Measurement Matters:
Financial Reporting and Productivity

John Barrios
University of Chicago Booth School of Business

Petro Lisowsky
Boston University

Michael Minnis
University of Chicago Booth School of Business

January 2019

Abstract

We examine the relation between financial measurement practices and firm-level productivity. Using two proprietary data sets, including a comprehensive panel of firm tax returns, we find that financial measurement quality, as measured by the use of an independent auditor, explains 10-20% of the intra-industry dispersion of total factor productivity (TFP), a magnitude similar to that accounted for by firm-level differences in R&D and other structured management practices identified in prior studies. Our results suggest two mechanisms. First, using plausibly exogenous differences in misreporting incentives we show that external auditors attenuate reporting biases in administrative data. Thus a portion of *measured* productivity heterogeneity is the direct result of reporting differences across firms. Second, cross-sectional and panel analyses are consistent with high-quality measurement as a management practice *causing* higher productivity, akin to personnel policies or target setting. While short of identifying causal treatment effects, the economic magnitude of the relation suggests that firms' accounting practices are an important area for explaining the vast heterogeneity in reported productivity.

Keywords: Management, productivity, accounting, auditing
JEL Codes: D24, G3, L2, M2, M40, O33

We thank Philip Berger, Nicholas Bloom, Matthias Breuer, Michelle Hanlon, Chang-Tai Hsieh, Christian Leuz, Valeri Nikolaev, Nemit Shroff, Andrew Sutherland, Chad Syverson, John Van Reenen, Thomas Wollmann, Luigi Zingales and workshop participants at Carnegie Mellon, MIT, Tilburg, UCLA and the Empirical Management Conference at Harvard for valuable comments. We thank June Huang for research assistance.

1. Introduction

Research finds substantial and persistent differences in productivity across firms even within well-defined industries. For example, Syverson (2004a) finds that within four-digit SIC industries in the U.S. manufacturing sector, the average difference in logged total factor productivity (TFP) between an industry's 90th and 10th percentile plant is 0.651. This estimate implies that firms in the 90th percentile produce almost twice the amount of output with the same level of measured input as the 10th percentile plant. Other studies have found similar dispersions in productivity within industries (Dhrymes 1991; Doms and Bartelsman 2000; Syverson 2004b; Hsieh and Klenow 2009; Fox and Smeets 2011).

Wide dispersion in firm-level productivity is a topic of significant debate (Syverson 2011; Bloom et al. 2013). One explanation for heterogeneity in firm-level productivity considers how researchers measure productivity. To estimate and measure productivity, one needs to observe inputs and outputs accurately and make assumptions about production technologies and demand. A rich literature shows how differing assumptions and measurements lead to differing estimates of firm-level productivity.[1] A second approach explains residual productivity with previously unmeasured determinants, such as the link between managerial practices and productivity. Bertrand and Schoar (2003) point to manager-specific effects relating to firms' policy decisions and outcomes. More recent studies document an association between specific management practices, such as hiring and firing policies, and firm-level productivity outcomes (Bloom and Van Reenen 2007). These management practices explain approximately one-fifth of the spread in productivity between the 10th and 90th percentiles within industries (Bloom et al. 2018).

While both the management practices and measurement explanations have begun to "put faces on" the dispersion in intra-industry productivity, much unexplained variation remains

[1] See for example, Olley and Pakes (1996); Levinsohn and Petrin (2003); Van Biesebroeck (2007); Foster et al. (2016); Kim, Petrin, and Song (2016); Haltiwanger et al. (2018); Collard-Wexler and De Loecker (2016).

(Syverson 2011, pg. 330). However, one prominent management practice with the potential to affect both the *reported* level of productivity and the *actual* level of productivity in administrative data sets which remains relatively unexplored is a firm's investment in its accounting and financial reporting system. Accounting is the process of measuring economic transactions and then mapping those activities into financial reports. The information produced by measuring and compiling financial reports can provide managers with useful information when making decisions (e.g., Bushman and Smith 2001; Kanodia and Sapra 2016). After producing financial reports, firm managers can then hire independent auditors who examine the processes management followed to produce the reports and collect evidence verifying the validity of the reported numbers. Ultimately, auditors assure that the reports represent the underlying economics of the firm within a materiality threshold. However, because the overwhelming majority of firms in the U.S. economy are not required to engage an auditor — and the majority do not — we view the production of audited financial reports as a management practice with scope for significant variation, even among the largest firms (Lisowsky and Minnis 2018).

Differential quality in firm-level financial measurement and auditing practices is potentially related to firm-level productivity for at least two reasons. First, data sets used to calculate firm-level productivity are often populated by information from firm financial reports. Misreported balances in these reports can thus directly affect *measured* productivity. For example, if firms intentionally under-report their production in the absence of external verification by auditors (e.g., to reduce tax liabilities), differential levels of auditing across firms will lead to differential measured productivity by the econometrician.[2] Second, financial measurement quality can affect *actual* productivity if high-quality reports provide managers with

[2] There is a large literature investigating financial reporting biases. Examples include Healy (1985), Schipper (1989), Fields et al. (2001), and Graham et al. (2005). Examples of papers examining tax reporting biases and the link to financial reporting include Burgstahler et al. (2006), Desai (2003), and Hanlon and Shevlin (2005).

more useful information for making decisions. For example, if a high-quality financial reporting system provides managers with more precise information about costs of production, they can better focus resource allocation decisions. As such, financial measurement and reporting can be a management practice akin to setting incentives or hiring high-quality labor.[3]

In this paper, we assess whether firms preparing high-quality financial reports verified by an independent auditor exhibit higher reported productivity compared to firms without external verification. We then attempt to discern whether these differences are explained by misreporting differences, actual productivity differences, or both. We use data from two independently compiled data sets to address this question. The first source of data is the comprehensive panel of tax returns for all private U.S. firms with at least $10 million of assets provided confidentially by the IRS. The second is a panel of small-to-medium sized privately held U.S. firms sourced from a wide array of accounting firms and compiled by the software and data analytics firm Sageworks. The datasets cover different time periods and firm sizes, and include different variables assuaging concerns about robustness and generalizability.

Using these data, we first confirm prior studies' findings of vast productivity heterogeneity across firms within well-defined industries with magnitudes similar to that of prior research.[4] We then measure financial reporting quality by coding the extent to which an external auditor verified the financial report.[5] We find that variation in financial reporting quality explains approximately 10% to 20% of the spread between the 10^{th} and the 90^{th} percentile of TFP, depending on the approach and data used. To put this into context, the explanatory magnitude of our financial reporting variable is similar in economic magnitude to other productivity drivers

---

[3] Examples of studies investigating the "real effects" of accounting include: McNichols and Stubben (2008), Cheng et al. (2013), Shroff (2017), and Harp and Barnes (2018). For discussions of the literature see Leuz and Wysocki (2016) and Roychowdhury et al. (2018).

[4] We use value added as our primary measure of firm output and, as such, our primary measure of productivity is "value added total factor productivity," or "TFP-VA." We discuss various issues with this measure and the alternative productivity measures we use in subsequent sections.

[5] The literature investigating the reporting quality improvements of auditing, similar to the biased reporting literature, is also large. See DeFond and Lennox (2014) for a review of this literature.

such as information technology, human capital, and management practices, which explain approximately 10 to 20% of the productivity dispersion (Bloom et al. 2018).

We run a battery of tests to ensure the robustness of the result and further understand why it manifests. First, we find similar inferences with alternative productivity measures, including gross production TFP (i.e., TFPR) and labor productivity.[6] Next, we ensure that observable differences across firms that also vary with reporting quality, such as size and external financing, do not explain the results (Rajan and Zingales 1998; Hsieh and Klenow 2009). While our main specification already accounts for the amount of inputs (e.g., levels of capital, labor, and materials), spatial differences (e.g., state fixed effects), and tightly specified industry characteristics and trends (4-digit NAICS by year fixed effects), we further account for differences in firm-level inputs by propensity-score matching on labor and capital within industry-year. The estimates are only slightly attenuated relative to our main results. Moreover, we create two-way sorts based on firms' level of debt and ownership dispersion and examine the relation between financial reporting and productivity *within each* of the partitions, with financial measurement quality continuing to explain differences in intra-industry productivity levels across firms. Thus, obvious observable differences in firm size and capital structure do not appear to explain the link between financial measurement quality and productivity.

We then consider two possible mechanisms for the link between productivity and investment in financial measurement quality. First, firms face an incentive to misreport their level of production to minimize taxes (e.g., Beck et. al. 2014; Balakrishnan et. al. 2018). External auditors, however, can reduce this misreporting (DeFond and Zhang 2014). Therefore, differential financial measurement quality could lead to heterogeneity in measured productivity because of differential firm-level reporting biases in administrative data sets. Exploiting

[6] We also consider production function estimates following the approach described in Ackerberg et al. (2015) to account for the endogenous link between productivity and management's choice of inputs and find similar inferences.

cross-sectional differences in incentives to bias reports based on state-level taxation differences, we find that the relation between reporting quality and productivity is approximately one-third lower in states with lower tax misreporting incentives (e.g., Texas) compared to states with higher misreporting incentives (e.g., California). This evidence suggests that one source of *measured* TFP dispersion using administrative data is differences in the amount of under-reported production which varies with the extent of external auditor engagement.[7]

We then consider the explanation that high-quality financial measurement is a management practice leading to higher *actual* productivity. The basic premise underlying our tests is that if better financial measurement provides better information, which in turn leads to better operating decisions, then we should find larger (smaller) productivity differences across report quality where measurement is more (less) important for management. One setting in which financial information may be particularly important is low margin industries where there is little room for managerial error. By contrast, firms in industries relying on innovation may have less scope for high-quality financial information with respect to their productivity. Consistent with these predictions, we find that the relation between financial measurement quality and productivity is higher in low margin industries (though statistically weak) and lower in industries focused on research and development. Second, consistent with research investigating learning-by-doing (Arrow 1962), we find that the relation between productivity and reporting quality attenuates with firm age. This is consistent with older firms benefiting less from outside auditors improving the precision of their financial reporting because of their accumulated knowledge.

We then examine time-series based tests to further support our inference that high-quality reporting is associated with higher *actual* productivity. Prior literature robustly finds that more productive firms are more likely to survive (e.g., Syverson 2011). We find that firms with more

[7] This is a complementary finding to the disciplining mechanism of tax audits on financial reporting (i.e., the reverse direction) as documented by Hoopes et al. (2012) and Hanlon et. al. (2014).

investment in financial measurement are significantly more likely to survive. Including both TFP and financial measurement quality in the same specification slightly attenuates both coefficients (consistent with a relation between the two), but both remain significant. The survival rate of firms with higher quality financial measurement is approximately 6.3 percentage points higher than those of low quality. We also compare *changes* in future productivity and growth and find that firms with better reporting measurement become more productive and grow faster, consistent with better managed and more productive firms attracting more resources.

As a final time-series test, we exploit the panel structure of the data to examine the pattern of measured productivity following a *change* in reporting quality regime. Under the assumption that an external audit cannot immediately improve a firm's *actual* productivity (because of a learning time lag), but can immediately reduce reporting *bias* (because auditor induced financial report adjustments immediately affect the financial report), a significant increase in measured TFP in the first year and little increase in the following year would suggest that the primary effect is bias reduction in *reported* productivity. On the other hand, if there is an increase in the second year of the audit engagement, this would be consistent with auditors facilitating an improvement in firm-level productivity through learning. Using a firm fixed effects design, we compare firms that increased their reporting quality in one year (i.e., engaged an external auditor) to firms which maintained a consistent reporting regime. We find an insignificant positive change in reported TFP in the first year of an audit (relative to the control firms without a reporting change), but a significant increase in TFP in the second year. While this test is subject to a several limitations, it is consistent with external auditing improving *actual* productivity by helping managers learn over time from higher quality financial measurement.

Collectively, we show that firms' investment in financial measurement practices are significantly related to firm-level productivity and highlight mechanisms for why this relation manifests. In doing so, our paper contributes to both the economics and accounting literatures.

First, we contribute to the the recent literature in economics which shows that management practices, such as hiring talent and providing incentives, can be viewed as a "technology" that enhances firm-level productivity (e.g., Bloom and Van Reenen 2007; Bloom et al. 2018). We extend this insight to a particularly important and widespread management practice: the firm's decision to produce, and have verified, high-quality financial statements.

Our findings also build on the recent accounting research linking reporting attributes to firm-level outcomes. For example, various papers link financial reporting quality to managerial decision-making and investment efficiency (e.g., McNichols and Stubben 2008; Cheng et al. 2013; Feng et al. 2015; Shroff 2017; Choi 2018). Miller et al. (2018) consider the attributes of the manager and find that entrepreneurs with accounting backgrounds start firms that are more likely to achieve profitability.[8] Our paper most closely relates to two recent papers which examine the link between accounting information and productivity dispersion. Breuer (2018) examines private European firms which are exposed to audit regulation requirements and finds a tighter left tail of the productivity distribution in industries were more firms are required by regulation to have a financial statement audit. Hann et al. (2018) analyze public U.S. firms and view reporting quality as an inherent industry-level characteristic. They find that productivity is more dispersed in industries with poorer accounting quality, inferring that better financial reports allow external financiers to better allocate their investments across firms. Our paper complements this literature by viewing accounting as a management practice which alters reported firm-level productivity. Importantly, our empirical approach also allows us to directly benchmark the economic magnitude of our results to that of the "management as a practice" literature.

It is also important to note what inferences cannot be made from our findings. Consistent with the recent literature associating management practices with productivity, we are not

[8] Another example of linking financial reporting information quality to decision-making is Gallemore and Labro (2015) who find that firms with higher internal information quality have higher tax efficiency (i.e., pay lower taxes).

identifying a treatment effect. Perhaps the most significant threat to a "treatment effect" inference that we are not able to assuage is that firms with high-quality measurement practices are also those with high-quality management practices along other dimensions, such as those identified by Bloom et al. (2018) — i.e., firms "bundle" the management practices. This possibility is intriguing because it reflects that high-productivity managers choose to expend resources on financial reporting practices, which may be otherwise simply viewed as an external communication mechanism. A related caveat — and consistent with the initial literature investigating other structured management practices — is that we do not investigate the question of why firms choose not to invest in improved financial measurement if it leads to enhanced productivity. Possibilities range from heterogeneous costs (and benefits — noting that we are only measuring potential reporting benefits for those firms choosing to invest in accounting practices) to competitive and behavioral explanations. We look forward to future research disentangling these explanations.

2. Financial reporting measurement and productivity.

2.1 Accounting rules and independent accountant attestation

Financial reporting has two broad dimensions: the set of accounting rules followed by the firm and the extent of independent accountant attestation (if any). Publicly traded firms in the U.S. are required by the SEC to obtain an audit and follow Generally Accepted Accounting Principles (GAAP). GAAP is an "accrual basis" of accounting wherein economic transactions can be realized and recorded prior to the receipt or payment of cash. Financial statements which contain significant accruals (and, thus, estimation) are subject to both estimation error and biased misreporting (e.g., Nikolaev 2017). To mitigate errors and bias in financial reports, managers (or owners and boards of directors) can choose to engage an independent accountant to verify the financial report prepared by managers, referred to as "attestation" (e.g., DeFond and Zhang 2014). During a financial statement audit, the independent accountant must collect evidence directly supporting the numbers reported by management in the financial statements. For example, accountants count inventory, observe property and equipment, and examine bank records for cash receipts from customers. Moreover, the independent accountant typically examines and tests the control systems firms use to record transactions and prepare the financial reports. For example, the accountants will examine how materials flow through the production process (i.e., are ordered, received, paid for, placed into production, and ultimately sold and delivered). Ultimately, the auditor assures that the financial statements present fairly, in all material respects, the financial position of the company and the results of the operations.

In this paper, we exploit the fact that non-listed firms can choose to follow any set of accounting rules as well as whether to obtain attestation (e.g., Allee and Yohn 2009; Lisowsky and Minnis 2018).[9] While we consider the combination of audited GAAP-basis financial statements as the highest quality of financial measurement, in the paper we use variation in attestation among private firms to examine the role of financial reporting and productivity for two main reasons. First, practically speaking, most substantive firms in the economy claim to follow GAAP-basis (or at least accrual basis) accounting. As such, the firms in our data sets overwhelmingly report to use similar accounting standards. It is the extent to which they *actually* follow them (as measured through the presence of attestation engagements) which produces the interesting variation. Figure 1, shows the substantial variation in the percentage of firms that engage an auditor both within as well as between industries with attestation rates per sector ranging between 20 to 60 percent of firms. Second, we rely on the financial reports themselves to measure productivity. Differential accounting standards could result in mechanical differences in measured productivity in our data. Moreover, given that attestation engagements are rare for

[9] See Appendix A for a description of the various sets of accounting rules as well as information on attestation.

firms following standards other than GAAP, these observations do not have much variation along the attestation dimension.[10]

Finally, while much literature motivates firms' reporting choices from the perspective of agency problems between the firm and shareholders or debtholders, descriptive evidence provides support for other determinants for firms' reporting choices. Figure 2 shows the amount of variation in firms' choice of an audit after conditioning on size, ownership, and debt. We find that 40% of firms with no debt and low ownership obtain audited financial statements. More strikingly is the fact that after conditioning on some debt and larger ownership dispersion the audited GAAP rate increase to over 50%. Moreover, we find that many firms with millions of dollars in external debt and large ownership dispersion still do not produce audited GAAP statements. These statistics reflect that agency problems and access to capital are not necessary conditions for private firms to engage in high-quality financial reporting and, more to this study's point, the potential for productivity enhancement to play a role in its adoption.

2.2 Linking financial measurement to productivity

We begin by discussing the measurement of productivity and then link financial reporting quality to productivity. As standard in the literature, we assume firm-level production follows a Cobb-Douglas production function:

$$Y_{it} = A_{it} K_{it}^{\alpha} L_{it}^{\beta} \qquad (1),$$

where Y is output measured as annual value added (sales less material inputs), K is the firm's capital stock, L is the annual labor inputs, and A is the (latent) total factor productivity (TFP)

[10] Because we do not exploit differential accounting standards, we think even more productivity dispersion can be explained by examining differences in reporting standards across firms with the availability of new data sources from the census.

term. TFP is then estimated as the residual after regressing the log of value added on the log of the input factors:[11]

$$\log\widehat{A_{it}} = \log Y_{it} - \widehat{\alpha}\log K_{it} - \widehat{\beta}\log L_{it} \qquad (2).$$

Estimated TFP measures Hicksian-factor neutral productivity differences, which would include, for example, differences in management practices (Bloom and Van Reenen 2007) or frictions in the capital markets (Hsieh and Klenow 2009).[12] We posit that financial reporting practices create differences in reported TFP for at least two reasons.

The first is a relatively straight-forward measurement explanation: high-quality auditing can attenuate the error and bias reported by firms in administrative data sets. By some estimates, potentially half of measured intra-industry TFP dispersion is the result of problems measuring inputs and outputs (Bloom and Van Reenen 2007). Given that the level of financial report attestation varies significantly across firms — even within well-defined industries — and research shows that financial misreporting decreases with the level of attestation, then a portion of TFP measured using administrative datasets could be the result of different levels of bias and noise mitigated by auditors.[13] For example, in the context of the production model above, firms which under-report production will show up in analyses as having low productivity. If some firms have auditors which force them to fully report their production and others do not, then this reporting difference generates heterogeneity in measured within-industry TFP.

While the notion that auditors reduce measurement issues in administrative data sets is perhaps an unsurprising prediction, the second explanation for a link between independent

[11] We also include both industry by year and geographic fixed-effects to account for industry, spatial, or temporal differences. Therefore, we do not adjust for industry-level input or output prices because this adjustment would be redundant with the inclusion of fixed effects.
[12] However, as has been discussed in the literature (e.g., Syverson 2011; Syverson 2018), because TFP is estimated as the residual of the production function, it literally captures anything not accounted for by the explicit inputs measured.
[13] For example, using variation in the level of attestation, Minnis (2011) finds that accruals are more predictive of future cash flows for firms with audits compared to firms with reviews or compilations.

accountant attestation and firm-level productivity is that auditing improves the *actual*, not just *reported*, level of productivity. In other words, the engagement of an independent auditor is a management practice technology akin to human resource policies, goal setting, or inventory management. Higher quality financial measurement and processes lead to better managerial information, which in turn lead to better decisions.

Under an information perspective, David et al. (2016) link imperfect information to resource misallocation and differentials in productivity. In a frictionless market, the optimal allocation of input factors across productive units requires the equalization of marginal products. Deviations from this outcome represent a misallocation of resources and translate into suboptimal aggregate outcomes and lower productivity. At a micro level, when a firm chooses inputs under limited information about their idiosyncratic fundamentals, the informational frictions in the firm lead to a misallocation of factors.[14] Under this framework, we can think of the firm facing a learning problem. While the firm and managers can learn from a variety of sources, the measured internal information is a prominent source of information for managers.[15]

Auditing does more than assure the quality of the financial report. Auditing procedures examine firms' controls and procedures. For example, they test for the existence and operational integrity of physical capital. They examine human resources and payroll policies to mitigate fraudulent (i.e., nonexistent) employees. Moreover, they provide advice about weaknesses in production processes such as inventory controls. Thus, the financial reporting system can be

[14] At a macro level misallocation across firms in an ex-post sense reduces aggregate productivity and output. The size of this misallocation is a function of the residual uncertainty at the time of the input choice, which is a function of the volatility of the fundamental shocks and the quality of information at the firm level (David, Hopenhayn, and Venkateswaran 2016).

[15] This decision theoretic analysis of information has a long history in economics and accounting (see Pratt, Raiffa, and Schlaifer 1965; Feltham 1968; Feltham and Demski 1970; Demski 1972). Moreover, research in accounting has shown that changes in accounting standards shape changes in firms behavior. For example, Amir and Benartzi (1999) suggest that firms avoid the recognition of an additional pension liability under SFAS 87 by reducing the volatility of pension assets. Amir et al. (2010) provide evidence consistent with firms changing their pension asset allocations to mitigate expected equity volatility from pension accounting changes in the U.K. and U.S. Shroff (2017) shows that changes in accounting standards may cause firms to learn new information from their internal systems, which in turn leads to higher investment efficiency.

considered an integral and essential component of the economic environment that determines how firms and managers allocate resources.[16]

A causal link between high-quality reporting and productivity is not a certainty, however. A plausible alternative view is that the primary purpose of audited financial reports is to serve merely as a financial communication designed for external users and not to facilitate internal decision-making. For example, a primary stated objective of GAAP is to provide investors with information about a firm's future cash flows to facilitate *external* investment decisions. The orientation of auditing and GAAP, therefore, is about improving the external information environment.

3. Data

We use two independently collected, proprietary panel data sets of private U.S. firms. The private U.S. firm setting has two advantages. First, these firms generally lack financial reporting mandates, making firm-level accounting policies a management practice with heterogeneity. Second, this setting includes the overwhelming majority of U.S. firms (99% of U.S. firms are private) and they control trillions of dollars in capital. Moreover, this setting does not simply contain small businesses — for example, there are three times as many firms exceeding $100 million in sales that are private compared to those that are public (See Allee and Yohn 2009; Minnis 2011; Lisowsky and Minnis 2018). We will now describe the two data sets and their relative advantages.

[16] Prior research finds that the management accounting systems that are used for internal decision making are closely linked to the financial accounting systems that are used for external reporting (Kaplan 1984; Dichev et al. 2013). Recent studies have begun to examine the extent to which managers act on faulty information as a result of their own earnings management decisions or ineffective internal controls over the financial reporting.These studies find that misreporting and deficiencies in the internal controls leads to inefficient investment (e.g., McNichols and Stubben 2008; Cheng, Dhaliwal, and Zhang 2013) and performance (Feng et al. 2015).

Our first data set is a comprehensive panel data set of all business tax returns for the years 2008 to 2010 for firms with at least $10 million in assets.[17] The data set was provided confidentially by the IRS and includes all filings for C-corporations, S-Corporations, partnerships, and limited liability companies. The fields in this data set include income and expense line items (i.e., "page 1" items used to calculate operating income) as well as balance sheet line items from Schedule L and the firm's NAICS industry code. Importantly for our study, the IRS forms also require firms to reveal two characteristics of their financial reporting system: the set of accounting standards the firm uses and whether the firm had its financial statements audited by an independent accountant.

The second data set we use was provided by Sageworks, Inc., a data and analytics company offering cloud-based services to accounting firms. Sageworks compiles firm-level data entered by accounting firms as the accounting firms conduct work for their clients. Sageworks provided us with the underlying firm-level data set for the years 2002 to 2008. The private firms in the data set are anonymized, but Sageworks provided identifying codes allowing us to track firms year-to-year. For each firm-year, the data set includes fields for income statement and balance sheet line items, the number of employees, an NAICS industry code, U.S. state of location, anonymized accounting firm identifier, and, importantly for this study, the extent of financial statement verification provided by the accounting firm. The data set also provides a broad categorization of accounting standards but does not provide the specific set of standards followed. Therefore, we follow Minnis (2011) and only use firms which follow an "accrual" basis of accounting.

We conduct analyses using both data sets because they offer relative advantages and complementarities ensuring the robustness and generalizability of our results. The IRS data set is

[17] The IRS data set starts in 2008 because this is the first year the IRS began asking firms to provide the set of standards followed and whether the financial statements were audited. The data set ends in 2010 because this was the last year provided by the IRS. See Lisowsky and Minnis (2018) for additional detail.

focused on medium-to-larger firms (those with at least $10 million in assets), while the Sageworks data set contains mostly smaller firms (the vast majority have less than $10 million in assets). While the Sageworks data set reports the number of employees, the IRS data set does not, so we measure labor inputs using the wage bill for the IRS analyses. Moreover, Sageworks provides the state of location for each firm, whereas the IRS did not provide us with firms' location data. The data sets also cover different time periods: our Sageworks panel covers 2002 to 2008; while the IRS data set covers 2008 to 2010. In addition, while the Sageworks data set is sourced from accounting firms which have opted to be customers of Sageworks (and therefore one may be concerned about participation bias), the IRS data set is a comprehensive set of filings, minimizing participation bias and allowing us to track the survival of firms from one year to the next with minimal error.[18] A final difference between the two data sets is the financial measurement quality specification we use. The IRS only asks firms if their financial statements are audited by an independent accountant; whereas the Sageworks data set records whether firms' financial statements are audited, reviewed, or compiled. (Recall the differences in these report types from Section 2.) Therefore, report quality is a binary variable for the IRS analyses and a count variable for Sageworks analyses.[19]

Table 1 presents the distribution of firms years across NAICS sectors for both data sets. The distributions are relatively similar across industries with slight differences in Construction and Retail trade. Compared to public firms based on Compustat data, private firms in both data sets are less focused in Manufacturing industries and more focused in Construction and Wholesale trade (see Lisowsky and Minnis 2018).

[18] In the Sageworks data set, measuring firm survival is considerably more noisy as firms could drop out of the data set for reasons other than death. In particular, accounting firms may choose to cease subscribing to Sageworks or, even if they continue to subscribe, may choose to stop entering a particular firm in the data set. One additional issue with the IRS data set is that the accounting variables can only be measured for the firms which "e-file" their return. Therefore, while we can track the existence of firms regardless of filing type, we can only measure the accounting choices of e-filing firms.

[19] All inferences and results are similar if we instead code the Sageworks data using a binary variable for whether the firm is audited.

In both data sets, we measure output as the log value added (*ln(va)*) calculated as the log of sales minus the cost of goods sold; capital as the log of total property, plant, and equipment (*ln(ppent)*); and materials as the log of cost of goods sold (*ln(cogs)*). In the analyses using the IRS (Sageworks) data, we measure labor (*ln(labor)*) as the log of the wage bill (number of employees).[20] Table 2 presents the descriptive statistics for these variables and reveals several aspects of the data. First, by construction, firms in the IRS data set are larger, on average, compared to the Sageworks data set. Second, firms with better reporting and financial measurement practices are larger, on average, than firms with lower quality measurement practices. However, while there are differences in size conditional on report type, there is also significant common support across the distributions — i.e., there are many large and small firms choosing each of the quality levels of financial measurement.[21] Nevertheless, we further consider these size differences in robustness analyses below. We also note from Table 2 that the majority of firms choose lower quality financial measurement practices, consistent with the accounting literature investigating these choices (e.g., Lisowsky and Minnis 2018).

[20] There are a few technical notes regarding the measurement of various inputs and outputs. First, using the wage bill to measure labor carries the issue of mixing the price and quantity of labor. To address this issue, the literature often indexes the wage bill by standard industry labor rates. We do not do this because we instead use industry by year fixed effects. Second, for the Sageworks data, we have sufficient years to regress revenues on *lagged* labor and capital to estimate TFP. Because the IRS panel is limited, we use *lagged* capital (because the tax return includes balance sheet information from t-1), but *contemporaneous* labor and materials. Finally, in neither data set does the cost of goods sold perfectly represent "materials" used in production because direct labor or capital used in the production process could also be included in this figure. Moreover, for the Sageworks-based analysis, cost of goods sold likely double-counts some figures (e.g., includes the number of employees, but also includes employee wages in cost of goods sold, which we are unable to disentangle); however, because the tax forms explicitly ask for cost of goods sold and wages separately — and therefore, these figures are only included in one place on the tax form — double-counting of inputs is not an issue in the IRS-based analyses. Simply omitting cost of goods sold from any of our analyses does not change our inferences, and generally increases the economic magnitudes of the results.

[21] To further ensure common support, we have truncated the distributions of both data sets based on size. In the IRS (Sageworks) data set, we require a minimum of $10 million ($500k) and a maximum of $1 billion ($250 million) in assets.

4. Results

4.1 Relation between reporting quality and productivity

We begin by estimating TFP in Table 3. We use logged value added as the dependent variable, so we are estimating value added TFP (i.e., TFP-VA). Columns 1 & 2 (3 & 4) reports the results for the IRS (Sageworks) sample. Both sets of analyses include 4-digit NAICS industry by year fixed effects and the Sageworks regressions also include the state of firm location fixed effects. Columns 2 & 4 are estimated on propensity matched samples where matching is done on the main set of variables (i.e., *ln(labor)* and *ln(ppe)*) within industry years. The reported coefficients represent the elasticities of production for labor and capital, which are similar in magnitude to those estimated in Bloom and Van Reenen (2007) and Hsieh and Klenow (2009). The residuals from these regressions are the estimated firm-level logged TFP-VA.[22]

As an initial assessment of the relation between TFP and financial measurement quality, Figure 1 plots the TFP distribution conditional on report type using the Sageworks data. Consistent with the hypothesis that higher quality measurement is associated with higher productivity, we find that TFP increases as financial measurement quality increases. This result holds not only at the mean but across the TFP-VA distribution. Additionally, we note that the variance of TFP-VA is not lower for higher reporting practices. Instead, there is a variance preserving rightward shift in the distribution.[23] This illustrates the primary finding of the paper: firms with better measurement systems are more productive.

[22] We also conduct our analyses in "one-stage" wherein we include the report variable directly in the production function estimation in Table 3. Not surprisingly, this leads to nearly identical inferences. Moreover, in additional supplementary analyses, we then fully interact the report variable with labor and capital to assess whether the elasticities of production differ conditional on report type. We find that the estimated coefficients on labor and capital do not differ across report types. We tentatively infer that this suggests that financial measurement is a Hicks-neutral outward shift in productivity.

[23] A conceptual concern with testing our hypothesis is that we are better able to measure TFP for firms with better measurement, which makes testing differences in TFP across firms with differing levels of measurement potentially problematic. However, our prediction is directional: firms with better measurement have higher TFP and more measurement should induce noise, not directional bias.

We formally test this result in Table 4. We regress estimated TFP-VA on our measure of financial report quality. Standard errors are clustered at the industry by year level. For the IRS sample, our variable of interest, *report,* equals 1 if the firm follows GAAP and receives a financial statement audit and 0 otherwise. Column 1 reports a significant coefficient of 0.108 on our *report* measure, which explains approximately 8.4% of the 10/90 unconditional TFP-VA spread in the IRS data set. For the Sageworks sample, *report* is an ordered variable equal to 0 (0.5, 1) for firms with compilations (reviews, audits). Column 3 reports a significant coefficient of 0.310, which indicates that going from a compilation to an audit is associated with a 20% increase in TFP-VA relative to the unconditional spread between the 10th and 90th percentile of TFP. To benchmark these magnitudes, Bloom et al. (2018) report that their 16-dimensional management score explains approximately 18% of the 10/90 TFP spread in U.S. firms. Using an 18 question management survey across four countries, Bloom and Van Reenen (2007) finds that management practices explain approximately 12% of the interquartile spread in TFP.[24] Thus, the explanatory power of our financial measurement quality variable is consistent with the findings using detailed management scores.

We next consider several robustness analyses. First, we consider two important observable differences across firms: size and access to capital markets. Regarding size, we note from Table 2 that while there is common support in the distribution of firm size (i.e., there are both very large and very small firms in each of the high and low-quality reporting buckets), higher financial measurement quality is also associated with larger firm sizes, on average. Thus, a natural concern is that size may be spuriously driving our results in the following way: larger firms are both more productive and have higher agency problems. If independent financial report verification is more likely to be used in the presence of agency problems, then the relation

[24] Because of differences in data availability and measurement, the comparisons across papers are not perfect. The closest comparison of economic magnitudes between management practices and financial reporting is our IRS analysis (Table 4, column 1) with Bloom and Van Reenen (2007) Table 1, column 2. Both use sales as the measure of production and include labor, capital, and materials as factors, along with time and 6-digit industry fixed effects.

between measurement quality and productivity is not the result of financial measurement quality, but rather spuriously driven through firm size.[25] Regarding capital market access, differential access to capital markets is a standard friction in productivity models predicting heterogeneity in productivity (e.g., Hsieh and Klenow 2009; Rajan and Zingales 1998). Firms with better access to capital markets can purchase more productive capital leading to observed heterogeneity across firms. This could be problematic for inferences in our setting because a standard reason for improving financial measurement (e.g., engage an auditor) is to access external capital. Therefore, our financial measurement variable could simply be identifying differential capital market access.

We address differences in firm size and capital market access across reporting quality in two ways. First, we propensity score match firms based on the level of inputs.[26] We force a match within 2-digit industry by year and require a caliper of 0.03 without replacement. Table 4, columns 2 and 4 report the results after including only those propensity-matched observations. The results are only slightly attenuated from those in columns 1 and 3. Second, we consider differences in capital structure across firms. In Table 5 we two-way sort firms in the IRS sample based on leverage and ownership dispersion.[27] In each cell, we report the sample size and portion of firms in the cell receiving a GAAP audit. We also re-estimate the Table 4, column 1 regression within partition and report the coefficient on *report*. The concern we are attempting to mitigate is that the productivity benefits of higher quality financial measurement are derived only

[25] We note that a prediction from models of productivity is that better managed firms attract more resources, grow more quickly, and therefore, are predicted to be larger than poorly managed firms (e.g., Syverson 2011; Bloom, Sadun, and Van Reenen 2017). Therefore, the amount of resources controlled (i.e., firm size) has been used as an outcome variable rather than a control variable.

[26] Specifically, in the Sageworks data we create a variable comp which equals 1 if the firm receives a compilation (the lowest report quality level) and 0 otherwise (effectively grouping observations receiving reviews and audits).Using comp as the dependent variable essentially results in mostly compilation to review comparisons because of the smaller number of audit observations. This also results in matches of the smaller firms in the sample. If we alternatively use an indicator for the firm receiving an audit as the dependent variable (which groups compilations and reviews) we have a much smaller sample (because of the smaller number of audit observations) but the results — both economically and statistically — remain unchanged.

[27] We cannot do this in the Sageworks data because we do not have ownership data.

from a capital market channel (i.e., those with better reporting have better access to capital markets, and thus, more access to productive inputs). Table 5 shows that the relation between financial measurement quality and productivity is positive across all cells (marginally insignificant in the upper left cell) and in fact becomes somewhat more positive when controlling for capital market access (e.g., the largest magnitude is the cell with the most leverage and ownership dispersion). While Table 5 is subject to the caveat that both capital structure and reporting quality are both equilibrium outcomes (and we do not examine the differences across the cells in Table 5), these results suggest that differences in capital structure do not explain our results.

Finally, we consider the generalizability of our results across industries. The analyses thus far include 4-digit industry by year fixed effects, but do not allow either the factor elasticities or the relation between reporting and productivity to differ across industries. Therefore, we re-estimate the Table 3 and Table 4 regressions for each sector reported in Table 1 with at least 500 observations (still including 4-digit industry by year fixed effects). Figures 3a and 3b plot the results. The figures show a persistent positive relation between TFP-VA and reporting measurement quality in each of the sectors in both data sets. Figure 4 reports the fraction of intra-industry 10/90 TFP dispersion explained by reporting measurement quality. We find statistically and economically significant results across all industries; thus, our results are not specific to a particular industry despite the heterogeneity. Nevertheless, the results in Figure 4 suggest potentially interesting variation across industries, which we will exploit in the next section.

To this point, we have shown that financial measurement reporting quality is strongly associated with firm-level productivity, of similar magnitude to other structured management practices; that differences in size or access to capital do not appear to explain the results; and that the association is persistent across industries, but that there are economically important differences across industries. We now turn to exploring mechanisms for these results.

4.2 Measurement bias and reporting as a technology

We examine the evidence for two possible explanations for a relation between financial reporting quality and productivity: biased differences in *reported* production and *actual* differences in the level of productivity.

4.2.1 Tax incentives to misreport

One explanation for the finding that higher reporting quality is associated with higher productivity is that firms with lower investment in reporting quality under-report their level of production.[28] Firms have incentives to under-report production to reduce tax liabilities and one role of external verification by independent auditors is to ensure managers do not bias the reported financial results (e.g., Coppens and Peek 2005; Burgstahler et al. 2006; Beck et. al. 2014; Hanlon et. al. 2014). If financial statement verification reduces management's under-reporting bias, then this could generate a positive relation between financial measurement quality and productivity. To examine this possibility, we exploit differences in incentives for under-reporting production across states driven by significantly different tax structures. For example, in 2008 the top state corporate tax rate was 12%, while at the same time four states had a 0% tax rate. Figure 5 demonstrates the extent of variation in corporate income tax rates across states in 2008. In short, incentives to under-report production (i.e., sales) differ significantly across states.

In Table 6, we examine how external verification of financial reporting attenuates cross-sectional incentives to under-report production. In column 1 we re-estimate the results

[28] While reduced measurement noise for inputs and outputs can reduce the dispersion in reported productivity, it would not generate a significantly positive relation between reporting quality and productivity. Also note that we are investigating the link between financial report auditing and tax misreporting. By contrast, several papers in accounting research have investigated the link how tax-related auditing (i.e., audits by the tax authorities) potentially mitigate financial misreporting (e.g., Hoopes et al. 2012; Hanlon et al. 2014).

from Table 4, column 3 (we do not have the state of location in the IRS data, so we can only use the Sageworks data for this analysis) but add an interaction term crossing the *high_corp* variable with *report*. The variable *high_corp* is an indicator equal to 1 if the state's corporate tax rate is above the median of all states and 0 otherwise.[29] For this analysis, we cluster the standard errors at the state level because the cross-sectional variable varies by state. The coefficient on the two-way interaction is significantly positive and about one-half the size of the coefficient on *report* indicating that external report verification is particularly important in states with high incentives for firms to under-report production. If the state and industry by year fixed effects identify all other differences across states, then this result highlights that at least one role of external verification is to mitigate reported production differences in administrative data sets.[30]

The results in column 1 of Table 6 rely on corporate tax rates, however, measuring the extent to which state taxes affect firms is not straightforward for several reasons. First, state taxation regimes are heterogeneous. Some states have standard corporate income tax rates based on the federal system, while others have taxation based on gross receipts. Still, others have hybrid approaches. Second, firm legal structures differ. Entities such as partnerships, limited liability companies, and S-corporations are so-called "pass-through" entities and taxes are assessed at the partner or owner level, not at the business level. As a result, the personal tax system could be quite relevant for assessing incentives to misreport rather than the corporate system. Still, other complications such as nexus, sales taxes, and certain exceptions to tax rules add further complexity. Therefore, to provide a robustness check to the results in column 1, we use summary tax data compiled by the U.S. Census which standardizes state taxes into five major categories (property, sales and gross receipts, licenses, income, and other) and then rank

[29] The median corporate tax rate during this time period was approximately 7%. Using the continuous state corporate tax rates rather than the median provides the same inferences. Note also that the main effect of *high_corp* is absorbed into the state fixed effect.

[30] In untabulated results, we find that this effect attenuates with firm size, consistent with the finding that smaller firms have higher tax avoidance on a percentage basis and that auditing is particularly effective for these firms.

them on a per capita basis by state. To operationalize this data for our purposes, we sum the "sales and gross receipts" and the "income" categories (per capita) and rank the 50 states. Our second measure of tax incentives to misreport is then a variable coded 1 (0.5, 0) for states in the top 10 (middle 30, bottom 10) of the per capita tax collection.[31] Column 2 shows that the results using this alternative measure are consistent with the results in column 1. In sum, the results suggest that one mechanism for the association between higher financial measurement quality and reported productivity is a reduction of production reporting bias.

4.2.2 Financial measurement quality as a management practice technology

A second explanation for the positive relation between financial measurement quality and firm-level productivity is that the external verification improves the productivity of the firm — i.e., the processes and information inside the firm are better because of the auditor's involvement. Ideally, to test this hypothesis, we would randomly assign treatment to firms; unfortunately, similar to much of the literature studying management practices (with the notable exception of Bloom et al. 2013) we are unable to do so, so we approach this issue using both cross-sectional and time-series based tests. Of course, these results remain suggestive and not causal.

Our cross-sectional tests follow one of two basic logical paths. First, if better financial measurement improves information for managers to make better productivity-related decisions, then our results should be stronger in situations in which better measurement is more important. We consider two settings in which the relative value of financial measurement might differ. Firms competing in industries with low margins — typically characterized by high competition — have little room for error regarding production decisions, and thus high-quality financial information and processes could be particularly relevant. By contrast, firms in industries reliant

[31] As an example of how this measure correlates with tax rates, note that California (with high personal and corporate tax rates) is coded as a "1", whereas Texas (with low personal and corporate tax rates) is coded as 0.

on innovation (i.e., developing new products through creativity and research) may not benefit from high-quality financial measurement for productivity purposes.

The second logical path for our cross-sectional tests is if firms have opportunities to gather information from their own experiences over time (i.e., learn) then this learning attenuates the relative importance of financial measurement quality for productivity. We measure a firm's "learning from itself" by its age. Older firms likely have more established processes; whereas newly established firms could benefit from the insights of auditors reviewing their processes which are still being established.

We examine these cross-sectional predictions in Table 7. In the first two columns, we use data from Compustat to construct the industry-based cross-sectional variables and industries are defined at the 3-digit NAICS level.[32] The cross-sectional variables in the first two columns are measured in deciles scaled to the interval [0,1] to facilitate interpretation of the coefficient to be a comparison between the top and bottom decile. The main effects of the cross-sectional variables are absorbed in the industry by year fixed effects. Column 1 measures margins at the industry median and is measured to be consistent with the Lerner index approach (i.e., 1 minus the gross margin such that the variable is increasing in lower margins). We find that the relation between financial measurement and productivity is higher in low-margin industries, but this result is not significant at the 10% level. In column 2, we measure the R&D intensity as the industry median level of R&D scaled by sales. We find that the ability for financial measurement quality to explain productivity dispersion is lower in high innovation industries. Finally, in column 3, we exploit the year the firm was founded on the corporate tax forms to measure firm

[32] We use Compustat data to construct the industry based cross sectional variables in Table 8 for two reasons. First, it allows us to use data "outside the system" to mitigate any mechanical or endogenous link between financial measurement and profitability levels. Second, R&D is not reported in either the IRS or Sageworks data sets. We define industry at the 3-digit level to consider the trade-off between a sufficient number of observations within each industry, while at the same time recognizing that industries can be very different at high levels of aggregation. We cluster standard errors at the 3-digit level to address the fact that variation in the cross sectional variables occurs at this level.

age. We create an indicator *young_age* which equals 1 for firms less than 4 years old.[33] We find that the interaction between *young_age* and *report* is significantly positive, supporting the idea that firms' processes become more efficient as they learn, muting the need for outside counsel. The cross-sectional tests of Table 7 thus reveal that financial measurement quality behaves consistently with a management practice technology.

To provide additional evidence consistent with financial measurement as a productivity-enhancing management technology, we exploit the panel structure of the IRS data set and examine several time-series tests. We first consider firm survival. A consistent finding in the economics literature is that productivity is strongly associated with firm survival. We revisit these results and examine whether financial measurement is associated with survival. For the IRS sample, we have the complete set of IRS tax returns, so *survive* is coded as 1 if the firm continues to file tax returns and 0 if not. In the main analysis, we use the year 2008 as the base year, and measure *survive* in 2010, but the results are virtually identical if we look just one year ahead.

In Table 8, we begin by replicating the result that TFP predicts survival. Column 1 reveals that our firm-level estimates of TFP-VA are strongly associated with firm survival. Given the consistent result in the literature, this validates our approach. In column 2 we then show that financial measurement quality is also associated with firms survival. Finally, column 3 includes both TFP-VA and financial measurement variables and the coefficients on both remain significant, though both are attenuated. We observe a 6.3% higher likelihood of survival for firms with high report quality compared to those with low report quality. We benchmark this estimate two ways. First, we note that this magnitude is slightly smaller than going from the 10th percentile to the 90th in TFP, which results in an 9% increase in the probability of surviving

[33] The sample size is smaller in columns 1 and 2 of Table 7 because we are only able to use data from corporations filing form 1120 (or 1120S). The year founded was not provided to us for other entity types. We use "less than 4 years old" as the indicator for young firm because Lisowsky and Minnis (2018) find that it is around this age where the relation between age and propensity to have audited GAAP financial statements begins to flatten significantly.

from 2008 to 2010 (0.070 x 1.284 = 0.09). Second, we also compare the magnitude to that found in Bloom et al. (2018) using their management practice survey. They find that a one standard deviation increase in the management score explains approximately 22% of the unconditional exit rate of their sample (their estimated coefficient of 0.153 times a one standard deviation in the management score of 0.172 divided by an unconditional exit rate of 11.8%, see Table 3). The unconditional exit rate in the IRS data from 2008 to 2010 is 24%, indicating that high financial measurement quality explains approximately 26% of this rate, similar to Bloom et al. (2018).

In columns 4 - 5 we examine the relation between reporting quality and survival after conditioning on firm size. Specifically, we split the sample based on median sales size and continue to find positive survival effects from reporting quality, with the effect being larger for smaller firms. In columns 6 - 7, we look at changes in firm-level performance, conditional on survival to 2010 and the financial measurement quality and level of performance in 2008. We find that firms with high quality financial measurement increase their productivity and sales. To be clear, similar to the cross-sectional tests these tests still do not establish causation. For example, one could be concerned that firms anticipating future growth are those that engage an independent accountant ex-ante (e.g., to attract external capital to facilitate the growth), resulting in an endogenous positive association between financial measurement quality and growth. At this point, we can simply say that our results are consistent with prior literature investigating management practices and are consistent with financial measurement quality facilitating higher firm performance.

We conduct one final changes analysis to assess potential mechanisms. Recall that there are two channels through which we suggest independent accountants can affect measured TFP heterogeneity: they reduce bias in the financial report (i.e., improve *reported* productivity) and they make the financial system more informative (i.e., improve *actual* productivity). These channels are not mutually exclusive and, indeed, our evidence so far is consistent with both.

However, one way to potentially disentangle these explanations is to observe what happens when when firms first engage an independent accountant. If the primary channel is through a reduction in bias (or other changes in the reported numbers), then these effects should arise in the first year. If the effects are primarily related to improving actual productivity, then these effects likely take time.[34]

In column 8 of Table 8, we restrict the sample to firms that exist in all three years of the panel and either (i) receive audited GAAP statements all three years; (ii) do not receive audited GAAP statements in any of the three years; or (iii) change from not receiving audited GAAP statements in 2008 to doing so in 2009 and continuing to do so in 2010. We then regress the level of TFP-VA for all years on firm fixed effects plus indicators for whether the firm initiated a GAAP audit. The coefficient on *start_gaap_audit_2009* reveals the change in TFP in the first year of the audit; and the coefficient on *start_gaap_audit_2010* reveals the incremental change in TFP-VA in 2010. The coefficient on *start_gaap_audit_2009* is positive but small and insignificant, suggesting little evidence of auditors substantially adjusting the reports for under-reporting bias in these firms. However, the coefficient on *start_gaap_audit_2010* is larger and significant. Therefore, compared to firms that did not change their financial measurement quality, those that improved their financial measurement quality had an increase in TFP in the second year of the engagement, but not the first, supporting a learning channel. This test remains subject to a variety of concerns (e.g., if in the first year of an audit, auditors are just as likely to be reducing upward bias in financial reports as downward bias, then this test does not rule out the reported productivity channel), but the time series evidence is consistent with a learning channel.

[34] An auditor does much of the audit-related work after a firm's fiscal year has already ended, therefore, they have the ability to affect the numbers as they are reported, but likely do not have an opportunity to affect actual productivity in the first year.

5. Discussion and conclusion

Understanding the vast heterogeneity in reported firm-level productivity is an important recent topic of interest. In this paper, we show that differential investments in firms' financial reporting measurement – i.e., information that not only informs management but also feeds into administrative data sets used by researchers – explain an economically large portion of this heterogeneity. Specifically, we find that firms engaging independent accountants, whose role is to examine firms' internal controls and provide assurance that the reported numbers materially represent the underlying economics of the firm, have significantly higher levels of reported productivity, survival, and growth and the magnitudes of these findings are consistent with research investigating a wide array of management practices.

We present evidence of two mechanisms for this relation. First, we show that auditors reduce bias in the financial reports. In particular, firms have incentives to report downward biased production figures to reduce tax burdens. Using plausibly exogenous cross-sectional variation in tax-based incentives across U.S. states, we show that outside auditors attenuate the downward bias of their audit clients, generating reported intra-industry productivity heterogeneity in administrative data sets. This result is particularly acute in smaller firms consistent with the tax literature finding that the majority of the "tax gap" is caused by small firms.

Second, we find results consistent with improved financial measurement improving actual firm-level productivity. That is, engaging an outside auditor is akin to a management practice or a "technology" which creates a Hicks-neutral outward shift in the level of productivity. Using cross-sectional tests, we show that the relation between auditing and productivity is stronger (weaker) where information precision is stronger (weaker): firms competing in low margin (high innovation) industries. Moreover, we find that learning by the firm moderates the auditing-productivity relation, further suggesting that the relation manifests through an information channel.

While we show that a straightforward characterization of a firms' reporting system explains nearly as much variation in intra-industry productivity as more detailed management practices used in studies such as Bloom and Van Reenen (2007) and Bloom et al. (2018), we are cautious that, much like their findings, we have not directly identified a causal mechanism linking higher financial measurement quality to higher productivity. Moreover, a quite relevant threat to our inferences is that we are merely identifying those firms with "good" management practices along the very same dimensions as those studies. Nevertheless, our results are intriguing for at least two reasons. First, auditing is often viewed as a practice required of firms with agency problems. However, should our results simply be explained away by "good" management practices, this suggests that high-quality firms are more likely to engage auditors. Second, our approach to measuring productivity is parsimonious and inexpensive to assimilate in administrative data sets. Merging this variable into other confidential governmental data sets such as the Census data set would allow for historical panel-level analyses and would not require teams of surveys.

Finally, our characterization of financial measurement is but one aspect of a firm's reporting system which may have systematic effects on measured productivity. Firms can account for the same economic transaction in a variety of ways which, in turn, would result in cross-sectional differences in productivity. At a minimum, our results suggest a more thorough accounting of firms' accounting would bear fruit in understanding dispersion in TFP.

References

Ackerberg, D., K. Caves, and G. Frazer. 2015. "Identification Properties of Recent Production Function Estimators." *Econometrica* 83: 2411-2451.

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

American Institute of Certified Public Accountants (AICPA). 2016. "Codification of Statements on Standards for Accounting and Review Services." Wiley.

Amir, E., and S. Benartzi. 1999. "Accounting Recognition and the Determinants of Pension Asset Allocation." *Journal of Accounting, Auditing & Finance* 14 (3): 321–43.

Amir, E., Y. Guan, and D. Oswald. 2010. "The Effect of Pension Accounting on Corporate Pension Asset Allocation." *Review of Accounting Studies* 15 (2): 345–66.

Arrow, K. 1962. "The Economic Implications of Learning by Doing." *The Review of Economic Studies* 29 (3): 155–73.

Balakrishnan, K., J. Blouin, and W. Guay, 2018. "Tax Aggressiveness and Corporate Transparency." *The Accounting Review*, forthcoming.

Beck, T., C. Lin, Y. Ma, 2014. "Why Do Firms Evade Taxes? The Role of Information Sharing and Financial Sector Outreach." *Journal of Finance* 69, 763-817.

Bertrand, M., and A. Schoar. 2003. "Managing with Style: The Effect of Managers on Firm Policies." *The Quarterly Journal of Economics* 118 (4): 1169–1208.

Bloom, N., E. Brynjolfsson, L. Foster, R. Jarmin, M. Patnaik, I. Saporta-Eksten, and J. Van Reenen. 2018. "What Drives Differences in Management Practices?" National Bureau of Economic Research. Working Paper Series.

Bloom, N., Eifert, B., Mahajan, A., McKenzie, D., & Roberts, J. 2013. "Does management matter? Evidence from India." *The Quarterly Journal of Economics*, *128*(1): 1-51.

Bloom, N., R. Sadun, and J. Van Reenen. 2017. "Management as a Technology?" National Bureau of Economic Research. Working Paper Series.

Bloom, N., and J. Van Reenen. 2007. "Measuring and Explaining Management Practices Across Firms and Countries." *The Quarterly Journal of Economics* 122 (4): 1351–1408.

Breuer, M. 2018. "How Does Financial-Reporting Regulation Affect Market-Wide Resource Allocation?" Working Paper.

Burgstahler, D., L. Hail, and C. Leuz. 2006. "The importance of reporting incentives: Earnings management in European private and public firms." *The Accounting Review* 81(5): 983-1016.

Bushman, Robert M., and A. Smith. 2001. "Financial accounting information and corporate governance." *Journal of Accounting and Economics* 32(1-3): 237-333.

Cheng, M., D. Dhaliwal, and Y. Zhang. 2013. "Does Investment Efficiency Improve after the Disclosure of Material Weaknesses in Internal Control over Financial Reporting?" *Journal of Accounting and Economics* 56 (1): 1–18.

Choi, J. 2018. "Accrual Accounting and Resource Allocation: A General Equilibrium Analysis." Working paper available at SSRN: https://ssrn.com/abstract=2977082.

Collard-Wexler, A., and J. De Loecker. 2016. “Production Function Estimation with Measurement Error in Inputs.” National Bureau of Economic Research working paper series.

Coppens, L. and E. Peek. 2005. “An Analysis of Earnings Management by European Private Firms.” *Journal of International Accounting, Auditing, and Taxation* 14(1): 1-17.

David, J., H. Hopenhayn, and V. Venkateswaran. 2016. “Information, Misallocation, and Aggregate Productivity.” *The Quarterly Journal of Economics* 131 (2): 943–1005.

Dechow, Patricia M., and I. Dichev. 2002. “The Quality of Accruals and Earnings: The Role of Accrual Estimation Errors.” *The Accounting Review* 77: 35–59.

Dechow, P., W. Ge, and C. Schrand. 2010. “Understanding Earnings Quality: A Review of the Proxies, Their Determinants and Their Consequences.” *Journal of Accounting and Economics* 50 (2): 344–401.

Dedman, E., A. Kausar., C. Lennox, 2014. The Demand for Audit in Private Firms: Recent Large- Sample Evidence from the UK. *European Accounting Review* 23, 1-23.

DeFond, M. and J. Zhang, 2014. “A review of archival auditing research.” *Journal of Accounting and Economics* 58, 275-326.

Demski, J. 1972. “Optimal Performance Measurement.” *Journal of Accounting Research* 10 (2): 243–58.

Desai, M. 2003. "The divergence between book income and tax income." *Tax policy and the economy* 17: 169-206.

Dhrymes, P. 1991. “The Structure Of Production Technology Productivity And Aggregation Effects.” 91-5. Center for Economic Studies, U.S. Census Bureau.

Dichev, I., J. Graham, C. Harvey, and S. Rajgopal. 2013. “Earnings Quality: Evidence from the Field.” *Journal of Accounting and Economics* 56 (2): 1–33.

Doms, M., and E. Bartelsman. 2000. “Understanding Productivity: Lessons from Longitudinal Microdata.” *Journal of Economic Literature* 38(3): 569-594.

Feltham, G. 1968. “The Value of Information.” *The Accounting Review* 43 (4): 684–96.

Feltham, G., and J. Demski. 1970. “The Use of Models in Information Evaluation.” *The Accounting Review* 45 (4): 623–40.

Feng, M., C. Li, S. McVay, and H. Skaife. 2015. “Does Ineffective Internal Control over Financial Reporting Affect a Firm’s Operations? Evidence from Firms' Inventory Management.” *The Accounting Review* 90 (2): 529–57.

Fields, T., T. Lys, and L. Vincent. 2001 "Empirical research on accounting choice." *Journal of Accounting and Economics* 31(1-3): 255-307.

Foster, L., C. Grim, J. Haltiwanger, and Z. Wolf. 2016. “Firm-Level Dispersion in Productivity: Is the Devil in the Details?” *The American Economic Review* 106 (5): 95–98.

Fox, J., and V. Smeets. 2011. “Does Input Quality Drive Measured Differences in Firm Productivity?” National Bureau of Economic Research working paper.

Gallemore, J., and E. Labro. 2015. “The Importance of the Internal Information Environment for Tax Avoidance.” *Journal of Accounting and Economics* 60 (1): 149–67.

Graham, J., and C. Harvey. 2001. “The Theory and Practice of Corporate Finance: Evidence from the Field.” *Journal of Financial Economics* 60 (2): 187–243.

Graham, J., C. Harvey, and S. Rajgopal. 2005. "The economic implications of corporate financial reporting." *Journal of Accounting and Economics* 40(1-3): 3-73.

Haltiwanger, J., R. Kulick, and C. Syverson. 2018. "Misallocation Measures: The Distortion That Ate the Residual." National Bureau of Economic Research working paper.

Hanlon, M., J. Hoopes, and N. Shroff. 2014. "The effect of tax authority monitoring and enforcement on financial reporting quality." *The Journal of the American Taxation Association* 36(2): 137-170.

Hanlon, M. and T. Shevlin. 2005. "Book-tax conformity for corporate income: An introduction to the issues." *Tax Policy and the Economy* 19 (2005): 101-134.

Hann, R., H. Kim., W. Wang, Y. Zheng. 2018. "Information Frictions and Productivity Dispersion: The Role of Financial Reporting Quality." Working Paper.

Harp, N.L. and B.G. Barnes. 2018. "Internal Control Weaknesses and Acquisition Performance." *The Accounting Review* 93(1), 235-258.

Healy, P. 1985. "The effect of bonus schemes on accounting decisions." *Journal of Accounting and Economics* 7(1-3), 85-107.

Hoopes, J., D. Mescall, and J. Pittman. 2012. "Do IRS Audits Deter Corporate Tax Avoidance?" *The Accounting Review* 87(5): 1603-1639.

Hsieh, C., and P. Klenow. 2009. "Misallocation and Manufacturing TFP in China and India." *The Quarterly Journal of Economics* 124 (4): 1403–48.

Kanodia, C. and H. Sapra. 2016. "A real effects perspective to accounting measurement and disclosure: Implications and insights for future research." *Journal of Accounting Research* 54(2), pp.623-676.

Kaplan, R. 1984. "The Evolution of Management Accounting." *In Readings in Accounting for Management Control*, 586–621. Springer.

Kim, K., A. Petrin, and S. Song. 2016. "Estimating Production Functions with Control Functions When Capital Is Measured with Error." *Journal of Econometrics* 190 (2): 267–79.

Levinsohn, J., and A. Petrin. 2003. "Estimating Production Functions Using Inputs to Control for Unobservables." *The Review of Economic Studies* 70(2): 317–41.

Lisowsky, P., and M. Minnis. (2018) "The Silent Majority: Private U.S. Firms and Financial Reporting Choices." Working paper.

Leuz, C., and P. Wysocki. 2016. ''The Economics of Disclosure and Financial Reporting Regulation: Evidence and Suggestions for Future Research.'' *Journal of Accounting Research* 54(2): 525-622.

McNichols, M., and S. Stubben. 2008. "Does Earnings Management Affect Firms' Investment Decisions?" *The Accounting Review* 83 (6): 1571–1603.

Miller, B., B. Williams, and T. Yohn. 2018. "Does Accounting Matter for (Start-up) Business Profitability?" Working paper.

Minnis, M. 2011. "The Value of Financial Statement Verification in Debt Financing: Evidence from Private U.S. Firms." *Journal of Accounting Research* 49 (2): 457–506.

Nikolaev, V. 2017. "Identifying accounting quality." Working Paper.

Olley, G.S., and A. Pakes. 1996. “The Dynamics of Productivity in the Telecommunications Equipment Industry.” *Econometrica* 64 (6): 1263–1297.

Pratt, J. W., H. Raiffa, and R. Schlaifer. 1965. Introduction to Statistical Decision Theory. McGraw-Hill, New York.

Rajan, R.G., and L. Zingales. 1998. "Financial Dependence and Growth." *The American Economic Review* 88(3): 559-586.

Roychowdhury, S., N. Shroff, and R. Verdi. 2018 “The Effects of Financial Reporting and Disclosure on Corporate Investment: A Review.” Working Paper

Schipper, K. 1989 “Commentary on earnings management.” *Accounting Horizons* 3: 91-102.

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

Slemrod, J. 2016. “Tax Compliance and Enforcement: New Research and Its Policy Implications.” Ross School of Business. Ross School of Business.

Syverson, C. 2004a. “Product Substitutability and Productivity Dispersion.” *The Review of Economics and Statistics* 86 (2): 534–50.

———. 2004b. “Market Structure and Productivity: A Concrete Example.” Journal of Political Economy 112(6): 1181-1222.

———. 2011. “What Determines Productivity?” *Journal of Economic Literature* 49 (2): 326–65.

Van Biesebroeck, J. 2007. “Robustness of Productivity Estimates.” *The Journal of Industrial Economics* 55 (3): 529–69.

Appendix A

Firm-level financial reporting has two broad dimensions: the set of accounting rules (or standards) followed by the firm and the extent of independent accountant attestation (if any). Figure A1 below illustrates the two dimensions as well as as the set of choices (non-listed) U.S. firms have. Accounting is the set of rules mapping economic events into financial reports. Firms not publicly listed can choose from different sets of accounting rules (e.g., Allee and Yohn 2009; Lisowsky and Minnis 2018). The most straightforward set of accounting rules is known as "cash basis" accounting in which economic transactions are simply recorded when cash is paid or collected by the firm. An alternative basis of accounting is "tax basis" in which the firm follows rules set by the Internal Revenue Service. All firms are required to file their annual tax form according to the tax basis of accounting. However, tax accounting standards are established by politicians and the main objective of tax rules is to collect tax revenues, not necessarily to portray the economic reality of the firm (Desai 2003; Hanlon and Shevlin 2005; Slemrod 2016). So while all firms are required to follow tax rules for filing annual forms with the IRS, many also follow more sophisticated practices to enhance the informativeness and contractability of the financial reports.

The most commonly understood and studied set of rules — and those required of publicly traded companies by the SEC — are referred to as Generally Accepted Accounting Principles (GAAP). GAAP is established by the Financial Accounting Standards Board (FASB) and is an "accrual basis" of accounting wherein economic transactions can be realized and recorded prior to the receipt or payment of cash. By necessity, the recording of accruals requires estimation on the part of managers because often one part of the economic transaction has not completed. For example, the firm has sold goods to a customer, but the customer has not yet paid. This transaction results in sales revenue and an accounts receivable accrual. The accounts receivable is essentially an estimate of how much cash will subsequently be collected from the customer. Financial statements contain significant accruals (and, thus, estimation) which are subject to both estimation error and biased misreporting (e.g., Dechow and Dichev 2002; Dechow et. al. 2010; Nikolaev 2017). Estimation error occurs when managers do not properly judge how future transactions will play out, but do so with noise (i.e., lack a direction to the future correction). Bias in the reports is an intentional — and directional — mischaracterization of the estimates often caused by various incentives. For example, managers compensated by annual bonuses could inflate the current year's reported production to the detriment of future years' performance (Healy 1985); while managers concerned with minimizing tax payments could underreport production levels by simply not recording sales (e.g., Slemrod 2016; Balakrishnan et. al. 2018).

To mitigate errors and bias in financial reports, managers (or owners and boards of directors) can choose to engage an independent accountant to verify the financial report prepared by managers, referred to as "attestation" (e.g., DeFond and Zhang 2014; Dedman et. al. 2014). The extent of work and testing independent accountants undertake when attesting to the financial report depends on the type of attestation engagement. The most rigorous — and the type of attestation required of public firms by the SEC — is an audit. During a financial statement audit, the independent accountant must collect evidence directly supporting the numbers reported by management in the financial statements. For example, accountants count inventory, observe property and equipment, and examine bank records for cash receipts from customers. Moreover, the independent accountant typically examines and tests the control systems firms use to record transactions and prepare the financial reports. For example, the accountants will examine how materials flow through the production process (i.e., are ordered, received, paid for, placed into production, and ultimately sold and delivered). Ultimately, the auditor assures that the financial statements present fairly, in all material respects, the financial position of the company and the results of the operations.

The second and third type of attestation engagements are significantly less rigorous than an audit (Minnis 2011). During a review engagement the accountant does not collect direct evidence supporting the reported balances in the financial statements, but instead conducts an inquiry of management about their financial reporting and management policies and performs high-level analyses of the financial reports (e.g., examines changes in balances over time and relationships between balances, looking for anything unusual). For a compilation engagement the independent accountant conducts no testing and provides no assurance about the balances in the reports at all. The purpose of the engagement is essentially “to assist management in presenting financial information in the form of financial statements” (AICPA 2016). Therefore, the independent accountant does little, if anything, to facilitate better reporting with a compilation engagement.

FIGURE A1
Two dimensions of financial reporting for non-listed U.S. firms

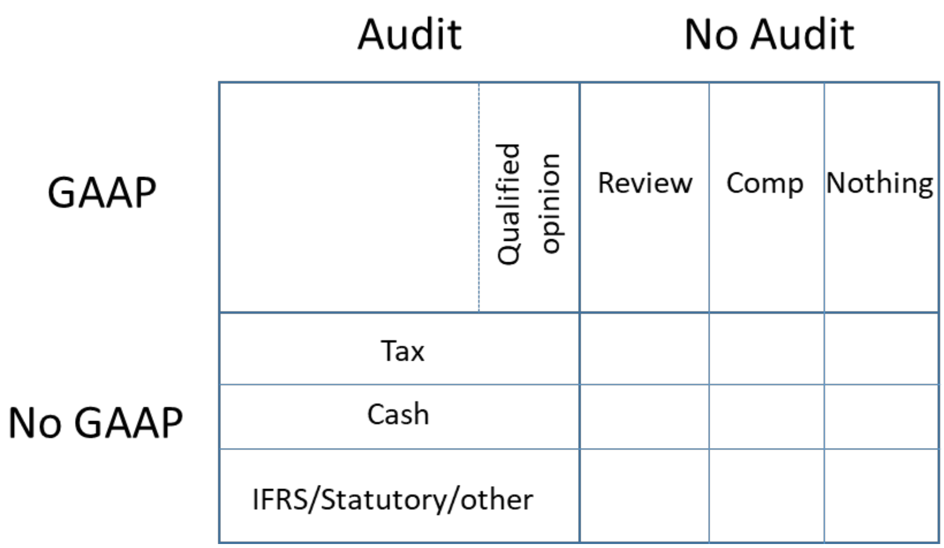

FIGURE 1
Variation in Financial Reporting Quality across Sectors

This figure reports the financial reporting quality variation within sector for those sectors with at least 500 observations. The data for the black bars is from the IRS data and reports the percentage of firms producing audited GAAP financial statements. The data for the gray bars is from the Sageworks data and reports the sector mean of the report variable, which equals 1 for audited, 0.5 for reviewed, and 0 for compiled financial statements. See Appendix A for definitions of these report types.

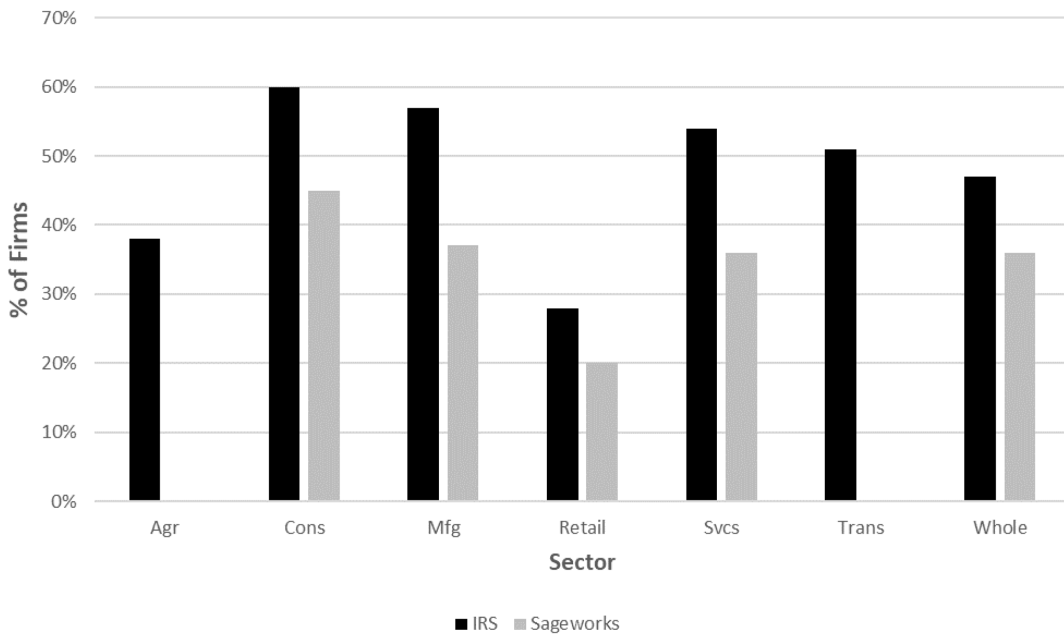

FIGURE 2
Variation in Financial Reporting Quality Conditional on Ownership and Debt

Figure 2a reports the financial report quality variation conditional on firm size (z-axis based on sales) and ownership dispersion (x-axis). The y-axis reports the percentage of firms producing audited GAAP financial statements. Figure 2b is identical but conditions on level of debt rather than ownership dispersion. The data for these plots is from the IRS data set.

Figure 2a: Conditioning on Size and Ownership dispersion

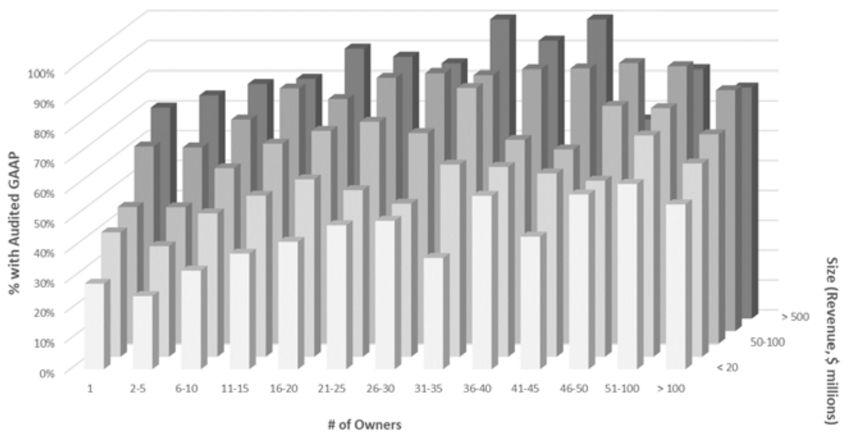

Figure 2b: Conditioning on Size and Debt

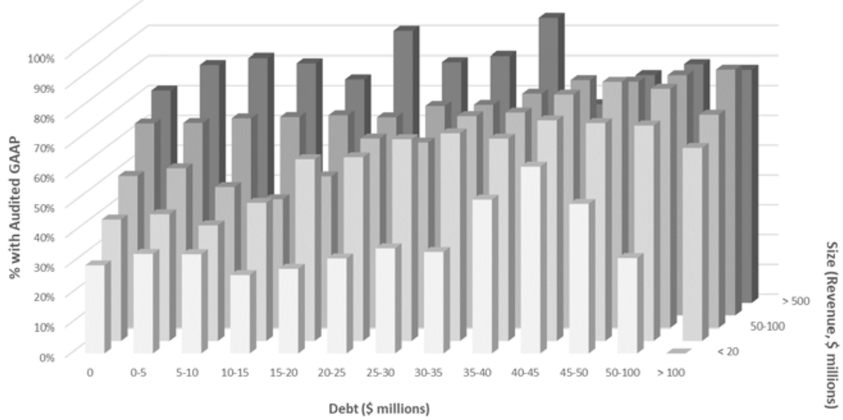

FIGURE 3

Distribution of TFP-VA by Industry Conditional on Financial Reporting Quality

These figures plot the distribution of TFP-VA by sector (for those with at least 500 observations), conditional on financial report quality. Figure 3a plots the results from the IRS data, while Figure 3b plots the results from the Sageworks data.

Figure 3a: IRS data

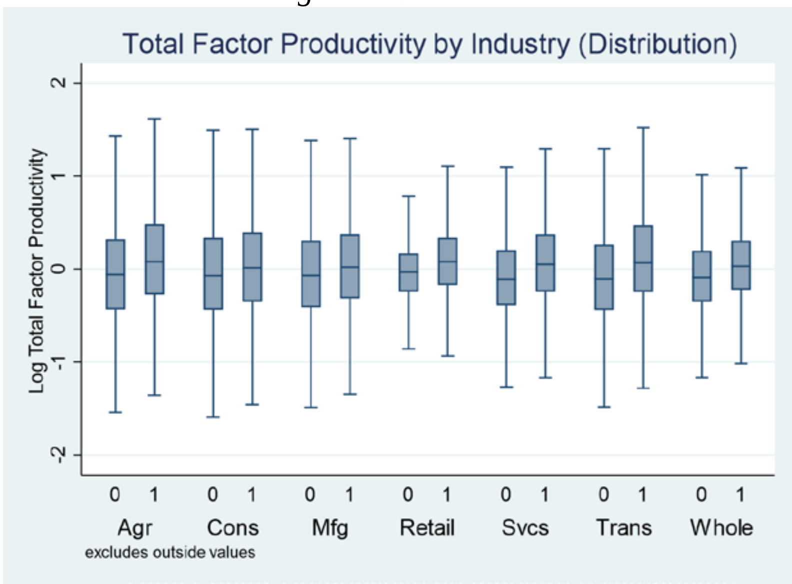

Figure 3b: Sageworks data

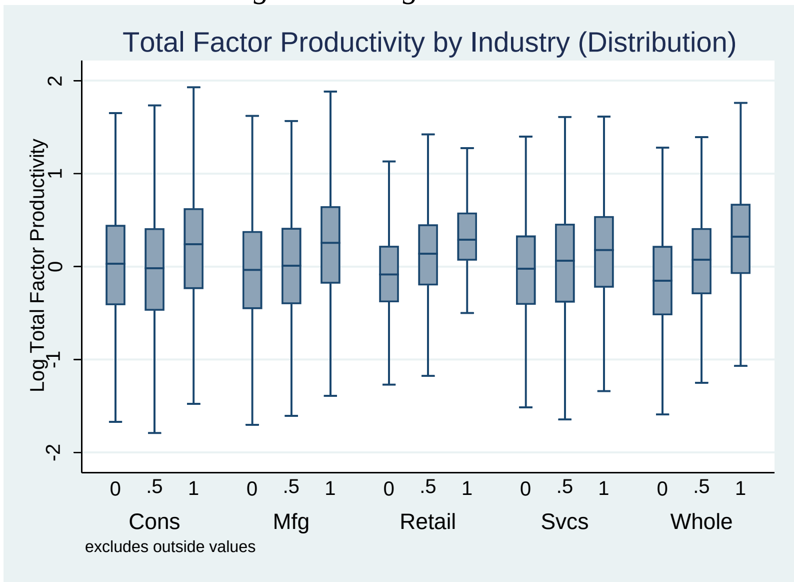

FIGURE 4
Portion of 10/90 TFP-VA Spread Explained by Industry

This figure plots the portion of the 10/90 TFP-VA spread explained by financial reporting quality for both the IRS (black) and Sageworks (gray) data sets.

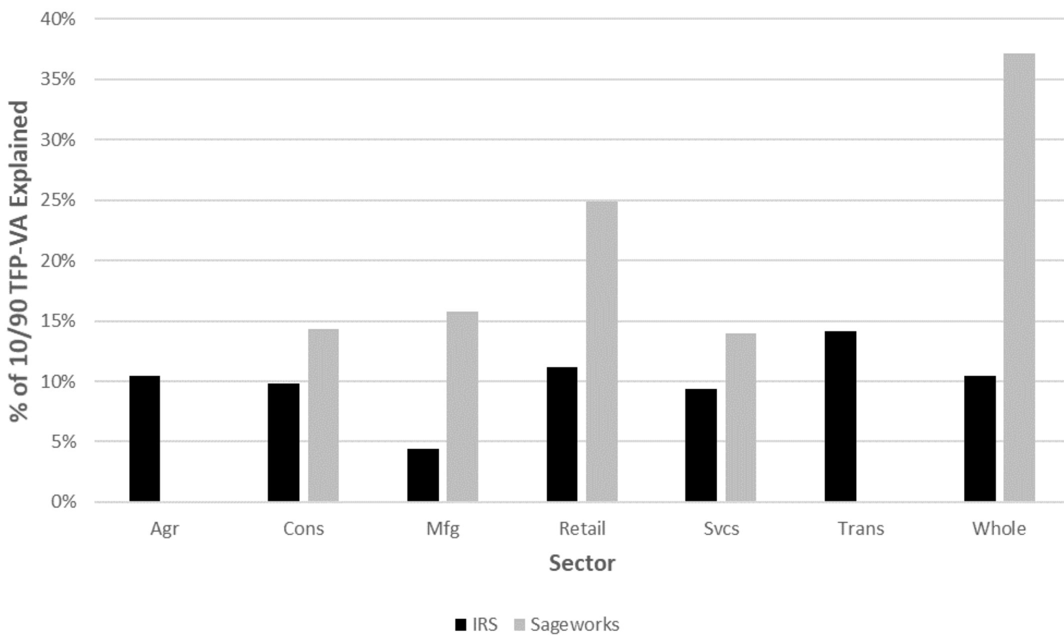

FIGURE 5
Corporate Taxation Rates across States

This map is shaded based on the corporate income tax rates for each state in the year 2008. Darker shades indicate higher corporate income taxes.

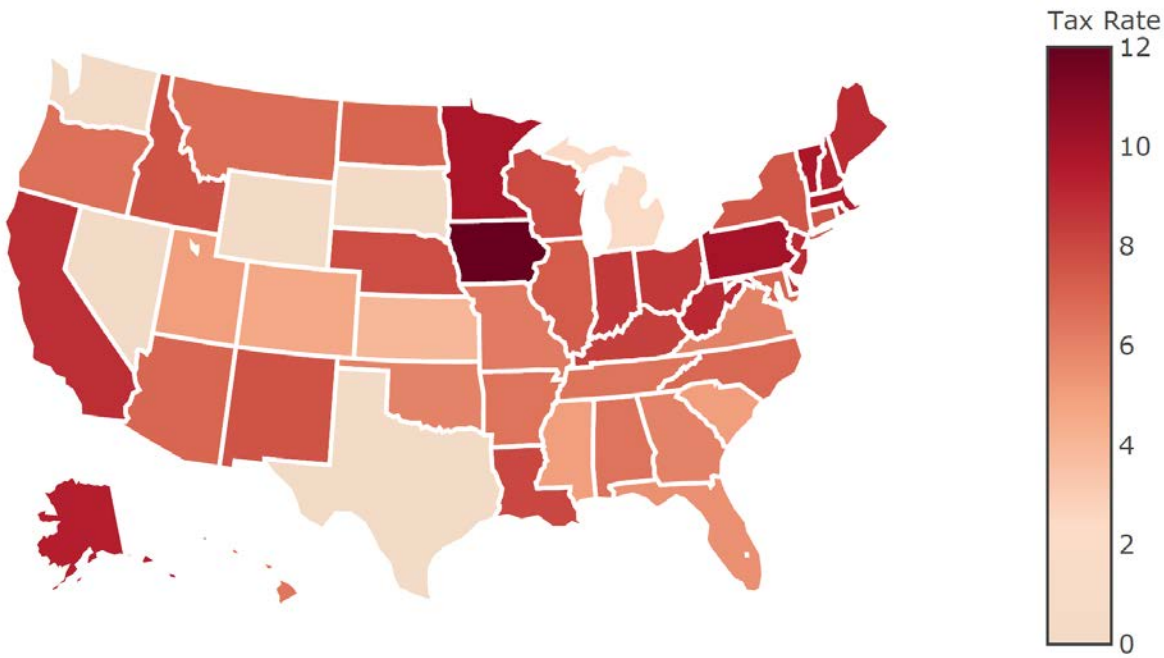

TABLE 1
Distribution of firm-years across industry

	IRS		*Sageworks*	
Industry	*n*	*%*	*n*	*%*
	(1)	*(2)*	*(3)*	*(4)*
Agriculture	1,011	1.7%	210	1.4%
Construction	7,919	13.3%	3,729	24.8%
Manufacturing	16,469	27.8%	3,731	24.8%
Mining	940	1.6%	70	0.5%
Retail trade	10,860	18.3%	2,194	14.6%
Services	10,438	17.6%	2,233	14.9%
Transportation	1,312	2.2%	276	1.8%
Utilities	244	0.4%	104	0.7%
Wholesale trade	10,117	17.1%	2,490	16.6%
Other	22	0.0%	n/a	n/a
Total	59,332	100.0%	15,037	100.0%

This table reports the distribution of firm-year observations across NAICS sectors for the IRS (columns 1 and 2) and Sageworks (columns 3 and 4) data sets.

TABLE 2

Descriptive statistics

Panel A: IRS

		Mean	*P10*	*P50*	*P90*	*sd*	*n*
Audit	ln(sales)	17.94	16.73	17.89	19.30	1.03	29,219
	ln(cogs)	17.42	15.85	17.52	19.02	1.40	29,219
	ln(va)	16.50	15.19	16.45	17.94	1.10	29,219
	ln(labor)	15.14	13.50	15.17	16.77	1.32	29,219
	ln(ppe)	15.18	13.07	15.30	17.22	1.67	29,219
No Audit	ln(sales)	17.39	16.15	17.43	18.55	1.00	30,113
	ln(cogs)	16.90	15.26	17.11	18.34	1.43	30,113
	ln(va)	15.89	14.77	15.88	17.05	0.97	30,113
	ln(labor)	14.56	12.99	14.66	15.99	1.24	30,113
	ln(ppe)	14.44	12.32	14.60	16.35	1.69	30,113

Panel B: Sageworks

		Mean	*P10*	*P50*	*P90*	*sd*	*n*
Audit	ln(sales)	16.52	15.04	16.51	17.97	1.14	2,214
	ln(cogs)	16.10	14.36	16.16	17.73	1.33	2,214
	ln(va)	15.06	13.61	15.12	16.47	1.14	2,214
	ln(labor)	4.02	2.48	4.01	5.44	1.19	2,214
	ln(ppe)	14.23	12.16	14.26	16.26	1.71	2,214
Review	ln(sales)	15.82	14.65	15.78	17.06	0.95	6,500
	ln(cogs)	15.44	14.11	15.42	16.80	1.08	6,500
	ln(va)	14.39	13.15	14.36	15.69	1.00	6,500
	ln(labor)	3.47	2.30	3.47	4.70	0.98	6,500
	ln(ppe)	13.31	11.39	13.38	15.20	1.54	6,500
Comp	ln(sales)	15.35	14.26	15.27	16.53	0.92	6,323
	ln(cogs)	14.85	13.49	14.85	16.24	1.15	6,323
	ln(va)	14.12	13.05	14.09	15.24	0.91	6,323
	ln(emp)	3.12	1.95	3.09	4.32	0.98	6,323
	ln(ppe)	13.18	11.47	13.28	14.81	1.44	6,323

This table presents summary statistics for the variables used in this paper partitioned by data set and conditional on financial reporting quality. Panel A reports the statistics from the IRS data set. *ln(sales)* is log gross sales; *ln(cogs)* is log cost of goods sold; *ln(va)* is log value added calculated as the difference between sales and cost of goods sold; *ln(labor)* is log of salaries and wages, all from page 1 of the tax return. *ln(ppe)* is lagged log property, plant, and equipment from Schedule L. Panel B reports the statistics from the Sageworks data set. *ln(sales)* is log of sales revenue; *ln(cogs)* is log of cost of goods sold; *ln(va)* is log value added calculated as the difference between sales and cost of goods sold; *ln(labor)* is lagged log of number of employees; *ln(ppe)* is lagged log property, plant, and equipment.

TABLE 3
Estimating Production Functions

Data set	*IRS*		*Sageworks*	
Sample	*Pool*	*Propensity*	*Pool*	*Propensity*
Dep. variable	*ln(va)*	*ln(va)*	*ln(va)*	*ln(va)*
	Coef	*Coef*	*Coef*	*Coef*
	SE	*SE*	*SE*	*SE*
	(1)	*(2)*	*(3)*	*(4)*
ln(labor)	0.598***	0.577***	0.587***	0.593***
	(0.01)	(0.01)	(0.01)	(0.02)
ln(ppe)	0.110***	0.100***	0.118***	0.094***
	0.00	0.00	(0.01)	(0.01)
State fixed effects	N	N	Y	Y
Industry x year fixed effects	Y	Y	Y	Y
Observations	59,332	38,636	15,037	8,423
R^2	0.697	0.625	0.593	0.548

This table presents the results from estimating the Cobb-Douglas production function with value added (log of sales less cost of goods sold) as the dependent variable. Columns 1-2 present the results from the IRS sample. Columns 3-4 present the results from the Sageworks sample. In the IRS analyses, ln(labor) is measured as the log of salaries and wages; in the Sageworks analyses, ln(labor) is measured as the log of total employees. All regressions include fixed effects for industry (4-digit NAICS) by year. The Sageworks analyses further include fixed effects for the state of location. The samples used in Columns 2 and 4 is restricted to the propensity matched samples as described in the text. Presented below the coefficients are robust standard errors clustered at the industry x year level. *, **, *** indicate significance at 10%, 5%, and 1%, respectively.

TABLE 4
Productivity and reporting quality

Data set	*IRS*		*Sageworks*	
Sample	*Pool*	*Propensity*	*Pool*	*Propensity*
Dep. variable	*TFPR-va*	*TFPR-va*	*TFPR-va*	*TFPR-va*
	Coef	*Coef*	*Coef*	*Coef*
	SE	*SE*	*SE*	*SE*
	(1)	*(2)*	*(3)*	*(4)*
report	0.108***	0.087***	0.310***	0.272***
	(0.01)	(0.01)	(0.02)	(0.03)
State fixed effects	N	N	Y	Y
Industry x year fixed effects	Y	Y	Y	Y
Observations	59,332	38,636	15,037	8,423
Share of 90-10 explained	8.4%	6.7%	20.2%	18.3%
R^2	0.007	0.005	0.022	0.017

This table represents the results of regresssing value added total factor productivity (estimated as the residuals from the regressions in Table 3) on financial measurement quality. Columns 1-2 present the results using the IRS sample and defines *report* as an indicator variable equal to 1 if the firm prepares financial statements according to GAAP and has them audited by an independent accountant, and 0 otherwise. Columns 3-4 present the results from the Sageworks sample and defines *report* as a variable equal to 0 (0.5, 1) if the firm has a compilation (review, audit). The sample used in Columns 2 and 4 is restricted to the propensity matched samples as described in the text. The share of 90-10 explained is the estimated coefficient on *report* divided by the spread in productivity between the 10th and 90th percentiles. Presented below the coefficients are robust standard errors clustered at the industry x year level. *, **, *** indicate significance at 10%, 5%, and 1%, respectively.

TABLE 5

Productivity and reporting quality conditional on capital structure

		Leverage		
		None	*>0 to 20%*	>20%
Owners	*1*	0.053 (0.035) R^2 = 0.166 n = 2,035 %Aud = 38.5%	0.083*** (0.025) R^2 = 0.130 n = 3,093 %Aud = 50.0%	0.089*** (0.015) R^2 = 0.122 n = 5,969 %Aud = 42.6%
	2 to 5	0.091*** (0.022) R^2 = 0.096 n = 5,456 %Aud = 39.7%	0.076*** (0.017) R^2 = 0.072 n = 7,490 %Aud = 46.9%	0.143*** (0.013) R^2 = 0.078 n = 14,203 %Aud = 39.7%
	>5	0.095*** (0.024) R^2 = 0.121 n = 4,082 %Aud = 57.1%	0.155*** (0.018) R^2 = 0.117 n = 6,458 %Aud = 63.2%	0.203*** (0.016) R^2 = 0.094 n = 9,271 %Aud = 64.1%

This table presents estimates from the model in Table 4, Column 1 after conditioning the sample based on the number of owners and amount of leverage. Leverage is defined as total outside (i.e., nonowner) debt divided by total assets from Schedule L of the tax return. Each cell of the table reports the estimated coefficient on the *report* variable, the robust standard error clustered at the industry x year level, the R^2, the sample size, and the portion of the sample in which *report*=1. The figures outside of the cells report the subtotals for the rows and columns. *, **, *** indicate significance at 10%, 5%, and 1%, respectively.

TABLE 6
Under-reporting bias and state taxation

Data set	*Sageworks*	
Dep. variable	*TFP-VA*	*TFP-VA*
Cross sectional variable	*Corp tax*	*Rank tax*
	Coef	*Coef*
	t-stat	*t-stat*
	(1)	*(2)*
report	0.241***	0.224***
	(0.04)	(0.05)
report x high_corp	0.129**	
	(0.05)	
report x high_rank		0.161*
		(0.09)
State FE	Y	Y
Industry x year FE	Y	Y
Observations	15,037	15,018

This table presents OLS regressions of value added total factor productivity regressed on financial report measurement quality and a variable measuring state level taxation intensity. The dependent variable is firm-year-level *TFP-va* estimated from the regressions reported in column 3 of Table 3. The variable *high_corp* categorizes U.S. states into high (=1 for above median) and low (=0 for equal to or below median) based on corporate tax rates. The variable *high_rank* categorizes U.S. states into high (=1 for states in the top 10), medium (=0.5 for states ranked 11 through 40), and low (=0 for states ranked in the bottom 10) based on the amount of sales, gross receipts, and income-based taxes scaled by the population and excludes the District of Columbia. The main effects of state taxation are absorbed in the state fixed effects. Presented below the coefficients are robust standard errors clustered at the state level. *, **, *** indicate significance at 10%, 5%, and 1%, respectively.

TABLE 7
Cross-sectional analyses

Data set	*IRS*		
Dep. variable	*TFP-VA*	*TFP-VA*	*TFP-VA*
Cross sectional variable	*Profit margin*	*R&D*	*Young Age*
	Coef	*Coef*	*Coef*
	t-stat	*t-stat*	*t-stat*
	(1)	*(2)*	*(3)*
report	0.085***	0.114***	0.098***
	(0.01)	(0.01)	(0.01)
report x CS var.	0.032	-0.054***	0.072***
	(0.02)	(0.02)	(0.02)
young age			0.003
			(0.02)
Industry x year FE	Y	Y	Y
Observations	54,547	54,547	51,240

This table presents OLS regressions of revenue total factor productivity regressed on financial report measurement quality and various cross-sectional variables or time indicators. The dependent variable is firm-year-level *TFP-va* estimated from the industry-level regressions reported in Table 3. The cross-sectional variables in Columns 1-2 are sourced from Compustat data using 3-digit NAICS industries annually. Profit margin is calculated as 1 minus the profit margin of the median firm in each industry-year. *R&D* is R&D scaled by sales for the median firm in each industry-year. Each of the cross-sectional variables in Columns 1-2 are deciled each year and scaled between 0,1 such that the magnitude of the coefficient can be interpreted as going from the first to the tenth decile of the cross-sectional variable. The main effects of the cross-sectional variable are absorbed in the industry x year fixed effects. The cross-sectional variable in Column 3 is an indicator variable equal to 1 if the firm is less than 4 years old as reported on the corporate (Form 1120 or 1120S) tax return (i.e., firms filing Form 1065 are omitted from this test). All regressions include industry x year fixed effects. Presented below the coefficients are robust standard errors clustered at the industry x year level. *, **, *** indicate significance at 10%, 5%, and 1%, respectively.

TABLE 8
Survival and changes in performance

Data set	*IRS*							
Sample	*Firm exists in 2008*					*Firm exists in 2008 and 2010*		
Size restriction	*None*	*None*	*None*	*>Median*	*<=Median*	*None*	*None*	*None*
Dep. variable	*Survive*	*Survive*	*Survive*	*Survive*	*Survive*	*TFP 2010*	*Sales 2010*	*TFP*
	Coef	*Coef*	*Coef*	*Coef*	*Coef*	*Coef*	*Coef*	*Coef*
	SE	*SE*	*SE*	*SE*	*SE*	*SE*	*SE*	*SE*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
tfp	0.074***		0.070***	0.021**	0.066***	0.741***		
	(0.01)		(0.01)	(0.01)	(0.01)	(0.02)		
report		0.071***	0.063***	0.029**	0.051***	0.032***	0.017**	
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
sales in 2008							0.941***	
							(0.006)	
PM in 2008								
start_gaap_audit_2009								0.010
								(0.01)
start_gaap_audit_2010								0.022***
								(0.01)
Industry FE	Y	Y	Y	Y	Y	Y	Y	N
Firm FE	N	N	N	N	N	N	N	Y
Year FE	N	N	N	N	N	N	N	Y
Observations	14,793	14,793	14,793	7,382	7,384	10,484	10,484	28,701
R^2	0.053	0.049	0.058	0.049	0.079	0.554	0.899	0.859

This table presents the results of OLS regressions of future firm performance conditioning on current performance and financial reporting quality. All analyses are restricted to the IRS sample. Columns 1-5 examine firm survival where *survive* is an indicator equal to 1 if the firm exists in 2010 and equal to 0 if the firm is not in the IRS data set in 2010 and *tfp* and *report* are measured in 2008. Column 4 limits the sample to firms above the median size by log sales and Column 5 limits the sample to firms below or equal to median size. The sample for the analyses in Columns 6 and 7 condition on the firm existing in all three years 2008 to 2010 in the IRS data set. Column 8 conditions the sample on firms which either have *report* = 1 each year, *report* = 0 each year, or change to *report* = 1 in 2009 and continue to have *report* = 1 in 2010 (i.e., eliminates firms which do not exist in all three years or reduce their report quality in any year or change their report quality more than once). The column reports a firm fixed effects regression of *TFP-va* on indicators for firms begining a GAAP audit in 2009 and 2010. Columns 1-7 include industry fixed effects; Column 8 includes firm and year fixed effects. Presented below the coefficients are robust standard errors clustered at the industry x year level. *, **, *** indicate significance at 10%, 5%, and 1%, respectively.