

Segment Reporting and Financial Statement Comparability: Evidence from Confidential Statistical Data

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July 23, 2026

ABSTRACT

Firms are required to report performance along different segments of their business (e.g., by industry or region) in their public financial statements. Segment reporting enables the public to assess a firm's operational structure (e.g., industries in which it operates) and disaggregated performance (e.g., sales by industry). This study uses information reported confidentially to the U.S. government as a proxy for a firm's internal operational structure, which we compare to the number of industry segments reported publicly. For many firms, especially large U.S. multinationals, their internal operational structures are more complex than what can be gleaned from their public disclosures. On average, multinationals confidentially disclose operations in 4.5 industries in our sample, while the same firms publicly report operations across 1.7 industries. We use these divergent measures to study an important outcome for capital markets, financial statement comparability, and investigate how distinguishing between disaggregation and organizational structure can matter. Specifically, we reexamine a financial accounting standard change (SFAS No. 131) using a differences-in-differences methodology, finding that incorporating confidential information about a firm's operational structure can substantively alter inferences about comparability that would otherwise be drawn from public information alone. Recognizing the challenge this presents to researchers without access to restricted data, we develop a synthetic measure of the number of industries that are confidentially reported, which we validate against a machine learning model using the restricted, internal data. This new measure can be constructed out-of-sample based solely on public data, assisting future researchers working with segment data.

JEL CODES: G14, M10, M41, F23, L20, L25

KEYWORDS: operational structure, segment reporting, multinational firms, industry diversification, financial statement comparability, SFAS No. 131, information disclosure, firm disaggregation

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I. INTRODUCTION

Under existing Financial Accounting Standards Board (FASB) standards, U.S. firms are required to break out financial performance metrics such as net sales or earnings by the segments in which they operate.¹ While smaller firms usually operate in a single industry or region, larger firms more often operate across multiple segments and disaggregate their performance in public financial statements along these lines. Investors and analysts view this information as “critically important” for understanding business activities (FASB 2023). In fact, a 2018 survey of financial analysts conducted by the CFA Institute reports that an overwhelming majority of analysts, about 90 percent, view segment disclosures to be at least as important as the overall consolidated results (Botosan, Huffman, and Stanford 2021).² Yet, most of these same users also believe the extent of disaggregation of segment information is insufficient, desiring more granular information for the purposes of assessing and forecasting performance (Botosan et al. 2021).³ Firms and the preparers of this information, on the other hand, are hesitant to provide more disaggregated segment information to the public out of concern that it could release costly proprietary information to competitors (Hayes and Lundholm 1996; Harris 1998; Botosan and Stanford 2005; Arya, Frimor, and Mittendorf 2010).⁴ This tension between public benefits for information and the costs to a firm’s revelation of its operations to its competitors is not unique to segment disclosures, but is a recurring issue at the heart of accounting more generally.

Given its potential cost and risk to firms, a natural empirical question arises: do we know whether additional segment information (or greater disaggregation) would convey measurable

¹ Specifically, SFAS No. 131 requires that firms, “...report information about operating segments in annual financial statements and requires that those enterprises report selected information about operating segments in interim financial reports...” (FASB 1997). See [FASB’s site](#) for a more detailed summary.

² The results from a 2012 Financial Accounting Foundation (FAF) user survey report a similar finding that, “91% of respondents rate segment information as very or somewhat important and 88% use segment information for investment decisions” (Botosan et al. 2021).

³ Stakeholders responded to FASB’s Invitation to Comment with the view that, “the list of required disclosures are helpful but often is insufficient” (FASB 2016), which comports with 65% of respondents from the 2018 CFA that had some level of agreement that, “the extent of disaggregation is insufficient to perform meaningful financial statement analysis” (Botosan et al. 2021).

⁴ The 2012 FAF survey found 72% of preparers agree with the statement that, “some companies aggregate operating segments to avoid reporting sensitive competitive information” (Botosan et al. 2021).

benefits to financial data users? For decades, accounting research has tackled variations of this question, finding that more disaggregated segment information improves, for example, the market's ability to predict future earnings (Berger and Hann 2003; Ettredge, Kwon, Smith, and Zarowin 2005), transparency of cross-segment variability of reported profits (Ettredge, Kwon, Smith, and Stone 2006), and capital allocation efficiency in internal capital markets (Cho 2015). Much of this line of research has relied on a 1997 change to FASB standards for segment reporting, SFAS No. 131, as a source of exogenous variation in how much firms disaggregate their financial performance. Substantively, this change shifted segment reporting from an *industry approach* using more traditional classifications of industry breakouts toward a *management approach* to allow firms to identify reportable segments based along the lines in which management internally makes decisions for the business. Effectively, SFAS No. 131 encouraged greater disaggregation of information and thus resulted in an increase in the number of segments firms reported, ultimately amplifying the relevance of segment reporting for evaluating firm performance (Berger and Hann 2003).

In our study, we revisit this question of evaluating the benefits of greater disaggregation of segment information, but with unique data that allow us to address a critical issue in prior work. One problem with evaluating the effect of an increase in segment information (even if it comes from a policy change such as SFAS No. 131) is that we ordinarily observe only what the firm discloses publicly, not necessarily a complete picture of its operating segments.⁵ Thus, when a firm publicly reports information across more industry segments, for example, it could mean that the firm has either 1) operationally changed and is disclosing information about industries it has expanded into (i.e., a “diversifier”) or 2) remained operationally the same but is simply disaggregating more information (i.e., a “disaggregator”).⁶ The accounting literature generally

⁵ In a 2012 Financial Accounting Foundation (FAF) survey, about 50% of financial statement users surveyed believed too few segments are reported; and, this opinion was heightened for single-segment firms, where 74% of user respondents believed these firms fail to provide adequate disaggregation (Botosan et al. 2021).

⁶ Botosan and Stanford (2005) found that about half (55%) of entities initiating new segments after SFAS 131 were in an industry that differs from their primary industry (indicating disclosure of more diverse operations).

assumes the latter, by necessity, because researchers lack internal operational information to distinguish between segment changes that correspond with diversification versus disaggregation. This may confound the causal interpretation of segment disaggregation insofar as the first source of activity plays an important role in outcomes from segment reporting. To investigate this potential limitation, we use confidential data from the U.S. Bureau of Economic Analysis (BEA) mandatory surveys of U.S. direct investment abroad, which cover U.S. multinational enterprise (MNE) parents and their foreign affiliates. Data reported on BEA's MNE surveys include more granular information on firms' industry operations than what is disclosed in public financial statements, such as those filed with Securities and Exchange Commission (i.e., Form 10-K). Information reported to BEA remains protected from disclosure in any form that would allow the identification of specific firms, and it is prohibited from release to other government agencies for purposes of taxation, investigation, or regulation.⁷ Thus, firms report detailed industry and other information to BEA with the expectation that the information will not be seen by their competitors or the general public. The ability to peek behind the veil with confidential BEA survey data affords a unique opportunity to sort out whether additional disaggregation of information is beneficial for evaluating an important financial statement attribute, the comparability of a firm's earnings to its industry peers ('comparability' hereafter), while distinguishing among diversifiers and disaggregators.

One reason standard setters argue that public segment disaggregation is critical for performance evaluation of firms is that they expect greater disaggregation to provide insight into how managers make decisions for different lines of a business (FASB 2016). Given that performance is often assessed relative to industry peers for these evaluations (De Franco, Kothari, and Verdi 2011; De Franco, Hou, and Ma 2023), understanding how information disaggregation

⁷ BEA's data collections are authorized by the International Investment and Trade in Services Survey Act (22 U.S.C. 3101-3108) ("Survey Act"). The Survey Act also defines the strict protections for the data. The BEA MNE survey data is intended for statistical purposes like constructing the U.S. national and international economic accounts and analytical research by BEA officials, employees, consultants, and contractors or by appointment as BEA Special Sworn Research Consultants (SSRCs).

contributes to comparability is a key part of understanding the role of segment reporting in performance evaluation. At the same time, the diversity of underlying industry operations is also relevant for understanding how comparable the firm's financial performance will be to its primary industry peers, given that operational structure is an important factor for a firm's productivity and investment decisions (Schoar 2002; Maksimovic and Phillips 2002; Ahn, Denis, and Denis 2006). Thus, for the purposes of this study, we focus on financial statement comparability as a setting where consideration for both constructs may be particularly important for the research design. Because there are many potential outcomes greater segment disaggregation can affect, a second goal of this paper is to provide a 'synthetic' firm-level measure of confidentially reported industries to aid future research on this topic.

Our initial dataset joins a panel of confidential firm-level data for U.S. multinationals from the BEA Direct Investment Abroad surveys to Compustat data for 1995-2001, covering the period surrounding the introduction of SFAS No. 131.⁸ BEA data include the U.S. reporter (the U.S. parent of the U.S. multinational) survey with its foreign affiliate surveys,⁹ which are linked by a U.S. reporter's ID and period year. Similar to Villalonga (2004), we use the number of unique industries reported by each firm in these confidential surveys as a measure for the firm's private information about the breadth of underlying industry operations.¹⁰ After aligning industry definitions across datasets, we then compare this measure with the number of unique industries reported publicly in segment data in Compustat, which captures the degree of public disaggregation of operational information in financial statements (10-Ks).

Our initial descriptive analysis shows that private reporting, information confidentially reported to the government, and public reporting of industry operations can differ substantially

⁸ We exclude the year of the change (1998).

⁹ This includes industry codes across all surveys in a given reporter-year but excludes those affiliates with minority ownership as this would not be included in consolidation.

¹⁰ To maintain a stable definition of industry over the period, we use three-digit SIC for the entire period. In the last few years of this analysis, the BEA surveys used NAICS codes instead of SIC codes. We convert four-digit NAICS codes in those years with survey data to three-digit SIC using a concordance between 1997 NAICS and 1987 SIC.

for multinationals. We observe that, on average, multinationals privately disclose operations in 4.5 industries, while the same firms publicly report operations across only 1.7 industries in the initial sample. Thus, multinationals operate in a considerably larger number of industries than can be gleaned from public information alone, and this difference is not limited to a particular time period.¹¹ Further, we find that most firms also had a change in their confidential reporting and that the number they reported moved closer to their industry peers. This held even for the group of firms with *no change* in their public reporting. Given BEA reporting requirements did not change at this time, this suggests that the change in industries reported to the BEA after the standard captures an underlying change in actual operations as opposed to a change in firm discretion with their BEA reporting.

When we incorporate the confidential segment information into a difference-in-differences (DiD) analysis surrounding SFAS No. 131, the results suggest this information plays an important role in understanding inferences from the FASB standard's change on comparability. Specifically, when we evaluate public reporting changes alone, and do not account for the confidentially reported information, we observe that firms with changes in their public reporting had less comparable financial statements to their industry peers following SFAS No. 131. This finding by itself might suggest a message consistent with what is predicted in prior literature on information disaggregation (Berger and Hann 2003; Ettredge et al. 2005; Ellis et al. 2012), that a standard change catalyzing greater public information disaggregation comes with an important trade-off (i.e., having less comparable financial statements amongst peers). However, when we incorporate the privately reported industry information into this analysis, we see this interpretation is incomplete. The negative association between a public reporting change and comparability is substantially diminished when we account for changes in privately reported information following the standard, both in the sense that the association is no longer statistically

¹¹ In the decades extending beyond our initial SFAS No. 131 sample (2002-2021), the number of privately reported industries fluctuated between 3 and 4 in most years, while the same MNEs reported approximately 1.5 industries publicly, on average. We discuss descriptive statistics in greater detail in Section III below.

significant at conventional levels and that the economic magnitude is much smaller. By contrast, we observe an increase in comparability for firms that had a private reporting change but *no change* in public reporting following SFAS No. 131. These firms had previously been treated as having no substantive change in reporting with analysis using public information, and they would have been part of the control group in traditional analysis with the standard. Collectively, our results suggest that financial statement comparability differences following SFAS No. 131 appear to be driven more by firm adjustments to move operations closer to industry norms (i.e., improved comparability from operations) rather than their adjustments in public information disaggregation.

The above findings present a challenge to future researchers interested in studying the role of public industry disaggregation using publicly reported segment data alone. Motivated by this, our final set of analyses in the paper works to provide an opportunity for future researchers to distinguish between the private industry reporting (diversification) and public reporting (disaggregation) constructs. To do this, we develop a model to predict the number of industries firms confidentially report using only firm characteristics that are publicly available. We evaluate the ‘synthetic measure’ of the number of industry segments reported to BEA against a machine learning model (i.e., conduct out-of-sample tests to evaluate the accuracy of the models’ predictions) and compare the synthetic measures to the actual measures of the number of industry segments (both those publicly and privately reported). Overall, the results from this analysis provide evidence that our synthetic segment measure is a reasonable approximation of the number of industry segments reported to BEA. Our expectation is that providing a reasonable proxy based on public data alone will help researchers that do not have access to BEA’s confidential data better distinguish between public and private reporting constructs in future work.

This study offers four contributions to the literature. First, the results speak to concerns from FASB and prior academic literature about the state of segment reporting and whether it is sufficient for capital market evaluations of firm performance. Bens et al. (2011) compare confidential U.S. Census Bureau plant-level data to publicly reported segment data, finding that

firms report more segments privately than they do publicly,¹² and further show that agency and proprietary costs can play a role in how much of the private information is publicly reported. Botosan et al. (2021) provide a detailed analysis of how public segment reporting has evolved over time and summarize stakeholders' perceptions about public segment reporting based on SEC comment letters and surveys from the Financial Accounting Foundation and the CFA Institute. Overall, the authors conclude that despite multiple attempts by standard setters to enhance segment reporting, there has been "little increase" in the extent of public segments reported, and there are continued concerns by stakeholders about the quality and usefulness of this information. The results in our analysis fill a void in the literature by showing that additional segment information that is not publicly reported has important consequences for accounting and financial outcomes in terms of financial statement comparability.

Second, the results in our study complement and strengthen prior work suggesting that public segment reporting is likely missing information that would be valuable for evaluating firm market value. Segment research in finance, for example, has shown that the so-called diversification discount is sensitive to the type of segment data used (Villalonga 2004), and that the scope of operations captured outside segment information can be highly valued by the market (Hoberg and Phillips 2025).¹³ Building on Villalonga (2004) and Hoberg and Phillips (2016), our results provide new evidence that public segment data may still not measure the construct that researchers think it measures, even following a major standard change (SFAS No. 131), and reinforces why segment information should be handled with care in finance and accounting research.

The third line of research we contribute to investigates capital market consequences of public information disaggregation (Ettredge et al. 2005; Ettredge et al. 2006; Cho 2015; Cho and

¹² They find firms report 5.1 industry segments privately but only 1.6 industry segments publicly on average, based on four-digit SIC codes.

¹³ Similar to our study, Villalonga (2004) uses confidential government data to construct an industry segment measure, albeit from a different survey (Business Information Tracking Series (BITS)), agency (Census Bureau), and timeframe (i.e., pre-1997). However, the diversification discount literature that Villalonga (2004) reexamines largely uses pre-1997 segment data that predates SFAS No. 131 (see footnote 3, p. 480 of her study).

Seo 2024). As noted above, prior research in this area has, by necessity, relied on public segment reporting alone to investigate the consequences of public information disaggregation. Research in this area typically attributes a positive association between a change in reported segments following SFAS No. 131 and an advantageous capital market outcome (e.g., forward earnings response coefficient, cross-segment profit variability, and capital market efficiency) as a benefit of information disaggregation with the standard. However, the results in our study reveal that inferences made for at least one important capital market consequence, financial statement comparability, using public segment reporting alone may lead to incomplete (or even misleading) inferences about the role of information disaggregation. We find that, once we account for adjustments to private operational changes that occurred around the standard (i.e., diversifiers), we no longer observe evidence supporting the association between public segment changes (i.e., disaggregators) and comparability that we observed when using public segment information alone. While it may be the case that operational changes play a more pronounced role in the outcome we investigate (comparability) relative to the others examined in prior literature, the results in our study suggest that variation in (private) operational reporting has the potential to play an important role for other capital market outcomes future researchers wish to study.

Fourth, to assist future researchers who may not have access to confidential firm data like that collected on BEA's MNE surveys, we contribute to the literature by constructing a 'synthetic measure' of the number of privately reported industry segments from a prediction model that uses only publicly available characteristics. Providing a 'synthetic' measure could enhance finance and accounting research in this area that primarily rely on public data and open new lines of inquiry. This approach is similar to Hanlon, Lester, and Verdi (2015), who develop a prediction model to identify the amount of foreign cash that is held by U.S. MNE (also captured in BEA survey data) using publicly observable information alone and then use this measure to study the effect of foreign cash on abnormal returns to M&A. Other examples of studies that contribute prediction models for outcomes from government or proprietary data include Wilson (2009), Lisowsky (2010), and Hepfer, Wilde, and Wilson (2023). Wilson (2009) and Lisowsky (2010)

predict whether a firm participates in a tax shelter (from IRS data) using publicly available information. Hepfer, Wilde, and Wilson (2023) similarly predict whether firms have a captive insurance entity (from proprietary data) using publicly observable characteristics. With the aid of these prediction models, studies that have cited these papers have been able to examine these constructs in other periods and settings to advance the tax literature (e.g., Kim, Li, and Zhang 2011; Chyz 2013).

II. BACKGROUND ON SEGMENT REPORTING AND THE CONSEQUENCES OF SFAS NO. 131

A long line of literature in finance and accounting presents theoretical reasons why operating in multiple lines of business can fundamentally change firm performance, as alternative lines of business may evolve differently in alternative economic environments (FASB 1997). Some of this research shows it can correspond with advantages, such as better internal capital markets, smoother income streams, and greater tax savings.¹⁴ However, other work shows diversification can create disadvantages as well, such as cross-subsidization of failing segments and overinvestment in a poorer set of investment opportunities.¹⁵ Accordingly, information about different lines of business can be valuable for evaluating firm performance, and aggregation of operating information across these lines may inhibit financial statement users' ability to make these evaluations.

At the same time, the accounting standard setters recognize firms' concern that too much disaggregation of a firm's profitability could release valuable, proprietary information to competitors (Botosan and Stanford 2005). Apple, Caterpillar, and Dell, for example, have argued additional segment disaggregation to the public would give away an important part of their competitive advantage (Ettredge, Kwon, and Smith 2002; Greenberg 2006; Arya, Frimor, and

¹⁴ For example, multiple divisions can correspond with internal capital markets that are more efficient than external capital markets, enable additional investment, and provide tax benefits (Weston 1970; Myers 1977; Lewellen 1971, Wentland 2023).

¹⁵ Meyer, Milgrom, and Roberts (1992) and Stulz (1990) predict operating in multiple lines of business may damage firm value by encouraging cross-subsidization of failing business segments with poor investment opportunities. Berger and Ofek (1995) suggest that diversified firms' value typically trades at a discount. However, later studies suggest other factors may contribute to differences in value (e.g., Campa and Kedia 2002).

Mittendorf 2010). For decades, standard setters have taken numerous steps to try to balance the needs for disaggregated product line and industry information and these concerns regarding proprietary information.

Historically, there have been several major shifts in how firms disaggregate this information for the public. In 1967, the Accounting Principles Board (APB) began by issuing a call for firms to voluntarily report information about product lines (Statement No. 2). Shortly afterward, in 1969 and 1970, the SEC began making line-of-business information for annual filings and registration statements mandatory. By 1976, the FASB sought to provide a more standardized framework for how to identify reportable segments along existing industry classifications such as the Standard Industrial Classification (SIC), geography, or quantitative thresholds based on sales by issuing SFAS 14, *Financial Reporting for Segments of a Business Enterprise*. It also provided a framework for the role of management's judgement in identifying reportable segments along these dimensions (Schipper 2022).

The next major shift in public segment reporting followed with the issuance of SFAS No. 131 (now ASC 280), *Disclosures about Segments of an Enterprise and Related Information*, in 1997. This change shifted segment reporting from an *industry approach* using more traditional classifications of industry breakouts toward a *management approach* to allow firms more discretion with identifying reportable segments based along the lines in which management internally makes decisions for the business. Prior academic research studying this change concluded that the standard increased the ability of the market to predict firm earnings (Ettredge et al. 2005) and improved disclosure about the diversity of multiple segment operations (Ettredge et al. 2006). However, as firms incorporated more management-specific information into their assessments of reportable segments, prior research also recognized there could be an important tradeoff with this discretion where segment reporting would become less comparable (Berger and Hann 2003; Ettredge et al. 2005) and potentially less useful for investors (Durney, Gee, and Wiebe 2025).

Consequences for comparability are important given investors, regulators, and academics

outline the critical role it plays for the usefulness of financial statements. For example, the FASB (1980, p. 40) explains that “investing and lending decisions essentially involve evaluations of alternative opportunities, and they cannot be made rationally if comparable information is not available.” Further, the reliance on financial ratios in financial statement analysis textbooks emphasizes the idea of comparable metrics for evaluating performance over time and across firms. While the prior literature above discussed concerns about comparability, it did not empirically examine the effect SFAS No. 131 had on comparability. The only study that has empirically examined the effect of the standard on comparability is Edmonds et al. (2018). They examine a single year before and after the policy change (1997 and 2002, respectively) and observe lower financial statement comparability following SFAS No. 131 adoption. Based on these results, the study concludes this reflects a predicted cost of the standard outlined above. However, as with other studies on SFAS No. 131, Edmonds et al. (2018) relies on publicly reported segment reporting changes alone, requiring the implicit assumption that underlying operational changes (private reporting) did not alternatively explain the observed differences in comparability.

Evidence from other research on SFAS No. 131 suggests plausible reasons why this assumption may not hold. Le et al. (2022) provide evidence that SFAS No. 131 altered firm restructuring activity in numerous ways. They find that firms were less likely to complete acquisition deals, but also that firms were less likely to spinoff components of the firm, which suggests that the standard may have altered operational diversification strategies. Further, Cho (2015) provides evidence that SFAS No. 131 influenced capital allocation differently for alternative types of firms. In terms of greenfield investment, their results show that SFAS No. 131 improved capital allocation efficiency in some firms, but that capital allocation efficiency declined in other firms. Thus, it is an open empirical question whether operational changes following SFAS No. 131 substantively altered the comparability of financial statements on average. Further, it is an open question whether accounting for operational changes influenced the role of additional information disaggregation with the standard.

III. SAMPLE CONSTRUCTION AND DESCRIPTIVE STATISTICS

III.A. Sample Construction for the Difference-in-Differences Analysis

We start with an initial sample of U.S. companies for the three years preceding and following the implementation of SFAS No. 131 for our diff-in-diff analysis (1995-2001, excluding 1998) and require that the firm has a single EIN for later merging with government survey data on private reporting. We use this data for comparability measures, public reporting information, and financial variables used for controls. We exclude duplicate firm-year observations (e.g., when a firm changes its fiscal year end) and observations without the comparability measure, without segment data to identify public reporting of industry information (based on SIC code),¹⁶ or with non-positive assets and sales. We then exclude companies where we cannot observe whether there is a public industry reporting change following the standard, requiring a firm has at least one observation in both the pre- and post-standard implementation periods. Similar to Hanlon et al. (2015), who also use BEA data merged with Compustat data, we then restrict the Compustat sample to large, U.S. multinationals with greater than \$100 million in assets and above median absolute pre-tax foreign income for the period. This results in a Compustat sample of 4,806 firm-year observations for large U.S. multinational firms with the public reporting information required in the analysis.

We then use firm-level panel data from the BEA Annual and Benchmark Surveys of U.S. Direct Investment Abroad to observe confidential industry reporting information for U.S. multinationals. We start with an initial sample of U.S. multinational firms for the same period (1995-2001, excluding 1998), eliminating duplicate firm-years and requiring the firm has one EIN and necessary information for the private industry reporting change measure (having at least one observation with industry information in both the pre- and post-standard implementation periods).¹⁷ We merge this sample with the Compustat sample above, which gives a final sample

¹⁶ Similar to Wentland (2023), we require that the sum of segment assets and sales be within 10 percent of Compustat firm-level assets and sales for completeness given SFAS No. 131 uses a 10 percent threshold to identify additional reportable segments.

¹⁷ To maintain a stable definition of industry over the period, we use three-digit SIC for the diff-in-diff analysis period.

of 1,022 firm-year observations that have both the private and public reporting change information and other analysis information required in the analysis.¹⁸

III.B. Descriptive Statistics and Initial Evidence for the Role of Confidential Reporting

We report descriptive statistics for the 1995-2001 diff-in-diff sample in the first set of columns in Table 2, which highlights several key takeaways about the set of firms for which the analysis is most relevant. For comparison to the broader Compustat population, we report the corresponding public information variable statistics for the broader set of large, multinational firms in Compustat (prior to the merge with the BEA survey data) and for the full Compustat sample for the diff-in-diff period. Overall, the analysis sample is similar to large, multinational Compustat firms, as these firms tend to be larger, with lower sales growth, and are slightly older.¹⁹ Considering our interest in distinctions with how firms report on multiple lines of business, the focus on this population is particularly relevant as larger, older firms are those most likely to be involved in multiple lines of business. This can be seen across columns in Table 2 with the public industry reporting as the samples with larger, older firms have a higher mean number of industries publicly reported.

In terms of the confidential reporting, the first striking takeaway from Table 2 is the difference in the statistics between the number of industries privately and publicly reported. Whereas the mean number of industries publicly reported is 1.703 (based on three digit SIC),²⁰ the number of industries these firms report privately is 4.456. Thus, consistent with Bens et al. (2011)

In the last few years of this analysis, the BEA surveys used NAICS codes instead of SIC codes. We convert four-digit NAICS codes in those years with survey data to three-digit SIC using a concordance between 1997 NAICS and 1987 SIC.

¹⁸ This merged Compustat-BEA sample (1,022 observations) is approximately 21.27 percent of the Compustat sample (4,806), which is comparable to the attrition Hanlon et al. (2015) observe with a similar Compustat-BEA merging process.

¹⁹ While we include a control for whether the firm has a loss in the current year in our analysis, we do not report descriptive statistics for this variable in Table 2 as the BEA disclosure guidelines suggest this could add disclosure risk. That said, we are able to report descriptive statistics for return on assets to give a sense of the typical profitability of firms in these samples.

²⁰ We use three-digit SIC as the BEA surveys in the later part of the analysis period switch from reporting in SIC to reporting industries in NAICS. Three-digit SIC was the finest level of industry granularity possible with a high degree of overlap in a crosswalk to four-digit NAICS in those later years. We use this same three-digit SIC code classification for all years in the diff-in-diff analysis to maintain a consistent benchmarking for industry definition in all years of this analysis. Our internal checks suggest that our private industry count is not particularly sensitive to this crosswalk.

and Botosan et al. (2021), firms have additional industry information that is not reported publicly.²¹ To avoid issues with the confidential industry-count trend mechanically reflecting changes in industry classification, we use a stable three-digit SIC definition throughout the DiD period and end the DiD sample before the 2002 NAICS revision.

In Figure 1, we then track the average private and public reporting of industries in the years surrounding the implementation of SFAS No. 131 (effective for fiscal years beginning after December 15, 1997) to see how this evolves over time. Consistent with prior research focused on the public reporting change following SFAS No. 131 (Ettredge et al. 2005), which finds some firms that reported only one industry publicly pre-SFAS 131 began reporting multiple industries post-SFAS 131, there appears to be a slight increase in the average public reporting on industries for MNEs. However, what is striking in the figure is that the change in public reporting focused on in prior literature is dwarfed by the decline in industry operations reported privately post-SFAS 131.²²

To understand more background about how privately reported operations changed following SFAS 131, in Table 3 we focus on firms that had a change in private reporting and consider: (1) whether this change moved them closer or further from operational norms in their industry and (2) whether the direction of the change in private reporting had a symmetric or asymmetric relationship with industry norms. We start by calculating the average number of industries privately reported by firms in each SIC 3 in the pre period and then the post period. We then took the absolute value of the difference (distance) between the number of industries firm i reported in the pre (post) period relative to the pre (post) period industry average. We looked at firms with a change irrespective of the direction of the initial change (i.e., either an increase or decrease in private reporting) and then separately for firms with increases versus decreases. For each comparison, we observed that the “distance” decreased between the firm and the average number of industries privately reported in its primary industry (SIC 3). This was true regardless of

²¹ Bens et al. (2011) use alternative government agency data from the U.S. Census Bureau on plants.

²² In untabulated analysis, we show that the figure is very similar if we use alternative BEA or Compustat samples.

whether the direction of the change in private reporting (i.e., a symmetric movement toward norms). Thus, in general, it appears that firm operations more commonly moved closer to their industry peers following the new standard.

To the extent that firms moved operations closer to their industry peers, this suggests that operational changes following SFAS No. 131 may have actually *enhanced* comparability on average. This prediction is in the opposite direction of the public segment reporting channel effect predicted with SFAS No. 131 in prior literature (Berger and Hann 2003; Ettredge et al. 2005), where the standard was expected to come with worse comparability as a trade-off for other benefits of the standard. Thus, the descriptive analysis in Figure 1 and Table 3 provide initial evidence that the operational diversifying/refocusing activity following SFAS No. 131 could play an important role in interpretations about comparability with the standard.

III.C. Sample Comparisons with the Literature

Before turning to the primary diff-in-diff analysis, we next also wanted to evaluate the degree of overlap in our merged Compustat-BEA sample relative to prior research focused on public reporting alone with the standard as a baseline. To do this, we prepared a more detailed breakout of pre-SFAS 131 and post-SFAS 131 reporting classifications following the style in Ettredge et al. (2005) and report these values in Table 4 Panels A.²³ Despite substantial differences in sample size with Ettredge et al. (2005) that focuses analysis on a much broader set of Compustat firms, we observe a very similar pattern (in terms of percentages) for the different classifications of public reporting for our sample.²⁴ This is reassuring, as it allays a potential concern that public reporting patterns might have been different for our smaller BEA-merged group of firms and that this could have played a role in results with our analysis.

²³ The breakouts from Table 2 in their paper.

²⁴ For example, we observe that 76% (86%) of single firms (multiple industry firms) continued publicly reporting single industry operations (multiple industry operations) following SFAS No. 131. Similarly, Ettredge et al. (2005) find 74% (87%) of their pre-131 single sample (multiple industry sample) retain these same classifications. Further, we observe that 24% of single industry firms switched to publicly report multiple industries post-131 and that 14% switched from publicly reporting multiple industries to publicly reporting a single industry. Ettredge et al. (2005) find that 26% of their sample switched from reporting a single industry to multiple industries and that 13% switched from reporting multiple industries to a single industry.

Next, we prepared an analogous breakout from Panel A for private reporting in Panel B to understand the differences in information when using private information for identifying information on multiple lines of business. A few takeaways stand out when comparing the private reporting breakouts in Panel B to those in Panel A. First, a much higher percentage of firms were reporting operations in multiple industries privately before SFAS 131 (84% did privately while only 36% did publicly) and a large proportion of firms (97%) continued to privately report multiple industries after SFAS 131. Thus, consistent with the descriptive statistics in Table 2 and Figure 1,²⁵ Table 4 also suggests firms tend to report operations privately in a greater number of industries both in the period before and the period after the standard.

In Table 4 Panel C, we then report a breakout to better understand how the *change* classifications differed between public and private reporting surrounding SFAS No. 131. The breakouts in Panel C highlight several important points. More than half of the sample had a change in the number of industries they privately reported (27 percent with a decrease and 34 percent with an increase). This stands in contrast with the implicit assumption in the segment literature that underlying operations did not change. Further, the private reporting changes were not limited to just one type of public reporting classification (i.e., when public segment reporting decreased, had no change, or increased). Instead, for *all* three of these public reporting change classifications, more than 50 percent of the firms in each classification had a change in the number of industries privately reported.²⁶ Further, in the two classifications when there was a change in public reporting (i.e., having an increase or a decrease in public segment reporting), the direction of the change in confidential reporting did not always move in the same direction as the public reporting change.²⁷

²⁵ Figure 1 reports the decline in yearly average number of industries privately reported while Table 4 Panel B only distinguishes if a single industry versus multiple industries are reported. The combination of the two sets of statistics suggests that multiple industries were reducing the number of industries they operated in on average but that they still maintained operations in more than a single industry.

²⁶ For the public reporting decrease classification, 84% of the firms (58%+26%) had a change in private reporting. For the public reporting increase classification, 66% (21%+45%) had a change in private reporting. For the public reporting no change classification, 56% (24%+32%) had a change in private reporting.

²⁷ For the firms that decreased the number of industries reported, 27% of these firms increased the number of industries they reported privately. For the firms that increased the number of industries reported, 21% decreased the number of industries they reported.

While the above, at first glance, suggests that public information alone is incomplete, this descriptive evidence does not evaluate whether the additional private reporting information was substantial enough to matter for accounting attributes that investors or the FASB would care about. Thus, in the next section, we use different combinations of public and private reporting changes to assess whether these differences can matter for at least one important accounting attribute, financial statement comparability.

IV. RESEARCH DESIGN

In our primary analysis, we employ a difference-in-differences (DiD) method in the three years preceding (1995-1997) and following (1999-2001) the implementation of SFAS 131 to investigate differential changes in comparability across firms.²⁸ The adoption of SFAS No. 131 is a setting in which prior research has interpreted changes in public segment reporting as changes in disclosure disaggregation. The goal of our design is to examine whether (or to what extent) those inferences change once we separately observe confidential changes in firms' underlying industry operations. Specifically, we estimate the following DiD specification:

$$comp_{it} = \beta_0 + \beta_1 post_t + \beta_2 ch_pub_i + \beta_3 ch_priv_noch_pub_i + \beta_4 post_t \times ch_pub_i + \beta_5 post_t \times ch_priv_noch_pub_i + \sum \beta controls + \beta_k + \epsilon_{it} \quad (1)$$

where $comp_{it}$ is defined following De Franco et al. (2011), which evaluates how similar a firm's earnings from its financial statements are relative to the other firms in that industry expected to face similar economic conditions.²⁹ The measure uses the 16 previous quarters of data to assess how close predicted earnings for firm i and each other firm j in the same industry reflect the same return (from firm i) where the return is used to represent common economic conditions faced by

²⁸ We end the post period in 2001 because of the role of industry classifications in BEA surveys at this time. In 1999, the BEA surveys switched to using four-digit NAICS industry classifications in place of the SIC-based codes used in prior surveys. For 1999-2001, crosswalks between three-digit SIC industry classifications and four-digit NAICS industry classifications had a very high degree of overlap. However, the degree of overlap decreased in 2002, when NAICS re-evaluated its industry classification list, a process that is done once every five years. By focusing on a balanced three-year pre and post period that ends in 2001, we ensure that the NAICS code classification change in 2002 does not affect our diff-in-diff interpretations.

²⁹ See Gross and Perotti (2017) for a more detailed review of the comparability literature and discussion of comparability measures, including limitations of the De Franco et al. (2011) measure.

both firms.³⁰ We set $post_t$ equal to one for fiscal years starting after December 17, 1997, when SFAS 131 became effective, and set it equal to zero otherwise.³¹ We incorporate industry fixed effects (β_k) at the SIC1 level in most specifications, but explore others (SIC2 and SIC3) for robustness.

We then create two time-invariant firm (group) variables that capture a firm’s public and private reporting response surrounding SFAS 131. The first, ch_pub , is an indicator for whether or not the firm changed the number of industries publicly reported following SFAS 131, and $ch_priv_noch_pub$ is an indicator for when the firm changed the number of industries it reported privately but did not change the number of industries reported publicly. Intuitively, when both terms are incorporated in the analysis, the category left off for comparison with these two groups are firms that did not change either form of reporting following SFAS 131. By contrast, evaluating ch_pub alone in the analysis without including a term for $ch_priv_noch_pub$ means that the benchmark group for ch_pub contains not only firms without any reporting changes but also firms that had private reporting changes but without public reporting changes. We interact each of these time-invariant firm indicators with $post_t$ to test whether comparability differed following SFAS 131 after distinguishing these groups. We use controls from De Franco et al. (2011) to account for variation in characteristics that influence the firm’s economic conditions, including industry and year fixed effects. We also consider additional controls from Conaway (2022) that capture differences between firm i and other firms in its primary industry. We provide a detailed description of the variables in eq. (1) in Appendix A.

Before turning to results with the equation above, we consider an important assumption

³⁰ We recognize that using a dependent variable that contains lagged earnings and returns means that some of the pre-period will contribute to the measurement of the dependent variable and that this can bias the interaction terms of interest (β_4 and β_5) toward zero. We address this in two ways. First, we exclude 1998 from the analysis to allow additional quarters in the post period to contribute to the comparability measure. Then, in Online Appendix Table A1, we re-estimate the analysis retaining only 2001 for the post period as this is where the pre-period contributes least to the post-comparability measure. Despite the fact that we lose roughly a third of the sample with this approach, results are very similar in terms of both economic magnitude and statistical significance. Finally, with the narrow time window of the panel, we elected not to estimate a dynamic panel given the likelihood of Nickell bias issues. The diff-in-diff estimator in panels with very small number of time periods are unlikely to exhibit much of a dynamic or autoregressive bias that would otherwise occur in longer panels with a persistent Y (Cornwall and Wentland 2025).

³¹ For robustness, in some specifications we incorporate year fixed effects (β_t), which we denote in our results tables.

with a difference-in-differences design; in our case, whether comparability would not have trended similarly for the different groups absent the standard change. While the parallel trends assumption is not directly observable, like many DiD studies, we examine differences in the periods prior to treatment, re-estimating eq. (1) above replacing the $post_t$ with one and two post terms for $t-1$ (1996) and $t-2$ (1995) and interact these alternative post terms with the different reporting groups. We report the corresponding analysis in Online Appendix Table A4. Overall, we observe that comparability was not evolving differently for those with a public reporting change ($ch_pub = 1$) relative to those without a public reporting change in the two years leading up to the standard, which suggests that it is unlikely these firms were altering their public reporting ahead of the when the standard became affective.

Alternatively, for those with an operational change but no public reporting change following the standard ($ch_priv_noch_pub = 1$), there is some indication that firms may have already begun to change operations in anticipation of implementation. While not statistically significant, the point estimate in $t-1$ (1996) is roughly half what we observe in the main analysis but is close to zero for $t-2$ (1995). In the case of operational adjustments, this does not necessarily undermine the parallel trends assumption because, unlike public reporting changes, the operational adjustments are not mandated for a particular date (i.e., fiscal years beginning after Dec. 17, 2017). Instead, firms may begin responding to the announcement of standard characteristics as soon as information about the standard seems likely to be applied. In this case, observing some anticipatory behavior in 1996 ($t-1$), but not significantly in 1995 ($t-2$), may reflect firms beginning to adjust their operations when the exposure draft came out in January of 1996. This evidence is consistent with Le, Oliver, and Tan (2022),³² which found that firms began changing their operations in 1996 ($t-1$), ahead of the standard's effect date. Hence, the coefficient for the 1995 ($t-2$) interaction may more credibly capture the pre-event period relevant for

³² Specifically, Le et al (2022) say that, “We believe that firms responded to the new regulation providing they were aware of it, in which case, they may remove some of their segments when they first learned about it. Therefore, we used January 2016, rather than December 2017, as the event year” (p.13-14).

evaluating the private reporting on operations, showing no significant difference across these two groups prior to 1996.³³

V. RESULTS

V.A. Baseline Analysis (with Public Reporting Changes Only)

We begin by reporting analysis of how comparability changed following the standard change when considering public reporting alone in line with approaches in prior literature. In the first three columns of Table 5, we perform this analysis on a broader sample of Compustat large MNC firms (4,806 firm-year observations) before requiring the merge with the BEA data. The last three columns of Table 5 then re-estimate this analysis on the merged sample but, again, only evaluate estimates when accounting for the public reporting classifications alone. The point estimate on the coefficient of interest, the interaction of the post period indicator and the indicator for whether there was a public reporting change (*postXch_pub*), is very similar across all columns ranging from -0.186 to -0.278. While the estimates on the smaller merged sample (roughly 78 percent smaller) are not statistically significant at conventional levels, the estimates are statistically significant on the interaction term at the 5 percent level or better when estimated for the broader sample, which is more in line with studies using publicly available data alone to make inferences about the standard. If the analysis for the eq. (1) stopped here with public reporting information alone, then the negative coefficient estimates from Table 5 might be interpreted as reinforcing the prediction that firms faced a cost with their reporting changes in terms of lower financial statement comparability following SFAS No. 131, which is in line with the predictions in Berger and Hann (2003) and Ettredge et al (2005).

However, as we will show, this interpretation is likely incomplete without consideration for private reporting classifications. Recall that when analysis focuses on public reporting changes (*ch_pub*) alone, such as in Table 5, it retains two groups as its counterfactual: (1) firms that had

³³ While we walk through these tests to provide context for the period leading up to the standard change, we caution against too strong of an interpretation of these tests, given the insights from Roth (2022) and their critique that pre-trends tests may be underpowered. In untabulated results, we explored alternative approaches proposed by Roth (2022) and Rambachan and Roth (2023), finding similar conclusions as the simple approach we report in Table A4.

no changes in either public or private reporting (i.e., ‘non-diversifier, non-disaggregator’ firms) and (2) firms that had a change in privately reported operations but that did not change their public reporting (i.e., ‘diversifier, non-disaggregator’ firms). If the second group of firms does not exist, or their operational changes that are not disclosed are not substantive, then underlying operational differences are unlikely to contribute to estimates with *ch_pub*. In this case, attributing the comparability differences with *ch_pub* to public reporting changes alone is reasonable. Alternatively, if the second group of firms *does* exist, and the unreported changes are meaningful, then including them as a counterfactual should confound the negative association between the interaction with *ch_pub* and comparability and Table 5. Recall from Table 3 that we know firms that changed their private reporting were generally moving their industry operations toward their industry peers. As a result, by including this second group above as part of the control group, this makes the counterfactual comparability benchmark artificially low in the post period. Hence, it would make it more likely to find a negative association between the *ch_pub* interaction in column (2) with comparability.

V.B. Main Diff-in-Diff Analysis (with Public and Private Reporting Changes)

In Table 6, we evaluate this possibility by incorporating a term for the group with a private reporting change, but no public reporting change (*ch_priv_noch_pub*), and an interaction for this term with the policy period (*postXch_priv_noch_pub*). However, we also include the public reporting analysis result from Table 5 column (4) for ease of reference with comparisons for this analysis. Consistent with the explanation above, we observe that the point estimate for the public reporting term interaction substantially diminishes (i.e., from -0.2782 to -0.0253). Further, we observe that the interaction term for the group with the confidential reporting change but no public reporting change (i.e., diversifier, non-disaggregator) is positive and significant in line with the expectation that an operational shift toward the industry norms would enhance comparability following SFAS No. 131. Compared to the sample average, this 0.4738 jump associated with the interaction term for *ch_priv_noch_pub* represents a 21 percent increase in comparability. Another valuable insight from the private reporting interaction term in column (2) is that it speaks to

whether the incremental information in private reporting not captured with public reporting is meaningful for understanding accounting attributes and how comparability changed following the new standard.³⁴

Next, we expand the set of private reporting groups evaluated for a more complete picture of how comparability changed with different types of private reporting following SFAS No. 131. In Table 6 column (3), we also consider the direction of the private reporting changes (increases or decreases). The results in this column continue to show the positive, significant coefficient estimate for firms that altered the number of confidentially reported industries but that did not alter public reporting (i.e., diversifier, non-disaggregators). Importantly, we find an increase in comparability for both groups that increased or decreased the number of industries they operated in based on private reporting (*postXincrease_priv_noch_pub* and *postXdecrease_priv_noch_pub*), which supports the symmetric movement toward peers we observed in Table 3.

V.C. Robustness, Unobservables, and Additional Analysis

In the remaining columns of Table 6 (columns (4)-(6)), we show inferences are similar when we re-estimate column (2) incorporating baseline controls from De Franco et al. (2011), industry fixed effects, and a fuller set of controls from Conaway (2022).³⁵ In Online Appendix Tables A2 and A3,³⁶ we perform a host of robustness tests, including evaluation of alternative combinations of industry fixed effects, controls, and narrower definitions peer groups,

³⁴ To be clear, this is an important but secondary result of the paper; so, we caution against too strong of a causal interpretation of this particular result as an effect of operational changes on comparability, given our current research design. Additional empirical work would need to be done (e.g., evaluating an ex ante treatment measure or predetermined exposure based on pre-SFAS public/private mismatch and further design changes) if this causal effect would be elevated as the main focus of the paper. Rather, the current paper emphasizes operational structure as a potential cofounder of analyses of publicly reported segment changes, based on similar designs from prior literature.

³⁵ When year fixed effects are included in columns (4)-(6) in Table 6, the Post term drops due to collinearity. Our DiD results are generally robust to whether we use either year fixed effects or a coarser Post term in the design.

³⁶ We do not incorporate firm fixed effects because the treatment indicators for different reporting groups are time-invariant firm classifications, and firm fixed effects would absorb the level effects of these group indicators (although the post-period interaction terms would remain identified). The disclosed specifications therefore report industry fixed effects and group main effects so that baseline group differences and post-period differential changes can be presented in a single table. Overall, the private reporting change interaction terms are robust to many alternative specifications despite the relatively small sample size (Online Appendix Table A2). While the public reporting change interaction term is not statistically significant in all specifications, this is consistent with the point raised in the discussion above regarding the noisy counterfactual issue when relying on public reporting classifications alone. Firm fixed effect specifications are a natural robustness exercise for future disclosure-cleared analyses.

respectively. In untabulated tests, we also verify results are robust to accounting for public and confidential (private) reporting for geography.³⁷ Critically, note that the coefficient estimates for the interaction term (*postXch_pub*), which is associated with publicly reported segment reporting changes after SFAS No. 131, is consistently near zero, relatively stable, and statistically insignificant across specifications in Tables 6 and A2. If anything, the point estimates move slightly positive (ranging from -0.02 to 0.11) as we add controls or incorporate finer variations of fixed effects into the specification. Taken together, the evidence suggests that the private segment information (as a proxy for operational differences) represents a substantial confounder for analysis of publicly reported segment changes and outcomes like financial statement comparability.

In Table 6 columns (4)-(6), we also evaluate the role of remaining unobservables. Given that Bens et al. (2011) find a number of different characteristics that can contribute to a firm's decision to publicly report segment data, it seems reasonable to evaluate the collective role of remaining unobservables that may not be captured by the baseline and fuller sets of observables. To do this, we perform a test from Oster (2019), which has been used in other recent accounting studies to evaluate the relative importance of unobservables as an alternative driver of results (e.g., Bao et al. 2022; Fox and Wilson 2023; Fox et al. 2026).³⁸ The Oster (2019) procedure produces a coefficient of proportionality, δ , to evaluate how important the set of additional unobservable determinants would have to be relative to the set of observable determinants already in the specification that would make the coefficient of interest indistinguishable from some test value parameter (typically, the null value of 0).

³⁷ We do not focus on interpretations of public and private reporting of geography in part because the dimension of geography in public segment reporting and BEA surveys are not standardized. For instance, public segments can represent a country, a state, or a continent while BEA surveys focus on the country as the dimension for geography. By contrast, industry codes help to standardize the scope of information comparisons between public segment disclosures and the information in BEA surveys. We include a test to control for variation in reporting about geography just to make sure our results with industry reporting are not instead alternatively explained by general 10-K vs. BEA reporting differences that are not related to industry specific information.

³⁸ While we follow the omitted-variable-bias sensitivity literature using Oster's (2019) implementation, we acknowledge that alternatives such as Cinelli and Hazlett (2020) offer a complementary framework that could be explored in future work.

Oster (2019) proposes an absolute value of delta of at least one as an indication that it is unlikely that unobservables alternatively drive the result. This value for delta suggests that collectively the remaining unobservables would have to be at least as important for the outcome of interest as the collective observables already accounted for in the specification. Applying the procedure to our analysis in columns (4)-(6) of Table 6 we use the parameters proposed in Oster (2019) where we assess the analysis in comparison with a $\beta = 0$ and the $R^2_{\max} = 1.3 \times \text{adjusted } R^2$. With this procedure, we find that the δ values for the positive coefficient estimate on the private reporting interaction term (*postXch_priv_noch_pub*) exceeds the threshold of one, ranging from 1.877 to 5.363 across columns (in absolute value terms).³⁹ Thus, the δ values suggest it is unlikely unobservables represent an alternative driver for the coefficient estimates we observe for the private reporting change.⁴⁰ Overall, the analysis in Table 6 combined with the robustness analysis in the appendix support the original inference that private reporting of industry operations likely played an important role in financial statement comparability following SFAS No. 131.

VI. PREDICTING CONFIDENTIALLY REPORTED INDUSTRIES

Throughout the paper, we have documented that firms tend to report a different number of industries in confidential survey data reported to BEA compared with their public filings. If this privately reported information is useful for research purposes, but is only available to internal researchers at BEA and those serving as Special Sworn Research Consultants at BEA (i.e., those legally allowed to access the raw data), then what other alternatives do external researchers have if they can only access publicly available data? In this section, we present an option that would provide researchers a predicted version of the BEA count of confidentially reported industries that varies by firm and year (has a panel data structure). This may help researchers better

³⁹ Given the interaction terms on public reporting are already indistinguishable from zero, this test for evaluating how important observables would have to be to make it indistinguishable from zero is not relevant. Consistent with this, if we perform the Oster (2019) test for the public reporting interactions in these columns, the absolute value of delta values are much smaller than one and nearer to zero.

⁴⁰ Intuitively, the values for δ with the private reporting interaction term suggest that collectively the unobservables would need to be as important as 1.877 to 5.363 times more important (in absolute value) than the standard observable determinants of comparability in prior literature (De Franco et al. (2011) and Conaway (2022)) that we account for in our analysis to result in a private interaction coefficient estimate that is indistinguishable from zero.

approximate firm operational structure alongside how much of this information is reported publicly. Our approach leverages public information from Compustat to predict the number of industries reported privately to BEA, without revealing the actual private survey information reported by firms. This concept is similar to synthetic data constructed by statistical agencies in the U.S. Government, which are meant to represent the underlying restricted data while concealing potentially sensitive information such as the Census Synthetic Survey of Income and Program Participation (SIPP).⁴¹

VI.A. Predictive Modeling Methods and Sample Restrictions

We pursue two different prediction models to construct a synthetic measure of industry segments privately reported to BEA (which we denote simply as “privately reported” in the tables and figures below). The first approach employs a simpler parametric model, a Poisson regression, to predict the confidentially reported number of segments. A parametric model is useful, even if it is not the best performing prediction model, because the coefficients can be reported to the general public. These coefficients can then be used by researchers to generate the predicted BEA-measure across all possible values (i.e., out of sample) using only publicly available information (via Compustat and other services that compile financial accounting information from public firms). Our second approach employs a machine learning model, Extreme Gradient Boosting (EGB), to predict the same synthetic BEA measure. While the machine learning approach has improved predictive capability across a number of metrics (which we discuss in greater detail below), it is still a non-parametric model that requires access to the internal BEA data to fully replicate predicted values out-of-sample. For external researchers interested in the second measure, we make the code available so that researchers who obtain Special Sworn Status at BEA

⁴¹ This exercise has a dual benefit to the public and the BEA. Statistical agencies use alternative data sources to model information reported on surveys to improve accuracy in the statistics using this data and help the agency better understand reporting anomalies, missing information, and various interpolations used internally. Statistical agencies like the BEA who administer surveys must also continuously reassess the response burden associated with the costliness of respondents filling out the surveys, often by evaluating and comparing data sources that collect similar information. However, we should be clear that the synthetic proxy and any results from this study are not an official data product from the BEA or U.S. Government.

can replicate these predictions for further study. For the purposes of this paper, however, the EGB model is primarily used as a benchmark for comparison purposes, and we summarize the methods in more detail in Online Appendix B.

For the prediction exercise in this section, we start with a dataset of U.S. multinationals in Compustat for 2002-2021 that requires non-missing values of public measures of determinants for operational diversification based on Campa and Kedia (2002) and has positive values of assets and sales, which results in a sample with 24,678 firm-year observations (3,102 firms).⁴² We then link this with the BEA data and identify the portion of this sample that also has the private industry variable (*ind private_{it}*) available, which gives a sample of 12,713 firm-years (1,672 firms). The sample period begins in 2002 based on the role of industry codes used in the surveys.⁴³ The sample ends in 2021, which was the last year the BEA survey data was available in electronic form at the time we conducted the analysis. Figure 2 illustrates the number of industries (based on four-digit NAICS) reported privately in BEA surveys (the solid black line) and publicly (the dashed black line) in the years after our diff-in-diff (2002-2021) period.⁴⁴

In the Online Appendix, we provide additional details for how these models are constructed along with additional analysis comparing the models and their predictions (see Appendix B.2 Prediction Results and Evaluation, and Appendix Tables A5 and A6). While it should not be surprising that the machine learning model outperforms the Poisson regression, the main question should be: are these synthetic measures reasonably representative of the actual number of industries privately reported to BEA? In Figure 3, we plot the smoothed kernel densities of the

⁴² We classify an observation as relating to a multinational if it reports non-zero pre-tax foreign income in Compustat. Campa and Kedia (2002) uses the following firm, industry, and year level characteristics: log(assets), earnings before interest and taxes/sales, capital expenditures/sales, two lags and recent averages of these prior three variables from Compustat, whether the firm is listed on the S&P (using CRSP), the number of firms in the company's primary industry that publicly report multiple industries (excluding the firm itself) and the percent of sales in the firm's primary industry that come from firms that publicly report multiple industries (excluding the firm itself) from SDC Platinum, the number of M&A deals for the year and the sum of transaction values for M&A deals for the year from SDC Platinum, is listed on a major exchange based on Compustat (codes 14, 11, and 12), and whether the current year and prior year are part of a contractionary period in the NBER business cycles.

⁴³ The surveys switched from SIC to NAICS in 1999 and 2002 was the first NAICS classification revision after this change in the survey. NAICS classifications are updated every 5 years.

⁴⁴ The sample used in Figure 2 is limited to firm-years with non-missing BEA and Compustat industry count data, positive assets, positive sales, and non-zero pre-tax foreign income.

privately reported industries (black solid line), public reported industries (black dotted line), and corresponding densities for synthetic measures of privately reported segments (EGB as the blue dotted line and Poisson as the red dotted line). Consistent with the findings from Appendix Table A6, the distribution of predicted values from the EGB model is closer to the actual reported values than those of the Poisson regression. However, compared to the publicly reported number of industries, it is clear from Figure 3 that the distribution produced by the Poisson model is a much closer fit to the distribution of privately reported industry operations.⁴⁵ The main takeaway from this evaluation is that the simple Poisson regression can provide a reasonable approximation of the number of industries firms report to BEA, functioning as a much closer proxy to a firm’s privately reported industry structure than its number of publicly reported industry segments. Thus, the coefficients reported in Appendix B, Equation A1 should allow future researchers to construct a reasonable prediction of privately reported segments using publicly reported data and conduct analysis where this synthetic measure would be relevant.

VII. CONCLUSION

Different streams of literature conclude that a firm’s operational structure⁴⁶ and the degree of information disaggregation firms report about this structure are important for many different aspects of capital markets.⁴⁷ However, standard setters and researchers have raised concerns about publicly reporting segment information that is often used (by necessity) to study these important constructs (FASB 2016; Botosan et al. 2021). Motivated by this important challenge, this study uses the number of industries reported to the U.S. BEA in its mandatory surveys of U.S. multinationals in comparison with the number of industries it publicly reports in firm financial

⁴⁵ In untabulated regressions, we re-ran the Poisson regression on the unconstrained training sample and included polynomial terms for the top five features by gain in the XGBoost model. This improved the overall predictions of the model an average of two percent over the three reported loss functions. While many of the added polynomial terms were highly significant, indicating relevant nonlinearities, there are likely many other interactions and nonlinear terms that are being missed in the Poisson regression framework.

⁴⁶ For example, see Lang and Stulz 1994; Berger and Ofek 1995; Graham et al. 2002; Campa and Kedia 2002; Laeven and Levine 2007; Hoechle et al. 2012, Liu and Lai 2012; Hann et al. 2013; Franco et al. 2016.

⁴⁷ See Berger and Hann 2003; Botosan and Stanford 2005; Ettredge et al. 2005; Ettredge et al. 2006; Berger and Hann 2007; Hope et al. 2009; Cho 2015.

statements to be able to account for and understand the role of both constructs. We use this merged data in a setting where prior research suggests both constructs arguably play an important role, the comparability of a firm's financials to its peers.

We use a differences-in-differences methodology that leverages a plausibly exogenous standard change in how firms were required to report their segments (SFAS No. 131). While prior research expected the timing of the standard change to be orthogonal to industry operations, we find that the results for the role of public reporting differs depending on whether we account for changes in private reporting of industry operations. Additionally, we find that when firms change the number of industries they privately report but do not disclose a change publicly that there is greater comparability with a firm's peers' financials in the period following the standard change. Combined, these results suggest that accounting for both the role of diversification and disaggregation, at least in our research setting with financial comparability, is important for inferences that standard setters and researchers care about.

The second set of analyses in our paper then moves on to provide an aid for future researchers who are limited to relying on public segment data. We prepare a model to predict private reporting of industries from (2002-2021) and use out-of-sample tests to provide evidence that this provides high discriminatory power to allow future researchers to distinguish the private reporting measure from the public reporting measure. This evidence is timely given renewed standard-setting attention to disaggregation, including FASB's ASU 2023-07 on improvements to reportable segment disclosures and ASU 2024-03 on disaggregation of income statement expenses. These developments underscore the need to distinguish between the usefulness of more disaggregated public reporting and the separate construct of firms' underlying operating scope.⁴⁸

⁴⁸ ASC 606 calls for greater information disaggregation in revenue recognition. Recent research discusses important trade-offs that are possible with greater disaggregation of information (Ellis et al. 2012; Hinson, Pündrich, and Zakota 2024).

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APPENDIX A – Variable Descriptions

Variable components come from Compustat unless otherwise labelled.		
Variable name	Description	Source(s)
comp	Following De Franco et al. 2011, we use the following two step process to estimate the comparability of a firm's earnings to its industry peers. First, for each firm year, we estimate the following equation using 16 previous quarters of data: $\text{Earnings}_{it} = \alpha_i + \beta_i \text{Return}_{it} + \varepsilon_{it} \quad (\text{A1})$ where Earnings_{it} is the ratio of quarterly income before extraordinary items (IB) to beginning-of-period market value of equity (stock price (PRCC_F) x shares of common stock (CSHO)) and Return is the stock price return (CRSP: RET) during the quarter. Using equation (A1) for firm i, the $\hat{\alpha}_i$ and $\hat{\beta}_i$ proxy for the accounting function for firm i. Similarly, using the equation for firm j, the $\hat{\alpha}_j$ and $\hat{\beta}_j$ proxy for the accounting function of firm j. We then estimate the “closeness” between firm i's and firm j's estimated accounting functions to predict their earnings, assuming they experienced the same economic events (i.e., if they had the same Return_{it}). We do this by estimating the following two accounting functions for each firm with the economic events of a single firm: $E(\text{Earnings})_{iit} = \hat{\alpha}_i + \hat{\beta}_i \text{Return}_{it} \quad (\text{A2})$ $E(\text{Earnings})_{ijt} = \hat{\alpha}_j + \hat{\beta}_j \text{Return}_{it} \quad (\text{A3})$ $E(\text{Earnings})_{iit}$ is the predicted earnings of firm i given firm i's return in period t and $E(\text{Earnings})_{ijt}$ is the predicted earnings of firm j given firm j's accounting function and firm i's return in period t. By using firm i's return in both predictions, this applies constant economic events for both accounting functions. Comparability is the negative value of the average absolute difference (distance) of the predicted earnings using firm i's and j's functions: $\text{comp}_{ijt} = -1/16 \times \sum_{t-15}^t E(\text{Earnings}_{iit}) - E(\text{Earnings}_{ijt}) \quad (\text{A4})$ Higher values of comp correspond with greater accounting comparability between firms. We estimate accounting comparability for each firm i – firm j pair for firms within the same industry (based on two-digit SIC) each year, producing a firm-year measure of firm i's accounting comparability.	Compustat fundamentals quarterly, CRSP
ind public	The number of unique industries (by three-digit SIC) reported by firm i in year t publicly in its segment disclosures.	Compustat Historical Segments
ind private	The number of unique industries (by three-digit SIC) reported by firm i in year t privately in its BE-10 or BE-11 international surveys for the BEA.	BEA surveys (BE-10 and BE-11)
Post	An indicator equal to 1 for fiscal years following implementation of SFAS No. 131 (fiscal years beginning after December 15, 1997) and zero in earlier periods.	Compustat Fundamentals

ch_pub	An indicator equal to 1 if the number of industries publicly report changed from the pre-SFAS No. 131 period to post-SFAS No. 131 period and set equal to zero if there was no change between periods. This requires at least one non-missing value of “ind public” in both the pre- and post-periods. If a firm reported industries publicly in more than one year in the pre (post) period, we use the maximum number of industries reported in a year within the pre (post) period for the comparison.	Compustat Historical Segments
ch_priv	An indicator equal to 1 if the number of industries privately report changed from the pre-SFAS No. 131 period to post-SFAS No. 131 period and set equal to zero if there was no change between periods. This requires at least one non-missing value of “ind private” in both the pre- and post-periods. If a firm reported industries privately in more than one year in the pre (post) period, we use the maximum number of industries reported in a year within the pre (post) period for the comparison. Based on BE-10 and BE-11 surveys.	BEA surveys (BE-10 and BE-11)
ch_pub_noch_priv	An indicator equal to 1 if the firm had a change in publicly reported industries (ch_pub = 1) but no change in privately reported industries (ch_priv = 0). Otherwise, the indicator is set equal to zero. Based on BE-10 and BE-11 surveys in addition to Compustat.	Compustat Historical Segments, BEA surveys (BE-10 and BE-11)
increase_pub_noch_priv	An indicator equal to 1 if the change in publicly reported industries was positive in the post period and there was no change in the privately reported number of industries. Otherwise, the indicator is set equal to zero. Based on BE-10 and BE-11 surveys in addition to Compustat.	Compustat Historical Segments, BEA surveys (BE-10 and BE-11)
decrease_pub_noch_priv	An indicator equal to 1 if the change in publicly reported industries was negative in the post period and there was no change in the privately reported number of industries. Otherwise, the indicator is set equal to zero. Based on BE-10 and BE-11 surveys in addition to Compustat.	Compustat Historical Segments, BEA surveys (BE-10 and BE-11)
ch_priv_noch_pub	An indicator equal to 1 if the firm had a change in privately reported industries (ch_priv = 1) but no change in publicly reported industries (ch_pub = 0). Otherwise, the indicator is set equal to zero. Based on BE-10 and BE-11 surveys in addition to Compustat.	Compustat Historical Segments, BEA surveys (BE-10 and BE-11)
increase_priv_noch_pub	An indicator equal to 1 if the change in privately reported industries was positive in the post period and there was no change in the publicly reported number of industries. Otherwise, the indicator is set equal to zero. Based on BE-10 and BE-11 surveys in addition to Compustat.	Compustat Historical Segments, BEA surveys (BE-10 and BE-11)
decrease_priv_noch_pub	An indicator equal to 1 if the change in privately reported industries was negative in the post period and there was no change in the publicly reported number of industries. Otherwise, the indicator is set equal to zero. Based on BE-10 and BE-11 surveys in addition to Compustat.	Compustat Historical Segments, BEA surveys (BE-10 and BE-11)

ch_both_priv_pub	An indicator equal to 1 if there was both a change in the number of industries privately reported (ch_priv) and the number of industries publicly reported (ch_pub). Otherwise, the indicator is set equal to zero. Based on BE-10 and BE-11 surveys in addition to Compustat.	Compustat Historical Segments, BEA surveys (BE-10 and BE-11)
size	The firm i 's market value of equity (stock price (PRCC_F) x shares of common stock (CSHO)) in year t .	Compustat fundamentals
lev	Firm i 's total debt (DLC + DLTT) scaled by total assets (AT)	Compustat fundamentals
btm	Firm i 's common ordinary equity for book (CEQ) scaled by the market value of equity (PRCC_F x CSHO) in year t .	Compustat fundamentals
age	The number of years firm i has been in Compustat (FYEAR – YEAR1).	Compustat fundamentals and names
roa	Firm i 's income before extraordinary items (IB) scaled by total assets (AT) in year t .	Compustat fundamentals
ret_vol	The standard deviation of daily returns (CRSP: RET) over the 12 months leading up to year-end. Based on CRSP data.	CRSP daily security file
sales_growth	Firm i 's the change in sales (SALE) from year $t-1$ to year t scaled by year $t-1$ sales.	Compustat fundamentals
loss	An indicator if firm i 's income before extraordinary items (IB) is negative in year t , set equal to zero if otherwise if not missing.	Compustat fundamentals
<i>Prediction variables from Campa and Kedia (2002)</i>		
log_at	The natural log of total assets (AT).	Compustat fundamentals
log_at_11 (log_at_12)	One and two year lags of log_at	Compustat fundamentals
ave_log_at	Moving average of log_at over years available	Compustat fundamentals
ebit_sale	Earnings before interest and taxes (EBIT) scaled by sales (SALE)	Compustat fundamentals
ebit_sale_11 (ebit_sale_12)	One and two year lags of ebit_sale	Compustat fundamentals
ave_ebit_sale	Moving average of ebit_sale over years available	Compustat fundamentals
capx_sale	Capital expenditures (CAPX) scaled by total sales (SALE)	Compustat fundamentals
capx_sale_11 (capx_sale_12)	One and two year lags of capx_sale	Compustat fundamentals

ave_capx_sale	Moving average of capx_sale over years available	Compustat fundamentals
psdiv_percent	The percent of sales in the firm's primary industry that come from firms that publicly report multiple industries (excluding the firm itself)	SDC Platinum, Compustat fundamentals
pndiv_percent	The number of firms in the company's primary industry that publicly report multiple industries (excluding the firm itself)	SDC Platinum
snp	Indicator equal to one if the firm is listed on the S&P	CRSP
majorex	Indicator if the firm is listed on a major stock exchange (EXCHG codes 14, 11, and 12).	Compustat fundamentals

FIGURES

Figure 1 – Trends in the number of publicly and privately reported industries surrounding SFAS No. 131 implementation (1995-2001)

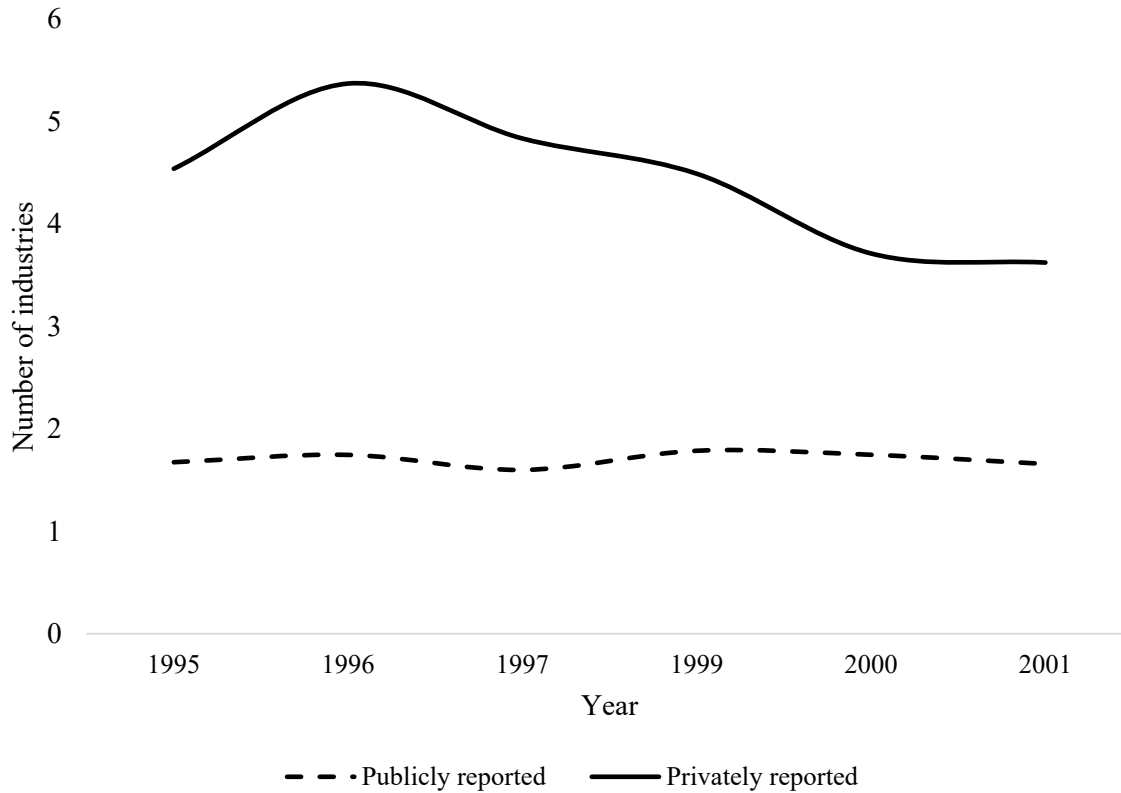


Figure 1 presents the average number of industries (based on three-digit SIC) reported privately in BEA surveys (the solid black line) and publicly (the dashed black line) in the years surrounding the implementation of SFAS No. 131 (fiscal years beginning after Dec. 15, 1997) for firm-years in our large multinational merged sample.

Figure 2 – Trends in the number of publicly and privately reported industries (2002-2021)

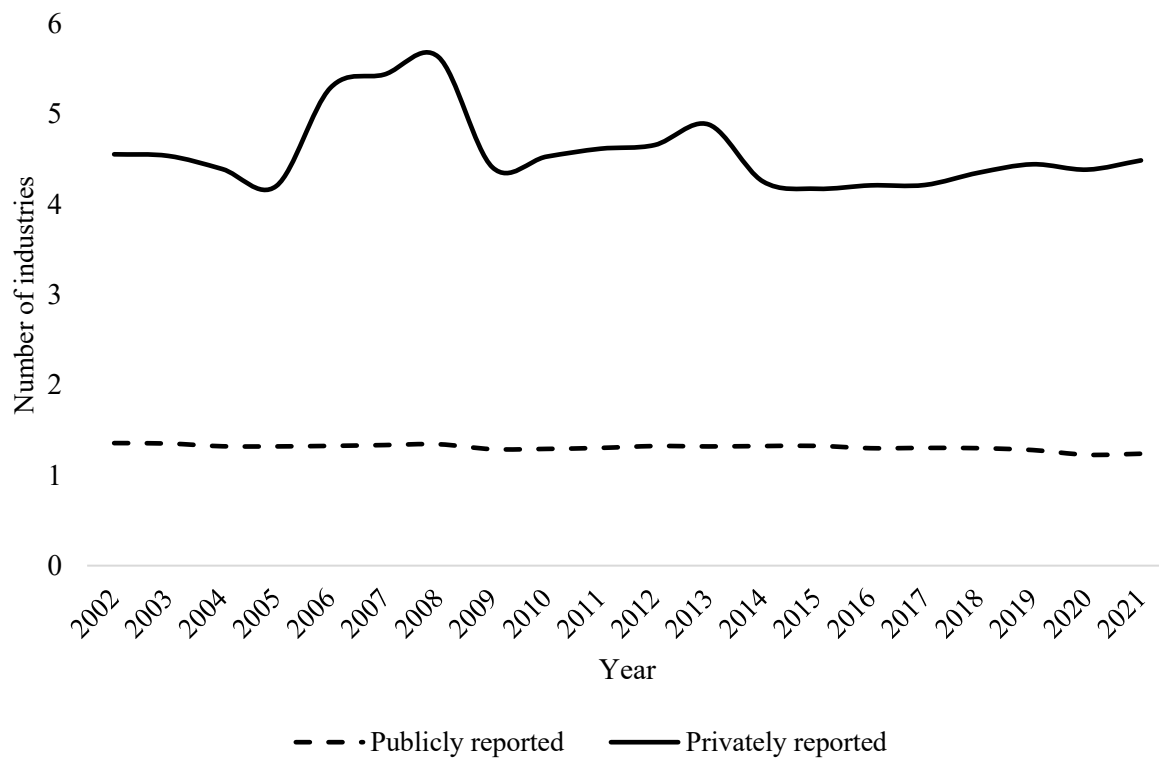


Figure 2 presents the number of industries (based on four-digit NAICS) reported privately in BEA surveys (the solid black line) and publicly (the dashed black line) in the years after our diff-in-diff (2002-2021) period for firm-years with non-missing BEA and Compustat industry count data, with positive assets and sales, and with non-zero pre-tax foreign income.

Figure 3 – Distribution (Smoothed Density) of Industry Segment Measures

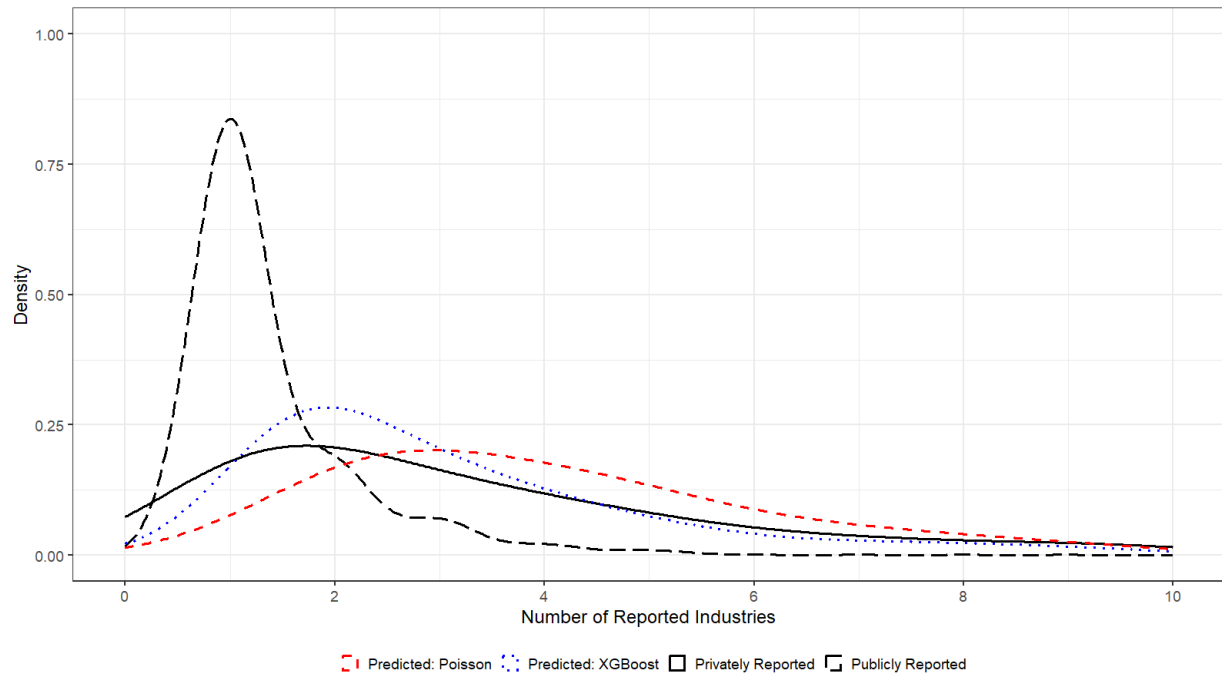


Figure 3 graphs the density of four different segment measures. The first two illustrate the smoothed distribution of the actual data – the number of segments privately reported to BEA (solid black line) and publicly reported via Compustat (dashed black line). The second two are synthetic BEA-measures discussed in the text, where the red dotted line represents the density of the synthetic measure estimated by a Poisson regression, while the blue dotted line corresponds to the measure predicted from the XGBoost machine learning model.

TABLES

Table 1 – Sample selection

<i>Panel A: Compustat Sample</i>		
	No. of obs. dropped	No. of obs. remaining
Initial sample of U.S. companies from 1995-2001 (excluding 1998) eliminating duplicate firm years and requiring the firm has one EIN		32,338
Eliminate observations without the main comparability measure and segment data to identify the number of industries (based on SIC 3), observations with non-positive assets and sales, and companies that do not have at least one observation in both the pre and post SFAS No. 131 periods	23,101	9,237
Limit to large multinationals (> median absolute pre-tax foreign income for the period and >\$100 million in assets in line with Hanlon et al. 2015)	4,431	4,806
<i>Panel B: BEA Sample</i>		
Initial sample of U.S. multinational firms from 1995-2001 (excluding 1998) eliminating duplicate firm years, requiring the firm has one EIN, and requiring necessary information for the BEA industry measure		10,740
Limit to large multinational firms (as defined in Panel A) with merge that have at least one BEA industry observation pre and post SFAS No. 131, information for control variables, and 1 and 2 year lead variables for parallel trends tests.	9,718	1,022
Table 1 reports the sample selection for the primary merged large multinational firm-year sample (n =1,022) with the separate steps for constructing the Compustat and BEA samples outlined in Panels A and B, respectively. The construction process and degree of attrition are similar to the process in Hanlon et al. (2015) that also use a merged Compustat-BEA data set.		

Table 2 – Descriptive statistics

	Large MNC merged sample (Primary)			Large MNC Compustat only sample			Full Compustat only sample		
	N	Mean	St dev	N	Mean	St dev	N	Mean	St dev
comp	1,022	-2.2685	1.1985	4,806	-2.3740	1.4995	9,237	-2.7818	1.7535
ind public	1,022	1.7006	0.9987	4,806	1.4869	0.8642	9,237	1.3415	0.7282
ind private	1,022	4.4755	3.9932	-	-	-	-	-	-
log(assets)	1,022	7.4117	1.4973	4,806	6.6848	1.4362	9,237	5.1826	2.0641
Size	1,022	6274.2	12,949.5	4,806	2,840.8	8,103.1	9,237	1,542.3	6,043.4
Lev	1,022	0.2576	0.1833	4,777	0.3094	0.2351	9,196	0.2546	0.2333
Btm	1,022	0.4842	0.3986	4,806	0.6464	0.5344	9,237	0.6865	0.6007
Age	1,022	23.804	9.9765	4,806	18.860	9.8604	9,237	16.788	8.5893
Roa	1,022	0.1240	0.0876	4,804	0.0972	0.0953	9,234	0.0425	0.2097
ret vol	1,022	0.1150	0.0631	4,806	0.1232	0.0723	9,237	0.1525	0.0942
sales growth	1,022	0.1214	0.2462	4,803	0.3272	7.7309	9,211	0.4109	7.6902

Table 2 presents the descriptive statistics for our primarily diff-in-diff analysis sample using a Compustat-BEA merged sample of large multinationals in the first three columns of statistics. We then report these statistics for two alternative, broader samples for comparison in remaining columns. Statistics in numeric columns 3-6 (7-9) corresponds with a sample of large multinationals in Compustat without restrictions for the merge to BEA data (a Compustat sample only requiring availability of the equation (1) variables and availability of at least one observation in both the pre- and post-periods). Overall, the statistics show that the primary analysis sample corresponds with larger, profitable firms with less sales growth.

Table 3 – Does private industry operations move closer or further from others in the industry?

	Abs. diff in no. industries for firm <i>i</i> and yearly avg. no. of industries privately reported for its primary industry <i>k</i>		
	Abs. diff pre-SFAS No. 131	Abs. diff post-SFAS No. 131	- move closer (+move further) from avg. no. of industry operations
For firms that privately:			
Changed no. of ind.	2.509	1.934	-0.575
Increased no. of ind.	2.147	1.829	-0.318
Decreased no. of ind.	2.924	2.091	-0.833

Table 3 presents the absolute difference between the number of industries reported by firm *i* and the average number of industries privately reported by firms in the pre- and post-SFAS No. 131 periods (based on three-digit SIC). We present the pre- and post- differences for each classification of private reported change to evaluate whether the private operation change was moving more in line or further away from the other firms in the industry. For each type of firm private operation change, the firms appear to be moving closer to other firms in their industry.

Table 4 – Large multinationals industry classifications using the merged Compustat-BEA sample

<i>Panel A: Breakout of public segment data industry classification</i>					
Pre-SFAS No. 131	Post-SFAS No. 131			<i>% of Total</i>	
	Single	Multi	Total		
Single	502	164	666		65%
<i>% of Total</i>	75%	25%	100%		
Multi	46	310	356		35%
<i>% of Total</i>	13%	87%	100%		
Total	548	474	1,022		
<i>% of Total</i>	54%	46%	100%		
<i>Panel B: Breakout of private survey data industry classification</i>					
Pre-SFAS No. 131	Post-SFAS No. 131			<i>% of Total</i>	
	Single	Multi	Total		
Single	100	57	157		15%
<i>% of Total</i>	64%	36%	100%		
Multi	25	840	865		85%
<i>% of Total</i>	3%	97%	100%		
Total	125	897	1,022		
<i>% of Total</i>	12%	88%	100%		
<i>Panel C: Overlap in direction of change in no. of ind. for public vs. private data after SFAS No. 131</i>					
Public no. of ind.	Private survey data no. of ind.				<i>% of Total</i>
	Decrease	No change	Increase	Total	
Decrease	66	17	31	114	11%
<i>% of Total</i>	58%	15%	27%		
No change	153	295	204	652	64%
<i>% of Total</i>	23%	45%	31%		
Increase	53	86	117	256	25%
<i>% of Total</i>	21%	34%	46%		
Total	272	398	352	1,022	
<i>% of Total</i>	27%	39%	34%		

Table 4 presents the breakout of public and private industry reporting (based on three-digit SIC) pre- and post-SFAS No. 131 implementation in a format similar to Table 2 in Ettredge et al. (2005) for the large multinational merged Compustat-BEA firm-year sample in Panels A and B, respectively. The percentage breakouts for public reporting in Panel A with the large multinational Compustat-BEA sample are very similar to those in Ettredge et al. (2005) that use a broader Compustat firm-year sample, which suggests the public reporting for the merged sample is reasonably similar to Compustat samples used with SFAS No. 131 analysis in prior literature. By contrast, the private reporting in Panel B for the same firms with Panel A shows much more frequent reporting of multiple industry operations than what is reported publicly with Panel A. Finally, Panel C compares classifications of whether or not the number of industries publicly or privately reported changed following SFAS No. 131 and for what percentages of the sample the publicly or privately reported match. Note that the percentage of mismatch in private for a given classification of public reporting change ranges from 42 percent (for the public reporting decrease classification: 15% (no change privately) +27% (an increase privately)) and with more than a 50% mismatch when public reporting has no change (23%+31% = 54%) or an increase (21%+34% = 55%).

Table 5 – Baseline Analysis - Comparability after SFAS No. 131 based on public reporting classifications alone

Dep. var.	comp					
	Large MNCs Compustat Only Sample			BEA-Comp Merged Large MNCs (primary sample)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post				-0.4991*** (-6.2610)	-0.4736*** (-6.3675)	-0.4947*** (-6.2681)
ch_pub	0.1592** (2.5547)	0.1434** (2.2394)	0.1195* (1.7529)	-0.0526 (-0.5283)	0.0869 (0.9331)	0.1086 (0.9795)
postXch_pub	-0.2693*** (-2.6557)	-0.2549** (-2.5354)	-0.2232** (-2.3318)	-0.2782 (-1.5117)	-0.2185 (-1.3594)	-0.1858 (-1.2843)
Controls	No	No	No	No	No	No
Ind fixed effects	SIC1	SIC2	SIC3	SIC1	SIC2	SIC3
Yr fixed effects	Yes	Yes	Yes	No	No	No
Obs	4,806	4,806	4,806	1,022	1,022	1,022
R-squared	0.1280	0.2464	0.3181	0.1038	0.3044	0.3923

Table 5 reports variations of equation (1) when focusing only on public reporting changes. Columns (1)-(3) report estimates for the broader large MNC Compustat sample before it is merged with the BEA data and uses year fixed effects in lieu of $Post_t$ term with the larger sample. Columns (4)-(5) re-estimate the analysis for the merged, smaller sample with the $Post_t$ indicator. Standard errors are robust and clustered by firm. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level (two-tailed), respectively.

Table 6 – Main Analysis - Comparability after SFAS No. 131 for firms with private and public industry classifications

Dep. var.	comp					
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.4991*** (-6.2610)	-0.7522*** (-6.7773)	-0.7522*** (-6.7707)			
ch_pub	-0.0526 (-0.5283)	-0.1492 (-1.3361)	-0.1491 (-1.3336)	-0.1344 (-1.1956)	-0.1560* (-1.6731)	-0.1298 (-1.4696)
ch_priv_noch_pub		-0.1667 (-1.2953)		-0.2983** (-2.3737)	-0.1742* (-1.7682)	-0.2508*** (-3.0083)
postXch_pub	-0.2782 (-1.5117)	-0.0253 (-0.1289)	-0.0254 (-0.1292)	0.0813 (0.5115)	0.1087 (0.6055)	0.1079 (0.6735)
postXch_priv_noch_pub		0.4738*** (3.1051)		0.3480** (2.5130)	0.2735* (1.9638)	0.2426** (1.9959)
increase_priv_noch_pub			-0.1679 (-1.0579)			
decrease_priv_noch_pub			-0.1652 (-1.0007)			
postXincrease_priv_noch_pub			0.4811*** (2.6342)			
postXdecrease_priv_noch_pub			0.4625** (2.4888)			
Controls	No	No	No	Baseline	Full	Full
Ind fixed effects	SIC1	SIC1	SIC1	SIC3	SIC1	SIC2
Yr fixed effects	No	No	No	Yes	Yes	Yes
Obs	1,022	1,022	1,022	1,022	1,022	1,022
R-squared	0.1038	0.1102	0.1102	0.4989	0.3576	0.4810
δ (postXch_priv_noch_pub)				-5.363	-1.877	-1.988

Table 6 begins with the default specification from Table 5 column (1) that uses the merged Compustat-BEA large MNC sample and evaluates public reporting changes alone. Column (2) then expands the analysis to move toward equation (1) by incorporating a role for the private reporting changes when there are no public reporting changes. Column (3) evaluates whether the role of private reporting is symmetric with increases versus decreases in private reporting. Columns (4)–(6) evaluate whether the role of private reporting is robust to alternative fixed effects and controls. Standard errors are robust and clustered by firm. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level (two-tailed), respectively.

ONLINE APPENDIX

Online Appendix A - Tables

Table A1 – Alternative Table 5 (Alternative Post Period)

Dep. var.	comp		
	(1)	(2)	(3)
Post	-1.1669*** (-7.5333)	-0.8935*** (-9.1840)	-1.1332*** (-7.9138)
ch_pub		-0.0620 (-0.6295)	-0.1739 (-1.5729)
ch_priv_noch_pub			-0.1884 (-1.4720)
postXch_pub		-0.7083* (-1.8572)	-0.4676 (-1.1878)
postXch_priv_noch_pub			0.4452** (2.3521)
Controls	No	No	No
Ind FE	SIC1	SIC1	SIC1
Year FE	No	No	No
Observations	684	684	684
R-squared	0.1805	0.1988	0.2018

Table A1 reports analysis from Table 6 leaving only 2001 for the post period, which reduces the sample by 33%. Standard errors are robust and clustered by firm. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level (two-tailed), respectively.

Table A2 – Table 6 – Additional Robustness Tests

Dep. var.	Comp								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post	-0.7522*** (-6.7773)	-0.7141*** (-6.9454)	-0.7244*** (-6.4194)	-0.6814*** (-5.5343)	-0.6274*** (-5.9757)	-0.6271*** (-5.3248)		-0.1296 (-0.9013)	
ch pub	-0.1492 (-1.3361)	-0.0498 (-0.5491)	0.0043 (0.0391)	-0.2088* (-1.8436)	-0.1301 (-1.3783)	-0.1095 (-0.9844)	-0.1344 (-1.1956)	-0.1622* (-1.7616)	-0.1560* (-1.6731)
postXch pub	-0.0253 (-0.1289)	0.0241 (0.1413)	0.0480 (0.2908)	0.0059 (0.0300)	0.0571 (0.3437)	0.0547 (0.3279)	0.0813 (0.5115)	0.1112 (0.6140)	0.1087 (0.6055)
ch_priv_noch_pub	-0.1667 (-1.2953)	-0.2420** (-2.3473)	-0.1892 (-1.5920)	-0.2526** (-2.0182)	-0.3228*** (-3.1701)	-0.2955** (-2.3356)	-0.2983** (-2.3737)	-0.1883* (-1.8993)	-0.1742* (-1.7682)
postXch_priv_noch_pub	0.4738*** (3.1051)	0.4456*** (3.0659)	0.4319*** (2.8622)	0.4509*** (2.9536)	0.3879*** (2.9085)	0.3759*** (2.6484)	0.3480** (2.5130)	0.2976** (2.1249)	0.2735* (1.9638)
Controls	No	No	No	Baseline	Baseline	Baseline	Baseline	Full	Full
Ind fixed effects	SIC1	SIC2	SIC3	SIC1	SIC2	SIC3	SIC3	SIC1	SIC1
Yr fixed effects	No	No	No	No	No	No	Yes	No	Yes
Obs	1,022	1,022	1,022	1,022	1,022	1,022	1,022	1,022	1,022
R-squared	0.1102	0.3097	0.3969	0.1702	0.3749	0.4502	0.4989	0.3259	0.3576

Table A2 presents robustness tests for Table 6. The columns show that the main DiD finding is robust to numerous common alternative specifications with alternative fixed effects and controls. Standard errors are robust and clustered by firm. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level (two-tailed), respectively.

Table A3 – Alternative comparability measures

Dep. var.	comp_med		comp_m10		comp_m4	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.3566*** (-3.8079)	-0.6887*** (-5.3619)	-0.0705 (-1.3970)	-0.2229*** (-3.3608)	-0.0476 (-1.2679)	-0.1383** (-2.5446)
ch_pub	0.0166 (0.1490)	-0.1470 (-1.2540)	0.0389 (0.5564)	-0.0426 (-0.5033)	0.0110 (0.2055)	-0.0323 (-0.4678)
postXch_pub	-0.3754* (-1.8084)	-0.0431 (-0.1947)	-0.3284** (-1.9871)	-0.1758 (-1.0467)	-0.2569* (-1.8809)	-0.1661 (-1.1857)
ch_priv_noch_pub		-0.2810** (-2.0165)		-0.1397 (-1.5168)		-0.0743 (-1.0328)
postXch_priv_noch_pub		0.6134*** (3.5128)		0.2801*** (2.9050)		0.1679** (2.2244)
Controls	No	No	No	No	No	No
Ind FE	SIC1	SIC1	SIC1	SIC1	SIC1	SIC1
Year FE	No	No	No	No	No	No
Observations	1,022	1,022	1,022	1,022	1,022	1,022
R-squared	0.0580	0.0668	0.0401	0.0440	0.0344	0.0366

Table A3 re-estimates variations of the Table 6 analysis replacing the dependent variable with alternative measures of comparability. Columns (1) and (2) use “comp_med” that retains the same comparison set of industry peers as the “comp” measure in the paper but uses the median distance to industry peers for firm i relative to these peers rather than the mean distance to industry peers. Columns (3) and (4) (columns (5) and (6)) then adjust the industry peer group used for the comparability measure to only include the 10 (4) most similar firms. Standard errors are robust and clustered by firm. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level (two-tailed), respectively.

Table A4 – Comparability regressions for parallel trends analysis

Dep. var.	comp			
	(1)	(2)	(3)	(4)
post ₋₁	-0.0172 (-0.132)	0.0542 (0.406)	-0.0838 (-0.476)	-0.0991 (-0.583)
post ₋₂	-0.0642 (-0.603)	-0.0245 (-0.201)	-0.0375 (-0.263)	-0.0573 (-0.412)
ch _{_pub}		0.0379 (0.339)	-0.0338 (-0.256)	-0.0819 (-0.611)
post ₋₁ Xch _{_pub}		-0.1885 (-1.064)	-0.0544 (-0.276)	-0.0447 (-0.232)
post ₋₂ Xch _{_pub}		-0.1262 (-1.001)	-0.1085 (-0.739)	-0.1135 (-0.793)
ch _{_priv_noch_pub}			-0.1239 (-0.700)	-0.1793 (-1.063)
post ₋₁ Xch _{_priv_noch_pub}			0.2449 (1.520)	0.2476 (1.548)
post ₋₂ Xch _{_priv_noch_pub}			0.0346 (0.197)	-0.0186 (-0.114)
Controls	No	No	No	Baseline
Ind FE	SIC1	SIC1	SIC1	SIC1
Year FE	Yes	Yes	Yes	Yes
Observations	1,022	1,022	1,022	1,022
R-squared	0.150	0.157	0.159	0.218

This table reports equation (1) after replacing “post” with two pseudo (false) post period terms and corresponding interactions that classify the policy as beginning one (post₋₁) and two (post₋₂) years before the actual implementation year for SFAS No. 131. Column (1) first presents a period only baseline analysis for comparability before incorporating comparison of reporting change and no change groups. Column (2) incorporates only the public segment change. Column (3) adds in the private reporting change, and column (4) re-estimates column (3) with baseline controls. Standard errors are robust and clustered by firm. T-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level (two-tailed), respectively.

Table A5 – Poisson prediction model for private industry reporting

	Poisson Full Sample (1)	Time-invariant Poisson (2)	EGB Gain (3)
log_at_l2	0.139*** (3.69)	0.139*** (3.72)	0.269
log_at	0.072* (1.78)	0.055 (1.38)	0.103
ave_log_at	0.128*** (10.5)	0.129*** (10.7)	0.093
ave_capx_sale	-0.057 (-1.48)	-0.051 (-1.43)	0.076
ave_ebit_sale	-0.005** (-2.34)	-0.004 (-1.56)	0.065
psdiv_percent	0.003*** (8.77)	0.004*** (9.46)	0.052
pndiv_percent	0.002*** (5.01)	0.002*** (4.97)	0.050
ebit_sale	-0.016 (-1.46)	-0.013 (-1.28)	0.033
ebit_sale_l1	-0.018 (-1.25)	-0.018 (-1.26)	0.030
capx_sale_l1	-0.296. (-1.82)	-0.356** (-2.20)	0.024
ebit_sale_l2	-0.002 (-0.159)	0.0008 (0.062)	0.024
capx_sale	-0.479*** (-3.83)	-0.434*** (-3.54)	0.022
capx_sale_l2	-0.243** (-2.13)	-0.193* (-1.74)	0.021
log_at_l1	-0.011 (-0.192)	-0.007 (-0.120)	0.020
snp	0.032 (1.42)	0.062*** (2.76)	0.005
majorex	0.196*** (6.54)	0.184*** (6.19)	0.003
Year Fixed Effects	Yes	No	
Observations	12,713	12,713	
Number Parameters	36	17	
Squared Correlation	0.375	0.365	
Pseudo R-squared	0.22	0.216	
Log-Likelihood	-32,149.53	-32,319.67	

This table reports model results for predicting the number of industries reported privately to BEA in surveys. Columns (1) and (2) report the coefficients from a Poisson regression model with and without year fixed effects, respectively. Column (3) reports the gain or improvement in prediction accuracy by each variable. The variables are ordered in this table in terms of gain in prediction accuracy by the EGB model, sorted by most important to least important variables for reducing uncertainty or entropy in the machine learning model. Robust standard errors are clustered by year. Standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level (two-tailed), respectively.

Table A6 – Evaluation of Prediction Models

	Constrained Test Sample (Includes Only Firms Reporting Ind. Segments Publicly)			Unconstrained (Full Test Sample)		
	Regression	XGBoost	%Δ	Regression	XGBoost	%Δ
Mean Absolute Error	1.851	1.112	39.940	2.495	1.473	34.530
Mean Absolute Percentage Error	0.503	0.385	23.497	0.572	0.412	28.470
Root Mean Square Error	2.804	2.058	26.604	3.793	2.512	31.230
Correlation - Private	0.563	0.846		0.612	0.870	
Correlation - Public	0.667	0.467		0.296	0.473	
Observations	1,400			2,500		
Correlation – Private (11+)	0.119	0.612		0.162	0.605	
Observations (11+)	74(45)			269(71)		

This table reports statistics to evaluate how well alternative estimation approaches predict privately reported industries to the BEA. The “Unconstrained” columns correspond with 20% of the full sample in col. 1 Table A5 used to test the prediction model trained on 80% of the sample (10,170 (train sample) +2,543 (test sample) = 12,713 firm-year observations). The “Constrained” columns correspond with a 20% test sample when taking the full sample and incorporating the restriction that it has public line of business segment reporting information.

Online Appendix B – Additional Details for Constructing and Evaluating Synthetic Measures

Appendix B.1 Model Construction

Our Poisson regression model follows the form,

$$ind_{it} \sim \text{Poisson}(\lambda_{it}),$$

with,

$$\log(\lambda_{it}) = x_{it}\beta + \alpha_t + \epsilon_{it}.$$

Here, $ind_{i,t}$ is the number of privately reported industries to BEA for firm i in period t ; $x_{i,t}$ is a vector of publicly reported determinants from Campa and Kedia (2002) (listed in Table A5); α_t is the time fixed effect; and, ϵ_{it} is an idiosyncratic component. Of course, similar to linear and other parametric models, the target of interest is $E(ind_{it}|x_{it})$ and the coefficients are interpretable as marginal effects (or, in this case, a change in the log of the expected count) that can be used for predicting values of $\widehat{ind}_{it}$. Finally, while OLS assumes a normal distribution, this approach is based on a Poisson distribution and is more suitable for non-negative discrete data like industry segments. Because the private industry reporting is skewed with a sizable right-tail (a large clustering of low industry numbers and values of a single industry), a Poisson distribution is also preferred as this distribution better fits skewed data better than a normal distribution. However, a Poisson regression may still underperform at the right tail (predicting high end industry values). We thus compare the Poisson regression prediction results with an iterative tree-based approach known as Extreme Gradient Boosting (Chen and Guestrin, 2016), which can better fit non-linearities and skewness in the data.⁴⁹ Extreme Gradient Boosting, EGB hereafter, is a scalable and computationally efficient implementation of gradient boosted trees with the core idea relying on building a sequence of decision trees where each successive tree learns from its predecessor's mistakes. At each iteration, $m \in 1, \dots, M$, EGB fits a new decision tree, $f_m(x)$, to the gradient of the loss function and predictions are updated as,

$$\tilde{y}_i^m = \tilde{y}_i^{(m-1)} + \eta f_t(x_i),$$

where $\eta \in (0,1]$ is the learning rate. The final prediction is the sum of all trees,

$$\tilde{y}_i^m = \sum_{m=1}^M \eta f_t(x_i).$$

Since our target is counts of industries as reported privately to the BEA, we assume the Gamma(μ, θ) distribution is appropriate where the log-likelihood function can be expressed as,

$$\sum_{i=1}^N \ln \Gamma\left(\frac{\tilde{y}_i}{\theta}\right) + \frac{\tilde{y}_i}{\theta} \ln(\theta) - \left(\frac{\tilde{y}_i}{\theta} - 1\right) \ln(y_i) + \frac{y_i}{\theta},$$

where θ is the unknown distribution parameter. Using this, the first derivative of the loss function is,

$$G_i = \frac{1}{\theta} \psi\left(\frac{\tilde{y}_i}{\theta}\right) + \frac{1}{\theta} \ln(\theta) - \frac{1}{\theta} \ln(y_i),$$

where $\psi(\cdot)$ is the digamma function. This is the gradient that new trees are fit to in the EGB algorithm. Hyperparameters of importance include the depth of each new tree (24), the learning rate (0.03), and sampling proportions for both the observation (0.8) and feature (0.8) set with values in parameters indicating the results of our grid search. Rather than coefficients, we report gain (i.e.,

⁴⁹ We also examined the use of the Random Forest (Breiman, 2001) and Gradient Boost Machines (Friedman, 1999) with similar results.

average improvement in model performance when a variable is used in a tree) by variable and overall model performance metrics (like mean absolute error and root mean square error) in the tables below.

Equation A1 – Full Poisson Model for Constructing a Synthetic BEA-measure of Industry Segments with Only Publicly Reported Information

Unconstrained Equation (not requiring public segment disclosures on line of business):

Researchers can construct the predicted private industry count as

$$\hat{E}[\text{ind private}_{it} | X_{it}] = \exp(X_{it}\hat{\beta}).$$

The following Poisson regression equation can be used to estimate a synthetic measure of the number of industries privately reported to BEA, where the estimated linear index is:

$$\begin{aligned} X_{it}\hat{\beta} = & -1.290831 + 0.054705 * \log_at - 0.013514 * \text{ebit_sale} - 0.434151 * \text{capx_sale} - 0.006875 * \\ & \log_at_11 - 0.017720 * \text{ebit_sale_11} - 0.356129 * \text{capx_sale_11} + 0.139177 * \log_at_12 + \\ & 0.000763 * \text{ebit_sale_12} - 0.193202 * \text{capx_sale_12} + 0.062031 * \text{snp} + 0.002341 * \text{pndiv_percent} \\ & + 0.003707 * \text{psdiv_percent} + 0.129412 * \text{ave_log_at} - 0.003569 * \text{ave_ebit_sale} - 0.051504 * \\ & \text{ave_capx_sale} + 0.184185 * \text{majorex} \end{aligned}$$

The synthetic predicted count is obtained by exponentiating this linear index. While this was modeled using a 20-year sample (2002-2021), it does not incorporate time fixed effects, making it a time-invariant equation for constructing the synthetic BEA-measure of industries out-of-sample. See Appendix Tables A5 and A6 for additional statistics reported for this equation.

The determinants for the above equations follow the definitions in Campa and Kedia (2002, Table V). Campa and Kedia (2002) uses the following firm, industry, and year level characteristics: log(assets), earnings before interest and taxes/sales, capital expenditures/sales, two lags and recent averages of these prior three variables from Compustat, whether the firm is listed on the S&P (using CRSP), the number of firms in the company's primary industry that publicly report multiple industries (excluding the firm itself) and the percent of sales in the firm's primary industry that come from firms that publicly report multiple industries (excluding the firm itself) from SDC Platinum, and is listed on a major exchange based on Compustat (codes 14, 11, and 12).

- ind private (4-digit NAICS) is the number of unique industries privately reported to the BEA by U.S. multinationals across for a U.S. reporter and its foreign affiliates
- ind public (4-digit NAICS) is the number of unique industries publicly reported in Compustat historical segment data in its line of business segments

Appendix B.2 Prediction Results and Evaluation

We tabulate the coefficient estimates for the firm characteristics in our prediction models in Appendix Table A5. In column (1), we provide coefficients for the Poisson regression for a 20-year sample frame (from 2002-2021). Column (2) tabulates a time-invariant model that excludes year fixed effects and can be used for out-of-sample predictions (with the full equation available in Equation A1 above). In column (3), we provide the contribution of each feature to the EGB model on total gain, as we only provide a single model of gains for the EGB since the missing publicly

reported industries are not an issue when predicting new observations as they are in the regression framework. As a result, the EGB model has been trained on the entire training set (80% of the total sample), and there are no constraints due to data reporting. Also note that this is not interpreted in the same way as the other columns, as a higher value in column (3) simply indicates it is a more important feature of the model, relatively speaking. Of the five most important features in our EGB model, not all of them are statistically significant at conventional levels in the Poisson specifications, underscoring the point that statistical significance is not necessarily a useful metric for evaluating prediction models (Mullainathan and Spiess 2017).

Next, we evaluate how well these models predict the private reported industry values. Recall that we split our sample randomly into a training and test set, where the training set is eighty percent of our post-2002 sample (10,170 observations). This split is done without any stratification and, as a result, it may be the case that some firms may appear entirely in the training or test set. Overall, there are 1,672 unique firm IDs in the post 2002 data (12,713 observations), there are 1,623 unique firm IDs in the training set (10,170 observations), and 1,134 unique firm IDs in the test set (2,543 observations). When comparing the predictions on the test set for only firms that reported public industry participation, we can see from Appendix Table A6 that predictions from EGB are preferred across all three commonly used loss functions. On average, the EGB predictions reduce out-of-sample loss relative to those of the Poisson regression, reducing mean absolute error (MAE), MA Percentage Error (MAPE), and root mean square error (RMSE) by 40%, 23%, and 27%, respectively. This pattern is repeated in the unconstrained training sample where we do not require the firm to have a value for public industry reporting in the Poisson regression. When we do a simple pairwise correlation between the predicted industry count and the actual private industry count in both of these samples, the predicted synthetic measure is highly correlated with the actual segment value (with a correlation coefficient of 0.61 and 0.87, for Poisson and EGB, respectively, in the unconstrained sample). In the tail of the distribution, where Poisson regressions are known to under-predict, the pairwise correlation is much higher for the predictions from the EGB model. For firms reporting 11+ segments to BEA, the correlation between the synthetic measure and the actual privately reported segment value is about 0.61 for EGB and 0.16 for Poisson, suggesting some caution in using the Poisson measure for firms with more than 10 industries.