Are Aggregate Industry Benchmarks Substitutes or Complements to Firm Disclosures? Evidence from the Stoppage of Government Current Industrial Reports

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Abstract

External aggregate industry information can either substitute for or complement a firm's own disclosures. Prior studies examine these mechanisms using peer disclosures and financial statement comparability, sources that are subject to endogeneity concerns. I study a new source of external information whose cancellation was likely exogenous to any individual firm: government-provided, product-level industry benchmarks. The U.S. Census Bureau's Current Industrial Reports (CIR) program supplied recurring physical-quantity statistics on production, shipments, inventories, and capacity for selected manufacturing industries until its cancellation in 2011. Using the CIR cancellation in a difference-in-differences design, I find that firms in affected industries significantly reduce the likelihood, frequency, and breadth of unit-based operating disclosure relative to firms in unaffected industries. The results extend to broader quantity disclosures and Management's Discussion and Analysis and are robust to a matched-sample design. Consistent with the complements explanation, the disclosure decline is stronger among firms with greater product-market similarity and in industries with greater public-firm presence, but is not systematically stronger in more competitive product markets. Following the CIR cancellation, affected firms also exhibit a modest deterioration in the information environment, with greater analyst forecast error, fewer analyst forecasts, and wider bid-ask spreads. Together, these additional results reinforce the interpretation that the CIR supplied economically useful benchmark information that complemented firm disclosure, and whose stoppage had adverse information effects.

1 Introduction

A fundamental idea in the disclosure literature is that external information helps investors interpret firm disclosures, particularly external information from peers. When market participants can compare firm-specific information to related firms within an industry, they can better assess or benchmark whether disclosed performance is unusual, persistent, or consistent with the firm's economic environment (e.g., Foster, 1981; De Franco et al., 2011; Shroff et al., 2017). This paper studies a particular form of external benchmark: aggregated government industry data. While prior research primarily examines individual peer firms' disclosures and financial statement comparability, aggregate public industry data represents a distinct source of external information that may shape firms' own disclosure decisions. Prior research on peer disclosure also suggests that firms respond to the surrounding information environment rather than choosing disclosure policies in isolation. In contrast to prior research focusing on disclosures by individual peer firms, I examine government-produced aggregate industry benchmarks from the Current Industrial Reports (CIR) and whether this information substitutes for or complements firms' own disclosures.

Predictions about the direction of this effect are ambiguous. One prediction is that public industry benchmarks complement firm disclosure. External information can increase firms' incentives to disclose by improving the information underlying disclosure (Verrecchia, 1990) and by strengthening the inferences investors draw from firms' disclosure choices (Dye and Sridhar, 1995). In particular, external information can allow investors to form expectations about firms that remain silent. When investors have a stronger benchmark against which to evaluate nondisclosure, silence can itself become more informative and potentially lead to more adverse inferences, increasing the cost of withholding information. Additionally, external information may reduce uncertainty over how disclosed information will be interpreted. When managers are uncertain about how different audiences will respond to a disclosure, remaining silent can reduce their exposure to those uncertain reactions (Bond and Zeng, 2022). Thus, external information can increase the relevance of disclosure while also increasing the cost of silence. Consistent with these theoretical predictions, Seo (2021) finds that disclosures by industry peers induce greater disclosure by focal firms.

A competing prediction is that public industry benchmarks substitute for firm disclosure. Outside information can reduce the need for firm disclosure by allowing investors to learn about the firm from other sources (Admati and Pfleiderer, 2000; Einhorn, 2018; Frenkel et al., 2020). As investors become better informed, the benefit of additional firm disclosure falls relative to its costs, reducing managers' incentives to disclose. Consistent with this theoretical prediction, Breuer et al. (2022) find that mandatory disclosures by regulated firms crowd out voluntary disclosures by related unregulated firms, with the reduction in voluntary disclosure increasing with the strength of the information spillovers.

Distinguishing between these competing predictions requires a setting in which changes in external information can be separated from other factors that also affect firm disclosure. Much of the prior literature examines external information supplied by individual peer firms. In these settings, peer and focal-firm disclosures may respond to the same underlying industry conditions. Additionally, peer disclosure may itself reflect expectations about how related firms will behave. Some studies use econometric approaches to mitigate these concerns. For instance, Seo (2021) uses instrumental variables, while Truong (2023) uses variation in peer disclosure timing. However, distinguishing between these predictions requires considering whether the source of variation affects focal firms through channels other than peer disclosure, such as whether peer firms make disclosure decisions while strategically anticipating focal firm responses.

Breuer et al. (2022) exploit changes in mandatory disclosure regulation for private firms in Germany to examine how information supplied by peer firms affects voluntary disclosure. They find that increased disclosure by regulated firms crowds out voluntary disclosure by related unregulated firms, particularly when information spillovers are stronger. While these findings demonstrate that peer information can substitute for firms' own disclosures, information supplied by individual firms reflects both underlying economic conditions and their disclosure decisions. In contrast, aggregate industry data summarize common industry conditions, providing benchmarks that investors can use to form expectations about multiple firms and evaluate firm-specific disclosures.

To examine these competing possibilities for aggregate industry information, I use the

cancellation of the U.S. Census Bureau's Current Industrial Reports (CIR) program. Since 1904, the CIR provided recurring product-level, physical-unit statistics for selected manufacturing industries, including physical quantities related to production, shipments, inventories, and capacity (U.S. Census Bureau, 2021a,b). These reports supplied public operating benchmarks that could help market participants interpret firm-level financial operating disclosures and benchmark voluntarily disclosed physical unit disclosures. They also provided information that investors could use to form expectations about firms that did not disclose comparable operating quantities. The CIR served a broad set of private and government users, and its proposed termination prompted concerns among both federal agencies and industry groups about the loss of the information.[1] Importantly, the CIR's discontinuation was driven by Census Bureau budget constraints rather than firms' disclosure decisions. The program was discontinued in Q4, 2011, eliminating a longstanding source of public product-level benchmark information in 2012 for covered industries (U.S. Census Bureau, 2011b, 2021a).

The CIR program cancellation therefore provides a useful setting for studying the role of public industry benchmarks in voluntary disclosure. The budget-driven cancellation allows me to examine firms' disclosure responses to a change in external industry information that was not initiated by the affected firms nor industry conditions. Additionally, the program covered some industries but not others, creating cross-sectional variation in exposure to the removal of public benchmark information. In the empirical analysis, I focus on unit-based operating disclosures because they are the firm-level disclosures most closely related to the type of information the CIR provided, but the broader question is whether the removal of public industry data changes firms' disclosure incentives.

One prediction is that firms exposed to the CIR program cancellation reduce physical operating disclosure. This prediction follows from the complementarity and adverse-inference view of benchmarks discussed above. When the CIR was available, investors could use public product-level data to interpret firm-level quantity disclosures and to form inferences about firms

[1] See Section 2.3 for additional discussion on CIR users and the economic value of the information around the time of termination.

that remained silent. After the CIR disappeared, disclosed quantities became harder to interpret and nondisclosure became less informative. If public benchmarks previously increased the benefits of disclosure or increased the adverse inference associated with silence, then removing those benchmarks should reduce firms' incentives to disclose operating quantities.

In contrast, another prediction is that firms exposed to the CIR program cancellation increase physical operating disclosure. If the CIR primarily substituted for firm disclosure by providing investors with industry-level information that reduced demand for firm-specific disclosure, its removal could lead firms to disclose more. Thus, whether the CIR program cancellation increases or decreases firm disclosure is an empirical question.

I test these predictions using public firms' Form 10-K disclosures from fiscal years 2008 through 2015, consistent with prior studies that examine the impact of events on annual disclosures (e.g., Kubick and Li, 2023). I classify firms as treated if they operate in industries covered by the CIR just before the program's cancellation in Q4, 2011 and compare their disclosure changes to firms in industries not affected by the termination. I measure operating quantity disclosure using dictionaries that identify physical units, production quantities, volume measures, capacity references, and related operating quantity terms in annual reports.

The results suggest that treated firms significantly reduce unit-based operating disclosure after the CIR program cancellation. Relative to control firms, treated firms experience declines in the likelihood, frequency, and breadth of physical-unit disclosure. The results are similar when I use broader measures of expanded quantity disclosure and when I restrict the analysis to Management's Discussion and Analysis. The findings are also robust to a matched-sample design comparing treated firms to firms in industries that lost CIR coverage during the pre-sample years of 1991 through 2007.[2]

An obvious alternative explanation for CIR stoppage and subsequent disclosure reduction by firms is that the underlying operations information was no longer relevant to investors. To rule

[2]The program-wide cancellation of the CIR was budget-driven. By contrast, changes to individual CIR survey coverage while the program was ongoing reflected changing stakeholder demand, data-user needs, and data-collection feasibility rather than the same across-the-board budgetary shock (U.S. Census Bureau, 2021a).

out this alternative, I next examine cross-sectional variation in sub-samples to distinguish among the competing explanations. If the CIR mattered because it helped investors benchmark firms and draw inferences from silence, the disclosure decline should be strongest where benchmarking is most relevant. Consistent with this prediction, the decline is concentrated among firms with high product-market similarity and in industries with greater public-firm presence. These are settings in which investors can more readily compare firms to similar public peers and use external information to evaluate missing firm-level disclosures. In contrast, the effect is not systematically stronger in high product-market competition (fluidity) settings, suggesting that the results are unlikely to be driven primarily by generic competitive threat or proprietary-cost pressure.

Finally, I examine whether the CIR program cancellation is associated with changes in the capital-market information environment. Following the program's discontinuation, affected firms also exhibit a modest deterioration in the information environment with higher analyst forecast error, fewer analyst forecasts, and wider bid-ask spreads. These patterns are consistent with the interpretation that the CIR supplied useful public benchmark information and that its removal reduced investors' ability to interpret firm-level operating activity. An irrelevance explanation would therefore need to account for not only the average disclosure decline, but also these sub-sample patterns and the capital-market effects.

This paper contributes to the disclosure literature by showing that public industry benchmarks can affect firms' voluntary disclosure incentives. Prior research finds that peer information and comparability affect investors' information processing and firms' disclosure behavior. I extend this literature by examining aggregate industry information following the budget-driven cancellation of a government statistical program, which removes a longstanding source of external information independently of firms' disclosure decisions. This setting allows me to examine which of the competing theoretical predictions dominates. I find that firms reduce operating disclosures after the removal of a government-provided industry benchmark, suggesting that the complementary role of aggregate industry information dominates its potential substitution effect. The broader pattern of results also makes it less likely that firms reduced disclosure simply because physical operating

information had become irrelevant. The disclosure decline varies across several cross-sectional dimensions in ways that are consistent with the benchmarking mechanism, and affected firms also experience worse analyst and trading information outcomes after the CIR ends. The results further suggest that public operating statistics can support firm disclosure by helping investors interpret disclosed information and by making nondisclosure more informative. Finally, the paper documents a capital-market implication of discontinuing a public aggregate industry data program that, although not designed for securities disclosure purposes, appears to have supported the public information environment.

The remainder of the paper proceeds as follows. Section 2 describes the CIR program and develops the hypotheses. Section 3 describes the sample, disclosure measures, and empirical design. Section 4 presents the main results and sub-sample evidence on benchmarking and proprietary-cost channels, and Section 5 concludes.

2 Hypothesis Development

2.1 Theory

Voluntary disclosure theory suggests that firms do not choose disclosure policies in isolation. In industry settings, disclosures may contain both firm-specific and common industry information, allowing investors to use one firm's disclosure to update beliefs about economically related firms. Dye and Sridhar (1995) formalize this idea in an industry-wide disclosure setting, showing that one firm's disclosure impacts how investors perceive the disclosure (or non-disclosure) decisions of peer firms. This framework provides a complementarity prediction by showing how information about peer firms can increase adverse inferences associated with a firm's choice to remain silent. However, it does not establish whether this complementarity effect dominates the competing substitution effect when investors also receive external, aggregated industry information.

One possibility is that public industry benchmarks complement firm disclosure. In the CIR setting, public product-level data could help investors interpret firm-level operating quantities

by providing a reference point for industry production, shipments, inventories, or capacity. A disclosed quantity is more informative when users can evaluate whether it is high or low relative to industry activity. Public benchmarks can also make nondisclosure more informative: if investors can form expectations about operating quantities using public industry data, then silence may generate a stronger adverse inference. This prediction is consistent with information-transfer models in which investors use disclosure and nondisclosure by related firms to update beliefs in information environments with multiple firms (Dye and Hughes, 2018). It also relates to Bond and Zeng (2022), who show that firms may remain silent when disclosure exposes them to uncertainty over how different audiences will interpret and respond. Physical operating disclosures may be especially subject to heterogeneous interpretation by investors, competitors, customers, suppliers, employees, or regulators. Public benchmarks can reduce this interpretive uncertainty by giving audiences a common reference point for evaluating the disclosed information. Under this view, the CIR increases the benefits of disclosure and raises the cost of silence; removing the CIR should therefore reduce firms' incentives to disclose physical operating quantities.

A competing possibility is that public industry benchmarks substitute for firm disclosure. If public industry data already allow investors to infer product demand, production conditions, capacity utilization, or other operating fundamentals, then managers may face less demand to provide their own firm-level operating quantities, which may have proprietary costs. This prediction follows from models in which outside information competes with firm disclosure and changes the marginal role of the firm's own signal. In particular, Frenkel et al. (2020) show that when outside information overlaps with information the firm could disclose, it can reduce the marginal benefit of voluntary disclosure relative to the costs of disclosure, including proprietary costs. Similarly, Einhorn (2018) shows that competing information sources can change disclosure equilibria because investors are not limited to information supplied by the firm itself. In my setting, the CIR supplies information that partially replaces firm disclosure rather than supporting it: when public industry data already reveal product demand, production conditions, or capacity utilization, managers may have less reason to disclose firm-specific operating quantities. Removing the CIR would then increase the marginal

role of firm-provided operating information and could lead firms to disclose more. Thus, the effect of the CIR program cancellation is theoretically ambiguous: public industry benchmarks may either increase disclosure by making firm-level information more interpretable, or reduce disclosure by supplying information that substitutes for firm-level disclosure.

2.2 Prior Findings

Prior empirical findings support the theoretical predictions that external information about related firms helps users interpret firm-specific information. De Franco et al. (2011) show that financial statement comparability is associated with greater analyst following, higher forecast accuracy, and lower forecast dispersion, consistent with comparable peer information reducing information-processing costs. Shroff et al. (2017) similarly find that the peer information environment is associated with a lower cost of capital, particularly when the focal firm's own information environment is weak. However, these studies do not directly predict whether firms will increase or decrease disclosure when public industry information changes.

One set of prior findings suggests that external information can complement firm disclosure by making firm-specific information more interpretable and by increasing the cost of silence or delay. Seo (2021) finds that peer disclosure induces focal-firm disclosure, consistent with peer disclosures providing information about industry conditions and increasing the capital-market cost of remaining silent. Truong (2023) provides related evidence on disclosure timing, showing that firms accelerate earnings announcements when industry peers announce early. Although the study focuses on timing rather than disclosure content, the findings are consistent with the broader idea that peer information changes disclosure incentives by increasing the cost of withholding or delaying information. Applied to the CIR setting, these studies suggest that public product-level benchmarks, even in aggregate, may support firm operating disclosure by helping users interpret disclosed quantities and by making nondisclosure more informative. However, the peer-disclosure setting also illustrates the empirical challenge: peer firms and focal firms may disclose in response to the same latent industry developments, making it difficult to separate a causal benchmark effect from

common changes in the industry information environment.

Other prior findings are consistent with the competing view that external information can substitute for firm disclosure. Breuer et al. (2022) find that mandatory disclosure by regulated firms crowds out voluntary disclosure by related unregulated firms, especially when information spillovers are stronger. Capkun et al. (2023) provide related evidence in a nonfinancial disclosure setting, showing that firms are less likely to disclose their own clinical trial results when closely related peer trial results have already been disclosed. These findings are especially relevant because they examine disclosure of operating or economically specific information rather than only financial statement information. However, settings such as clinical trial disclosure also involve strategic interactions among competitors, since firms may consider how rivals will respond to information about product development, technological progress, or market opportunities. Thus, prior evidence supports the possibility of substitution, but it also underscores why the relation between external benchmarks and voluntary disclosure is difficult to isolate empirically. Applied to the CIR setting, public industry statistics may reduce demand for firm-level operating quantities when they allow users to infer product demand, production conditions, or capacity utilization. The prior empirical literature therefore reinforces the theoretical ambiguity: public benchmarks may either complement firm disclosure by improving interpretation and increasing the cost of silence, or substitute for firm disclosure by supplying related information from outside the firm.

2.3 Current Industrial Reports Setting

This section describes the Census Bureau's Current Industrial Reports (CIR) program, its historical development, its role within the broader system of public manufacturing data, its discontinuation, and the timing of the resulting information shock. The CIR was a long-running federal statistical program that provided recurring measures of U.S. industrial activity for selected manufactured products. The program traced its origins to 1904, when the Census Bureau began collecting current industrial statistics through early cotton and fats-and-oils surveys (U.S. Census Bureau, 2021a,b). The program developed gradually over the twentieth century rather than through

a single expansion event. The selection and continuation of surveys reflect a combination of Census priorities, federal agency demand, industry demand, private-sector relevance, and data-collection feasibility. Most CIR surveys were funded with Census Bureau funds, but some were funded fully or partially by other federal agencies or private trade organizations (U.S. Census Bureau, 2021a,b).

Over time, the early current product surveys became part of a broader Census manufacturing statistics system that also included the half-decade Economic Census and the Annual Survey of Manufactures. The CIR occupied a distinct role within this system. While the Economic Census and Annual Survey of Manufacturers provided broad measures of manufacturing activity, the CIR provided recurring information at a more detailed product level.

By the late 2000s, the CIR had become a sizeable but selective manufacturing information program. In its mature form, the program consisted of approximately 47 surveys covering about 40,000 establishments and roughly one-third of manufactured products in the U.S. manufacturing sector (U.S. Census Bureau, 2021a). The covered areas included textiles and apparel, chemicals, primary metals, computer and electronic components, industrial equipment, consumer goods, cotton, fats and oils, aerospace equipment, and other selected areas of manufacturing products (U.S. Census Bureau, 2021a,b). The Census also updated survey panels for most reports annually, reconciled the CIR estimates to the Annual Survey of Manufactures and Economic Census, and periodically modified surveys to provide more current information on products produced. When a small number of producers existed, the CIR surveys could cover all known producers of a product, while products with larger producer populations were covered using cutoff and random sampling techniques (U.S. Census Bureau, 2021b).

2.3.1 The Current Industrial Report Data

A central feature of the CIR was that it reported product-level operating quantities, rather than dollar values of manufacturing activity. Many federal manufacturing datasets describe activity at the industry level using the value of shipments, orders, inventories, or other accounting-based measures. The CIR reports were different because they often tabulated the number of products

produced or shipped within specific categories of manufactured products. Depending on the product, these quantities could be reported in units, tons, pounds, square feet, barrels, or other physical measures. This made the CIR relevant to data users who needed to observe real activity in physical terms rather than infer quantities from dollar sales.

This distinction is important because dollar-based manufacturing data and quantity-based manufacturing data are not interchangeable. Dollar shipment values combine both price and quantity. A change in the dollar value of shipments may reflect more units sold, higher prices, changes in product mix, inflation, or some combination of these factors. By contrast, physical quantity data more directly measure the underlying scale of production or shipments and in greater detail. For users interested in product flows, capacity, demand, inventories, waste generation, or benchmarking across firms and industries, recurring product-level data provide information that dollar values alone cannot fully recover.

The CIR also differed from broader manufacturing datasets in the frequency and speed with which it provided product-level information. Monthly CIR surveys were targeted for release within 30 to 45 days, quarterly surveys within 60 to 90 days, and annual surveys within 6 to 9 months after the reference period. Annual surveys were mandatory, whereas the monthly and quarterly versions were voluntary (U.S. Census Bureau, 2021b). I focus on the annual version of the surveys. The loss was not simply the disappearance of a historical data series; it was the disappearance of recurring public product-level information that was available between broader Economic Census and Annual Survey of Manufactures releases.

The CIR therefore occupied a different role from other Census manufacturing products. Other Census manufacturing products continued to provide relevant information after the CIR ended, but they captured different aspects of industrial activity. The Manufacturers' Shipments, Inventories, and Orders survey and the Annual Survey of Manufactures continued to provide information on manufacturing activity, but primarily in dollar terms and with less detailed physical quantity information than the CIR. The Economic Census covered nearly the full manufacturing sector and provided detailed establishment, industry, and product information every five years (U.S.

Census Bureau, nd, 2000). These sources remained important for tracking the level and composition of manufacturing activity. However, they did not preserve the CIR's combination of recurring frequency and detailed product-level physical quantity information. Thus, the loss of the CIR did not eliminate public Census manufacturing data; rather, it reduced public access to a specific type of information: detailed recurring measures of physical manufactured products.

Facing tighter federal spending constraints in preparing its fiscal year 2012 budget, the Census Bureau undertook a broader review of its ongoing and cyclical statistical programs and concluded that some programs would need to be reduced or terminated to finance higher-priority initiatives (U.S. Census Bureau, 2021a). The CIR was ultimately terminated as part of this broader effort to fund major population and statistical initiatives, including expansion of the American Community Survey, early research and testing for the 2020 Census, and preparations for the 2012 Economic Census (U.S. Census Bureau, 2011a). The fiscal year 2012 budget proposed eliminating the program's 35 full-time positions and $4.0 million in base funding (U.S. Census Bureau, 2011a). As part of the review, the Census Bureau consulted key data users about the consequences of eliminating the CIR and concluded that other manufacturing statistics could partially mitigate its loss (U.S. House of Representatives, Committee on Appropriations, 2011).

The proposed termination nevertheless raised concerns among users of CIR information. Census documentation identifies federal agencies, private firms, trade associations, and market analysts as users of CIR data. Private firms used the data to evaluate company performance relative to industry performance, assess current business conditions, conduct market analysis, and plan future operations (U.S. Census Bureau, 2021a). In 2011, five industry associations wrote to the Department of Commerce emphasizing the importance of the CIR to U.S. manufacturing and noting that smaller firms were particularly unlikely to have the resources to replicate the information when making internal operating decisions (U.S. Congress, Joint Economic Committee, 2012). Agricultural stakeholders similarly raised concerns about the loss of CIR information following the cancellation announcement (U.S. Department of Agriculture, National Agricultural Statistics Service, 2014). Some CIR surveys had also historically been funded fully or partially by other federal agencies

or private trade organizations (U.S. Census Bureau, 2021a). The consequences also extended to government users. For example, the Federal Reserve later reported that the CIR discontinuation "significantly affected" its industrial production indexes and required a search for alternatives (Board of Governors of the Federal Reserve System, 2014). Despite these concerns, the Census Director characterized the termination as one of several difficult program choices made in anticipation of tight spending limits (Groves, 2011). Given the target release windows of six to nine months after the reference period for annual surveys (U.S. Census Bureau, 2021b), users of annual CIR reports likely did not experience the resulting cutoff in industrial data until 2012.

I therefore treat 2012 as the first clean post-treatment year following the cancellation of the CIR program in Q4 2011. This change removed a source of detailed quantity information that allowed outside users to observe how much of particular products were produced or shipped within detailed manufacturing industries. Since the discontinuation was driven by Census budget constraints rather than by firms' disclosure choices, the setting provides a plausibly exogenous change in the availability of public operating information.

2.4 Hypothesis Development

The CIR is a recurring source of public industry operating data. The effect of removing such data on firms' own disclosure choices is not obvious. Public industry data may either complement firm disclosure or substitute for it, and these alternatives imply different predictions for how firms respond when the CIR disappears.

One possibility is that public industry data complements firm disclosure by providing an external benchmark for investors. When investors can compare a firm's operating disclosures to related industry activity, the disclosed information becomes easier to interpret: investors can better assess whether the firm's performance is strong, weak, or typical. The same benchmark also makes nondisclosure more informative because investors can form expectations about the firm's operating activity and draw stronger inferences when comparable information is withheld. Thus, public industry data can increase disclosure incentives by making both disclosure and nondisclosure

more meaningful. This logic is consistent with evidence that peer information shapes firms' disclosure choices, such as Seo (2021), which shows that industry-peer disclosures induce firms' own disclosures. After the CIR disappears, investors have less public information with which to interpret firm-level operating quantities or evaluate silence, reducing firms' incentives to disclose under the complementarity view.

A competing possibility is that public industry data substitutes for firm disclosure. If investors can infer enough about firm-level conditions from public industry data, managers may face less demand to provide their own operating disclosures. This logic is consistent with evidence that outside disclosures can crowd out voluntary disclosure by related firms. Breuer et al. (2022) show that mandatory disclosures by some firms can reduce voluntary disclosure by other related firms when those mandatory disclosures create information spillovers. Applied to the CIR, this substitution view implies that the public industry data provided by the CIR may have reduced demand for firms' own operating disclosures. If so, the removal of the CIR could increase firm disclosure because managers may need to replace information that investors previously obtained from the public benchmark.

Applying these competing views to the CIR program cancellation yields two alternative predictions. If the CIR complemented firm disclosure by making firm-level information more interpretable and nondisclosure more informative, treated firms should reduce operating disclosure after the CIR ends. If the CIR substituted for firm disclosure by providing investors with information that reduced demand for firm-specific disclosure, treated firms should increase operating disclosure after the CIR ends.

Applying these competing views to my setting leads to the first set of hypotheses:

H1a: Following the CIR program cancellation, treated firms reduce unit-based operating disclosure relative to control firms.

H1b: Following the CIR program cancellation, treated firms increase unit-based operating disclosure relative to control firms.

The distinction between these hypotheses suggests differing mechanisms. A decline in

disclosure would be more consistent with the view that the CIR complemented firm disclosure by making disclosed quantities more interpretable. An increase in disclosure would be more consistent with the view that the CIR substituted for firm disclosure by providing investors with information that firms later needed to replace.

An obvious alternative explanation for CIR stoppage and subsequent disclosure reduction by firms is that the underlying operations information was no longer relevant to investors. To rule out this alternative, I examine several cross-sectional variations. If the CIR provided a relevant benchmark, its loss should matter most when investors could apply that benchmark to the firm. Product-market similarity provides such a setting. If the decline in disclosure merely reflected an irrelevance of physical operating information, one would have to explain why the response appears to vary systematically with settings in which the CIR was more relevant as a benchmark. The CIR reports supplied standardized product-level quantity information that individual peer disclosures generally do not provide. When firms have similar product-market peers, investors can more easily use the CIR's industry-level data to evaluate whether a firm's disclosed quantities appear high, low, or unusual. Thus, similar peers make the CIR information more useful for interpreting firm-level disclosure and nondisclosure. Under the benchmark-complement view, the disclosure decline should therefore be strongest among firms with more similar product-market peers.

H2: The decline in unit-based operating disclosure following the CIR program cancellation is more pronounced among firms with greater product-market similarity.

A related implication is that the CIR should matter more when public peer benchmarking is more relevant. The CIR reports provide standardized product-level industry data, but investors must still connect those data to the public firms they value. This connection is easier in industries with more public firms, because investors can combine the CIR benchmarks with public peers' filings, financials, and market information to form expectations about the focal firm's operating quantities and form conclusions about silence. In industries dominated by private firms, the CIR may describe overall product-market activity, but investors have fewer publicly observable peer firms with which to interpret the benchmark. Therefore, under the benchmark-complement view,

the decline in unit-based disclosure following the CIR program cancellation should be stronger in industries with greater public-firm presence.

H3: The decline in unit-based operating disclosure following the CIR program cancellation is more pronounced in industries with greater public-firm presence.

A separate alternative explanation is based on proprietary costs, which is a major cost in papers examining information crowding out other disclosures. The CIR program cancellation may have changed the proprietary cost of firm-specific operating disclosures. If public industry statistics are no longer available, a firm's own physical quantity disclosures may become more informative to competitors. Under this view, firms reduce disclosure not because benchmark-enabled inference weakened, but because firm-specific quantity information became more competitively sensitive after the public benchmark disappeared. This explanation predicts that the disclosure decline should be strongest where competitive velocity or product-market movement is greatest.

H4: The decline in unit-based operating disclosure following the CIR program cancellation is more pronounced among firms facing greater product-market fluidity.

Finally, if the CIR supplied useful public benchmark information, its removal should have implications beyond firms' disclosure choices. Losing product-level benchmarks should make it harder for analysts and investors to interpret firm-level operating activity. I therefore examine whether the CIR program cancellation is associated with changes in the capital-market information environment.

H5: Following the CIR program cancellation, treated firms experience higher analyst forecast error, fewer analyst forecasts, and wider bid-ask spreads relative to control firms.

Together, these hypotheses organize the empirical tests. H1a and H1b distinguish whether the CIR complemented or substituted for firm disclosure. H2 and H3 test whether the disclosure response is concentrated where benchmarking should be most useful. H4 evaluates whether the results are better explained by proprietary-cost concerns arising from competitive threat. H5 examines whether the removal of the CIR is associated with broader information-environment consequences.

3 Data

3.1 Current Industry Report Data

I begin with available annual CIR reports, which are stored in the Southern New Hampshire University Archive, and use these reports to identify CIR coverage over the period 1991 through 2015 (Southern New Hampshire University, nd). I use the data from the reports to construct an industry-year history of CIR coverage and to identify which industries were impacted by the discontinuation of the program, which was due to the government budget in contrast to year-to-year changes in program coverage that were in response to stakeholder demand (U.S. Census Bureau, 2021a,b).

The Census Bureau discontinued the CIR data collection during Q4, 2011, canceling the standard annual release of the 2011 annual reports that would have been primarily released in 2012 (U.S. Census Bureau, 2011b). Therefore, I define fiscal year 2012 as the first clean post-treatment year.

I classify firms as treated if they were covered during the final reporting year of 2010. Firms in these NAICS-6 industries were exposed to the loss of recurring public industry data when the CIR ended. Firms in industries not covered by the CIR serve as control firms. Firms that had coverage dropped during the 2008 through 2011 pre-period are dropped from the sample. This treatment definition captures exposure to the withdrawal of a public industry benchmark rather than whether an individual public firm directly responded to a Census survey.

3.2 Firm-Year Panel, Disclosure Measures, and Data Sources

I construct the main analysis sample at the firm-year level. I begin with U.S. public firms in Compustat and merge them to the 10-K disclosure database using firm identifiers and fiscal year. The use of a firm-year panel follows the annual nature of firms' Form 10-K disclosures and the annual timing of the primary CIR reports. I then merge the CIR industry-year coverage file described in Section 3.1 to the Compustat/10-K panel using firms' industry classifications. This

merge assigns each firm-year to an NAICS-6-level CIR treatment status.

The main sample covers fiscal years 2008 through 2015, where 2008 through 2011 serve as the pre-period and 2012 through 2015 serve as the post-period. Firms are classified as treated if their NAICS-6 industry was covered by the CIR in the final CIR release. Firms in industries not exposed to the loss of final-period CIR coverage serve as control firms. Because treatment is assigned at the NAICS-6 industry level, I require each firm-year to have a valid NAICS-6 industry classification and a valid pre-period CIR coverage classification.

Table 1 reports the sample-construction process using unique NAICS-6 industry and firm counts. Panel A reports the CIR-covered portion of the sample. The historical Compustat sample identifies 170 distinct six-digit NAICS industries classified as treated over the 1991–2010 period, corresponding to 6,921 distinct Compustat firms in treated industries. Restricting the sample to the main 2008–2015 window reduces the CIR-covered portion to 170 distinct industries and 6,045 distinct firms. Dropping industries that lost CIR coverage in 2008–2009 leaves 170 distinct industries and 6,045 distinct firms. Requiring valid 10-K disclosure data reduces the CIR-covered portion to 163 distinct industries and 3,271 distinct firms, and requiring non-missing baseline regression variables yields 162 distinct CIR-covered industries and 2,893 distinct firms.

Panel B reports the corresponding full Compustat/10-K sample. The Compustat universe over 1991–2015 contains 1,295 distinct six-digit NAICS industries and 23,728 distinct firms. Restricting the sample to 2008–2015 reduces the sample to 1,053 distinct industries and 16,842 distinct firms. Dropping industries that transitioned in the pre-period leaves 1,027 distinct industries and 15,696 distinct firms. Requiring valid 10-K disclosure data reduces the sample to 878 distinct industries and 7,608 distinct firms, and requiring non-missing baseline regression variables yields the main regression sample of 854 distinct industries and 6,915 distinct firms.

Based on the discussion in Section 2.2, I focus on unit-based operating disclosures as the primary firm-level disclosure outcome. These disclosures are the annual-report disclosures most directly related to the type of operating information removed by the CIR program cancellation. I construct three primary measures. The first is an indicator equal to one if the firm provides at

least one unit-based operating disclosure during the fiscal year. The second is the log frequency of unit-based operating disclosure terms in the filing. The third is the log breadth of unit-based operating disclosure, measured using the number of distinct disclosure categories appearing in the filing. These measures capture whether firms provide operating quantity information, how frequently they provide it, and how broadly they discuss different types of operating activity.

The primary disclosure dictionary focuses on physical-unit references, which consist of a number and its physical unit of measurement, because those disclosures are most closely aligned with the product-level operating data provided by the CIR. For robustness, I also construct a broader set of expanded quantity disclosure measures that include related operating quantity terms, including production, volume, shipments, inventories, capacity, and activity-based disclosures. As with the primary measures, I construct binary, frequency, and breadth versions of the expanded quantity measure. In additional analyses, I repeat the disclosure measurement using only the Management's Discussion and Analysis section of the 10-K to examine whether the results appear within the section of the filing most directly related to operating performance and business conditions.

I use Form 10-K filings because the disclosures of interest are annual operating disclosures and because the CIR information shock is naturally aligned with fiscal-year reporting. In all baseline specifications, I also control for the log of filing word length to ensure that the results are not driven by changes in the overall length of firms' annual reports.

I obtain firm financial statement variables from Compustat. The main control variables are log total assets, return on assets, leverage, property, plant, and equipment scaled by assets, inventory scaled by assets, and log filing word length. These controls capture differences in firm size, profitability, capital structure, asset intensity, inventory intensity, and disclosure length. I merge these variables to the disclosure panel using firm identifiers and fiscal year.

I also use CIR coverage histories to construct a matched-sample robustness test. For each NAICS-6 industry, I identify the last year in which the industry was covered by the CIR. Industries covered during the 2010 reporting year form the main treated group (the CIR program was cancelled in Q4, 2011). Industries that lost CIR coverage before the main sample period of 2008 through

2015 provide a comparison group because they were historically covered by the CIR but were not exposed to the program-wide termination after the final 2010 reports.[3] This matched-sample design compares treated firms to firms in industries with more similar historical exposure to CIR reporting.

To examine whether the CIR program cancellation affected the capital-market information environment, I obtain analyst forecast data from IBES and market data from CRSP. I use IBES annual EPS forecasts to construct analyst forecast error and analyst forecast count at the firm-year level. I merge IBES to the Compustat panel using the standard IBES-CRSP and CRSP-Compustat links. I obtain bid-ask spreads from CRSP daily data and aggregate the measure to the firm-year level. These variables allow us to test whether the loss of the CIR is associated with changes in analyst information production and trading frictions.

I also construct a NAICS-6 industry-level measure of public-firm presence using the Statistics of U.S. Businesses (SUSB) and the Compustat public-firm sample (U.S. Census Bureau, 2013). For each NAICS-6 industry, I compare the number of public firms identified in Compustat with the total number of employer firms reported by SUSB using the 2008 through 2011 pre-period. The resulting measure captures the share of firms in an industry represented by public firms in Compustat and is intended to proxy for the availability of same-industry public peers that investors can use for benchmarking. Because SUSB does not separately identify public and private firms, the measure, which combines SUSB and Compustat should be interpreted as a noisy proxy for public-firm presence rather than a measure of public firms' market share. I merge the resulting industry-level measure to the main panel using NAICS-6 classifications.

To explore product-market fluidity, I use the Hoberg-Phillips product-market fluidity measure (Hoberg et al., 2014) to examine whether the results are driven by proprietary-cost or competitive-threat channels. Product-market fluidity captures movement in firms' product markets and provides a proxy for competitive threat or product-market instability. If the disclosure decline is primarily driven by proprietary costs, the effect should be stronger in high-fluidity settings. I use this measure to distinguish the benchmarking interpretation from the alternative explanation that the CIR program

[3] See footnote 2 for a discussion of changes to CIR survey coverage while the program was ongoing.

cancellation increased the competitive sensitivity of firm-specific quantity disclosures.

Finally, to examine the benchmarking channel, I use product-market similarity measures from Hoberg and Phillips (2016). I construct pre-period measures (2008 through 2011) of product-market similarity using data before the CIR program cancellation and merge these measures to the main Compustat/10-K panel. The primary measures are average product-market similarity and total pairwise similarity score. These variables capture the extent to which a firm has economically similar product-market peers. If the CIR supported disclosure by helping investors benchmark firms, the disclosure response to the CIR program cancellation should be stronger among firms with greater product-market similarity.

3.3 Descriptive Statistics

Table 2 presents descriptive statistics for the variables used in the main analyses. The main unit of observation is the firm-year. The descriptive sample includes 40,228 firm-years with valid CIR treatment classification, while the baseline regression specifications requiring non-missing control variables use 35,798 firm-years. Appendix A contains detailed descriptions of all variables.

In the descriptive sample, approximately 41 percent of firm-years are in industries affected by the CIR program cancellation, and approximately 50 percent of observations occur in the post period. Physical-unit disclosure appears in 28.3 percent of firm-years, while expanded quantity disclosure appears in 39.3 percent of firm-years. The frequency and breadth measures are right-skewed, as reflected in median values of zero for the disclosure-count variables. This pattern is expected because many firms do not provide physical quantity disclosures in a given annual report, while firms that do disclose may provide multiple unit references or refer to multiple categories of operating quantities.

The control variables indicate substantial variation in firm characteristics across the sample. The average log of total assets is 6.239, while the median is 6.440, which is a bit smaller than the median of 7.095 in Hoitash and Hoitash (2018).[4] The median return on assets is 1.2 percent,

[4]Hoitash and Hoitash (2018) covered the fiscal years 2011–2014, which overlaps with the latter years of my sample.

although the mean is negative, reflecting the presence of loss firms. The median leverage ratio is 18.4 percent, and the median ratios of property, plant, and equipment and inventory to assets are 11.1 percent and 1.7 percent, respectively. The mean log filing length is 10.722, consistent with substantial variation in the length of annual reports, which is similar to the 11.05 length in Hoitash and Hoitash (2018).

4 Results

4.1 Testing Hypothesis H1: The Main Result

Table 3, Panel A presents the baseline difference-in-differences estimates of the effect of the CIR program cancellation on firms' unit-based operating disclosures. All specifications include firm and fiscal-year fixed effects, and standard errors are clustered by NAICS-6 industry. The dependent variables capture three margins of operating quantity disclosure: whether the firm provides any unit-based disclosure, the frequency of unit-related disclosure, and the breadth of distinct unit categories disclosed. For each outcome, I estimate specifications with and without the log of total 10-K word length. The word-length control helps distinguish changes in unit disclosure from broader changes in annual-report length.

Panel A reports results for physical-unit disclosure, the primary disclosure measure. Across all specifications, the coefficient on Treat × Post is negative and statistically significant. In column (1), the coefficient on Treat × Post is –0.018, indicating that treated firms are 1.8 percentage points less likely to disclose physical-unit information after the CIR program cancellation relative to control firms. This effect represents approximately a 6 percent decline relative to the sample mean of 28.3 percent. In column (2), adding the log of word length reduces the coefficient modestly to –0.014, but the estimate remains statistically significant. This suggests that the decline is not simply attributable to treated firms filing shorter annual reports after the CIR program cancellation.

Columns (3) and (4) examine the frequency of physical-unit disclosure. The coefficient on Treat × Post is –0.032 after controlling for filing length. These estimates imply that treated

firms reduce the intensity with which they discuss physical operating units after the CIR program cancellation. Columns (5) and (6) examine the breadth of physical-unit disclosure. The coefficient on Treat × Post is –0.022 without the word-length control and –0.017 with the word-length control, indicating that treated firms also reduce the range of unit categories discussed in their annual reports. Overall, Panel A provides consistent evidence that the CIR program cancellation is associated with a decline in physical-unit disclosure along the extensive, intensive, and breadth margins.

Following the baseline specification, Figure 1 plots coefficients from an event-study specification to examine pre-treatment dynamics and the evolution of treated-control differences around the CIR program cancellation. The 2008 through 2011 coefficients are close to zero and statistically insignificant, providing no evidence of differential pre-trends between treated and control firms. The estimated effects become negative after the CIR program cancellation, with the strongest declines appearing in the early post-period. This pattern is consistent with treated firms reducing physical-unit disclosure after the loss of the CIR rather than continuing pre-existing disclosure trends.

4.2 Additional Tests of H1: Expanded Quantity Disclosure and MD&A

Table 3, Panel B repeats the analysis using the broader expanded quantity disclosure measure. The results are similar. The coefficient on Treat × Post is negative across all six specifications and remains statistically significant after controlling for filing length. In the word-length-controlled specifications, treated firms are 1.8 percentage points less likely to provide expanded quantity disclosure, reduce the frequency of expanded quantity terms by 0.034, and reduce the breadth of expanded quantity disclosure by 0.023. These results indicate that the decline is not limited to a narrow physical-unit dictionary. Instead, the CIR program cancellation is associated with a broader reduction in firms' operating quantity disclosures.

Table 4 examines unit disclosure within Management's Discussion and Analysis. This analysis helps address the possibility that the main results are driven by boilerplate or by sections of the 10-K less directly related to managers' operating discussion. The estimates again show a negative disclosure response. In the word-length-controlled specifications, the coefficient on Treat

× Post is -0.013 for the binary disclosure measure, -0.030 for frequency, and -0.014 for breadth. These results suggest that the decline in unit disclosure appears within MD&A, where operating performance and business conditions are most directly discussed.

Together, Table 3, Panel B and Table 4 provide additional support for H1a. The disclosure decline appears in both broader operating quantity measures and in the MD&A section, suggesting that the baseline result is not unique to one dictionary definition or one part of the filing.

4.3 Robustness of H1: Matched Control Firms with Prior CIR Coverage Loss

Table 5 presents a matched-sample robustness test. This analysis compares firms affected by the CIR program cancellation to firms in industries that previously lost CIR coverage before the sample period. The purpose of this test is to compare treated firms to a control group with more similar historical exposure to CIR reporting.

The estimates remain negative across all disclosure outcomes. The binary outcomes are negative but not statistically significant, while the frequency and breadth outcomes are statistically significant for both physical-unit and expanded quantity disclosure. In the word-length-controlled matched sample, treated firms reduce physical-unit disclosure frequency by 0.081 and breadth by 0.035, and reduce expanded quantity disclosure frequency by 0.094 and breadth by 0.045. These results suggest that the main findings are not driven by comparisons between CIR-covered industries and wholly unrelated control industries.

In sum, the baseline and matched-sample analyses support H1a. Firms exposed to the loss of the CIR reduce unit-based operating disclosure after the program cancellation. The evidence is inconsistent with the simple substitution view, under which firms would replace the missing public benchmark by increasing their own quantity disclosures. Instead, the results are more consistent with the view that the CIR complemented firm disclosure by making disclosed quantities more interpretable and nondisclosure more informative. When the public benchmark disappears, treated firms disclose less.

4.4 Testing Hypothesis H2: Product-Market Similarity and Benchmarking

Table 6, Panel A examines whether the disclosure decline following the CIR program cancellation is concentrated in firms for which benchmarking should be most useful. If the CIR operated as a public product-level benchmark, its removal should matter more when investors can compare the firm to economically similar product-market peers. In these settings, disclosed quantities are easier to interpret, and nondisclosure is more informative because investors can evaluate the firm against similar public firms and an external distribution of product-level activity. Hypothesis H2 therefore predicts that the disclosure decline should be stronger among firms with greater product-market similarity.

To test this prediction, I use Hoberg-Phillips product-market similarity measures constructed before the CIR program cancellation (2008 through 2011). In Panel A, I classify firms into low and high average similarity groups based on whether their pre-period average similarity is below or above the sample median. I then estimate the baseline difference-in-differences specification separately within each subsample. The dependent variables are the binary, frequency, and breadth measures of physical-unit disclosure, and all specifications include firm and fiscal-year fixed effects, firm controls, and the log of filing word length.

The results support Hypothesis H2. In the low average similarity subsample, the coefficient on Treat $\times$ Post is small and statistically insignificant across all three disclosure outcomes. The estimated coefficients are 0.005 for the binary disclosure measure, –0.009 for disclosure frequency, and 0.002 for disclosure breadth. In contrast, the estimated effects are negative and statistically significant in the high average similarity subsample. The coefficient on Treat $\times$ Post is –0.042 for the binary measure, –0.060 for frequency, and –0.034 for breadth. These estimates indicate that the decline in physical-unit disclosure is concentrated among firms with more similar product-market peers.

Taken together, Table 6, Panel A supports the benchmark-enabled inference interpretation of the main results. The CIR program cancellation does not appear to reduce unit disclosure uniformly across all treated firms. Instead, the effect is concentrated among firms for which investors had more

similar product-market peers to use as benchmarks. This pattern is consistent with the idea that the CIR made firm-level operating quantities more useful and nondisclosure more informative in settings where peer comparison was more feasible.

4.5 Testing Hypothesis H3: Public-Firm Presence and Public Peer Benchmarking

Table 6, Panel B examines a second implication of the benchmarking channel. If the CIR helped investors evaluate firm disclosures relative to public peers, the effect of the CIR program cancellation should be stronger in industries with greater public-firm presence. In these industries, investors can compare a firm's operating disclosures to a larger set of public firms in the same product market. Public-firm presence should therefore make the CIR benchmarks more useful and make nondisclosure decisions starker. Hypothesis H3 predicts that the decline in unit-based operating disclosure is more pronounced in industries with greater public-firm presence.

To test this prediction, I construct a NAICS-6 industry-level public-share measure using Statistics of U.S. Businesses data and the Compustat public-firm sample. Because SUSB reports the total number of employer firms but does not distinguish public from private firms, I measure public-firm presence as the number of public firms divided by the total number of employer firms in each NAICS-6 industry. This measure should be interpreted as a noisy proxy for public-firm presence rather than as a measure of public-firm market share. I classify firms into low and high public-share groups based on the 2008 through 2011 pre-period median and estimate the baseline specification separately within each group.

The results in Table 6, Panel B are consistent with Hypothesis H3. In industries with low public-firm presence, the coefficient on Treat $\times$ Post is small and statistically insignificant across the three disclosure outcomes. The estimates are –0.005 for the binary measure, –0.002 for frequency, and 0.002 for breadth. In contrast, in industries with high public-firm presence, the coefficients are negative and statistically significant. The coefficient on Treat $\times$ Post is –0.021 for the binary measure, –0.061 for frequency, and –0.038 for breadth. These estimates indicate that the disclosure

decline is concentrated in industries where public peer benchmarking is more feasible.

Together, Table 6, Panel B provides additional support for the benchmarking interpretation. The disclosure decline is larger when investors have more public peers to use in evaluating firm-level operating quantities and nondisclosure. This pattern is difficult to reconcile with a simple substitution view, under which firms should replace the lost public information by increasing their own disclosures. Instead, the evidence suggests that the CIR complemented firm disclosure most strongly in settings where public peer comparisons were available.

4.6 Testing Hypothesis H4: Proprietary Costs and Product-Market Fluidity

Table 6, Panel C examines whether the disclosure decline is stronger in high product-market fluidity settings. This test helps assess whether the results are primarily driven by proprietary-cost concerns, as an alternative to the complementary mechanism. If the CIR program cancellation made firm-specific quantity disclosure more competitively sensitive, the decline should be strongest among firms facing greater product-market movement or competitive threats.

The results do not show a clear high-fluidity pattern. The binary disclosure estimates are nearly identical in the low- and high-fluidity subsamples, with coefficients of –0.016 and –0.017, respectively, and neither estimate is statistically significant. For disclosure frequency, the effect is larger and statistically significant in the low-fluidity subsample, with a coefficient of –0.042, while the high-fluidity estimate is smaller and insignificant. For disclosure breadth, the high-fluidity estimate is negative and marginally significant, but it is similar in magnitude to the low-fluidity estimate.

Overall, Table 6, Panel C does not support the prediction that the disclosure decline is concentrated in high-fluidity industries. These results do not rule out proprietary-cost considerations, but they suggest that generic competitive threat or product-market movement is unlikely to be the primary driver of the main findings. The sub-sample evidence is more consistent with the benchmarking interpretation, where the decline is concentrated in settings with greater product-market similarity and public peer benchmarking.

4.7 Testing Hypothesis H5: Capital-Market Information Environment

Table 7 examines whether the CIR program cancellation is associated with changes in the capital-market information environment. If the CIR supplied useful public benchmark information, its removal should make it harder for analysts and investors to interpret firm-level operating activity. Hypothesis H5 therefore predicts that treated firms experience a deterioration in information-environment outcomes following the CIR program cancellation.

I examine three outcomes. The first is analyst forecast error, which captures the accuracy of analysts' earnings expectations. The second is analyst forecast count, which captures the amount of analyst forecast activity or updating. The third is the bid-ask spread, which captures trading frictions and adverse-selection costs. All specifications include firm and fiscal-year fixed effects, firm controls, and the log of filing word length.

The results are consistent with a deterioration in the information environment. In column (1), the coefficient on Treat × Post is 0.004 for forecast error and is statistically significant at the ten-percent level. This estimate indicates that analysts' forecasts become less accurate for treated firms after the CIR program cancellation relative to control firms. In column (2), the coefficient on Treat × Post is –0.036 for forecast count and is statistically significant at the ten-percent level, indicating a decline in analyst forecast activity. In column (3), the coefficient on Treat × Post is 0.003 for bid-ask spreads and is statistically significant at the one-percent level, indicating higher trading frictions after the CIR program cancellation.

These results suggest that the CIR program cancellation affected more than firms' disclosure wording. The loss of public product-level benchmarks is associated with worse analyst information outcomes and wider bid-ask spreads. This pattern is consistent with the interpretation that the CIR supplied useful benchmark information to capital-market participants, rather than being irrelevant, and that its removal reduced investors' ability to evaluate firm-level operating activity.

Overall, the evidence across Tables 6 through 7 supports the benchmark-enabled inference channel. Treated firms reduce unit-based operating disclosure after the CIR program cancellation, and this decline is concentrated where product-market and public-peer benchmarking should be

most useful. The information environment also deteriorates following the loss of the CIR. In contrast, the disclosure decline is not systematically stronger in high-fluidity settings, suggesting that proprietary-cost concerns related to generic competitive threat are unlikely to be the dominant explanation.

5 Conclusion

A fundamental question in the disclosure literature is how external information affects firms' incentives to provide their own disclosures. Prior research has primarily examined information supplied by peer firms or through financial statement comparability. I examine a distinct form of external information: government-provided aggregate industry benchmarks. The U.S. Census Bureau's Current Industrial Reports supplied recurring product-level physical quantity information on production, shipments, inventories, capacity, and related operating activity for selected manufacturing industries. The budget-driven cancellation of the CIR program, despite push-back from industry groups and government agencies(U.S. Census Bureau, 2011b; U.S. Congress, Joint Economic Committee, 2012) provides a setting in which a longstanding source of external industry information is removed independently of firms' own disclosure choices, allowing me to examine how firms respond when an aggregate public benchmark disappears.

The effect of removing such information on firms' disclosure incentives is theoretically ambiguous. Public industry benchmarks may substitute for firm disclosure by supplying information that investors would otherwise demand from firms. Alternatively, they may complement firm disclosure by making firm-specific operating information easier to interpret and by making nondisclosure more informative. Using the cancellation of the CIR program in a difference-in-differences design, I find that firms in affected industries reduce unit-based operating disclosure relative to firms in unaffected industries. The decline appears in the likelihood, frequency, and breadth of physical-unit disclosure, extends to broader operating quantity measures and disclosures within MD&A, and is also evident in a matched-sample design using industries with prior CIR coverage loss as controls.

These findings suggest that, in this setting, the complementary role of aggregate industry information dominates its potential substitution effect.

An obvious alternative explanation for CIR stoppage and subsequent disclosure reduction by firms is that the underlying operations information was no longer relevant to investors. To rule out this alternative, I examine various sub-sample evidence and it to be more consistent with the complementarity rather than the irrelevance interpretation. The disclosure decline is concentrated among firms with greater product-market similarity and in industries with greater public-firm presence, settings in which investors should be better able to use aggregate industry information and public peers to interpret firm-level operating disclosures and draw inferences from nondisclosure. In contrast, the decline is not systematically stronger in high product-market fluidity settings, suggesting that generic competitive-threat or proprietary-cost concerns are unlikely to be the primary explanation for the disclosure response. I also find that affected firms experience higher analyst forecast error, fewer analyst forecasts, and wider bid-ask spreads following the CIR program cancellation. These findings are consistent with the CIR supplying information that was useful to capital-market participants rather than merely changing firms' disclosure choices.

Overall, the evidence contributes to the disclosure literature in three ways. First, it provides evidence on which of the competing theoretical effects of external information dominates: removing a public industry benchmark reduces rather than increases firm disclosure, consistent with aggregate industry information complementing firms' voluntary disclosures. Second, the setting provides variation in external information generated by the budget-driven cancellation of a government statistical program rather than by the disclosure choices of related firms, helping separate the effect of the information environment from strategic interactions among disclosing firms. Third, the findings extend prior research on peer information by showing that aggregate information about common industry conditions can itself shape firms' disclosure incentives. More broadly, the results highlight a role for government statistical programs beyond the direct information contained in the published data. By providing common benchmarks against which investors evaluate both disclosed information and firms' decisions to remain silent, these programs can also affect firms' incentives to

produce information themselves.

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Figure 1: Pre-trend Visualization via Treatment × Year Physical-Unit Disclosure Coefficients

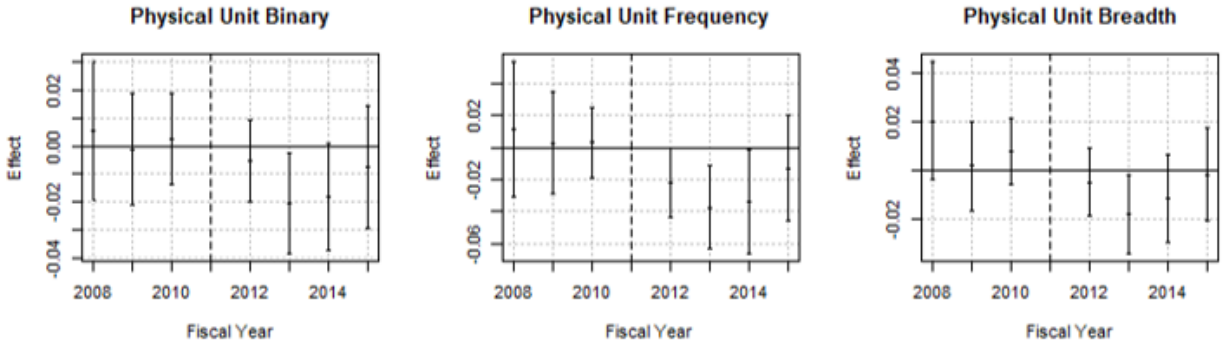
I represent physical-unit disclosure with the coefficients $\widehat{\beta}_y^j$ in regressions of physical-unit disclosure outcomes on interactions between indicators for each fiscal year and an indicator for firms in industries covered by the Census Bureau's Current Industrial Reports in its final reporting year of 2010 (the CIR program was canceled in Q4, 2011), controlling for firm and fiscal-year fixed effects, firm-level controls, and filing length.

$$Y_{i,t}^j = \sum_{\substack{y=2008 \\ y\neq 2011}}^{2015} \hat{\beta}_y^j \left(\mathbf{1}\{fyear_t = y\} \times \text{Treat}_i\right) + \text{FirmFE}_i + \text{YearFE}_t + \text{Controls}_{i,t} + \varepsilon_{i,t}^j \tag{1}$$

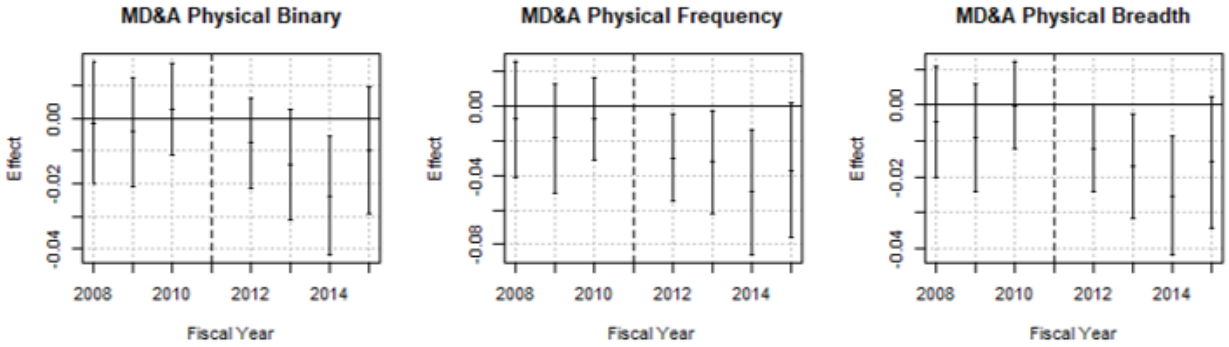
The dependent variable, $Y_{i,t}^j$, represents one of three physical-unit disclosure measures (indexed by j) for firm i in fiscal year t (see Appendix A and Table 1 for variable and sample details). The first measure is an indicator equal to one if the firm provides at least one physical-unit disclosure in its Form 10-K. The second measure is the log frequency of physical-unit disclosure, calculated as the natural logarithm of one plus the number of physical-unit references identified in the filing. The third measure is the log breadth of physical-unit disclosure, calculated as the natural logarithm of one plus the number of distinct physical-unit categories appearing in the filing. Thus, j indexes the extensive margin, intensive margin, and breadth of physical-unit disclosure. $Treat_i$ is an indicator equal to one for firms operating in industries previously covered by the Census Bureau's Current Industrial Reports. $Controls_{i,t}$ includes firm size, profitability, leverage, property, plant, and equipment, inventory, and filing length controls. Fiscal year 2011 is the omitted reference year, so each coefficient $\widehat{\beta}_y^j$ captures the treated-versus-control difference in disclosure outcome j in fiscal year y relative to the treated-versus-control difference in 2011.

The figure compares disclosure patterns in 2008 through 2011 against 2012 through 2015, which represents years before and after the cancellation of the CIR program in Q4, 2011. The coefficients in the years before the program cancellation (2008 through 2011) show no systematic differential trend between treated and control firms, consistent with the parallel-trends assumption. After the cancellation of the CIR program in Q4, 2011, the estimated coefficients generally decline during 2012 through 2015, indicating that firms in industries previously covered by CIR reduce disclosure relative to firms in industries not covered by CIR. This pattern is shown separately for the full 10-K, excluding exhibits, and for MD&A only, and across binary, frequency, and breadth measures of physical-unit disclosure.

Entire 10-K, Not Including Exhibits



MD&A Only



Notes: The figure plots event-study coefficients relative to the omitted fiscal year, 2011. Bars indicate confidence intervals based on standard errors clustered by NAICS-6 industry. The dashed vertical line marks the program cancellation of the Census Bureau's Current Industrial Reports.

Appendix A: Variable Definitions

Variable	Definition (i=firm, t=fiscal year)
$Treat_i$	An indicator equal to one if firm i operates in a Compustat NAICS-6 industry covered by the Current Industrial Reports program in its final reporting year of 2010 (the CIR program was canceled in Q4, 2011), and zero otherwise. Firm-years in NAICS-6 industries whose last CIR coverage year was 2008 or 2009 are excluded from the main sample. CIR coverage is merged to Compustat by Gvkey, fiscal year, and fiscal year-end date. Source: Constructed from CIR coverage files and Compustat NAICS-6 industry classifications.
$Post_t$	An indicator equal to one for fiscal years 2012 through 2015 and 0 in fiscal years 2008 through 2011 (the CIR program was canceled in Q4, 2011). Source: Constructed from Compustat fiscal year.
Log Physical Unit Frequency$_{i,t}$	The natural logarithm of one plus the total number of valid physical-unit disclosure term matches identified in firm i's Form 10-K for fiscal year t. The measure is constructed by tokenizing the Form 10-K text and searching for terms in the physical-unit dictionary. The dictionary combines production unit terms, mass and volume terms, energy unit terms, and logistics unit terms. Production unit terms include phrases such as units sold, units shipped, units produced, units delivered, unit shipments, unit volume, in millions of units, and in thousands of units. Mass and volume terms include tons, tonnes, barrels, kilograms, pounds, boe, and bbl. Energy terms include megawatts, gigawatts, kilowatts, mwh, and gwh. Logistics terms include shipments, loads, deliveries, passengers, containers, teus, and railcars. A physical-unit term is counted only when it appears directly adjacent to a numerical value, so that the measure captures quantity-bearing physical-unit disclosure rather than general references to units or operating activity. Matches whose surrounding text indicates non-operating or accounting uses of unit terminology, such as business unit, reporting unit, unit cost, unit price, unit margin, unit of account, or unit of measure, are excluded. The variable is computed as $\log(1 + \text{Physical Unit Frequency}_{i,t})$. Source: Constructed from parsed Form 10-K filings.
Physical Unit Binary$_{i,t}$	An indicator that equals one when Physical Unit Frequency$_{i,t}$ is greater than zero.
Log Physical Unit Breadth$_{i,t}$	The natural logarithm of one plus the number of distinct physical-unit disclosure categories identified in firm i's Form 10-K for fiscal year t. Valid physical-unit matches are defined as in Log Physical Unit Frequency$_{i,t}$. Physical-unit categories are based on the dictionary subfamilies: production units, mass and volume units, energy units, and logistics units. A category is counted when the filing contains at least one valid matched term from that category. The variable is computed as $\log(1 + \text{Physical Unit Breadth}_{i,t})$.

Variable	Definition
Log Expanded Unit Frequency$_{i,t}$	The natural logarithm of one plus the total number of valid expanded operating quantity disclosure term matches identified in firm i's Form 10-K for fiscal year t. The measure is constructed by tokenizing the Form 10-K text and searching for terms in the expanded operating quantity dictionary. The expanded dictionary includes all valid physical-unit terms used to construct Log Physical Unit Frequency$_{i,t}$ and adds broader non-physical operating quantity references related to production activity, volume, inventory, capacity, utilization, throughput, and other operating activity measures. Incremental production and activity terms include words and phrases such as production, produced, output, sales volume, production volume, volume growth, activity levels, operating activity, and production rates. Incremental inventory, capacity, and utilization terms include inventory, inventories, capacity, production capacity, utilization, capacity utilization, throughput, run-rate, operating rate, and occupancy. Other activity-based terms include subscribers, customers, customer count, locations, stores, beds, rooms, wells, acreage, and similar operating-count measures. An expanded-unit term is counted only when it appears directly adjacent to a numerical value, so that the measure captures quantity-bearing expanded-unit disclosure rather than general references to units or operating activity. Matches whose surrounding text indicates non-operating or accounting uses of unit terminology, such as business unit, reporting unit, unit cost, unit price, unit margin, unit of account, or unit of measure, are excluded. Repeated appearances of the same valid expanded operating quantity term are counted separately. The variable is computed as $\log(1 + \text{Expanded Unit Frequency}_{i,t})$. Source: Constructed from parsed Form 10-K filings.
Expanded Unit Binary$_{i,t}$	An indicator that equals one when Expanded Unit Frequency$_{i,t}$ is greater than zero.
Log Expanded Unit Breadth$_{i,t}$	The natural logarithm of one plus the number of distinct expanded operating quantity disclosure categories identified in firm i's Form 10-K for fiscal year t. Valid expanded operating quantity matches are defined as in Log Expanded Unit Frequency$_{i,t}$. Expanded operating quantity categories are based on the expanded operating quantity dictionary subfamilies, including the physical-unit categories used to construct Log Physical Unit Breadth$_{i,t}$ and the incremental non-physical categories described in Log Expanded Unit Frequency$_{i,t}$. A category is counted when the filing contains at least one valid matched term from that category. The variable is computed as $\log(1 + \text{Expanded Unit Breadth}_{i,t})$.
Log of Total Assets$_{i,t}$	The natural logarithm of total assets for firm i at the end of fiscal year t, computed as $\log(AT_{i,t})$, where AT is Compustat total assets. The variable is set to missing when total assets are missing or less than one. Source: Compustat Annual.

Variable	Definition
Return on Assets$_{i,t}$	Net income divided by total assets for firm i in fiscal year t, computed as $NI_{i,t}/AT_{i,t}$, where NI is Compustat net income and AT is Compustat total assets. The variable is set to missing when net income or total assets is missing, or when total assets are less than one. Source: Compustat Annual.
Leverage$_{i,t}$	Total debt divided by total assets for firm i in fiscal year t, computed as $(DLC_{i,t} + DLTT_{i,t})/AT_{i,t}$, where DLC is debt in current liabilities, $DLTT$ is long-term debt, and AT is total assets. Missing debt components are set to zero, and negative debt components are set to zero before summing. The variable is set to missing when total assets are missing or non-positive. Source: Compustat Annual.
PPE$_{i,t}$	Property, plant, and equipment divided by total assets for firm i in fiscal year t, computed as $PPENT_{i,t}/AT_{i,t}$, where $PPENT$ is Compustat net property, plant, and equipment and AT is total assets. The variable is set to missing when property, plant, and equipment or total assets is missing, or when total assets are less than one. Source: Compustat Annual.
Inv$_{i,t}$	Inventory divided by total assets for firm i in fiscal year t, computed as $INVT_{i,t}/AT_{i,t}$, where $INVT$ is Compustat inventory and AT is total assets. The variable is set to missing when inventory or total assets is missing, or when total assets are less than one. Source: Compustat Annual.
Log of Word Length$_{i,t}$	The natural logarithm of one plus the total number of words in firm i's parsed Form 10-K for fiscal year t, computed as $\log(1 + \text{Document Word Count}_{i,t})$. Source: Constructed from parsed Form 10-K filings.
Avg Similarity$_i$	A 2008 through 2011 product-market similarity measure for firm i constructed from the Hoberg-Phillips TNIC pairwise similarity file. The TNIC pair file contains focal-firm–peer-firm observations by year, identified by $gvkey1$, $gvkey2$, and a pairwise similarity score. I first restrict the TNIC pair data to years 2008–2011, exclude self-pairs and observations with missing or non-finite similarity scores, and compute the mean pairwise similarity score across all TNIC peers of focal firm i in each year. I then average these annual firm-level means over the available years from 2008 through 2011 and assign the resulting value to all firm-years for firm i in the sample. Source: Constructed from Hoberg-Phillips TNIC product-market similarity data.
Proportion of Public Firms$_i$	The proportion of firms in NAICS-6 industry that are publicly traded firms, measured as the number of Compustat public firms in the industry divided by the total number of employer firms in the same industry from the Statistics of U.S. Businesses. Public firms are counted as distinct Compustat Gvkeys in the industry, and total employer firms are taken from SUSB national industry-level firm counts. The measure is constructed using the 2008 through 2011 period and assigned to firm-years in the sample. Source: Constructed from Compustat and Statistics of U.S. Businesses.

Variable	Definition
Product Market Fluidity$_i$	A 2008 through 2011 product-market fluidity measure for firm i constructed from the Hoberg-Phillips product-market fluidity data. The Hoberg-Phillips file provides an annual firm-level product-market fluidity score, $prodmktfluid$, which captures changes in the product-market space surrounding firm i due to changes in related firms' product offerings. I first restrict the product-market fluidity data to years 2008–2011, exclude observations with missing or non-finite fluidity scores, and then average the annual firm-level fluidity scores over the available years from 2008 through 2011. I assign the resulting pre-period average to all firm-years for firm i in the sample. Source: Constructed from Hoberg-Phillips product-market fluidity data.
Forecast Error$_{i,t}$	The natural logarithm of one plus the price-scaled absolute analyst forecast error for firm i in fiscal year t. Annual EPS forecasts are taken from IBES Detail and restricted to EPS forecasts with annual periodicity and fiscal-period indicator one when those fields are available. Forecasts are linked to Compustat through the IBES ticker–CRSP PERMNO link and the CRSP/Compustat Merged link. A forecast is matched to a Compustat fiscal-year target when the IBES forecast-period end date is within 30 days of the Compustat fiscal year-end date and the forecast announcement date is between zero and 90 days before the Compustat fiscal year-end date. For each analyst-firm-year, only the latest forecast before fiscal year-end is retained. The raw forecast error is computed as $\lvert ActualEPS_{i,t} - MedianForecast_{i,t}\rvert / Price_{i,t}$, where $Price_{i,t}$ is the absolute value of Compustat fiscal year-end price and is required to exceed one. The raw scaled error is winsorized at the 5th and 95th percentiles, and the final variable is $\log(1 + \text{winsorized scaled forecast error}_{i,t})$. Source: Constructed from IBES Detail, IBES–CRSP link data, CRSP/Compustat Merged data, and Compustat Annual.
Forecast Count$_{i,t}$	The natural logarithm of one plus the number of individual annual EPS forecasts available for firm i in fiscal year t. Forecasts are counted using the same IBES Detail matching procedure used to construct Forecast Error: EPS forecasts are matched to a Compustat fiscal-year target when the IBES forecast-period end date is within 30 days of the Compustat fiscal year-end date and the forecast announcement date is between zero and 90 days before the fiscal year-end date. This variable counts individual forecast observations rather than distinct analysts. The variable is computed as $\log(1 + \text{Forecast Count}_{i,t})$. Source: Constructed from IBES analyst forecast data, IBES–CRSP link data, and CRSP/Compustat Merged data.

Variable	Definition
Bid-Ask Spread$_{i,t}$	The average daily relative bid-ask spread for firm i during fiscal year t. CRSP daily observations are linked to Compustat Gvkeys using the CRSP/Compustat Merged link table and assigned to fiscal years when the CRSP trading date falls between the prior fiscal year-end date plus one day and the current fiscal year-end date. For each valid daily observation, the relative spread is computed as $2(ASKHI_{i,d} - BIDLO_{i,d})/(ASKHI_{i,d} + BIDLO_{i,d})$, where $ASKHI$ is CRSP daily ask high and $BIDLO$ is CRSP daily bid low. The daily spread is set to missing when either quote is missing or non-positive, or when $ASKHI < BIDLO$. The firm-year raw spread is the mean of valid daily relative spreads during the fiscal year. The final variable is winsorized at the 5th and 95th percentiles. Source: Constructed from CRSP daily data and CRSP/Compustat Merged data.

Table 1: Sample Construction

Table 1 reports the construction of the main estimation sample. I begin by identifying annual coverage under the Census Bureau's Current Industrial Reports (CIR) program. CIR began in 1904 and provided recurring, aggregated manufacturing information at the NAICS-6 industry level for certain industries. The CIR program was fully canceled in Q4, 2011 due to budget constraints though certain industries lost their coverage earlier.

I obtain available annual CIR reports from the Southern New Hampshire University archive, which contains annual CIR reports from 1991 through the final annual CIR reporting year of 2010 (the CIR program was cancelled in Q4, 2011). I review these reports to identify the product and NAICS-6 categories covered by CIR in each year to construct an annual CIR coverage file.

I use the CIR coverage file to assign treatment status. A NAICS-6 industry is classified as treated if it was covered by an annual CIR report in the final reporting year 2010. Firms operating in these industries were exposed to the loss of recurring public industry data when the CIR program ended. Firms in industries not covered by CIR serve as control firms. Firms whose NAICS-6 industries pre-emptively lost CIR coverage during 2008 through 2009 are dropped from the sample entirely.

I then merge the CIR treatment classification to the Compustat/10-K disclosure sample using firms' NAICS-6 industry classifications. The main sample is a firm-year panel covering fiscal years 2008 through 2015. Fiscal years 2008 through 2011 are classified as the pre-period, and fiscal years 2012 through 2015 are classified as the post-period. I treat fiscal year 2012 as the first clean post-treatment year because CIR data collection ended during Q4, 2011 and the final reporting year was in 2010.

Panel A: CIR-Treated Portion of the Sample (Treatment Firms)

Sample construction step	Distinct treated NAICS-6	Distinct treated firms
Historical Compustat sample, 1991–2010	170	6,921
Restrict to main sample window, 2008–2015	170	6,045
Drop industries that lost CIR coverage in 2008–2009	170	6,045
Require valid 10-K disclosure data	163	3,271
Require non-missing baseline regression variables	162	2,893

Panel B: Full Compustat/10-K Sample (Treatment + Control Firms)

Sample construction step	Distinct Compustat NAICS-6	Distinct Compustat firms
Compustat universe, 1991–2015	1,295	23,728
Restrict to main sample window, 2008–2015	1,053	16,842
Drop industries that lost CIR coverage in 2008–2009	1,027	15,696
Require valid 10-K disclosure data	878	7,608
Require non-missing baseline regression variables	854	6,915

Table 2: Descriptive Statistics

This table reports descriptive statistics for the firm-year variables used in the analysis. Each observation is a firm-year. Number of observations may not match the number of observations in other tables due to regressions requiring all variables for a given firm-year. All variables are defined in Appendix A.

Variable	N	Mean	P25	Median	P75
Treat_i	40,228	0.413	0.000	0.000	1.000
Post_t	40,228	0.497	0.000	0.000	1.000
Physical Unit Binary$_{i,t}$	40,228	0.283	0.000	0.000	1.000
Log Physical Unit Frequency$_{i,t}$	40,228	0.561	0.000	0.000	0.693
Log Physical Unit Breadth$_{i,t}$	40,228	0.281	0.000	0.000	0.693
Expanded Unit Binary$_{i,t}$	40,228	0.393	0.000	0.000	1.000
Log Expanded Unit Frequency$_{i,t}$	40,228	0.707	0.000	0.000	1.099
Log Expanded Unit Breadth$_{i,t}$	40,228	0.381	0.000	0.000	0.693
Log of Total Assets$_{i,t}$	37,641	6.239	4.645	6.440	7.954
Return on Assets$_{i,t}$	37,573	-0.098	-0.048	0.012	0.055
Leverage$_{i,t}$	39,550	1.339	0.030	0.184	0.399
PPE$_{i,t}$	36,262	0.227	0.025	0.111	0.352
Inv$_{i,t}$	37,214	0.082	0.000	0.017	0.119
Log of Word Length$_{i,t}$	40,228	10.722	10.446	10.779	11.055
Avg Similarity$_i$	29,182	9.935	1.175	1.967	6.287
Proportion of Public Firms$_i$	37,005	0.032	0.002	0.017	0.038
Product Market Fluidity$_i$	29,109	7.231	4.569	6.422	9.369
Log Forecast Error$_{i,t}$	23,283	0.017	0.001	0.003	0.011
Log Forecast Count$_{i,t}$	23,283	2.232	1.609	2.197	2.890
Bid-Ask Spread$_{i,t}$	27,039	0.044	0.026	0.038	0.057

Table 3, Panel A: Impact of the CIR Program Cancellation on Physical Unit Disclosure

This table reports baseline difference-in-differences estimates of the effect of the cancellation of the Census Bureau's Current Industrial Reports program on firms' physical-unit disclosure. The sample is a firm-year panel covering fiscal years 2008 through 2015. The sample period is divided into a pre-cancellation period of 2008 through 2011 and a post-cancellation period of 2012 through 2015. Firms that lost CIR coverage in 2008 through 2009 are dropped from the sample. The dependent variables are Physical Binary, Physical Frequency, and Physical Breadth. Treat × Post equals one for treated firms in 2012 through 2015, where treated firms are those operating in industries previously covered by CIR. Standard errors are clustered by NAICS-6, and firm and fiscal-year fixed effects absorb the main effects of Treat and Post. See Appendix A for variable definitions and Table 1 for sample construction details.

Variable	Physical Unit Binary		Physical Unit Frequency		Physical Unit Breadth	
	Base	+ Words	Base	+ Words	Base	+ Words
$\text{Treat}_i \times \text{Post}_t$	-0.018**	-0.014*	-0.045***	-0.032**	-0.022***	-0.017**
	(0.007)	(0.007)	(0.015)	(0.016)	(0.008)	(0.008)
Log of Total $\text{Assets}_{i,t}$	0.016***	0.006	0.053***	0.022*	0.021***	0.009*
	(0.005)	(0.005)	(0.012)	(0.011)	(0.006)	(0.005)
Return on $\text{Assets}_{i,t}$	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$\text{Leverage}_{i,t}$	-0.000	-0.002	0.004	-0.001	0.003	0.001
	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)
$\text{PPE}_{i,t}$	0.069**	0.061*	0.186**	0.158*	0.074**	0.063**
	(0.035)	(0.035)	(0.083)	(0.082)	(0.031)	(0.031)
$\text{Inv}_{i,t}$	0.025	0.018	0.119	0.096	0.033	0.024
	(0.057)	(0.057)	(0.089)	(0.087)	(0.051)	(0.051)
Log of Word $\text{Length}_{i,t}$		0.112***		0.368***		0.144***
		(0.022)		(0.086)		(0.034)
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Num. obs.	35,798	35,798	35,798	35,798	35,798	35,798
R^2	0.835	0.837	0.933	0.935	0.890	0.892
Adj. R^2	0.796	0.798	0.917	0.920	0.864	0.866

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 3, Panel B: Impact of the CIR Program Cancellation on Expanded Unit Disclosure

This table reports baseline difference-in-differences estimates of the effect of the cancellation of the Census Bureau's Current Industrial Reports program on firms' expanded quantity disclosure. The sample is a firm-year panel covering fiscal years 2008 through 2015. The sample period is divided into a pre-cancellation period of 2008 through 2011 and a post-cancellation period of 2012 through 2015. Firms that lost CIR coverage in 2008 through 2009 are dropped from the sample. The dependent variables are Expanded Binary, Expanded Frequency, and Expanded Breadth. Treat × Post equals one for treated firms in 2012 through 2015, where treated firms are those operating in industries previously covered by CIR. Standard errors are clustered by NAICS-6 industry, and firm and fiscal-year fixed effects absorb the main effects of Treat and Post. See Appendix A for variable definitions and Table 1 for sample construction details.

Variable	Expanded Physical Unit Binary		Expanded Physical Unit Frequency		Expanded Physical Unit Breadth	
	Base	+ Words	Base	+ Words	Base	+ Words
$\text{Treat}_i \times \text{Post}_t$	-0.023**	-0.018*	-0.050***	-0.034*	-0.030***	-0.023**
	(0.010)	(0.010)	(0.018)	(0.019)	(0.009)	(0.010)
Log of Total $\text{Assets}_{i,t}$	0.015***	0.002	0.057***	0.020	0.024***	0.007
	(0.005)	(0.005)	(0.014)	(0.012)	(0.006)	(0.005)
Return on $\text{Assets}_{i,t}$	-0.001	-0.001	-0.001	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$\text{Leverage}_{i,t}$	-0.000	-0.002	0.004	-0.001	0.003	0.001
	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)
$\text{PPE}_{i,t}$	0.065*	0.053	0.207**	0.174**	0.076**	0.061*
	(0.038)	(0.037)	(0.086)	(0.084)	(0.033)	(0.033)
$\text{Inv}_{i,t}$	0.047	0.037	0.168*	0.140	0.036	0.023
	(0.060)	(0.059)	(0.100)	(0.095)	(0.054)	(0.053)
Log of Word $\text{Length}_{i,t}$		0.156***		0.437***		0.196***
		(0.023)		(0.087)		(0.037)
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Num. obs.	35,798	35,798	35,798	35,798	35,798	35,798
R^2	0.779	0.781	0.914	0.918	0.854	0.857
Adj. R^2	0.726	0.729	0.894	0.898	0.819	0.823

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Impact of the CIR Program Cancellation on Unit Disclosure within MD&A

This table reports difference-in-differences estimates of the effect of the cancellation of the Census Bureau's Current Industrial Reports program on unit disclosure within Management's Discussion and Analysis. The sample is a firm-year panel covering fiscal years 2008 through 2015. The sample period is divided into a pre-cancellation period of 2008 through 2011 and a post-cancellation period of 2012 through 2015. Firms that lost CIR coverage in 2008 through 2009 are dropped from the sample. The dependent variables capture whether the firm discloses any unit information in MD&A, the frequency of that disclosure, and the breadth of distinct unit categories discussed in MD&A. Treat × Post equals one for treated firms in 2012 through 2015, where treated firms are those operating in industries previously covered by CIR. Standard errors are clustered by NAICS-6 industry, and firm and fiscal-year fixed effects absorb the main effects of Treat and Post. See Appendix A for variable definitions and Table 1 for sample construction details.

Variable	Physical Unit Binary		Physical Unit Frequency		Physical Unit Breadth	
	Base	+ Words	Base	+ Words	Base	+ Words
$\text{Treat}_i \times \text{Post}_t$	-0.016**	-0.013*	-0.036**	-0.030*	-0.017***	-0.014**
	(0.007)	(0.007)	(0.015)	(0.015)	(0.006)	(0.006)
Log of Total $\text{Assets}_{i,t}$	0.014***	0.012***	0.041***	0.036***	0.014***	0.012***
	(0.005)	(0.005)	(0.011)	(0.011)	(0.004)	(0.004)
Return on $\text{Assets}_{i,t}$	0.000*	0.000*	0.001*	0.001*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$\text{Leverage}_{i,t}$	0.002	0.002	0.005	0.005*	0.002	0.002
	(0.001)	(0.001)	(0.003)	(0.003)	(0.001)	(0.001)
$\text{PPE}_{i,t}$	0.042	0.035	0.119*	0.105*	0.034	0.028
	(0.030)	(0.030)	(0.063)	(0.062)	(0.029)	(0.028)
$\text{Inv}_{i,t}$	0.064	0.058	0.154**	0.141*	0.057	0.052
	(0.046)	(0.046)	(0.073)	(0.073)	(0.036)	(0.036)
Log of Word $\text{Length}_{i,t}$		0.025***		0.056***		0.023***
		(0.006)		(0.014)		(0.006)
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Num. obs.	34,189	34,189	34,189	34,189	34,189	34,189
R^2	0.782	0.786	0.860	0.864	0.809	0.814
Adj. R^2	0.729	0.734	0.826	0.831	0.763	0.768

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Impact of the CIR Program Cancellation on Unit Disclosure: Matched Sample with Firms that Lost CIR Program Coverage in 1991 through 2007

This table reports matched-sample difference-in-differences estimates of the effect of the cancellation of the Census Bureau's Current Industrial Reports program on unit disclosure. The sample is a firm-year panel covering fiscal years 2008 through 2015. The sample period is divided into a pre-cancellation period of 2008 through 2011 and a post-cancellation period of 2012 through 2015. Treated firms are compared with firms in industries that lost CIR coverage before the main sample period. The control group consists of firms that were previously covered by CIR but lost coverage during 1991 through 2007, providing a control group with more similar historical exposure to CIR reporting. Firms that lost CIR coverage in 2008 through 2009 are dropped from the sample. The dependent variables include physical-unit disclosure measures and expanded quantity disclosure measures. Treat × Post equals one for treated firms in 2012 through 2015, where treated firms are those operating in industries previously covered by CIR. Standard errors are clustered by NAICS-6 industry, and firm and fiscal-year fixed effects absorb the main effects of Treat and Post. See Appendix A for variable definitions and Table 1 for sample construction details.

Variable	Physical Unit Disclosure			Expanded Quantity Disclosure		
	Binary	Frequency	Breadth	Binary	Frequency	Breadth
$\text{Treat}_i \times \text{Post}_t$	-0.024	-0.081**	-0.035**	-0.027	-0.094***	-0.045**
	(0.016)	(0.033)	(0.016)	(0.019)	(0.035)	(0.019)
Log of Total $\text{Assets}_{i,t}$	0.003	0.045*	0.014	0.001	0.053**	0.014
	(0.013)	(0.023)	(0.011)	(0.012)	(0.024)	(0.011)
Return on $\text{Assets}_{i,t}$	0.000	0.001**	0.000	0.000	0.001**	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$\text{Leverage}_{i,t}$	0.008	0.018	0.012	0.005	0.018	0.010
	(0.008)	(0.013)	(0.009)	(0.008)	(0.014)	(0.009)
$\text{PPE}_{i,t}$	0.044	0.328*	0.082	0.071	0.368**	0.093
	(0.075)	(0.170)	(0.060)	(0.077)	(0.168)	(0.061)
$\text{Inv}_{i,t}$	0.087	0.151	0.062	-0.010	0.135	-0.018
	(0.104)	(0.130)	(0.083)	(0.103)	(0.148)	(0.088)
Log of Word $\text{Length}_{i,t}$	0.096***	0.265***	0.108***	0.160***	0.349***	0.171***
	(0.027)	(0.069)	(0.029)	(0.031)	(0.073)	(0.034)
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Num. obs.	8,451	8,451	8,451	8,451	8,451	8,451
R^2	0.804	0.912	0.854	0.745	0.889	0.810
Adj. R^2	0.760	0.893	0.821	0.689	0.865	0.768

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 6, Panel A: Impact of the CIR Program Cancellation on Unit Disclosure by Low and High Average Similarity

This table reports subsample difference-in-differences estimates after splitting firms by pre-period average product-market similarity. The sample is a firm-year panel covering fiscal years 2008 through 2015. The sample period is divided into a pre-cancellation period of 2008 through 2011 and a post-cancellation period of 2012 through 2015. Firms that lost CIR coverage in 2008 through 2009 are dropped from the sample. Firms are classified as low or high average similarity using the sample median of the pre-cancellation Hoberg-Phillips average similarity measure. The dependent variables are physical-unit disclosure measures: Physical Binary, Physical Frequency, and Physical Breadth. Treat × Post equals one for treated firms in 2012 through 2015, where treated firms are those operating in industries previously covered by CIR. Standard errors are clustered by NAICS-6 industry, and firm and fiscal-year fixed effects absorb the main effects of Treat and Post. See Appendix A for variable definitions and Table 1 for sample construction details.

Variable	Physical Unit Binary		Physical Unit Frequency		Physical Unit Breadth	
	Low Avg. Sim	High Avg. Sim.	Low Avg. Sim.	High Avg. Sim.	Low Avg. Sim.	High Avg. Sim.
$\text{Treat}_i \times \text{Post}_t$	0.005	-0.042***	-0.009	-0.060***	0.002	-0.034***
	(0.012)	(0.011)	(0.020)	(0.021)	(0.011)	(0.010)
Log of Total $\text{Assets}_{i,t}$	0.004	0.010	0.033**	0.038**	0.015*	0.012
	(0.009)	(0.007)	(0.015)	(0.018)	(0.009)	(0.008)
Return on $\text{Assets}_{i,t}$	0.000	-0.003	0.000	-0.009	0.000	-0.004
	(0.000)	(0.005)	(0.000)	(0.007)	(0.000)	(0.003)
$\text{Leverage}_{i,t}$	-0.016	0.000	-0.023	0.004	-0.011	0.002
	(0.010)	(0.002)	(0.018)	(0.004)	(0.009)	(0.002)
$\text{PPE}_{i,t}$	-0.055	0.172***	0.175	0.264***	0.004	0.110**
	(0.082)	(0.050)	(0.155)	(0.092)	(0.062)	(0.050)
$\text{Inv}_{i,t}$	0.083	0.187*	0.028	0.336**	0.076	0.123
	(0.097)	(0.108)	(0.139)	(0.136)	(0.083)	(0.099)
Log of Word $\text{Length}_{i,t}$	0.134***	0.049**	0.300***	0.210**	0.137***	0.067**
	(0.027)	(0.024)	(0.057)	(0.084)	(0.027)	(0.032)
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Num. obs.	13,630	13,199	13,630	13,199	13,630	13,199
R^2	0.785	0.876	0.912	0.953	0.841	0.919
Adj. R^2	0.743	0.852	0.895	0.943	0.810	0.903

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 6, Panel B: Impact of the CIR Program Cancellation on Unit Disclosure by Low and High Public-Firm Presence

This table reports subsample difference-in-differences estimates after splitting firms by pre-cancellation public-firm presence in the NAICS-6 industry. The sample is a firm-year panel covering fiscal years 2008 through 2015. The sample period is divided into a pre-cancellation period of 2008 through 2011 and a post-cancellation period of 2012 through 2015. Firms that lost CIR coverage in 2008 through 2009 are dropped from the sample. Industries are classified as low or high public-firm presence using the median value of the public-share measure constructed from Compustat and Statistics of U.S. Businesses data. The dependent variables are physical-unit disclosure measures: Physical Binary, Physical Frequency, and Physical Breadth. Treat × Post equals one for treated firms in 2012 through 2015, where treated firms are those operating in industries previously covered by CIR. Standard errors are clustered by NAICS-6 industry, and firm and fiscal-year fixed effects absorb the main effects of Treat and Post. See Appendix A for variable definitions and Table 1 for sample construction details.

Variable	Physical Unit Binary		Physical Unit Frequency		Physical Unit Breadth	
	Low Public Firm Pres.	High Public Firm Pres.	Low Public Firm Pres.	High Public Firm Pres.	Low Public Firm Pres.	High Public Firm Pres.
$\text{Treat}_i \times \text{Post}_t$	-0.005	-0.021**	-0.002	-0.061*	0.002	-0.038**
	(0.013)	(0.009)	(0.019)	(0.031)	(0.012)	(0.015)
Log of Total $\text{Assets}_{i,t}$	0.006	0.007	0.017*	0.039*	0.010*	0.012
	(0.007)	(0.008)	(0.010)	(0.020)	(0.006)	(0.009)
Return on $\text{Assets}_{i,t}$	-0.002	0.000	-0.002	0.001	-0.001	0.000
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)
$\text{Leverage}_{i,t}$	-0.002	-0.001	0.003	-0.003	0.002	0.000
	(0.007)	(0.002)	(0.011)	(0.003)	(0.007)	(0.002)
$\text{PPE}_{i,t}$	0.041	0.069	0.082	0.191	0.055	0.059
	(0.056)	(0.050)	(0.111)	(0.127)	(0.052)	(0.042)
$\text{Inv}_{i,t}$	-0.027	0.071	0.001	0.270	-0.057	0.165*
	(0.072)	(0.100)	(0.104)	(0.170)	(0.058)	(0.099)
Log of Word $\text{Length}_{i,t}$	0.115***	0.108***	0.231***	0.438***	0.108***	0.158***
	(0.026)	(0.031)	(0.044)	(0.143)	(0.021)	(0.057)
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Num. obs.	16,188	16,825	16,188	16,825	16,188	16,825
R^2	0.795	0.872	0.902	0.951	0.839	0.918
Adj. R^2	0.746	0.841	0.878	0.939	0.800	0.899

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 6, Panel C: Impact of the CIR Program Cancellation on Unit Disclosure by Low and High Product-Market Fluidity

This table reports subsample difference-in-differences estimates after splitting firms by pre-cancellation product-market fluidity. The sample is a firm-year panel covering fiscal years 2008 through 2015. The sample period is divided into a pre-cancellation period of 2008 through 2011 and a post-cancellation period of 2012 through 2015. Firms that lost CIR coverage in 2008 through 2009 are dropped from the sample. Firms are classified as low or high fluidity using the sample median of the pre-cancellation Hoberg-Phillips product-market fluidity measure. The dependent variables are physical-unit disclosure measures: Physical Binary, Physical Frequency, and Physical Breadth. Treat × Post equals one for treated firms in 2012 through 2015, where treated firms are those operating in industries previously covered by CIR. Standard errors are clustered by NAICS-6 industry, and firm and fiscal-year fixed effects absorb the main effects of Treat and Post. See Appendix A for variable definitions and Table 1 for sample construction details.

Variable	Physical Unit Binary		Physical Unit Frequency		Physical Unit Breadth	
	Low Fluidity	High Fluidity	Low Fluidity	High Fluidity	Low Fluidity	High Fluidity
$\text{Treat}_i \times \text{Post}_t$	-0.016	-0.017	-0.042*	-0.025	-0.014	-0.018*
	(0.013)	(0.013)	(0.022)	(0.021)	(0.012)	(0.011)
Log of Total $\text{Assets}_{i,t}$	0.007	0.006	0.032	0.032**	0.011	0.014*
	(0.012)	(0.008)	(0.020)	(0.015)	(0.011)	(0.008)
Return on $\text{Assets}_{i,t}$	0.011*	0.000	0.021**	0.000	0.007	0.000
	(0.006)	(0.000)	(0.009)	(0.000)	(0.005)	(0.000)
$\text{Leverage}_{i,t}$	-0.014	0.000	0.001	0.002	-0.009	0.002
	(0.026)	(0.002)	(0.045)	(0.004)	(0.022)	(0.002)
$\text{PPE}_{i,t}$	0.034	0.043	0.145	0.225*	0.051	0.036
	(0.060)	(0.061)	(0.121)	(0.128)	(0.059)	(0.060)
$\text{Inv}_{i,t}$	0.232***	-0.104	0.333***	-0.138	0.180**	-0.081
	(0.090)	(0.112)	(0.117)	(0.155)	(0.075)	(0.096)
Log of Word $\text{Length}_{i,t}$	0.110***	0.076***	0.305***	0.212***	0.114***	0.090***
	(0.024)	(0.028)	(0.058)	(0.075)	(0.025)	(0.035)
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Num. obs.	13,718	13,741	13,718	13,741	13,718	13,741
R^2	0.791	0.864	0.901	0.954	0.839	0.914
Adj. R^2	0.752	0.836	0.882	0.945	0.808	0.897

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Impact of the CIR Program Cancellation on Forecast Accuracy, Forecast Activity, and Bid-Ask Spreads

This table reports difference-in-differences estimates of the effect of the cancellation of the Census Bureau's Current Industrial Reports program on capital-market information-environment outcomes. The sample is a firm-year panel covering fiscal years 2008 through 2015, subject to the availability of analyst forecast and trading data. The sample period is divided into a pre-cancellation period of 2008 through 2011 and a post-cancellation period of 2012 through 2015. Firms that lost CIR coverage in 2008 through 2009 are dropped from the sample. The dependent variables are Log Forecast Error, Log Forecast Count, and Bid-Ask Spread. Treat × Post equals one for treated firms in 2012 through 2015, where treated firms are those operating in industries previously covered by CIR. Standard errors are clustered by NAICS-6 industry, and firm and fiscal-year fixed effects absorb the main effects of Treat and Post. See Appendix A for variable definitions and Table 1 for sample construction details.

Variable	Log Forecast Error	Log Forecast Count	Bid-Ask Spread
$\text{Treat}_i \times \text{Post}_t$	0.004*	-0.036*	0.003***
	(0.002)	(0.019)	(0.001)
$\text{Log of Total Assets}_{i,t}$	-0.002	0.350***	-0.005***
	(0.002)	(0.017)	(0.001)
$\text{Return on Assets}_{i,t}$	-0.001	-0.011	-0.001
	(0.002)	(0.018)	(0.001)
$\text{Leverage}_{i,t}$	0.022***	-0.047	0.005**
	(0.004)	(0.035)	(0.003)
$\text{PPE}_{i,t}$	0.020***	0.475***	0.017***
	(0.007)	(0.093)	(0.003)
$\text{Inv}_{i,t}$	0.033**	-0.048	0.011**
	(0.016)	(0.157)	(0.005)
$\text{Log of Word Length}_{i,t}$	0.009**	-0.056*	0.008***
	(0.004)	(0.031)	(0.001)
Fixed Effects	Firm, Year	Firm, Year	Firm, Year
Num. obs.	21,858	21,858	26,504
R^2	0.656	0.870	0.826
Adj. R^2	0.568	0.837	0.786

Notes: Standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.