Because it is clear that the majority of our sample firms are manufacturing firms, we believe that WBES provides an appropriate sample with which to investigate export activities. For example, the garment industry has the highest export intensity, followed by textiles. These two industries are highly representative of the features that we would expect to find in industries in developing countries: they have low wages and low economic value added[17]. Interestingly, the percentage of audited firms does not exhibit much variation across industries. In other words, the firms that are audited do not cluster into a selected few industries in our sample.

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[14] The World Bank asks respondent firms the following question: Is access to financing, which includes availability and cost (interest rates, fees, and collateral requirements), no obstacle (0), a minor obstacle (1), a moderate obstacle (2), a major obstacle (3), or a very severe obstacle (4) to the current operations of this establishment?

[15] We investigate a subsample of firms that are interviewed twice in Section 4.3.

[16] The results and inferences are similar if we cluster by firm.

[17] Seethe article on the garment sector in developing countries at http://wiego.org/informal-economy/occupational-groups/garment-workers.

Table 2. Summary Statistics of Our Sample

Panel (a): Industry breakdown				
WBES industry	Observations (no.)	Observations (%)	Firms audited (%)	Firms that export (%)	Mean export intensity
Retail and wholesale trade	12,516	21.44	49.27	12.14	0.04
Other services	6,121	10.49	57.23	18.71	0.09
Food	6,089	10.43	56.51	25.14	0.11
Other manufacturing	5,565	9.53	50.93	27.44	0.11
Metals and machinery	5,546	9.50	59.97	29.34	0.11
Garments	3,890	6.66	45.55	39.41	0.25
Nonmetallic and plastic materials	3,691	6.32	62.21	25.30	0.09
Other: construction, transportation, etc.	3,143	5.38	57.05	13.33	0.05
Textiles	3,034	5.20	56.26	38.13	0.18
Chemicals and pharmaceuticals	2,779	4.76	62.83	36.70	0.10
Hotels and restaurants	2,688	4.60	52.75	8.74	0.04
Wood and furniture	1,236	2.12	39.16	25.32	0.14
Electronics	1,031	1.77	72.36	31.62	0.15
Auto and auto components	713	1.22	78.26	22.16	0.08
Leather	331	0.57	50.45	20.85	0.10
Total	58,373	100	54.75	23.15	0.10

Panel (b): Country breakdown				
Country	Observations (no.)	Observations (%)	Firms audited (%)	Firms that export (%)	Mean export intensity
India	7,031	12.04	83.70	15.22	0.07
China	2,062	3.53	69.01	23.33	0.11
Russian Federation	2,041	3.50	24.60	12.35	0.03
Mexico	1,684	2.88	46.14	22.39	0.07
Colombia	1,439	2.47	58.72	32.24	0.08
Argentina	1,357	2.32	73.62	48.71	0.12
Chile	1,312	2.25	49.31	29.04	0.08
Bangladesh	1,199	2.05	44.54	24.02	0.21
Nigeria	1,162	1.99	24.10	26.85	0.15
Peru	1,131	1.94	32.89	37.40	0.15
Kenya	1,108	1.90	77.08	34.12	0.15
Turkey	1,090	1.87	57.71	57.61	0.28
Bulgaria	1,089	1.87	39.76	32.97	0.18
Pakistan	990	1.70	34.14	17.17	0.11
Egypt, Arab Republic	876	1.50	71.23	15.18	0.07
Brazil	866	1.48	21.02	20.09	0.04
South Africa	804	1.38	78.23	26.12	0.06
Zambia	766	1.31	63.71	13.45	0.05
Ghana	736	1.26	45.92	16.85	0.06
Ethiopia	704	1.21	72.87	10.09	0.06
Uganda	696	1.19	46.84	16.52	0.07
Philippines	654	1.12	92.05	32.57	0.24
Uruguay	637	1.09	39.72	33.91	0.14
Nepal	622	1.07	81.03	10.93	0.06
Senegal	615	1.05	29.76	15.12	0.05
Tanzania	612	1.05	54.41	15.36	0.05
Croatia	599	1.03	40.57	40.90	0.17
Guatemala	593	1.02	60.71	36.26	0.13
El Salvador	590	1.01	92.37	37.29	0.14
Ecuador	578	0.99	52.25	21.28	0.06
Ukraine	548	0.94	22.26	18.80	0.07
Vietnam	538	0.92	29.55	35.69	0.23
Kazakhstan	501	0.86	20.56	3.39	0.01
Paraguay	484	0.83	32.02	19.63	0.08
Angola	470	0.81	13.40	2.77	0.01
Iraq	470	0.81	42.13	7.66	0.04
Macedonia, FYR	466	0.80	38.63	35.19	0.17
Tunisia	465	0.80	78.28	48.82	0.26
Mali	460	0.79	25.87	13.04	0.05
Nicaragua	435	0.75	42.53	16.55	0.06
Bolivia	426	0.73	78.17	24.18	0.10
Honduras	423	0.72	58.87	16.55	0.08
Serbia	417	0.71	53.72	37.41	0.10
Mozambique	411	0.70	38.69	4.87	0.02
Mongolia	397	0.68	82.37	9.32	0.05
Yemen, Republic	392	0.67	31.12	12.50	0.05
Poland	384	0.66	27.34	28.13	0.09
Israel	382	0.65	94.76	27.49	0.11
Slovenia	376	0.64	34.04	60.64	0.23
Botswana	375	0.64	70.40	12.53	0.04
Bosnia and Herzegovina	370	0.63	57.03	33.78	0.13
Namibia	369	0.63	86.72	14.91	0.06
Sri Lanka	356	0.61	66.29	17.13	0.13

Table 2. (Continued)

Panel (b): Country breakdown				
Country	Observations (no.)	Observations (%)	Firms audited (%)	Firms that export (%)	Mean export intensity
Hungary	352	0.60	67.33	32.67	0.11
Moldova	349	0.60	26.07	15.76	0.08
Panama	342	0.59	81.87	22.22	0.11
Malawi	319	0.55	56.74	10.66	0.04
Latvia	316	0.54	62.03	36.71	0.17
Myanmar	313	0.54	19.49	11.50	0.10
Burundi	312	0.53	26.92	5.45	0.02
Estonia	309	0.53	56.96	38.83	0.18
Armenia	308	0.53	20.45	12.99	0.05
Czech Republic	307	0.53	51.47	49.51	0.21
Lithuania	306	0.52	33.66	40.85	0.19
Lao PDR	293	0.50	22.87	22.18	0.18
Lebanon	285	0.49	91.23	48.77	0.19
South Sudan	281	0.48	26.69	1.78	0.01
Belarus	280	0.48	42.86	29.64	0.11
Trinidad and Tobago	274	0.47	80.29	23.36	0.06
Jordan	268	0.46	66.04	39.55	0.18
Cameroon	265	0.45	67.55	14.34	0.05
Mauritania	265	0.45	26.42	15.09	0.10
Rwanda	265	0.45	49.81	10.19	0.03
Slovak Republic	256	0.44	48.83	35.55	0.16
Costa Rica	254	0.44	61.42	30.31	0.13
Madagascar	250	0.43	51.60	26.80	0.20
… (49 countries not displayed)	—	—	—	—	—
Total	58,373	100.00	54.75	23.15	0.10

Panel (c): Year breakdown		
Fiscal year	No. of observations	Percentage of sample
2006	8,659	14.83
2007	5,219	8.94
2008	1,120	1.92
2009	8,431	14.44
2010	8,272	14.17
2011	1,393	2.39
2012	3,850	6.60
2013	11,244	19.26
2014	9,745	16.69
2015	440	0.75

Panel (d): Summary statistics for main variables						
Variable	No.	Mean	P25	Median	P75	Standard deviation
Export	58,373	0.099	0.000	0.000	0.000	0.248
Has export	58,373	0.231	0.000	0.000	0.000	0.422
Audit	58,373	0.547	0.000	1.000	1.000	0.498
Size	58,373	16.840	14.771	16.649	18.757	2.888
ROS	58,373	0.413	0.166	0.380	0.760	0.431
State	58,373	0.002	0.000	0.000	0.000	0.015
Foreign	58,373	0.072	0.000	0.000	0.000	0.242
Block	58,373	0.786	0.500	1.000	1.000	0.264
MEXP	58,373	17.658	10.000	15.000	25.000	10.716
Part	58,373	0.167	0.000	0.000	0.000	0.373
Main	58,373	0.732	0.600	0.850	1.000	0.328
Govcont	58,373	0.154	0.000	0.000	0.000	0.361
Quality	58,373	0.240	0.000	0.000	0.000	0.427
Foreign tech	58,373	0.091	0.000	0.000	0.000	0.288
Email	58,373	0.737	0.000	1.000	1.000	0.440
Finance	58,373	0.398	0.000	0.000	1.000	0.489
FinCon	58,373	0.264	0.000	0.000	1.000	0.441
Access debt	58,373	0.149	0.000	0.000	0.000	0.356
Age	58,373	19.375	10.000	15.000	24.000	13.894
Website	58,373	0.464	0.000	0.000	1.000	0.499

Panel (b) in Table 2 shows the sample breakdown by country. For the sake of brevity, we do not present the full list of 125 countries. Instead, we present the countries with more than 250 firm-year observations. We observe several features from this panel. First, the countries represented in the sample are developing countries. The country with most observations is India, with more than 7,000 firm-year observations, followed by China (2,062 observations), Russia (2,041 observations), Mexico (1,684 observations), and Colombia (1,439 observations). Second, the percentage of firms that are audited exhibits considerable variations across countries. For example,

more than 83% of private firms in India have auditors, but less than 25% of private firms in Russia are audited. Panel (c) in Table 2 presents the sample breakdown by year. Approximately half of the observations in our sample come from years after 2010[18]. Panel (d) of Table 2 presents the sample descriptive statistics. On average, 9.9% of a firm's sales are from exports, and 54.7% of our sample firms have auditors. Turning to the control variables, we observe that 24% of firms have obtained an internationally recognized quality certification, whereas 9.1% of firms use foreign technology.

Table 3. Baseline Test

Panel (a): Using *Export* as the dependent variable

	Export	
	(1)	(2)
Audit	0.069***	0.067***
	(4.60)	(4.51)
Size	0.060***	0.061***
	(5.60)	(5.83)
ROS	−0.022	−0.027
	(−1.09)	(−1.35)
State	1.055	1.049
	(0.82)	(0.82)
Foreign	0.359***	0.358***
	(7.10)	(7.13)
Block	−0.151***	−0.150***
	(−3.52)	(−3.52)
log(MEXP)	0.042***	0.040***
	(3.20)	(3.09)
Part	0.025	0.024
	(1.22)	(1.13)
Main	0.087**	0.079**
	(2.16)	(1.97)
Govcont	−0.122***	−0.123***
	(−7.15)	(−7.17)
Quality	0.237***	0.236***
	(13.52)	(13.50)
Foreign tech	0.105***	0.105***
	(4.84)	(4.80)
Email	0.282***	0.278***
	(5.74)	(5.65)
Finance	0.062***	0.061***
	(3.17)	(3.11)
FinCon	0.005	0.006
	(0.23)	(0.30)
Access debt	0.051***	0.049***
	(2.82)	(2.72)
log(Age)	−0.001	−0.001
	(−0.11)	(−0.06)
Website	0.180***	0.179***
	(5.90)	(5.85)
Institution		0.034
		(0.44)
GDP		−0.242**
		(−2.50)
GDP growth		0.016***
		(2.79)
Population		1.629***
		(5.29)
Inflation		−0.007
		(−1.48)
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Pseudo R^2	0.209	0.210
N	58,373	58,373

Table 3. (Continued)

Panel (b): Using *Has export* as the dependent variable

	Has export	
	(1)	(2)
Audit	0.102***	0.098***
	(4.72)	(4.65)
Size	0.099***	0.102***
	(6.38)	(6.66)
ROS	−0.034	−0.040
	(−1.09)	(−1.32)
State	1.261	1.248
	(0.59)	(0.59)
Foreign	0.528***	0.527***
	(8.70)	(8.70)
Block	−0.237***	−0.238***
	(−3.83)	(−3.84)
log(MEXP)	0.067***	0.065***
	(3.26)	(3.16)
Part	0.041	0.039
	(1.22)	(1.13)
Main	0.007	−0.002
	(0.13)	(−0.03)
Govcont	−0.087***	−0.088***
	(−3.76)	(−3.83)
Quality	0.408***	0.408***
	(11.83)	(11.72)
Foreign tech	0.210***	0.210***
	(5.07)	(5.03)
Email	0.360***	0.355***
	(7.10)	(6.97)
Finance	0.139***	0.137***
	(5.87)	(5.87)
FinCon	0.016	0.018
	(0.54)	(0.61)
Access debt	0.128***	0.126***
	(4.73)	(4.62)
log(Age)	0.070***	0.071***
	(3.53)	(3.55)
Website	0.325***	0.324***
	(8.77)	(8.75)
Institution		0.020
		(0.18)
GDP		−0.366*
		(−1.78)
GDP growth		0.025***
		(2.79)
Population		2.133***
		(4.48)
Inflation		−0.007
		(−1.20)
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Pseudo R^2	0.247	0.248
Area under ROC curve	0.827	0.827
N	58,373	58,373

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[18] Léon (2015) also employs data from the second World Bank survey (similar to us). However, his sample ends in 2011 (due to the time of publication) and therefore does not include observations that become available after 2011. In our study, we use a more updated WBES version provided by the World Bank and include observations after 2011.

Notes. Panel (a): Results of a Tobit regression of Export on Audit and firm characteristics. The dependent variable Export is defined as the export sales scaled by total sales. The key independent variable, Audit, is an indicator variable that takes a value of one if the firm has an auditor and zero otherwise. We control for firm size (Size), profitability (ROS), the proportion of shares held by the government, foreign owners and the largest owners (State, Foreign, and Block, respectively), managerial experience (log(MEXP)), whether the firm belongs to a larger group (Part), product concentration (Main), government contract (Govcont), internationally recognized quality certification (Quality), foreign licensed technology (Foreign Tech), email communications (Email), bank loans (Finance), financing constraints (FinCon), access to new debt (Access debt), firm age (log (Age)), and the availability of the corporate website (Website). We further control for institutional environment (Institution), GDP per capita (GDP), the growth rate of GDP per capita (GDP growth), population (Population), and the inflation rate (Inflation). We include industry, country, and year fixed effects. T-statistics are in parentheses and standard errors are clustered by country. Variables are defined in the appendix. Panel (b): Results of a Probit regression of Has export on Audit and firm characteristics. The dependent variable, Has export, is defined as an indicator variable that takes a value of one for exporters and zero for non-exporters. We control for firm and country characteristics as in panel (a). We include industry, country, and year fixed effects. Z-statistics are in parentheses and standard errors are clustered by country.
*p < 0.1; **p < 0.05; ***p < 0.01.

4.2. Baseline Regressions

We present the results of estimating our baseline Equation (1) in panel (a) of Table 3. The dependent variable is the export intensity, Export, defined as the export sales scaled by total sales. To the extent that Export takes a value of zero for firms without ex-porting activities, we use the Tobit regression to estimate Equation (1)[19]. The main independent variable of interest is Audit, defined as an indicator variable that takes a value of one if the firm has an auditor and zero otherwise. We first regress Export on Audit with firm characteristics, country, industry, and year fixed effects. In column (2) of panel (a), we add country characteristic controls. The coefficient on Audit is positive and statistically significant at the 1% level in both regressions, consistent with Hypothesis 1 insofar as firms with audited financial statements have more export sales. In column (2), the average marginal effect of Audit on export intensity is 0.013 (untabulated)[20]. This is also economically significant given that the sample's unconditional mean of Export is 0.099. In other words, among firms with an average export intensity of 9.9%, moving from a firm that was not audited to an otherwise identical audited firm results in an increase of 13% in export intensity. We also assess the impact of Audit relative to other firm characteristics. For example, the average marginal effect of Finance (the indicator variable indicating whether the firm has a loan/line of credit), is about 0.012, which is very close to that of Audit.

Panel (a) of Table 3 also reveals that the control variables are related to export intensity in manners consistent with our predictions. The coefficient on Size is positive and statistically significant at the 1% level, consistent with the idea that larger firms have a greater capacity to export. Turning to the other control variables, we find that firms with a manager that has more specialized industry experience, firms with more concentrated products, firms with an internationally recognized quality certification, firms that use foreign technology, firms that use emails to communicate with business partners, firms that have access to the debt market, and firms with websites have more exports. On the contrary, firms with government contracts have fewer exports. Speaking to the ownership structure, firms with higher foreign ownership have more exports and firms with higher block ownership have fewer exports. The coefficients on Institution is in-significantly different from zero, possibly due to the fact that a full set of country fixed effects help absorb the effect of country-level institutions. The coefficient on Population is positive, possibly consistent with the idea that emerging markets export mainly labor-intensive products.

In panel (a) of Table 3, the dependent variable is the export intensity with a lower bound of zero and an upper bound of one. To assess the robustness of our result to this intensity measure, in panel (b), we employ a dichotomous variable, Has export, which takes a value of one for exporters and zero for non-exporters. We use the Probit model to estimate this regression and expect that those firms that are audited are more likely to have exports. Consistent with our prediction, the coefficient on Audit is positive and statistically significant at the 1% level in both regressions. The area under the ROC curve is 0.827, indicating a good performance of the model. To the extent that we are interested in not only whether the firm exports, but also in the intensity of exports, we use the continuous variable, Export (as in panel (a)), as the dependent variables in all subsequent tests[21].

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[19] The results and inferences are similar if we use ordinary least squares regressions.

[20] Because we estimate a Tobit function in Table 3, the coefficient on Audit should not be interpreted as the marginal effect.

[21] As a robustness check, in untabulated tests, we also employ the natural logarithm of export sales (unscaled) as the dependent variable. The results and inferences are similar to the baseline analysis using the intensity measure. The average marginal

4.3. Change Model Design

One nice feature of the WBES database is that the survey has some firms interviewed twice. If the World Bank has surveyed the same firm twice in different years, it assigns the same firm identifier across surveys. This "panel data" feature allows us to investigate whether a firm that had no auditor previously, but later appointed an auditor, has increased its export intensity. This approach largely eliminates firm-specific and time-invariant characteristics.

Panel (a) of Table 4 shows the univariate tests of the difference in the mean export intensity between the firms' two rounds of surveys. Overall, we have 2,812 firms from 55 countries with repeated surveys. We report the mean value of Export for three subgroups of firms: firms for which the audit status remained unchanged during the two surveys (1,999 firms); firms that were not audited in the first survey, but were later audited in the second survey (432 firms); and firms that were initially audited, but were not audited in the second survey (381 firms). For the first group, the mean export intensity is slightly higher in the second round. The difference, however, is statistically insignificant at conventional levels. For the second group where the firms were not audited in the first survey; but were later audited in the second round, the mean export intensity is higher in the second round and the difference is statistically significant at the 10% level. This comparison is consistent with our prediction that firms export more when they switch from not being audited to being audited. For the third group, where firms were audited in the first round, but were not audited in the second round, the mean export intensity is lower in the second round and the difference is statistically significant at the 10% level.

We next estimate Equation (1) in the first-order difference for firms that have two surveys in the WBES. We use ΔExport to indicate the change in export intensity and ΔX to indicate the change in the right-hand-side independent variables. We are interested in the coefficient on ΔAudit. We present the results in panel (b) of Table 4. The coefficient on ΔAudit is positive (β =0.016, t-statistic = 2.41) and statistically significant at the 5% level, which is consistent with the idea that firms that previously had no auditor, but later appointed an auditor, experienced an increase in export intensity.

5.   Identification

5.1. Regression Discontinuity Design

To identify causality, in this section, we employ a fuzzy RDD to ascertain the causal relationship between financial statement verification and exports. For private firms, some countries impose the requirements to have their financial statements audited. For example, Hungary has the following requirement:

All business entities are obliged to apply double-entry bookkeeping (including branches of foreign corporations) and are subject to a statutory annual audit. However, companies with an average turnover of below HUF 200 million and a headcount of fewer than 50 employees (in two full consecutive business years) are by default exempt from the obligation to have their books audited. (UHY International 2018, p. 25)

In this requirement, we understand that the threshold for the mandatory financial audit is represented by annual sales of Hungarian forint (HUF) 200 million or the number of employees of 50. This requirement enables us to implement a RDD using firms around the stipulated requirement. Around the threshold, firms whose number of employees is slightly above the threshold are selected into the treatment group whereas similar firms whose number of employees is slightly below the threshold serve as the control group. As such, the exogenous mandatory audit regulation triggers a forced increase in the probability of receiving financial audits for the treatment group. Within a narrow range around the threshold, firm characteristics are similar except that the treatment group is subject to mandatory audits. Thus, the difference in Export between these two groups of firms can be attributed to the forced audit effect.

To do this, we first manually collect a sample of mandatory financial audit thresholds around the world. Similar to the example of Hungary, the thresholds are usually contingent on a firm's annual sales, total assets, or number of employees. Unfortunately, we find that in the WBES, when firms respond to the annual sales question, they round their numbers to the nearest base-10 thresholds (i.e., 1,000, 10,000, or 100,000 local currency units). For example, we find that 28.2% of our sample firms report their sales to the nearest 10,000,000, 26.9% of our sample firms report their sales to the nearest 1,000,000, 20.1% of our sample firms report their sales to the nearest 100,000, 8.3% of our sample firms report their sales to the nearest 10,000, and 5.4% of our sample firms report their sales

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effect of Audit suggests that moving from a firm that was not audited to an otherwise identical audited firm results in an increase of 37.7% in export sales.

to the nearest 1,000. This rounding is particularly damaging to the RDD, since rounding prohibits us from distinguishing between firms that are slightly above and below the sales thresholds[22].

Panel (a): Changes in *Export* in three subsamples

	(1) $\Delta Audit = 0$	
	Mean of *Export*	N
Round 1	0.098	1,999
Round 2	0.104	1,999
Difference	0.006	
p-value (*t*-test)	0.207	

	(2) $\Delta Audit = +1$	
	Mean of *Export*	N
Round 1	0.073	432
Round 2	0.095	432
Difference	0.022	
p-value (*t*-test)	0.072*	

	(3) $\Delta Audit = -1$	
	Mean of *Export*	N
Round 1	0.098	381
Round 2	0.075	381
Difference	−0.023	
p-value (*t*-test)	0.077*	

Notes. Panel (a): Changes in Export for three subgroups of firms: firms for which the audit status remained unchanged during the two rounds; firms that were not audited in the first round, but were later audited in the second round; and firms that were initially audited, but later became not audited in the second round. Panel (b): Results of estimating Equation (1) in the first-order difference form using firms with two surveys in the WBES. Δ is the change operator. T-statistics are in parentheses and standard errors are clustered by country.
*p < 0.1; **p < 0.05; ***p < 0.01.

Panel (b): First-difference regression

| Dependent variable	(1) $\Delta Export$
$\Delta Audit$	**0.016****
	(2.41)
$\Delta Size$	0.006*
	(1.86)
ΔROS	−0.009
	(−0.79)
$\Delta State$	0.032
	(0.11)
$\Delta Foreign$	0.022
	(1.10)
$\Delta Block$	−0.016
	(−0.94)
$\Delta log(MEXP)$	0.005
	(0.96)
$\Delta Part$	0.008
	(1.02)
$\Delta Main$	−0.005
	(−0.43)
$\Delta Govcont$	0.011
	(1.32)
$\Delta Quality$	0.031***
	(2.77)
$\Delta Foreign\ tech$	0.033*
	(1.67)
$\Delta Email$	0.015
	(1.03)
$\Delta Finance$	0.022**
	(2.66)
$\Delta FinCon$	−0.007
	(−1.06)
$\Delta Access\ debt$	−0.011
	(−1.47)
$\Delta log(Age)$	−0.010
	(−0.63)
$\Delta Website$	0.021***
	(2.68)
Country fixed effects	Yes
Year fixed effects	Yes
Industry fixed effects	Yes
Adjusted R^2	0.036
N	2,812

In contrast, we find that firms do not round their responses to the question about number of employees. To first validate that firms with employee numbers above the threshold indeed have a higher likelihood of having auditors, we plot our data in Figure 1. The horizontal axis indicates the relative distance between firms' actual employee numbers and the mandatory audit threshold in the country (i.e., relative distance = (firm i's employee number− the threshold)/the threshold). We use the relative distance to the stipulated thresholds on the horizontal axis rather than the absolute distance because thresholds are different across countries. Distance = 0 indicates the threshold. In panel (a) of Figure 1, the vertical axis plots the mean of Audit. Each circle is generated by averaging Audit across firms within a 0.01 range of the relative distance (i.e., bin size = 0.01), weighted by the number of firms in each bin. A bigger circle represents more firms in each bin. A quadratic function of best fit is drawn to detect the jump in the mean of Audit around the cutoff point. From the plot, we observe that there is a discontinuity in Audit

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[22] WBES does not provide the total assets data.

around the threshold Distance $=0$[23]. In panel (b), the vertical axis plots the mean of Export. Each circle is generated by averaging Export across firms within a 0.01 range of the relative distance (i.e., bin size = 0.01), weighted by the number of firms in each bin. Again, we observe a discontinuity of an approximately 10% increase in the export intensity across the threshold line[24].

Figure 1. (Color online) *Audit* and *Export* Around the Thresholds Requiring a Financial Audit

(a)
Audit on the two sides of the threshold

(b)
Export on the two sides of the threshold
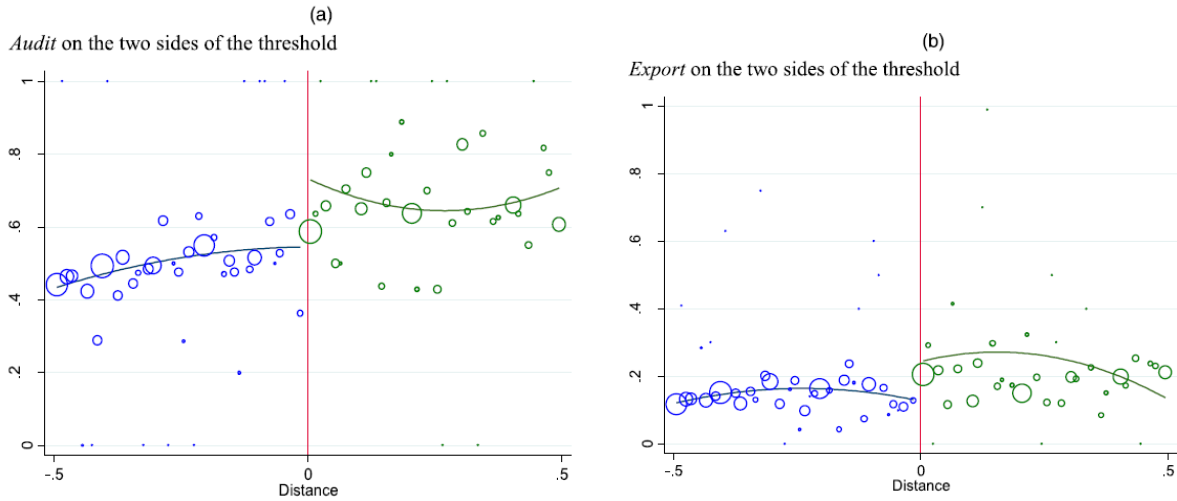
In this narrow bandwidth, firm characteristics are similar around the threshold except that the firms slightly exceeding the threshold are subject to mandatory audits and the firms slightly below the threshold are not. Thus, any difference in Export between these two groups can be ascribed to the forced audit effect. Because both Above and Export may be correlated with firm size, to control for the potential confounding effect of firm size, we include the relative distance of a firm's actual number of employees from the threshold number (Distance). Including Distance as a control helps isolate the treatment effect of the forced audit on Export around the discontinuity point (Falato and Liang 2016). We also add a nonlinear term, that is, the squared term of Distance, to control for the

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[23] In a fuzzy RDD, treatment is determined partly by whether the assignment variable crosses a cutoff point. As discussed in Lee and Lemieux (2010), this situation is common in practice for a variety of reasons, when factors other than the threshold rule affect the probability of treatment. Unlike the case of the sharp RDD, where the probability of treatment jumps from 0 to 1 at the cutoff, in the fuzzy RDD case, the probability jumps by less than one. Specifically, in our setting, other factors, such as the size of revenues and total assets, may jointly determine whether a firm is a treatment firm (i.e., has a mandatory audit) or not. However, with the WBES data, we cannot utilize the revenue threshold because rounding prohibits us from distinguishing between firms that are slightly above and below the sales thresholds. We cannot use the total assets variable either because WBES does not contain the total assets variable. As with the models with measurement errors, this fuzzy RDD would work against finding results.

[24] An implicit assumption underlying this test is that firms do not manage the number of employees. Although it is possible for firms to actively manage the number of employees by hiring fewer employees for a short period, it is difficult for firms to do so in the long run. This is because deliberately hiring fewer employees than the optimal level for an extended period would be harmful to the firm's operations and will impede the firm's growth. Also, it may not be easy for firms to hide the number of employees from the government. For example, the number of employees can easily be cross-verified with the payroll offices, social insurance agencies, and other local and state government agencies. To ascertain whether firms actively manage the number of employees to avoid mandatory audits, we conduct several tests. First, in untabulated tests, we plot the continuity of the sample density around the threshold using the approach developed by McCrary (2008). If firms actively manage the number of employees to escape the mandatory audit requirement, we would expect that the density of observations that report the number of employees just below the threshold is significantly higher than the density of observations that report the number of employees just above the threshold, that is, there would be a discontinuity around the threshold. From the untabulated plot, we find that the density of observations just below the threshold is actually lower than the density of observations just above the threshold. This is inconsistent with the conjecture that firms manipulate the number of employees to avoid audit. Second, we estimate the abnormal number of employees by using the natural logarithm of the firm's fixed assets plus firm characteristics and fixed effects in our baseline regression to predict the "normal" number of employees. We subtract the normal number of employees from the actual number of employees to obtain the abnormal number of employees. If firms actively manage the number of employees to escape the mandatory audit requirement, we would expect the abnormal number of employees to be lower in firms that report the number of employees just below the threshold. However, when we plot the Abnormal employment, we observe no discontinuity around the threshold.

potential nonlinear effect related to the forcing variable (i.e., the number of employees). We include all control variables and fixed effects as in our baseline regression, but we do not report them in the table.

In Table 5, we provide a formal test of our hypothesis using parametric regressions. The dependent variable is Export and the variable of interest, Above, is a binary variable indicating whether a firm meets the employee number threshold for a required audit. We estimate the parametric regressions using the sub samples of observations that are close to the mandatory audit threshold. We define closeness as having an employee number within the ranges of $\pm15\%$, $\pm20\%$, $\pm25\%$, or $\pm30\%$ of the threshold value. Panel (a) of Table 5 presents the results of estimating the augmented Equation (1) using the discontinuity sample, that is, firms close to the discontinuity. Consistent with our hypothesis, we find that the coefficient on Above is positive and statistically significant at the 5% level or better in columns (1) to (4) for the RDD sample. This is consistent with the conjecture that among those firms that are close to the mandatory audit threshold (and thus share an array of similar characteristics), meeting the threshold itself (and experiencing a consequent rise in the possibility of having an auditor) has a positive effect on export intensity[25].

To guard against the possibility that the observed positive relation between Export and Above is due to the parallel trends in the number of employees and export intensity, we conduct two falsification analyses in the spirit of Falato and Liang (2016). In the first falsification test, we create a pseudo-threshold for each country year. We define the pseudo-threshold as the median of the forcing variable (i.e., the number of employees) in the subsample above the actual threshold[26]. We then use the firm-year observations around the pseudo-threshold to run the regressions. We alternately use observations with an employee number within ranges of $\pm15\%$, $\pm20\%$, $\pm25\%$, or $\pm30\%$ of the pseudo-threshold value in the placebo tests. As before, Above is a binary variable indicating whether a firm is above the pseudo-threshold. Similarly, in the second falsification test, we define the pseudo-threshold as the median of the forcing variable (i.e., the number of employees) in the subsample below the actual threshold. We also alternately use observations with an employee number within the same range in the placebo regressions.

We present the placebo test results using these two constructed pseudo-thresholds in panels (b) and (c) of Table 5, respectively. If our RDD results merely capture the parallel trends in firm size and export intensity, we should observe a similar positive coefficient on Above for those placebo thresholds. However, panels (b) and (c) in Table 5 indicate that none of those placebo thresholds has a significant impact on exports. This is because compared with the firms slightly below the pseudo-thresholds, firms slightly above the pseudo-thresholds do not have a sharp increase in Audit. Collectively, our results suggest that only those real thresholds that lead to a forced jump in Audit generate more exports, providing further evidence on the causality of our results.

5.2. Difference-in-Differences Design

We further address the endogeneity concern by exploiting the countries with exogenous regulation shocks to the mandatory audits. Several countries have revised their number-of-employees-based thresholds. Such a setting provides us with a quasi-natural experiment to perform a DiD design to identify the effect of the change in mandatory auditing status on exports. We expect that firms that are exempted from auditing due to such an exogenous shock have differential exports after the regulation change.

We first assess the possibility of using the WBES sample for this design. Although we collect information on the employee-number-based mandatory thresholds for audits at the country-year level, there is little variation across years. Specifically, we observe only four countries with such changes during our sample period: Estonia, Montenegro, Slovakia, and Turkey. Starting in 2009, Montenegro increased the threshold from 50 employees to 250 employees. Unfortunately, all firms from Montenegro with two waves of surveys report their number of employees either below 50 or above 250. In other words, these firms are unaffected by the shock on the threshold. Starting in 2009, Slovakia increased the threshold from 20 to 30 but all survey firms from Slovakia were

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[25] To further mitigate the potential bias due to potential employee number manipulation, we conduct a robustness check by excluding the observations where firms report the number of employees that are immediately below the threshold (the "suspect" manipulation firms). Specifically, we repeat the test in panel (a) of Table 5 by excluding suspect firms that report the number of employees in the $[-0.05, 0)$ range below the threshold from the bandwidths of $\pm15\%$, $\pm20\%$, $\pm25\%$, or $\pm30\%$ around the threshold value. The coefficient on Above remains positive and statistically significant at the 5% levels or better.

[26] For example, in 2009 Hungary had an actual threshold of 50. That is, firms with a number of employees larger than 50 are required to have their financial statements audited. To create the pseudo-threshold, we collect all Hungarian firms with more than 50 employees into a subsample (call it the "above" subsample) and define the median number of employees in the above subsample as the pseudo-threshold for the subsample.

interviewed after 2009. Turkey lowered the threshold in 2014 but all surveys from Turkey were conducted before 2014. Starting in 2010, Estonia increased the threshold from 10 to 30 employees. However, we observe only 10 firms with two years' data around the change. Limited by the WBES data structure, we cannot employ the DiD design with the WBES sample.

We turn to alternative data sources to employ the DiD design. The Orbis database offered by Bureau Van Dijk provides us with an opportunity to employ this test. Specifically, the online version of Orbis provides historical financial data (for a limited set of variables) for the past 10 years for European public and private firms. We extract data for Estonia, Montenegro, Slovakia, and Turkey. Unfortunately, Orbis does not cover firms in Montene growth sufficient financial statement data(total assets, sales, etc.).The coverage for Turkish firms is also incomplete. We are only able to identify seven Turkish private firms in 2014 with sufficient data but only two of them have available values for the number of employees. Export data for Slovak firms is unavailable in Orbis. In contrast, the coverage for Estonian firms is rich. As such, we implement the test on a sample of firms from Estonia.

We follow the regulation change in Estonia since January 1, 2010, to design our test. Before this date, firms that satisfy two of the following—(1) revenue > 0.64 million euros, (2) assets > 0.32 million euros, and (3) number of employees> 10—must have mandatory financial audits. Since January 1, 2010, firms that satisfy two of the following—(1) revenue > 2 million euros,(2) assets > 1 million euros, and (3) number of employees > 30—must have mandatory financial audits.

Table 5. The Regression Discontinuity Design

Panel (a): Results of parametric regressions using real thresholds				
	Export			
Distance	(1) ±15%	(2) ±20%	(3) ±25%	(4) ±30%
Above	0.271**	0.263**	0.212**	0.178**
	(2.05)	(2.55)	(2.45)	(2.56)
Distance	0.060	−0.418	−0.322	−0.168
	(0.07)	(−0.79)	(−1.11)	(−0.91)
Distance^2	9.851**	2.499	−0.685	−1.050
	(2.17)	(0.82)	(−0.50)	(−1.02)
Control variables	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Pseudo R^2	0.287	0.289	0.235	0.219
N	476	551	858	947
Panel (b): Falsification analysis using pseudo-thresholds constructed using observations above the real threshold				
Above	0.014	−0.021	0.100	0.047
	(0.12)	(−0.22)	(1.27)	(0.64)
Distance	−0.105	0.380	−0.423*	−0.106
	(−0.17)	(1.06)	(−1.83)	(−0.55)
Distance^2	−4.722	−2.149	−0.200	0.299
	(−1.33)	(−1.18)	(−0.20)	(0.44)
Control variables	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Pseudo R^2	0.304	0.276	0.263	0.271
N	427	565	729	829
Panel (c): Falsification analysis using pseudo-thresholds constructed using observations below the real threshold				
Above	−0.121	0.075	0.065	0.051
	(−0.60)	(0.62)	(0.89)	(0.82)
Distance	1.053	0.008	0.122	0.108
	(1.08)	(0.02)	(0.53)	(0.60)
Distance^2	−6.769	0.815	1.109	−0.024
	(−0.90)	(0.25)	(0.87)	(−0.02)
Control variables	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Pseudo R^2	0.210	0.204	0.204	0.210
N	1,196	1,445	1,990	2,245

Notes. Bold indicates variables of interest. Panel (a): Results of estimating an augmented version of Equation (1). The dependent variable is Export, and the variable of interest, Above, is a binary variable set to one if a firm meets the employee number threshold for a required audit, and zero otherwise. We include the relative distance (with the sign) of a firm's number of employees from the threshold number (Distance) as well as the squared term to control for the effect of firm size on the export intensity. We include all other firm characteristics and fixed effects in the regression. T-statistics are in parentheses and standard errors are clustered by country. Panel (b): We define the pseudo-threshold as the median of the forcing variable (i.e., the number of employees) in the subsample above the actual threshold. We alternately use observations with an employee number within ranges of ±15%, ±20%, ±25%, or ±30% of the pseudo-threshold value. Above is a binary variable indicating whether a firm meets the pseudo-threshold. We include the relative distance (with the sign) of a firm's number of employees from the pseudo-threshold (Distance) as well as the squared term to control for the effect of firm size on the export intensity. We include all other firm characteristics and fixed effects in the regression. T-statistics are in parentheses and standard errors are clustered by country. Panel (c): We define the pseudo-threshold as the median of the forcing variable (i.e., the number of employees) in the subsample below the actual threshold. We alternately use observations with an employee number within ranges of ±15%, ±20%, ±25%, or ±30% of the pseudo-threshold value. Above is a binary variable indicating whether a firm meets the pseudo-threshold. We include the relative distance (with the sign) of a firm's number of employees from the pseudo-threshold (Distance) as well as the squared term to control for the effect of firm size on the export intensity. We include all other firm characteristics and fixed effects in the regression. T-statistics are in parentheses and standard errors are clustered by country.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Alternatively, firms that satisfy one of the following—(1) revenue > 6 million euros, (2) assets > 3 million euros, and (3) number of employees > 90—must have mandatory financial audits.

We first clean the data from Orbis using the same data requirement that we work with the WBES. We remove all public firms and require that firms have available total assets, number of employees, and income before extraordinary items. To form a meaningful set of control firms that are comparable to the treatment firms, we require that firms have at least revenues of 0.32 million euros and total assets of 0.16 million euros, respectively[27]. We choose these filters since these numbers are half the revenue and total assets thresholds for mandatory audit before 2010 (under the old regime), respectively. Finally, we exclude firms in the banking industries since these firms are unlikely to export products to foreign countries. Our firm years in this DiD design are 2007 to 2012 (i.e., three years before and after the change, respectively).

We calculate export intensity as the revenue from export scaled by sales revenue. Because Orbis does not cover data such as whether the firm has government contracts, or whether the firm uses a foreign technology, we estimate a "parsimonious" equation by including Size, Financing, Access debt, and ROS. In particular, Financing is an indicator variable that takes a value of one if the firm has interest-bearing debt and zero otherwise. Access debt is an indicator variable that takes a value of one if the firm has new debt and zero otherwise. Like our baseline regression, we also include the industry and year fixed effects. We define industries using the Fama-French 48 industries.

It is clear that the new regime, in general, increases the threshold for mandatory audits. Following the new regulation in Estonia, we define Exempted as an indicator variable that takes a value of one if a firm is required to be audited in 2009 (under the old regime) but is exempted from the mandatory audit in 2010 (under the new regime). We find that 25% of our sample firms are identified as Exempted firms as they needed to be audited under the old regime but are exempted from audit under the new regime. Post takes a value of one for years after 2010 (inclusive). Control variables are defined in the same way as in the main text. We estimate the following regression:

$$Export_{t+1} = \beta_0 + \beta_1 Post \times Exempted + \beta_2 Exempted + Firm\ controls + fixed\ effects. \qquad (2)$$

We employ the export data from year $t+1$ as our dependent variable. To keep some degree of consistency with our main test using WBES data, we also employ export data from year t as an alternative dependent variable. The variable of interest is the interaction term Post × Exempted. Because the key variable of interest is the interaction term, to facilitate interpretation, we use the OLS regression (Ai and Norton 2003). A negative coefficient on Post × Exempted would imply that a firm's export declines after the firm is exempted from mandatory audit under the

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[27] As of January 14, 2011, when the euro became the sole legal tender in Estonia, the value of one Estonian kroon is 0.064 euros.

new regime. The standalone variable Post is automatically dropped when we include year fixed effects. We present the results in Table 6. The coefficient on Post × Exempted is negative and statistically significant at the 1% level in column (1). This is consistent with the idea that firms that are exempted from the mandatory audit have lower exports in subsequent years[28]. To the extent that Orbis only covers selected fundamental financial variables, we cannot control for many firm characteristics (such as management experience) as we do with WBES data. To overcome this, in column (2), we include firm fixed effects to help absorb the time-invariant firm characteristics that may contribute to export intensity. The standalone variable Exempted is automatically dropped when we include firm fixed effects. Again, the coefficient on Post × Exempted is negative and statistically significant at the 5% level after controlling for firm fixed effects. Results in columns (3) and (4) are similar. This is consistent with the prediction that firms that are exempted from the mandatory audit have lower exports in subsequent years. Taken together, evidence in this DiD design confirms our previous findings that having fi-financial statements audited helps promote exports. Meanwhile, since the sample in this test is based on a single country, we also acknowledge the generalizability limitations.

6. Cross-Sectional Variation Tests

6.1. Moderating Role of Audit Effectiveness

To further infer causality, we next test Hypothesis 2a, that is, the moderating role of audit effectiveness. In these tests, we vary the effectiveness of external auditing. If auditing plays an important role in promoting exports, the effect of auditing on exports should be stronger when auditing is more effective.

Table 6. The Difference-in-Differences (DiD) Design Using the Sample with a Change in the Threshold of Mandatory Audit

	$Export_{t+1}$		$Export_t$	
	(1)	(2)	(3)	(4)
Post × Exempted	−0.172***	−0.154**	−0.113**	−0.097*
	(−3.21)	(−2.12)	(−2.53)	(−1.79)
Exempted	0.029		−0.030	
	(1.35)		(−1.11)	
Size	0.048**	0.187***	0.046**	0.137***
	(2.12)	(3.30)	(1.97)	(2.81)
ROS	0.319*	0.000	0.364**	0.209
	(1.91)	(0.00)	(2.25)	(1.08)
Financing	0.009	0.000	0.000	0.038
	(0.27)	(0.01)	(0.00)	(0.79)
Access debt	0.009	0.013	0.013	−0.012
	(0.39)	(0.50)	(0.58)	(−0.55)
Industry fixed effects	Yes	No	Yes	No
Firm fixed effects	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.396	0.664	0.388	0.716
N	966	966	1,155	1,155

Notes. Bold indicates variables of interest. Results of a DiD regression using the Estonian sample between 2007 and 2012. Exempted is an indicator variable that takes a value of one if the firm is required to be audited in 2009 (under the old regime) but is exempted from the mandatory audit in 2010 (under the new regime). Post takes a value of one for years after 2010 (inclusive). The dependent variables are export sales (scaled by total sales) in year t + 1 in columns (1) and (2) and export sales (scaled by total sales) in year t in columns (3) and (4), respectively. We include Size (defined as the natural logarithm of total sales), ROS (defined as net income scaled by sales), Financing (defined as an indicator variable that takes a value of one if the firm has interest-bearing debt and zero otherwise), and Access debt (defined as an indicator variable that takes a value of one if the firm has new debt and zero otherwise). We include industry and year fixed effects in columns (1) and (3) and include firm and year fixed effects in columns (2) and (4), respectively. T-statistics are in parentheses and standard errors are clustered by firm.
*p < 0.1; **p < 0.05; ***p < 0.01.

We employ the audit profession development score (APD score) developed in Michas (2011) as our first measure of audit effectiveness. Michas (2011) classifies the discussions on the APD in the World Bank reports into four broad aspects: (A) auditor education, (B) auditing standards, (C) auditor independence, and (D) auditor oversight.

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[28] It is also possible that some importers would continue to import products from the exporter even if the exporter no longer has an auditor because of the specific operational needs. Meanwhile, some exempted firms may have voluntary audits. These possibilities would work against finding the results.

Then, he identifies several subcomponents within each aspect. For example, under the auditor education category, Michas searches the discussion for the following four questions: A1: Are universities' accounting education curriculum standards the same for all universities within a country? A2: Are auditors required to perform on a professional examination before being licensed to practice as an auditor? A3: Are accountants required to gain professional experience before being licensed as an auditor? A4: Are auditors required to fulfill continuing education requirements on an annual basis? Appendix B in Michas (2011) presents the questions for each subcomponent within each aspect and the coding scheme.

We obtained the coded data file from Michas[29]. The expanded APD index now covers 51 countries and is updated to our sample year following the coding scheme in Michas (2011). Then, for each aspect, we average the scores for the subcomponents to obtain a subscore. The final APD score is the average of the four sub-scores for the four aspects. A higher APD score indicates more effective external audits. We partition our sample by the median of this APD index. We present the results in panel (a) of Table 7. The coefficient on Audit is 0.119 and statistically significant at the 1%level for the high APD subsample. In contrast, for the low APD subsample, the coefficient on Audit is merely one-half of the magnitude (0.059), when compared with the coefficient in the high APD subsample. The difference is also statistically significant (p =0.051), consistent with the idea that auditing plays a much more important role in promoting exports when the overall quality of the audit profession is higher.

We construct our next measure for audit effectiveness directly from the WBES data. Specifically, this measure is based on the response to the following question:
Do you strongly disagree, tend to disagree, tend to agree, or strongly agree (scoring 1–4, respectively) that "the court system is fair, impartial and uncorrupted"?

The rationale is that, if the court system is fair, it is more likely that auditors face strong litigation risks. As such, we expect that the financial audits are conducted effectively. On the contrary, if the court system is unfair, it is more likely that the auditors are not subject to strong litigation risks, and that they forfeit audit quality to cater to the management or simply spare efforts. Therefore, we expect that in countries where the court systems are not perceived as fair, audits are less effective. Khurana and Raman (2004) find that the perceived audit quality (measured by auditees' ex-ante cost of equity capital) is higher in the United States than in other economically similar but less litigious countries, supporting the idea that perceived audit quality is shaped by the auditors' litigation exposure.

To construct this macro-level country-specific measure, Court fairness, we average the firm-level responses from each country to arrive at a country-level measure[30]. As such, a higher score represents the public perception of a fair court in that country. We then perform the cross-sectional variation test to examine whether the court system's fairness moderates the effect of financial statement verification on ex-port intensity. We split our sample by the perceived fairness score into five quintiles and run the baseline regression using the lower (Q1 to Q4) quintiles and the highest (Q5) quintile, respectively[31]. We report the results in panel (b) of Table 7. The coefficient on Audit in the highest quintile is statistically significantly larger than the coefficient in the lower quintiles, consistent with the idea that the effect of financial audits in countries with fairer court systems (hence higher litigation risks to auditors) is stronger in promoting foreign trades.

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[29] We thank Professor Paul Michas at the University of Arizona for generously sharing with us.

[30] We first calculate the country-level court fairness by averaging firms' individual responses because conceptually court fairness should reflect the common view of the court fairness in each country.

[31] In untabulated tests, we also split the entire sample into two subsamples by the mean of the perceived fairness score. The co-efficient on Audit in the high subsample is two-thirds larger than the coefficient on Audit in the low subsample. However, the test of statistical significance reveals that the difference is not statistically significant at the conventional levels. This may be because the countries in our sample are emerging economies and these countries, in general, have weak court systems. As such, in panel (b) of Table 7, we conduct the test using the firms with the strongest court fairness vs. the firms with weak court fairness.

Table 7. Cross-Sectional Variation Tests Partitioned by Proxies for Audit Effectiveness

Panel (a): Partition by APD score	Export		Panel (b): Partition by the perceived fairness of the court system	Export	
Partitioning variable: *APD score*	(1) *Low*	(2) *High*	Partitioning variable: *Court fairness*	(1) *Low* (Q1–Q4)	(2) *High* (Q5)
Audit	**0.059*** **	**0.119*** **	**Audit**	**0.062*** **	**0.155*** **
	(3.65)	**(3.49)**		**(3.92)**	**(3.47)**
Control variables	Yes	Yes	Control variables	Yes	Yes
Country fixed effects	Yes	Yes	Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes	Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes	Industry fixed effects	Yes	Yes
Difference in the coefficient on *Audit*	0.060		Difference in the coefficient on *Audit*	0.093	
p-value (H$_1$: $\beta_{Audit}^{(1)} < \beta_{Audit}^{(2)}$)	0.051*		p-value (H$_1$: $\beta_{Audit}^{(1)} < \beta_{Audit}^{(2)}$)	0.023**	
Pseudo R^2	0.235	0.223	Pseudo R^2	0.211	0.226
N	18,936	17,695	N	46,804	11,569

Panel (c): Partition by auditor litigation risk

	Export	
Partitioning variable: *Auditor litigation risk*	(1) *Low* (*Auditor litigation risk* < 1)	(2) *High* (*Auditor litigation risk* = 1)
Audit	**0.065*** **	**0.126*** **
	(3.13)	**(5.24)**
Control variables	Yes	Yes
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Difference in the coefficient on *Audit*	0.061	
p-value (H$_1$: $\beta_{Audit}^{(1)} < \beta_{Audit}^{(2)}$)	0.030**	
Pseudo R^2	0.197	0.282
N	38,643	5,012

Notes. Bold indicates variables of interest. Panel (a): Results of a Tobit regression of Export on Audit and firm characteristics in subsamples partitioned by the APD score. Following Michas (2011), this index is calculated based on the World Bank reports for auditor education, auditing standards, auditor independence, and auditor oversight. A higher APD score indicates more effective audit quality. We split our sample by the median of the APD score. T-statistics are in paren-theses and standard errors are clustered by country. Panel (b): Re-sults of a Tobit regression of Export on Audit and firm characteristics in subsamples partitioned by the perceived fairness of courts in a country. T-statistics are in parentheses and standard errors are clustered by country. Panel (c): This panel reports the results of a Tobit regression of Export on Audit and firm characteristics in subsamples partitioned by the level of the auditor's litigation risk T-statistics are in parentheses and standard errors are clustered by country.

Finally, we employ an index that directly measures the auditor litigation risk to capture audit effectiveness. We expect that audits are more effective in countries where the auditors' litigation risk is high. To construct the auditor litigation risk index, we manually collect data from the World Bank. Specifically, the World Bank issues Reports on the Observance of Standards and Codes—Accounting and Auditing (ROSCs) for its member countries. In each ROSC report, the World Bank describes the level of liability faced by auditors in each specific country. We manually read each ROSC and convert the qualitative discussions into a numerical value. Following Michas (2011), we code Auditor litigation risk as 0 if the liability is non-existent, 0.33 if low, 0.67 if mid-level, and 1 if the liability is high, respectively. We then examine whether Auditor litigation risk moderates the effect of auditing on exports.

We obtain Auditor litigation risk for 72 countries. Because the ROSC reports for the rest of countries are unavailable on the internet, we use a smaller sample. Consistent with our intuition, on average, the auditors in our sample countries face low to moderate liability risks. The mean Auditor litigation risk is 0.49, falling between 0.33 (low liability risk) and 0.67 (mid-level liability risk). Nevertheless, about 12.5% of countries have a score of 1 (the auditor's liability risk is high). We then perform a cross-sectional variation test to examine whether Auditor litigation risk moderates the effect of auditing on foreign export intensity. We split the full sample into two subsamples: high (Auditor litigation risk =1) and low (Auditor litigation risk < 1), respectively. We report the results in panel (c) of Table 7. In the subsample where Auditor litigation risk is high, the coefficient on Audit is positive and approximately twice as high as the coefficient on Audit in the low Auditor litigation risk subsample. The difference in the coefficients is also statistically significant at the 5%level. This finding is consistent with the idea that the effect of auditing on promoting exports is more pronounced if the auditors face high litigation risk. Taken together, the evidence from this section suggests that the effect of auditing is more pronounced in countries where audits are more effective.

6.2. Role of Alternative Information Sources About the Exporter

We next perform several cross-sectional variation tests to examine whether the availability of other information sources moderates the effect of financial statement verification on export intensity (Hypothesis 2b). These tests rest on the prediction that other information in the supplier-customer relationship may crowd out the effect of financial statement verification. The first partitioning variable we consider is the age of the exporter. When an exporter has a long-established history, information about the exporter is rich. As such, foreign importers are less likely to rely on fi-financial statement verification to assess the potential risk when they do business. The second partitioning variable we consider is the availability of the exporter's website. When the exporter has a website, foreign im-porters may obtain useful information about the ex-porter from the online interface.

We present the results in Table 8. We calculate Firm age as the number of years between the survey year and the year in which the firm began its operations. We obtain the availability of the exporter's website from the response to the following question:

Thinking about the present time, does this establishment use any of the following in its communications with clients or suppliers? a. Email; b. Its own website.

We present the results using subsamples partitioned by the median of Firm age in panel (a) of Table 8 and present the results using subsamples partitioned on the availability of the exporter's website in panel (b) of Table 8, respectively. Consistent with our predictions, the coefficients on Audit are higher in firms that are young and those without websites. The p-values to test the differences between the coefficients on Audit reveal that the differences are statistically significant at the 1% level.

7. Additional Analyses

7.1. Controlling for Financial Reporting Quality

In this section, we further control for financial re-porting quality. Because our sample firms from WBES are all private firms and WBES provides too limited financial variables to calculate the commonly used accruals-based measures of financial reporting quality, we implement an alternative design by using country-year-level financial reporting quality, estimated using public firms in these countries. Specifically, we follow Leuz et al. (2003) and develop a country-year-level measure of the overall financial reporting quality using public firms across these countries. To do so, we obtain data from Compustat Global and calculate the following four components of the country-year-level score. EM1 is the country's median ratio of the firm-level standard deviations of operating income over operating cash flow (both scaled by lagged total assets). The cash flow from operations is equal to operating income minus accruals, where accruals are defined following Sloan (1996). EM2 is the country's Spearman correlation between the change in accruals and the change in cash flow from operations (both scaled by lagged total assets). EM3 is the country's median ratio of firm-level absolute value of accruals to the absolute value of the cash flow from operations. EM4 is the number of small profits divided by the number of small losses for each country. A firm-year observation is classified as a small profit (loss) if net earnings scaled by lagged total assets are in the range $[0, 0.01]$ ($[-0.01, 0)$). Among these four measures, EM1 captures the degree of earnings smoothing, with the idea that more aggressive smoothing indicates more earnings management. EM2 captures the correlation between changes in accruals and changes in cash flows, with the idea that a more negative correlation indicates more earnings management. EM3 captures the magnitude of accruals and a higher value indicates more earnings management. EM4 captures small loss avoidance, and a higher percentage of firms reporting small profits indicates more earnings management. Then, for each country year, we rank these four earnings management metrics such that a higher score indicates a higher level of earnings management. Finally, we average the four ranked values in each country-year pair to calculate the score to capture the aggregate financial reporting quality. We define the aggregate score as FRQ. A higher FRQ indicates poorer financial re-porting quality.

We then add FRQ into our baseline regression. To the extent that the inclusion of FRQ further reduces the sample size, we include FRQ in a separate panel, that is, panel (a) of Table 9. The coefficient on FRQ is statistically insignificant at the conventional levels, possibly due to the fact that a full set of country fixed effects help absorb the variations in financial reporting quality across countries. Most importantly, the coefficient on Audit remains positive and statistically significant at the 1% level.

Table 8. Cross-Sectional Variation Tests Partitioned by Proxies for Alternative Information Source

	Panel (a): Partition by firm age			Panel (b): Partition by the availability of the corporate website		
		Export				*Export*
Partitioning variable: *Firm age*	(1) Younger	(2) Older	Partitioning variable: *Website*	(1) No	(2) Yes
Audit	0.099***	0.045***	*Audit*	0.125***	0.048***
	(4.38)	(2.85)		(4.04)	(3.42)
Control variables	Yes	Yes	Control variables	Yes	Yes
Country fixed effects	Yes	Yes	Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes	Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes	Industry fixed effects	Yes	Yes
Difference in the coefficient on *Audit*	0.054		Difference in the coefficient on *Audit*	0.077	
p-value (H$_1$: $\beta_{Audit}^{(2)} < \beta_{Audit}^{(1)}$)	0.009***		p-value (H$_1$: $\beta_{Audit}^{(2)} < \beta_{Audit}^{(1)}$)	0.007***	
Pseudo R^2	0.218	0.204	Pseudo R^2	0.195	0.173
N	29,263	29,110	N	31,316	27,057

Notes. Bold indicates variables of interest. Panel (a): Results of a Tobit regression of Export on Audit and firm characteristics in subsamples partitioned by the firm age. Firm age is calculated as the number of years between the survey year and the year in which the firm began its operations. We split the sample by the median firm age. T-statistics are in parentheses and standard errors are clustered by country. Panel (b): Reports the results of a Tobit regression of Export on Audit and firm characteristics in subsamples partitioned by the availability of a firm's website. T-statistics are in parentheses and standard errors are clustered by country.
*p < 0.1; **p < 0.05; ***p < 0.01.

Table 9. Robustness Checks

Panel (a): Controlling for country-year-level financial reporting quality	
	(1) *Export*
Audit	0.062***
	(3.79)
FRQ	0.000
	(0.35)
Control variables	Yes
Country fixed effects	Yes
Year fixed effects	Yes
Industry fixed effects	Yes
Pseudo R^2	0.206
N	43,142

Notes. Bold indicates variables of interest. Panel (a): Results of a Tobit regression of Export on Audit and firm characteristics after controlling for country-year-level financial reporting quality. Following Leuz et al. (2003), we develop a country-year-level measure of the overall financial reporting quality using public firms from Compustat Global in our sample countries. We control for firm characteristics and include industry, country, and year fixed effects. T-statistics are in parentheses and standard errors are clustered by country. Panel (b): Results of a Tobit regression of Export on Audit and firm characteristics for a sample that excludes countries with an extremely large or small number of observations. In column (1), we exclude the top five countries with the most observations, that is, India, China, Russia, Mexico, and Colombia. In column (2), we exclude 23 countries with fewer than 100 observations. We control for firm characteristics and include industry, country, and year fixed effects. T-statistics are in

Panel (b): Excluding the top five countries with the most observations or those countries with fewer than 100 observations

	Export	
	(1) Excluding five countries with most observations	(2) Excluding countries with fewer than 100 observations
Audit	0.066***	0.067***
	(3.72)	(4.46)
Control variables	Yes	Yes
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Pseudo R^2	0.216	0.212
N	44,116	57,001

Panel (c): Construct validity of WBES responses

	Export	
	(1) More restrictive	(2) Most restrictive
Audit	0.072***	0.080***
	(3.53)	(3.85)
Control variables	Yes	Yes
Country fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Pseudo R^2	0.205	0.210
N	34,722	17,534

parentheses and standard errors are clustered by country. Panel (c): Results of a To bit regression of Export on Audit and firm characteristics for a sample that meets more stringent requirements regarding the truthfulness and precision of the responses. In column (1), we require that the responses to questions regarding opinions and perceptions are rated by interviewers as

"truthful," and that the responses to questions regarding figures are rated as "taken from establishment" or "estimates computed with some precision." In column (2), we further require that the interviewers rate the responses to questions regarding figures as "taken from establishment" only. We control for firm characteristics and include industry, country, and year fixed effects. T-statistics are in parentheses and standard errors are clustered by country.

*p < 0.1; **p < 0.05; ***p < 0.01.

7.2. Excluding the Top Five Countries with the Most Observations and Those Countries with Fewer than 100 Observations

We discover that around one-fourth of the observations in our sample are firm-year observations from India, China, Russia, Mexico, and Colombia. We also have 23 countries with fewer than 100 firm-year observations. To ensure that our results are not driven by these countries, in this subsection, we exclude firms from India, China, Russia, Mexico, and Colombia, as well as firms from countries with fewer than 100 firm-year observations, respectively.

We present the results in panel (b) of Table 9. We remove firm-year observations from India, China, Russia, Mexico, and Colombia in column (1) and remove those countries with fewer than 100 firm-year observations in column (2), respectively. The economic and statistical significance of the effect of Audit on Export remains similar when we remove these observations from the sample. We conclude that our results are not driven by the selected countries with the most observations or by those with very few observations.

7.3. Construct Validity of the WBES Responses

One concern is that the responses to the WBES survey questions might not accurately describe the real operations of the surveyed firms. Indeed, the World Bank is concerned about this potential construct validity problem and thus asks the interviewer to complete two questions about the quality and creditworthiness of the responses. At the end of each survey, the WBES requires the interviewer to rate responses to questions regarding (1) opinions and perceptions and (2) figures, including productivity and employment numbers. Specifically, the two diagnostic questions to the interviewer are as follows:

Q1: It is my perception that the questions regarding opinions and perceptions are: (1) truthful, (2) some-what truthful, or (3) not truthful.

Q2: The questions regarding specific figures (productivity and employment numbers) are: (1) taken directly from establishment records, (2) estimates computed with some precision, or (3) arbitrary and unreliable numbers.

Our baseline test employs the sample using firm-year observations that give type-1 or type-2 opinions (truthful or somewhat truthful) and type-1 or type-2 figures (taken from establishment records or reasonable estimations). In other words, we exclude those observations with untruthful responses or unreliable numbers. To further ensure that our results are not driven by poor quality observations, we impose stricter requirements regarding response accuracy. We present the results in panel (c) of Table 9. In column (1), we require that the responses to Q1 are rated by inter-viewers as "truthful" and that the responses to Q2 are rated by interviewers as "taken from establishment" or "estimates computed with some precision," respectively. We refer to this as the "more restrictive" sample. In column (2), we further require that the interviewers respond to Q2 as "taken from establishment" only. We refer to this as the "most restrictive" sample. After applying these additional data filters, our sample size shrinks to 34,722 and 17,534 observations in columns (1) and (2), respectively. Most importantly, using these subsamples yields similar coefficients on Audit in both columns when compared with those reported in the baseline test.

8. Conclusion

We investigate the importance of auditing in promoting exports. Using a sample of private firms from 125 countries between 2006 and 2015, we show that firms that have their financial statements audited by an auditor have more exports than firms that do not have their financial statements audited after con-trolling for firm and country characteristics, as well as country, industry, and year fixed effects. To further establish causality, we employ the RDD and the DiD design. Our test procedures help mitigate the risk that our result simply documents the association between having an external auditor and high export intensity, although we cannot completely rule out that possibility.

In addition, we show that the effect of auditing is stronger in countries with more effective audits and for firms with less available information. Emphasizing the product market effect of financial audits in an international setting, our findings highlight the microeconomic foundation for the benefits of financial audits in promoting economic development.

Appendix: Variable Definitions

This table defines the main variables used in this study.

Variables	Definitions
Export	Export intensity, that is, a firm's export sales scaled by total sales
Has export	An indicator variable that takes a value of one if the firm exports, and zero otherwise
Audit	An indicator variable that takes a value of one if the firm has an auditor, and zero otherwise
Size	The natural logarithm of total sales
ROS	Net income scaled by total sales, where net income is imputed by subtracting the costs of production from the total sales
State	The proportion of shares held by government
Foreign	The proportion of shares held by foreign investors
Block	The proportion of shares held by the largest shareholders
log(MEXP)	The natural logarithm of the number of years that the top manager has been working in the industry sector
Part	An indicator variable that takes a value of one if the firm is a part of a larger group, and zero otherwise
Main	The proportion of total sales derived from the main product
Govcont	An indicator variable that takes a value of one if the firm secures (or attempts to secure) a government contract, and zero otherwise
Quality	An indicator variable that takes a value of one if the firm has obtained an internationally recognized quality certification, and zero otherwise
Foreign tech	An indicator variable that takes a value of one if the firm uses technology licensed from a foreign-owned company, and zero otherwise
Email	An indicator variable that takes a value of one if the firm uses email to communicate with business partners, and zero otherwise
Finance	An indicator variable that takes a value of one if the firm has a loan or line of credit from a bank, and zero otherwise
FinCon	An indicator variable that takes a value of one if the degree of obstacle is three or greater for access to finance, and zero otherwise
Access debt	An indicator variable that takes a value of one if the firm finances its fixed assets with new debt, and zero otherwise
Age	Number of years between the survey year and the year in which the firm began operations
Website	An indicator variable that takes a value of one if the firm has a website, and zero otherwise
Institution	The first principal component of the six indicators (government effectiveness, regulatory quality, corruption, political stability, rule of law, voice and accountability) in the World Development Indicators
GDP	The natural logarithm of GDP per capita
GDP growth	The growth rate of GDP per capita
Population	The natural logarithm of population
Inflation	The rate of inflation
APD score	The Audit Profession Development (APD) score developed in Michas (2011), an index calculated from four aspects: auditor education, auditing standards, auditor independence, and auditor oversight
Court fairness	The country average of firm-level responses from the question asking the extent to which the firm regards the court system in the country as "fair, impartial, and uncorrupted" (assigning an integer of 1–4 for strongly disagree, tend to disagree, tend to agree, and strongly agree, respectively)
Auditor litigation risk	Coded 0 if the auditor litigation liability is non-existent, 0.33 if low, 0.67 if mid-level, and 1 if the liability is high, respectively; data manually collected from the World Bank's ROSCs
Distance	The firm's number of employees less the threshold number for mandatory audits, scaled by the threshold number
Above	An indicator variable that takes a value of one if the firm meets the employee number threshold for a required audit, and zero otherwise

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