Public Firm Presence, Financial Reporting, and the Decline of US Manufacturing

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ABSTRACT: We examine the relation between public firm presence and import competition. The information created by public firm presence may provide importers with insights they can use for competing with domestic firms. Consistent with this possibility, we document a positive relation between public firm presence and import competition. We find similar results when using differences in the expected costs of the Sarbanes-Oxley Act as a source of plausibly exogenous variation in public firm presence after the act. We use differences in the proportion of German firms reporting publicly around a major enforcement reform as a natural mechanism experiment, and find evidence that financial reporting is a channel through which public firm presence relates to import competition. Additional mechanism tests and a falsification test estimated in the United Kingdom, where public and most private firms report publicly, further support this inference. In total, our evidence is consistent with foreign competitors using the information created by public firm presence, including what public firms disclose in financial reports, to compete with domestic firms. Consequently, our results provide evidence of competitors using the proprietary information disclosed in financial reports to compete with the disclosing firms and of information frictions affecting trade.

1. Introduction

We examine whether the information generated by the presence of publicly-traded manufacturing firms facilitates foreign import competition in the US. Although foreign importers enjoy many trade advantages, such as lower labor costs and a lighter regulatory burden, they also face significant information frictions. Potential frictions include uncertainty about demand, consumer preferences, and the competitive landscape, all of which may increase the riskiness of competing in the US market and discourage foreign imports. Publicly-traded firms are one information source that may ameliorate these frictions.

The US Securities and Exchange Commission (SEC) requires firms publicly-traded in US capital markets to prepare financial reports for capital market participants. These reports must detail firms' investments, financial performance, exposure to risk factors, material contracts, expansion plans, and production schedules. Beyond these mandated disclosures, the managers of public firms also often release forecasts of future earnings and financial decisions and discuss firm performance with analysts, who in turn produce their own forecasts. Although investors are the intended beneficiaries of much of this information, competitors can also use it (Badertscher et al., 2013; Bernard et al., 2020; Kim, 2019). For example, foreign importers can draw on information about production schedules, investments, profitability, accruals, sales, and risk factors to understand US market demand and consumer preferences, as well as the US competitive landscape. This information can thus reduce foreign firms' uncertainty about the US market.

Prior work finds that reduced uncertainty increases investment on average, suggesting that reduced uncertainty can also increase importing on the margin (e.g., Guiso and Parigi, 1999). Because the information generated by the presence of public firms can also reveal domestic firms' operational strengths and weaknesses, such as financial health and competitive capabilities, it can

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ameliorate information frictions and encourage import competition even when the public firms' profitability is low. Despite these arguments, public firm presence could also decrease import competition or have no effect on it, because the information produced also benefits domestic firms (Badertscher et al., 2013). For example, the information produced as a result of public firm presence may be more relevant to and accessible by domestic firms. If this factor outweighs any benefits of ameliorating importers' more severe information frictions, then public firm presence will benefit domestic firms to a greater degree, decreasing import competition. Moreover, public firm presence could even discourage importers insofar as public firms are superior competitors due to their greater access to liquid capital, their responsiveness to investment opportunities, and their ability to invest in projects designed to help them deter competition (e.g., greater information about domestic firms' strengths could deter import competition).[1] Consequently, the relation between public firm presence and import competition is an open question.

We employ several approaches in investigating the relation between public firm presence and import competition. We begin by descriptively documenting whether variables that we expect to affect import competition are also determinants of public firm presence (defined for each industry-year as the ratio of public firm sales to total US production). We find that public firm presence is greater in industries with greater economies of scale, a higher concentration, and higher imports in other high-income countries. We also find that public firm presence increases in industries that are growing and are becoming more labor intensive.

We next examine the relation between public firm presence and foreign import competition using industry-level panel regressions of import competition. In these regressions, we control for the variables included in our determinants analysis. We measure import competition as the ratio

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[1] E.g., Rajan and Zingales (1998), Aghion et al. (2005), Michaely and Roberts (2011), Badertscher et al. (2013), Maksimovic et al. (2013), Gilje and Taillard (2016), Acharya and Xu (2017), and Aghion et al. (2018).

of non-related party imports to domestic production, and we regress this measure on lagged public firm presence. The results from our preferred specification suggest that moving from the median to the 75th percentile of public firm presence within an industry increases subsequent import competition in that industry by about 1.3 percentage points. For comparison, the elasticity of subsequent import competition to public firm presence is slightly smaller than the elasticity of import competition to the domestic production worker wage rate, 1/3rd of the elasticity to tariff uncertainty resolution calculated as in Pierce and Schott (2016), and 1/20th of the elasticity to value added. In total, we find evidence of a robust, albeit second-order, association between public firm presence and subsequent import competition.

Although the association between public firm presence and import competition is consistent with a causal link, other reasonable explanations for it may exist. For example, our determinants analysis suggests that industry growth is a determinant of public firm presence and may encourage importing. Consequently, growth options revealed via a mechanism other than public firm presence and that our controls do not capture could drive the association between public firm presence and import competition.

Motivated by this potential endogeneity concern, we follow Badertscher et al. (2013) and use a natural experiment created by the Sarbanes-Oxley Act (SOX) to provide evidence on the causal effect of public firm presence on import competition. SOX imposed high compliance costs that vary by industry, causing firms in some industries to avoid public listing.[2] We use inter-industry differences in the expected costs of SOX as instruments for differences in public firm

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[2] Engel et al. (2007) and Leuz et al. (2008) find that SOX caused public firms to deregister with the SEC and no longer provide public financial reports. Iliev (2010) finds that SOX imposed costs equal to 12% to 35% of firm value for small firms that were likely on the margin between deregistering and remaining public. Financial Executives International (2005) survey 217 large companies and find that the one-year increase in direct compliance costs due to SOX was over $4 million.

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presence, after the act's passage. We find that moving from the median to the 75th percentile of expected costs of SOX decreases public firm presence by 9.5 percentage points after the passage of the act. This result suggests that the expected costs of SOX on the decisions to go dark, delist, and avoid initial public listing jointly resulted in a significant decline in public firm presence in manufacturing industries where the costs were greater, on the margin (Engel et al., 2007; Leuz et al., 2008). We then regress import competition on the fitted values of public firm presence from the first stage in a "fuzzy difference-in-differences" approach (Armstrong et al., 2018). We find that the estimated relation between the fitted values of public firm presence and subsequent import competition is similar in magnitude to the relation between public firm presence and subsequent import competition estimated by our prior tests.[3]

While our SOX tests help mitigate potential endogeneity concerns, they do not establish the mechanism(s) through which public firm presence affects import competition. Thus, we next investigate whether financial reporting by public firms is an important channel through which public firm presence affects import competition. We begin by examining a natural mechanism experiment (Ludwig et al., 2011). Building on Bernard (2016), we use a plausibly exogenous change in reporting enforcement and availability in Germany as a source of variation in mandated financial reporting in a difference-in-differences approach (see also Breuer et al. (2019) and Breuer (2021)). Although nominally required to disclose publicly, most German limited liability firms did not do so until sweeping enforcement reforms increased compliance.[4] Using the increase in financial reporting due to this plausibly exogenous increase in enforcement in a difference-in-

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[3] One potential concern with SOX as a natural experiment is that it imposed regulatory costs on public firms, potentially making them less able to compete with foreign firms. However, this potential effect would, if anything, work against our finding that those industries for which the expected costs of SOX are greatest are also the industries where import penetration relatively decreases.

[4] See Henselmann and Kaya (2009), Bernard (2016), Breuer et al. (2019), Breuer (2021).

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differences design, we find that increases in financial reporting cause increases in subsequent import competition.

To strengthen our inferences and provide additional insight into potential mechanisms, we conduct a series of cross-sectional tests. We expect the relation between public firm presence and import competition to be stronger when public firms generate a richer information environment. Consistent with this, we find that when public firm financial reports are more informative to investors, the relation between public firm presence and import competition is stronger. Similarly, we find that when managers forecast future gross margins or capital expenditures, and when more analysts forecast future earnings per share (EPS), sales, gross margins, or capital expenditures, the relation between public firm presence and subsequent import competition is again greater. Moreover, the magnifying effect of analyst EPS forecasts on the relation between public firm presence and import competition is monotonically increasing in the horizon of the forecast (e.g., the effect is greater for five year ahead forecasts than it is for one year ahead forecasts).

Finally, we follow Badertscher et al. (2013) and conduct a falsification test to further strengthen our inference that public financial reporting is a mechanism through which public firm presence can affect import competition. If financial reporting is an important driver of the relation between import competition and public firm presence, then we should observe a weaker or no relation in countries where both public and private firms must report publicly. Consistent with this, we find that import competition is not sensitive to public firm presence in the UK, where public and most private firms must report publicly (we also find similar null results in the German setting where public firm presence is likewise uncoupled from public reporting requirements).

In total, although the role of public firm presence is not directly observable, the evidence from our association tests, natural experiment, natural mechanism experiment, mechanism tests,

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and falsification test provide consistent evidence that public firm presence and public firm financial reporting reduces information-based trade costs and increases import competition. This evidence contributes to the accounting literature on the proprietary costs of disclosure by documenting evidence that financial reporting is an important mechanism through which public firm presence affects import competition. The proprietary costs literature argues that product market competition discourages disclosure, based on the assumption that financial reports can provide competitors with enabling information (see Beyer et al. (2010) for a review). However, evidence consistent with this assumption is scarce. As Roychowdhury et al. (2019) state, "…the lack of evidence showing that competitors indeed incorporate peer firms' proprietary disclosures into their decision-making is somewhat surprising." We contribute to this literature by providing evidence of international competitors incorporating domestic firms' disclosures into their decision-making.

Several related studies in the proprietary costs literature suggest a "real effect" of financial reporting on competitive outcomes.[5] However, none examine differences in competitor sales. Instead, these papers indirectly suggest changes in competitor decisions by documenting changes in industry-aggregate profitability dispersion or in disclosing firms' outcomes (e.g., equity market price declines). We build on these studies by documenting direct evidence of changes in competitors' sales decisions. Further, several related studies provide evidence of firms responding to peer firm information by changing their investment decisions or accessing peer firm information in response to investment opportunities.[6] The most closely related of these prior studies is

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[5] E.g., Bernard (2016), Berger et al. (2019), Christensen et al. (2020), Hann et al. (2020), and Breuer (2021).
[6] E.g., Badertscher et al. (2013), Shroff et al. (2014), Shroff et al. (2017), Bernard et al. (2020), Kim and Olbert (2021), and Sani (2021).

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Badertscher et al. (2013). Badertscher et al. (2013) show that increased public firm presence improves the investment efficiency of private domestic firms.

We build on Badertscher et al. (2013) by documenting how public firm presence shifts industry production towards imports (i.e., import competition). Whether public firm information will increase import competition to a greater degree than it will increase domestic firm production is ex ante unclear.[7] Specifically, the information generated by public firms may not be useful in overcoming the frictions faced by foreign importers. Similarly, gathering and understanding this information may be more costly for foreign competitors due to potentially greater information processing costs (Blankespoor et al., 2020).[8] Our results suggest that foreign importers do benefit from public firm information to a greater degree than do domestic firms. Consequently, our results have potential policy implications to the extent that policymakers seek to differentially influence foreign and domestic competition.[9]

We also contribute to the trade literature by documenting direct evidence of information frictions affecting trade. Head and Mayer (2014) review this literature and argue that the distance between trading partners affects international trade more than transport costs or tariffs can independently explain, and they suggest that one potential explanation is that information frictions also impede trade. Prior work suggests that potential information frictions in trade include search costs, as well as uncertainty about partner quality, market conditions, consumer demand or

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[7] Moreover, whether increased investment efficiency results in more production is also ex ante unclear. Although firms will generally increase an activity when that activity becomes more efficient, investment efficiency could reduce investment if, for example, doing so mainly curbs overinvestment.

[8] A concurrent working paper, Yang (2019), finds evidence that US segment disclosures increase import competition in the US. Another concurrent working paper, Zhou (2021), finds evidence that improved financial reporting quality increases exporting from and importing into a country.

[9] Consistent with policymakers seeking to differentially influence the two, government-set trade tariffs discourage foreign import competition while blocking anti-competitive mergers encourages domestic competition (U.S. Department of Justice and Federal Trade Commission, 2010).

preferences, government policy, growth opportunities, and potential profitability.[10] However, direct evidence of information frictions affecting trade is scarce (Steinwender, 2018). By documenting evidence that the information produced by public firm presence encourages import competition, we help fill this gap in the literature.

2. Background and predictions

Distance affects trade by more than can be explained by direct costs such as tariffs or transport costs (Disdier and Head, 2008). Head and Mayer (2013) argue that a potential explanation for this phenomenon is that information frictions impede trade. Prior research suggests several sources of potential information frictions. Rauch and Casella (2003) and Rauch and Trindade (2003) use analytical models to show that search costs and uncertainty about partner quality can discourage trade. Albornoz et al. (2012) uses an analytical model to show that uncertainty about consumer preferences, business practices, and institutional environments can do the same; they find that their model helps explain exporting from Argentina. Allen (2014) analytically models uncertainty about market conditions, documenting that it too can impede trade, and that incorporating uncertainty into the model helps explains rice trading in the Philippines. Sager and Timoshenko (2019) model uncertainty about profitability and find a similar inhibitory effect on trade. In total, prior studies suggest that information frictions can hinder trade.

Despite the theoretical and conceptual reasons information frictions might inhibit trade, Steinwender (2018) notes that direct evidence of information frictions affecting trade is scarce. She shows that the completion of the transatlantic telegraph in 1866 decreased the volatility and

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[10] See, e.g., Rauch and Casella (2003), Rauch and Trindade (2003), Albornoz et al. (2012), Allen (2014), Shroff et al. (2013), Pierce and Schott (2016), Handley and Limão (2017), and Sager and Timoshenko (2019). We discuss these papers in more detail in Section 2.

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level of differences in the price of cotton between New York and Liverpool and increased the amount of cotton shipped from the former to the latter. Pierce and Schott (2016) and Handley and Limão (2017) find that the US granting Permanent Normal Trade Relations (PNTR) status to China, which reduced policy uncertainty about tariff rates, increased trade from China to the US. Their results suggest that uncertainty about government policy can affect trade. We build on these prior studies by examining a novel source of information that is potentially useful to foreign importers: the information produced by public firm presence, particularly via public firms' financial reports.

The SEC oversees financial reporting by US public firms, with the mission of ensuring fair, orderly, and efficient capital markets. To accomplish this mission, the SEC strives for equal, public access to decision-relevant information:

> All investors, whether large institutions or private individuals, should have access to certain basic facts about an investment prior to buying it, and so long as they hold it. To achieve this, the SEC requires public companies to disclose meaningful financial and other information to the public. This provides a common pool of knowledge for all investors to use to judge for themselves whether to buy, sell, or hold a particular security. Only through the steady flow of timely, comprehensive, and accurate information can people make sound investment decisions. [11]

The SEC requires public firms to disclose annual and quarterly financial statements, current reports of material events (SEC form 8-K), notifications of transactions by insiders, and other financial reports. These required disclosures reveal financial information about the profitability, financial health, and investments of US firms. Public financial reports also contain a tremendous amount of non-financial information, including the existence of trade secrets (Glaeser, 2018), discussions about the material risks firms face (Smith and Heinle, 2017), material contracts (Costello, 2013),

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[11] https://www.sec.gov/Article/whatwedo.html.

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the identities of key customers, and even mine safety records (Christensen et al., 2017). The information in required disclosures is often forward looking, either explicitly, due to the accruals system or by SEC mandate, or implicitly, due to the serial correlation between past performance and investment and future performance and investment.

Public firms' information environments are not limited to required disclosures. The owners of public firms are disparate investors who are uninvolved in the daily operation of the firm. These investors consequently demand, and frequently receive, additional information for monitoring and allocating their investments. This information includes management forecasts of future earnings and investment and other voluntary disclosures (see Armstrong et al. (2010) and Dechow et al. (2010) for reviews of the literature on investor demand for information).[12] Information intermediaries such as financial analysts further contextualize, extend, and disseminate information about public firms.[13] In total, public firm presence directly and indirectly generates a tremendous amount of information.

Although the information generated by US public firms is for the intended benefit of investors, competitors may also use it (Roychowdhury et al. (2019) review the literature). Indirectly consistent with competitors using such information, a large accounting literature documents evidence of a negative relation between product market competition and voluntary disclosure.[14]

A growing literature also documents evidence of financial reporting requirements affecting industry profitability dispersion and disclosing-firm profitability. Bernard (2016) finds that

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[12] Bloomfield and Tuijn (2019) and Glaeser and Landsman (2021) document evidence that capacity and patent disclosures by public firms can discourage import competition by signaling product market strength. Our results suggest that the information produced by public firm presence on average encourages import competition, even if some specific disclosures can discourage import competition.

[13] E.g., Bushee et al. (2010), Engelberg and Parsons (2011), Dougal et al. (2012), and Kelly and Ljungqvist (2012).

[14] For example, Huang et al. (2016) find that tariff rate reductions cause firms to reduce their disclosure of earnings forecasts.

financially-constrained German firms forced to disclose public financial information dispose of fixed assets and lose market share. Berger et al. (2019) find that profitability dispersion is greater in Korean industries in which more firms do not disclose costs of goods information. Hann et al. (2019) find that profitability dispersion is greater in US manufacturing industries in which financial reporting provides less information about the productivity of assets. Christensen et al. (2020) find that increased public oversight of UK firms' financial reporting results in equity market prices declines when the firm is more profitable. Breuer (2021) finds that broader financial reporting requirements lead to the founding of more new firms and decreases in market concentration. These studies provide indirect evidence of competitors benefiting from public firm information.

Prior literature also finds that private firms are more sensitive to their investment opportunities when they operate in industries with greater public firm presence, which is directly consistent with competitors benefiting from public firm information (e.g., Badertscher et al., 2013; Shroff et al., 2017).[15] Further, previous research also finds that investment opportunities cause firms to acquire accounting information about their rivals (Bernard et al., 2019). In total, the literature provides indirect and direct evidence of domestic firms benefiting from the information produced as a result of public firm presence.

We extend this logic to import competition from foreign competitors. We argue that the information disclosed by public firms in their financial reports can also help foreign competitors

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[15] Sadka (2006) and Beatty et al. (2013) present evidence that fraudulent misreporting causes competitors to increase their own investment, consistent with competitors using peer firm financial statements to inform their investment decisions. Their findings also suggest that financial reporting may be misleading and therefore harmful to foreign competitors. However, we believe that the evidence that investors and competitors rely on financial reports and the comparative rareness of fraudulent misreporting suggests that on average, financial reporting generates information that is useful to competitors.

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understand market opportunities, US firms' competitive positions and plans, and what has and has not worked for US firms (e.g., financial reports may reveal why poorly performing US firms did not succeed, helping foreign competitors enter the market despite US firms' poor performance). Consequently, on the margin foreign competitors should be more willing and able to compete in the markets in which US firms operate when the proportion of public firms is higher, and hence may increase their import competition in these markets.

However, this prediction is not without tension. Public firm presence could decrease import competition if domestic competitors derive greater benefit from the information than do importers. For example, the information produced as a result of public firm presence could be more relevant and accessible to domestic firms. If these forces dominate the benefits of ameliorating the greater information frictions importers face, then public firm presence will decrease import competition. Further, public firm presence could even directly discourage foreign importers because public firms are stronger competitors given their greater access to liquid capital, their responsiveness to investment opportunities, and their ability to invest in projects designed to help them deter competition. Consequently, the relation between public firm presence and import competition is an open question.

3. Empirical approach and results

3.1. Sample and descriptive statistics

We investigate the relation between public firm presence and import competition in the US manufacturing setting. While our focus on manufacturing limits the generalizability of our inferences, examining this sector offers several important benefits (Glaeser and Guay, 2017). Manufacturing firms are particularly vulnerable to import competition because manufactured

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products can be easily produced in one market and sold in another. The US Census also collects extensive data about manufacturing firms, including private manufacturing firms. These data allow us to measure the prevalence of public firms in each manufacturing industry ex post. However, these data are likely of limited use to foreign competitors because the census reports them in an aggregated fashion after a delay of over a year, and because they lack the additional information included in financial reports (e.g., information about risk exposure).

Table 1 reports descriptive statistics for our sample in Panel A and pairwise correlations between variables in Panel B. Our main sample begins in 2000 (1999 is the earliest year we can obtain related party trade data by industry and we need one year of lag data) and ends in 2016 (the most recent year the census data are available). We include each four-digit manufacturing NAICS industry (3111-3399), resulting in 85 industries.[16] When we draw the data from multiple datasets, we winsorize ratios at the 1% and 99% percentiles to minimize the effects of outliers. Throughout our analyses, we adjust standard errors for clustering within industries to address potential serial dependence within industries over time and years to address cross-sectional dependence due to common time effects (e.g., inflation).

3.2. Determinants of public firm presence

We begin our empirical analyses by descriptively documenting whether variables that we expect to affect import competition are determinants of public firm presence. To do so, we estimate the following industry-level regression:

$$PublicPresence_{i,t} = \beta'X_{i,t} + \gamma_i + \delta_t + \varepsilon_{i,t,} \tag{1}$$

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[16] The NAICS system underwent revision during our sample period, resulting in changes to the definitions of 14 of the four-digit classifications in our sample. To ensure that our results are not a byproduct of these classification changes we estimate a robustness test after excluding the industries that changed (untabulated). Our inferences remain the same.

where i indexes 4-digit NAICS industries and t indexes calendar years. We follow Badertscher et al. (2013) and measure public firm presence, or *PublicPresence*, as sales by US firms in industry i as reported in the Compustat database, scaled by total US production as reported by the Census Bureau. For multi-segment firms, we allocate industry segment sales to each segment's industry i.[17] *PublicPresence* reflects the ratio of public firm production to total domestic production, not the share of firms that report publicly.

We use data from the US Census Bureau's Annual Survey of Manufactures and Census of Manufacturers (ASM/CMF) to measure US production at the 4-digit NAICS industry-year level. The Census Bureau conducts the ASM in years when there is no full census, which allows us to use ASM production information in non-census years. The census uses several stratifications of the sample, supplemental data from the IRS and Social Security Administration, and full sampling of the largest manufacturing firms, which collectively account for 72% of manufacturing production, to ensure the ASM is almost as accurate as the census.[18] Consistent with the survey returning extremely accurate estimates, the mean, median, and modal sampling error of the 2016 survey at the granular NAICS 6 industry code level was 2.08%, 2.85%, and 0%.[19]

Eq. (1) includes fixed effects for each industry (γ_i) to account for time-invariant differences between industries, and year fixed effects (δ_t) to control for general macroeconomic effects (e.g.,

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[17] We perform this adjustment using business segment data. Because firms in Compustat can have operations in multiple countries, the numerator of our *PublicPresence* measure does not perfectly capture US production from public firms. We adjust for geographic, instead of industry, segments to exclude non-US sales and find similar results (untabulated). Because firms typically produce either geographic or industry segment disclosures and not the interaction of the two, we cannot make both segment adjustments simultaneously.

[18] https://www.census.gov/programs-surveys/asm/technical-documentation/methodology.html.

[19] https://www.census.gov/programs-surveys/asm/data/tables.html; we examine errors at the NAICS 6 level because it is unclear how to aggregate sampling errors to the NAICS 4 level. Prior studies that examine non-manufacturing industries cannot rely on the ASM and hence may have to interpolate values from census years to non-census years. Given the potential estimation error from interpolating census values, these studies often also examine counts of the number of firms as an alternative measure. However, this count measure comes at the cost of no longer value-weighting firms (which, e.g., treats very large and very small firms similarly). Because we believe the ASM is extremely accurate, because the count measure also includes some measurement error, and because we believe that the sales-weighted measure is more appropriate, we choose not to use the count measure in our setting.

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inflation). The vector *X* includes potential determinants that may affect importers' willingness and ability to enter the domestic market that may also affect public firm presence. To account for direct costs of trade, we include *Tariff*, measured as the realized duty paid, which we obtain from the Census Bureau import data. We also include *NTRGap × Post2001*, measured as in Pierce and Schott (2016).[20] Higher values of *NTRGap × Post2001* represent greater uncertainty resolution about trade policy after Congress granted China Permanent Normal Trade Relations status. Tariffs and trade policy uncertainty may increase public firm presence by reducing the risk public firm disclosures will encourage import competition. Finally, we follow Autor et al. (2013) and Acemoglu et al. (2016) and include total industry *i* imports in other high-income countries, or *ImportsOHIC*. *ImportsOHIC* controls for other determinants of industry imports that may affect managers' willingness to list publicly.[21]

We include *ValueAdd*, defined as the total value of industry shipments less the cost of raw materials and fuel, scaled by total shipments, because differences in margins and economies of scale may affect import and listing decisions. We also examine differences in labor intensity and skill with *Payroll*, defined as total industry payroll expenses divided by the total value of industry shipments, and *WageRate*, defined as the hourly wage rate for the average production worker in the industry. Differences in labor skill requirements and wages may affect import competition and listing decisions (e.g., due to potential differences in access to skilled workers or the ability to undercut wages between public and private firms, as well as between domestic and foreign).

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[20] Specifically, we measure *NTRGap × Post2001* as the difference between the normal trade relations (NTR) tariff rate and the higher non-market economies tariff rate in 1999 per industry, interacted with an indicator for the period after Congress granted China Permanent Normal Trade Relations status. We obtain these data from the Pierce and Schott (2016) data appendix.

[21] The other countries are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland (Autor et al., 2013; Acemoglu et al., 2016).

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We control for the size and market control of major industry firms using *Concentration*, defined as the percentage of US production by the top 20 industry firms (public or private) by shipments. Industry concentration may encourage public listing in order to compete or coordinate with other large firms, and may similarly encourage import competition if concentrated industries are otherwise less competitive. We include *IndustryGrowth*, measured as the change in industry sales, scaled by industry sales in the prior year, because differences in growth opportunities may affect public firm presence and encourage importing. We also include measures of investment, *CapEx* and *TotalInv*, to account for differences in industry investment that may affect listing decisions and import competition (e.g., due to entry deterrence; Dixit, 1980). These measures are only available from 2003 onwards. We obtain *IndustryGrowth*, *ValueAdd*, *Payroll*, *WageRate*, *Concentration*, *CapEx*, and *TotalInv* from the US Census Bureau ASM/CMF data.

In Table 2, we present the results of estimating Eq. (1). Columns (1) and (2) show the results without industry fixed effects, while columns (3) and (4) include industry fixed effects. In columns (2) and (4), we include *CapEx* and *TotalInv*, which shortens the sample window due to data availability. The results in columns (1) and (2) suggest that in the cross-section, public firm presence is greater in industries with greater economies of scale, more concentrated industries, and in industries with higher imports in other high-income countries (i.e., *ValueAdd*, *Concentration*, and *ImportsOHIC* are positive and statistically significant). These results are consistent with greater economies of scale, a desire or need to list publicly to coordinate or compete with other large firms, and other variables that affect import competition in high-income countries increasing public firm presence.

The results in columns (3) and (4) suggest that public firm presence increases in industries that are growing and becoming more labor intensive (i.e., *Payroll* and *IndustryGrowth* are positive

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and statistically significant). These results are consistent with public firms having greater access to highly-paid skilled labor, and with growth options encouraging public listing. We find some evidence that *Tariffs* encourage public presence (large and marginally statistically significant coefficient estimates in columns (1), (2), and (4)). This result suggests barriers to trade may encourage public firm presence, potentially by reducing the risk that public disclosures will increase import competition. We find limited evidence that *Capex* or *TotalInv* relate to public firm presence (only one of four coefficient estimates is statistically significant, with mixed signs across the four).

3.3. Public firm presence and subsequent import competition

We next examine the association between public firm presence and subsequent import competition using the model:

$$ImportComp_{i,t} = \alpha_1\ PublicPresence_{i,t-1} + \beta'X_{i,t-1} + \gamma_i + \delta_t + \varepsilon_{i,t,} \qquad (2)$$

where X includes the determinants of public firm presence from Eq. (1), except *CapEx* and *TotalInv* (which are only available after 2003 and weakly and inconsistently relate to *PublicPresence*). *ImportComp* is the ratio of imports, excluding related party imports, to total US production in industry i in year $t,$ and it captures the competitive pressure foreign firms exert on US manufacturers.[22] We use US Census Bureau import data from Peter Schott's website to calculate the measure (Schott, 2008).[23] These data measure imports at the harmonized code (i.e., product) and exporting-country level; we aggregate them to the primary (4-digit) NAICS industry-year level. We use the US Census Bureau's related party trade reports to remove related party trade

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[22] Our measure is akin to import penetration by foreign firms. However, a key difference is that our measure includes US production that is ultimately exported out of the US. We do so because US sales lost to importers are often offset by increased export sales by US firms (e.g., Kletzer, 2001). Our interest is in US production relative to foreign competition—not how US demand is satisfied. By including US exports, we also capture how US financial reporting helps foreign firms to compete with US firms outside the US.

[23] http://faculty.som.yale.edu/peterschott/sub_international.htm. We thank Peter Schott for making these data publicly available.

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from total imports to ensure that we capture foreign competition and not a multinational firm's decision to move production abroad.

Because our industry fixed effects limit our analyses to within-industry variation, using the full sample standard deviation of *PublicPresence* to assess economic magnitudes would likely overstate the magnitudes (Mummolo and Peterson, 2018). To capture feasible variation in the dependent variable, we first demean *PublicPresence* by industry and then standardize the demeaned values to unit variance. This approach eases interpretation such that Eq. (2) estimates coefficients for a within-industry standard deviation increase in *PublicPresence* (note that this transformation does not affect the statistical significance of the coefficient estimates).

We also modify and re-estimate Eq. (2) to examine possible nonlinearities in the relation between public firm presence and import competition. For example, the first few public firms in an industry may provide the bulk of the relevant information, and further public firm presence may only provide a negligible amount of additional information. In light of these possibilities, we include the square of *PublicPresence*, or *PublicPresence^2*, in Eq. (2). We also re-estimate Eq. (2) after taking the inverse hyperbolic sine of all variables (e.g., *ImportComp* and *PublicPresence* become *sinh^{-1}(ImportComp)* and *sinh^{-1}(PublicPresence)*).[24] The inverse hyperbolic sine transformation leads to similar interpretations, and similarly reduces the influence of outliers, as the natural logarithm transformation (Burbidge et al., 1988). A benefit of the inverse hyperbolic sine transformation in our setting is that it is defined for the zero values of import competition and public firm presence in our data (Johnson, 1949; Burbidge et al., 1988).

Panel A of Table 3 presents the result of estimating Eq. (2). Column (1) presents the results without controls and column (2) presents the results of our preferred specification including the

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[24] Because the inverse hyperbolic sine transformation results in coefficient estimates that can be approximately interpreted as percentage changes, we do not demean and standardize in these specifications.

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vector of controls. The coefficient on *PublicPresence* in column (2) suggests that moving from the median to the 75th percentile of public firm presence within an industry increases subsequent import competition in that industry by about 1.3 percentage points (*t*-statistic of 2.43). The inclusion of controls in column (2) attenuates the coefficient estimate on *PublicPresence*. A potential concern is that the included controls imperfectly capture correlated omitted factors and that additional bias remains. Using the maximum R^2 and delta heuristics proposed by Oster (2019), we conclude that our inferences are robust to this potential source of bias.[25]

Column (3) presents the results of estimating Eq. (1) after including *PublicPresence²*. We find some evidence that the relation between *PublicPresence* and *ImportComp* is weakly concave (*PublicPresence² t*-statistic of 1.67).[26] Finally, Panel B presents the results of estimating Eq. (1) after taking the inverse hyperbolic sine transformation of all variables. The results suggest that the elasticity of *ImportComp* to *PublicPresence* is 0.197 (*t*-statistic of 3.13). The elasticity of subsequent import competition to public firm presence is slightly smaller than the elasticity of import competition to the domestic production worker wage rate, 1/3rd of the elasticity to *NTRGap* post 2001, and 1/20th of the elasticity to value added.[27] In total, the evidence suggests that public firm presence is an important, but second-order, determinant of import competition.

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[25] We also explore which variables are responsible for most of the attenuation and find that *Payroll* is responsible for the vast majority of the coefficient reduction. To ensure that the functional form of this variable is not limited in its ability to properly control for labor intensity, unreported results simultaneously include unscaled payroll, logged unscaled payroll, and scaled payroll squared (each lagged). These results are robust to their inclusion.

[26] The weak evidence of nonlinearities is consistent with the literature on financial reporting and profitability dispersion. This literature finds significant dispersion in profitability within industries due to a lack of information sharing (e.g., Berger et al., 2019; Hann et al., 2020; Breuer, 2021). Consequently, this literature suggests that one firm's financial information provides a very incomplete picture of the competitive environment.

[27] The positive elasticity of import competition to the domestic worker wage rate is consistent with foreign importers being able to undercut domestic wages. The positive elasticity to the NTR gap post 2001 is consistent with the results in Pierce and Schott (2016) and Handley and Limão (2017) that the resolution of trade uncertainty due to US granting Permanent Normal Trade Relations status to China increased import competition. The positive elasticity to value add is consistent with foreign importers targeting industries with greater economies of scale and higher domestic margins.

We next turn to the question of whether the positive relation between *PublicPresence* and *ImportComp* is driven by increases in the numerator of *ImportComp* (i.e., *NonRPImports*), decreases in the denominator of *ImportComp* (i.e., *USProduction*), or both. Specifically, we separately re-estimate Eq. (2) after replacing *ImportComp* with *NonRPImports* alone in column (1) and with *USProduction* alone in column (2). The results, reported in Table 3, Panel C, suggest that public firm presence increases import competition and decreases domestic production by approximately the same amount. In other words, public firm presence appears to encourage import competition that crowds out domestic firm production, without significantly affecting the overall level of industry production.

We also examine how related party trade, or *RPImports*, relates to public firm presence. Relative to other importers, related parties should have no, or at least significantly less, information asymmetry about market conditions because related parties have at least partial common ownership. Consequently, if our results are due to public firm presence reducing information frictions, then related party imports should relate less to public firm presence than should non-related party imports. Consistent with public firm presence reducing information frictions driving our results, the coefficient estimate on *PublicPresence* when using *RPImports* as the dependent variable in column (3) is 1/10th of the magnitude of the coefficient when using *NonRPImports* and is not statistically significant (*t*-statistic of 0.35). Therefore, to explain our results, a correlated and omitted variable must correlate with public firm presence and imports, but not with related party trade.

We next re-estimate our baseline specification using the change in the dependent variables and controls and excluding the industry fixed effects (fixed industry differences are "differenced out" when using the change specification). We then replace *PublicPresence* with the tercile of the

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change in public firm presence in each of the three preceding years. We examine the three preceding years to document how the relation between changes in public firm presence and changes in subsequent import competition evolves over time and because changes specifications are more sensitive to timing assumptions than are fixed effects specifications (Greene, 2003). The results, reported in Table 3, Panel S, column (1), suggest that changes in public firm presence in each of the three preceding years positively relate to changes in subsequent import competition. The magnitudes of these relations is similar to those documented in Panel A. For example, the results in column (1) suggest that moving from the midpoint of the first tercile of public firm presence to the third in the prior year would increase import competition by 2.6 percentage points (*t*-statistic of 1.86).

In column (2), we separately examine increases and decreases in public firm presence. We expect increases in public firm presence to have a greater and swifter relation with changes in subsequent import competition than decreases do because increases immediately improve the information environment while decreases deteriorate it only as existing information grows stale. We disaggregate the tercile change in public firm presence into separate indicators for whether the change is in the third tercile (entirely positive) or the first tercile (entirely negative).

We find that decreases in public firm presence negatively relate to subsequent import competition and increases positively relate to subsequent import competition. Perhaps unsurprisingly, given that we are disaggregating variation in public firm presence and imposing strict timing restrictions, 4 of the 6 coefficients are statistically insignificant at conventional levels. However, consistent with our expectations, we find that the relation between subsequent import competition and increases in public firm presence is greater than the relation for decreases in public firm presence. We also find that this asymmetry in magnitudes ameliorates at longer horizons (e.g.,

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the coefficient on increases at *t-1* is 157% greater in magnitude than the coefficient on decreases, while at *t-3* the coefficient on increases is only 53% greater than the coefficient on decreases).

3.4. SOX natural experiment

A potential concern with Eq. (2) is that ownership type (public or private) is an endogenous choice. For example, our determinants analysis suggests that growth options positively relate to greater public firm presence, and they may also encourage import competition. To the extent that our controls imperfectly capture potential growth options, then correlated and omitted growth options may bias our results. Consequently, we follow Badertscher et al. (2013) and use the introduction of SOX as a natural experiment to obtain plausibly exogenous variation in public firm presence.

The US government enacted SOX in response to the Enron and Worldcom accounting scandals (Romano, 2004). Importantly, these accounting scandals were unexpected, as evidenced by the high market value of the two firms immediately prior to their collapse. Enron was an oil and natural gas company and Worldcom was a telecommunications company, suggesting that their behavior had little relation to import competition in *manufacturing* industries, beyond triggering SOX. Consequently, SOX is plausibly exogenous with respect to manufacturing import competition, suggesting that we can use cross-sectional and time-series variation in SOX to draw causal inferences about the effects of public firm presence on import competition.

Prior work finds that SOX imposed large net costs on firms, to which some firms responded by either deregistering or forgoing public listing in the first place.[28] Consequently, we anticipate that the expected costs of SOX will negatively affect public firm presence. To calculate SOX's expected costs, we follow Zhang (2007) and use firms' buy and hold abnormal returns from July

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[28] See Engel et al. (2007), Zhang (2007), Leuz et al. (2008), and Iliev (2010).

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8th, 2002 to July 20th, 2002, when significant news about the likelihood of success and the severity of potential SOX legislation became public.[29] We measure firm-level abnormal returns using the Fama-French three-factor model plus the momentum factor (Carhart, 1997) and average returns by 4-digit NAICS to obtain our industry-level proxy for the expected costs of SOX, *SOXBHAR$_i$*.

We find that *SOXBHAR$_i$* is large and negative (a mean of -4.3% and a median of -3.7%), consistent with SOX imposing significant costs on firms and in line with the findings of Zhang (2007). *SOXBHAR$_i$* varies considerably across industries (a standard deviation of 5.9%), suggesting heterogeneity in these costs across industries. We anticipate that this heterogeneity in costs will result in different rates of "compliance" with the SOX treatment. In industries with higher costs, more firms will respond to SOX by deregistering or forgoing listing in the first place and public firm presence will relatively decline. In industries with lower costs, fewer firms will respond to SOX by deregistering or forgoing listing in the first place and public presence will remain relatively unchanged. Thus, we can estimate a "fuzzy" difference-in-differences model (Armstrong et al., 2018), where how aggressively public presence changes in response to SOX is a function of *SOXBHAR$_i$*.[30]

Practically speaking, our fuzzy difference-in-differences design embeds an instrumental variables system of equations in a difference-in-differences specification:

$$PublicPresence_{i,t-1} = \theta_1 (SOXBHAR_i \times Year2003_t) + \cdots +$$

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[29] During this period, the Senate passed the SOX bill with added amendments to strengthen its impact, President Bush delivered speeches in support of rulemaking on corporate reform, and House Republicans reportedly retreated from efforts to dilute the bill. We estimate abnormal returns as the residual from a model of expected returns based on the Fama-French and momentum factors. We estimate firms' factor exposures using firm returns over the 100-day window (requiring at least 70 return observations per firm) prior to a 50-day gap before the event using the WRDS event study application.

[30] While we use cross-sectional differences in the industry-level costs of SOX in conjunction with time-series variation in the timing of the act, prior studies use alternative sources of variation. For example, Gao et al. (2009), Iliev (2010), and Glaeser et al. (2020) use variation in firms' proximity to size-based compliance thresholds and Armstrong et al. (2019) use differences in firms' fiscal year ends. We expect these sources of variation to be too narrow to detect aggregate cross-industry effects.

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$$\theta_6 (SOXBHAR_i \times Year2008_t) + \rho'X_{i,t} + \lambda_i + \nu_t + \eta_{i,t}; \qquad (3a)$$

$$ImportComp_{i,t} = \alpha_1 \widehat{PublicPresence}_{i,t-1} + \beta'X_{i,t} + \lambda_i + \nu_t + \varepsilon_{i,t}. \qquad (3b)$$

The first stage equation, Eq. (3a), estimates treatment compliance with $SOXBHAR_i$ over time. We interact $SOXBHAR_i$ with indicators for each year after SOX enactment to allow the model to reflect the effects of SOX on *PublicPresence* over time.[31] Doing so is important because while *PublicPresence* may quickly reflect the effect of firms deregistering after SOX enactment, the effect of firms forgoing public listing may take more time to accumulate.

Eq. (3) includes industry fixed effects (λ_i), which isolate variation in *PublicPresence* across industries time and absorb the main effect of $SOXBHAR_i$. Eq. (3) also includes year fixed effects (ν_t) that control for common macroeconomic effects and reflect the main effects of $YearXXXX_t$. The vector *X* includes time-varying industry factors from Eq. (1) that might affect *PublicPresence*. The second stage equation, Eq. 3(b), regresses import competition on the fitted values of $PublicPresence_{i,t-1}$ from the first stage, our control variables, and industry and year fixed effects.

Like all instrumental variables models, fuzzy difference-in-differences models require the relevance condition to hold in order to produce causal estimates. We examine the relevance condition by estimating Eq. (3a), and we report the results in Table 4, Panel A. In column (1), we find a strong positive relation between $SOXBHAR_i$ and $PublicPresence_{i,t-1}$ for $Year2005_t$ through $Year2008_t$ (i.e., $PublicPresence_{i,t-1}$ is a function of SOX costs beginning in 2005). The positive coefficients are consistent with higher costs of SOX causing a decline in public presence ($SOXBHAR_i$ is more negative when costs are higher). Based on when SOX begins to affect

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[31] Subscripts are consistent for observations across Eq. (3a) and (3b)—i.e., import competition in year *t* will be a function of public presence in year *t*-1. Thus, the year indicators in the first stage equation refer to year *t* whereas the dependent variable is lagged. For example, $Year2003_t$ turns on for the value of $PublicPresence_{i,t-1}$ corresponding with 2002 (the year of enactment) and so on.

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PublicPresence, we simplify the model in column (2) and use an indicator for *t* greater than or equal to 2005 (public presence in 2004 and after).

The results suggest that moving from an industry at the 75th percentile of *SOXBHAR$_i$* to the median industry (equivalent to an increase in the expected costs of SOX equivalent to 3.8% of market value), would result in a 0.171 within-industry standard deviation decrease in public presence. This decrease is equivalent to 9.5 percentage points (*t*-statistic of -2.60), and reflects the combined effect of any public firms going dark (Leuz et al., 2008) or going private (Engel et al., 2007), as well as the effect of any private firms deciding not to list publicly. Combined, these effects suggest that SOX meaningfully decreased public firm presence in manufacturing industries where *SOXBHAR$_i$* is more negative. We conclude that *SOXBHAR$_i$* is a relevant instrument for public firm presence.

We report the results of estimating Eq. (3b) in Table 4, Panel B. The magnitude of the coefficient estimates on the instrumented values of *PublicPresence* are similar in magnitude to the coefficient estimates documented in Table 3. These results suggest that public firm presence causes an increase in subsequent import competition (*t*-statistics of 1.84 and 2.13). However, we note that instrumental variables models require the exclusion restriction to hold in order to produce causal estimates. In fuzzy difference-in-differences models, the exclusion restriction is equivalent to the parallel trends assumption in standard difference-in-differences models.

We expect the parallel trends assumption to hold for several reasons. Because the abnormal returns we use to construct *SOXBHAR$_i$* are by construction unexpected, we do not expect them to relate to selection by individual firms. Because malfeasance by non-manufacturing firms triggered SOX, we do not expect that regulators designed SOX with respect to the characteristics of different manufacturing industries. Moreover, we do not expect *SOXBHAR$_i$* to reflect significant growth

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opportunities because we purge it of the returns to the market, size, market-to-book, and momentum factors, and because additional news about industry growth options between July 8th, 2002 and July 20th, 2002 is unlikely.[32] We also estimate parallel trends falsification tests to examine this assumption.

In particular, we estimate two models, regressing either *PublicPresence* or *ImpComp* on our control variables, industry fixed effects, and $SOXBHAR_i$ interacted with year indicators. Because this test is a falsification test, we estimate the latter in reduced form to increase the power to detect differential effects prior to the act.[33] We report the results relative to the year the act passed (2002). We find no evidence that *PublicPresence* or *ImportComp* trends differently based on *SOXBHAR* prior to the act, consistent with the parallel trends assumption. Moreover, we find that the respective relations between *SOXBHAR* and both *PublicPresence* and *ImportComp* increases gradually after the passage of the act, before reaching a steady state. This gradual increase is consistent with public firm presence responding to SOX after a delay as firms delist and forgo listing in response to the act, and with importers responding to the resulting gradual decay in the information environment. This gradual increase is also inconsistent with the presence of a correlated omitted variable, such as omitted growth options, unless the omitted variable gradually affects importers' behavior after the act.

3.5. German enforcement reform natural mechanism experiment

A potential concern with the preceding analyses is that while they investigate the relation between *PublicPresence* and *ImportComp*, they do not isolate the mechanism(s) through which

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[32] It is possible that high costs due to SOX may put US firms in that industry at a competitive disadvantage relative to foreign firms. If so, it would work against finding our results.

[33] Similarly, we do not remove related party trade from this test because that data begins in 2000, limiting our ability to detect pre-treatment effects. For the same reason, we exclude the lagged variable, *IndustryGrowth*. The results are almost identical if we do not make these adjustments (although slightly less statistically significant in the post-period when removing related party trade).

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this relation arises. Consequently, we build on Bernard (2016) and examine a natural mechanism experiment around major financial reporting enforcement reforms in Germany (Ludwig et al., 2011). Since 1987, German limited liability firms are subject to E.U. reporting mandates that require them to publicly disclose certain annual financial statement information. However, local German courts originally tasked with enforcing these reporting requirements did not impose significant penalties for non-compliance, which caused most firms to ignore them (Henselmann and Kaya, 2009; Bernard, 2016).

The low rate of compliance changed in November 2006 when a series of court cases and increasing pressure from the European Commission caused the German government to enact the Bill on the Electronic Registers for Commerce, Companies and Associations (Electronic Commercial and Company Registrar, Gesetz über elektronische Handelsregister und Genossenschaftsregister sowie das Unter- nehmensregister).[34] Effective for financial statements with fiscal years ending on or after December 31, 2006, the bill created an electronic publication register, centralized enforcement, and enacted escalating fines for non-compliance. Unsurprisingly, financial reporting compliance rapidly increased (Bernard, 2016; Breuer et al., 2019; see Figure 2, Panel A for compliance over time in our sample).

We predict that in German industries where more production is publicly reported after the increase in enforcement, subsequent import competition will relatively increase. To examine this prediction, we estimate the following baseline regression:

$$GermanImportComp_{i,t} = \alpha_1 Post\ 2007_t \times DiscShift_i + \gamma_i + \delta_t + \varepsilon_{i,t}. \tag{4}$$

We measure the increase in public reporting due to increased enforcement using the difference in the sales-weighted proportion of firms reporting publicly between 2008 and 2006 in industry i, or

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[34] See Henselmann and Kaya (2009), Bernard (2016), Breuer et al. (2019), and Breuer (2021).

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DiscShift. Post 2007 is an indicator for the period after 2007 (Bernard, 2016; Breuer et al., 2019; Breuer, 2021). We measure disclosed sales using Bureau van Dijk's Orbis database and total German production using Eurostat. *GermanImportComp* is defined as the ratio of imports to total German production in industry *i* in year *t*. We obtain German import data from the BACI international trade database. Because Eq. (4) is a generalized difference-in-differences specification, the main effect of *DiscShift* is absorbed by the industry indicators, γ_i, and the main effect of *Post 2007* is absorbed by the year indicators, δ_t.

We modify Eq. (4) to include US import competition (*ImportComp*) and its interaction with *Post 2007* to control for any common features between the German market and US market related to imports. We are also able to modify Eq. (4) by including the ratio of sales by German public firms in Bureau van Dijk's Orbis database with non-missing market capitalization to total production from Eurostat in industry *i* and year *t*, or *GermanPublicPresence*, and interact it with *Post 2007*. The inclusion of *GermanPublicPresence* allows us to estimate the respective relations of financial reporting with import competition and public firm presence.

Table 5 presents the results of estimating Eq. (4). We find evidence that mandated financial reporting positively effects import competition in the German setting. For example, the results in column (1) suggest that moving from the median industry to an industry at the 75th percentile of *DiscShift* (equivalent to a 42 percentage point increase in sales-weighted industry financial reporting) increases import competition in that industry by 13 percentage points (*t*-statistic of 1.86). This result is similar in magnitude in all columns and statistically significant in columns (1) through (4), but it becomes statistically insignificant in column (5) when including all variables and interactions. However, the insignificance is not due to a decrease in magnitudes, as the coefficient estimate on *Post 2007* × *DiscShift* in column (5) actually becomes slightly larger in

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magnitude relative to the coefficient estimate in column (4). Instead, an increase of about 50% in the standard error of the estimate drives the insignificance. In total, the results suggest that increases in reporting requirements cause increases in importing, consistent with public reporting requirements acting as a channel through which public firm presence affects import competition in the US setting.

In contrast, we find no evidence that *GermanPublicPresence* relates to *GermanImportComp* (e.g., a coefficient estimate of 0.038 and *t*-statistic of 0.33 in column (3)). This latter result suggests that other public firm characteristics, such as their access to liquid capital, are unrelated to import competition. This latter result is also inconsistent with a correlated omitted variable or selection problem such as omitted growth options biasing our results in the US setting (unless that variable or selection problem is not present in Germany).

Similar to our SOX analysis, we also evaluate the parallel trends assumption in the German enforcement setting by interacting the year indicators with *DiscShift*. We present the results, reported relative to 2007, in Figure 2, Panel B. We find no evidence that import competition varies significantly with *DiscShift* prior to 2008. However, we find that import competition increases as a function of *DiscShift* beginning in 2008. We note that import competition responds more quickly in the German setting than in the SOX setting. Because information production relatively increases in the German setting and relatively decreases in the SOX setting, the quicker effect in the German setting suggests that imports respond more quickly to increases in information than they do to decreases (consistent with the results of our changes design reported in Table 3, Panel C).

The delay between the bill's passage in late 2006 and after the 2007 fiscal year, when firms began to disclose in response to the bill's mandate, also helps rule out growth options biasing our results in the German setting. If the German government enacted the bill because of industry-level

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differences in growth options, or if the European Commission pressured the German government because of industry-level differences in growth options, these growth options would have to have no affect import competition until 2008. We think it unlikely that the German government enacted the bill in November 2006 with foresight about industry-level differences in 2008 growth options. We also think it is unlikely that the German government enacted the bill with foresight about industry-level differences in 2008 growth options, but not with foresight about industry-level differences in 2006 or 2006 growth options.

3.6. US mechanism tests

In this section, we estimate or summarize a series of tests in the US market that examine whether the production of information is the mechanism through which public firm presence affects import competition. These tests provide insight into why public firm presence benefits importers to a greater degree than it does domestic firms. Further, these tests provide insight into how our prior inferences generalize to different settings and subsamples (Glaeser and Guay, 2017).

3.6.1 Informativeness of US financial reports

We first estimate Eq. (2) after including measures of the informativeness of US financial reports and their interaction with *PublicPresence*. We predict that when competitors find US financial reports more informative, the relation between *PublicPresence* and *ImportComp* will be greater because public firms' financial reports will resolve importers' uncertainty to a greater degree. We use equity market responses to disclosures as a measure of their informativeness to competitors (e.g., Ball and Brown, 1968; Beaver, 1968; see Dechow et al. (2010) for a review).[35] Specifically, we estimate four similar measures of financial report informativeness, *ICScore1-4*,

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[35] We assume that foreign competitors and investors find the same kind of information informative. We believe this assumption is reasonable because many of the forces that affect domestic firm value should affect the attractiveness of their markets to importers (e.g., domestic firms' risks, opportunities, and performance should affect foreign importers' entry, exit, and production decisions).

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using the R^2 from regressions of either trading volume or absolute returns on disclosure dates. The four measures use various permutations of earnings announcement dates or the 10-K/10-Q release dates of the focal firm and other firms in the industry. Full details of the measurement estimation can be found in the online appendix. The results in Table 6 suggest that foreign competitors respond more to public firm presence when financial statements are more informative (*t*-statistics of 1.84 to 2.53 on the coefficient estimate for the interaction of *PublicPresence* and *ICScore*).

3.6.2 Forecasts of US financial information

We next modify Eq. (2) to examine the potential moderating effect of different types of manager and analyst forecasts on the relation between *PublicPresence* and *ImportComp*. We predict that when managers or analysts forecast future financial information, the relation between *PublicPresence* and *ImportComp* will be greater because forecasts help resolve foreign importers' uncertainty. To examine this prediction, we begin by separating public firm sales into those by firms with managers who provide future earnings guidance and those by firms with managers who do not. We then scale each by total production, resulting in a measure of guided public firm presence, *GuidedPublicPresence*, and a measure of unguided public firm presence, *UnguidedPublicPresence*. We then re-estimate Eq. (2) after replacing *PublicPresence* with *GuidedPublicPresence* and *UnguidedPublicPresence*. We report the results in Table 7, Panel A. We separately repeat the process for EPS, sales, gross margin, and capital expenditure forecasts in columns (1)-(4) to explore how each type of forecast differentially affects the relation between public firm presence and subsequent import competition.

Across all four columns, we find that public firm presence relates positively to subsequent import competition, regardless of whether the manager issued a forecast. Moreover, we find that the relation is greater when the manager forecasts futures gross margins and capital expenditures

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(chi-squared test statistics of 2.999 and 4.593 and *p*-values of 0.083 and 0.032 in columns (3) and (4)). This result is consistent with foreign importers responding to public firm presence when uncertainty about future profitability and capital expenditures is ameliorated by manager forecasts.

In Panel B of Table 7, we turn to analyst forecasts. Following Badertscher et al. (2013), we take the number of analysts following each firm, and aggregate these counts at the industry-year level. We then interact the industry-year aggregate count of analyst following with the variables in the model and then repeat the process for each different forecast type. For all four types, we find that subsequent import competition is more responsive to public firm presence when more analysts forecast subsequent firm outcomes (*t*-statistics on the interaction of *Analysts* and *Public Firm Presence* range from 3.13 to 5.00). One potential explanation for the seemingly higher value of analyst, relative to manager, forecasts is that analysts' information advantage is about the macro-economy and importers may care more about macroeconomic information (Hutton et al., 2012). Another potential explanation is that analysts tend to issue longer horizon forecasts, and potential importers care more about long-term outcomes.

Regardless, one advantage of examining analyst forecasts is that unlike managers, many analysts forecast longer term earnings. Consequently, we are able to examine how longer horizon analyst EPS forecasts, particularly horizons of up to five or more years, moderate the relation between public firm presence and import competition. The results, presented in column C, suggest that the amplifying effect of analyst forecasts on the relation between public firm presence and subsequent import competition is monotonically increasing in the horizon of the analyst forecast (e.g., the moderating effect of forecasts that are five years or more ahead in column (5) is over four times greater than the moderating effect of the one year ahead forecasts in column (1)).

3.6.3 Country-level analyses

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In this section, we summarize several analyses conducted at the exporting country-industry-year level that we report and explain in detail in the online appendix. These analyses explore when and why public firm presence benefits some foreign firms to a greater degree than others, holding the value of that information to domestic firms fixed.

We expect public firm presence to benefit importers more when they can more easily process US financial reports (i.e., when importers face lower information processing costs and information frictions). Consequently, we examine how the similarity of a country's accounting standards to US GAAP, as measured by Bradshaw et al. (2004), moderates the relation between public firm presence and subsequent import competition from that country. Consistent with our predictions, we find that public firm presence has a stronger relation with import competition originating from countries that have similar accounting standards to US GAAP. We also examine whether foreign competitors appear to access public disclosures made by US firms. Consistent with this, we find that increases in EDGAR downloads of US financial statements in a given industry by users in a foreign country precede increases in import competition from that country and industry.

3.7. UK falsification test

In this section, we follow Badertscher et al. (2013) and estimate a falsification test in the UK to bolster our inference that public financial reporting is an important mechanism through which public firm presence affects import competition. The UK's Financial Reporting Council (FRC) requires both public and private firms to report audited financial statements. Further, UK enforcement of financial reporting requirements, unlike German enforcement, was historically high. Consequently, we should find not find any relation between changes in public firm presence

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and import competition in the UK due to financial reporting because both public and all but the smallest private firms must report publicly.

We focus on the UK because it is in many other ways culturally and economically similar to the United States (e.g., the Special Relationship; see Griffith et al. (2006)). For example, the UK received the highest score on the Brookings' institute's global manufacturing scorecard (78), while the US received an almost identical score (77).[36] Similarly, the United Nations Conference on Trade and Development notes that manufacturing accounts for a similar proportion of both countries' production (e.g., 10% vs 12% in 2016).[37] Consequently, we anticipate that any endogenous relation between changes in public firm presence and import competition will also be present in the UK. We also anticipate that any non-financial reporting characteristic of public firms that causally affects import competition will also be present in the UK (i.e., we expect alternative mechanisms to also be present in the UK). Therefore, we estimate the following regression:

$$UKImportComp_{i,t} = \alpha_1 UKPublicPresence_{i,t-1} + \alpha_2 UKDisclosure_{i,t-1} + \gamma_i + \delta_t + \varepsilon_{i,t}. \quad (6)$$

We measure *UKImportComp* analogously to how we measure import competition in Germany and the US (i.e., as the ratio of imports to total UK production in industry i in year t).

We obtain UK import data from the BACI international trade database. We measure *UKPublicPresence* as sales by UK public firms, as inferred by non-missing market capitalization divided by UK production, and obtained from the Orbis database and Eurostat. We measure *UKDisclosure* as disclosed sales divided by UK production, which are also from the Orbis database and Eurostat. Because the FRC historically enforced disclosure requirements, it may be

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[36] https://www.brookings.edu/research/global-manufacturing-scorecard-how-the-us-compares-to-18-other-nations/.
[37] https://unctad.org/statistics.

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comparatively more difficult to identify an incremental effect of disclosure in the UK, relative to Germany.[38] Finally, we again include both *USImportComp* and *USPublicPresence*.

Table 8 presents the results of estimating Eq. (6). Column (1) reports the baseline results and column (2) reports the results including the US variables. Columns (3) and (4) present the results from repeating the sequence using the inverse hyperbolic sine transformation of the independent and dependent variables of interest. Across all four columns, we find no evidence of a relation between public firm presence and import competition in the UK where both public and private firms report publicly (*t*-statistics of 0.16 to 0.78).

However, we note that while the UK is similar to the US in many ways, it is still not a perfect counterfactual. One prominent difference between the two economies is that import competition in the former is significantly more saturated (e.g., the median import competition in the UK is 0.798, while in the US it is 0.125). To ensure these differences in saturation do not drive our results, in unreported tests we repeat the main tests documented in Table 3, Panel A, column (2) using quantile regression. Quantile regression allows us to draw inferences about the relation between *PublicPresence* and specified percentiles of the conditional distribution of *ImportComp*. Consequently, quantile regression allows us to document the relation between *PublicPresence* and *ImportComp* in the US at saturations of *ImportComp* that are equivalent to the mean level in the UK, as well as at the median. We find that our inferences are similar at these saturation levels.

In total, the results in Table 8 suggest that the positive relation between import competition and public firm presence is not present in the UK. Therefore, a difference between the UK and the US is responsible for the differential relation between public firm presence and import competition

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[38] Unlike in the German setting, where changes in the disclosure behavior of large firms help to identify the effect of *DiscShift*, in the UK setting only changes in the disclosure behavior of very small firms identify the effect of *UKDisclosure*.

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in the two countries. Arguably, the most significant difference is that the SEC does not require US private firms to publicly report, while the FRC requires that UK private firms to publicly report.[39] Consequently, the results in Table 8 suggest that public financial reporting requirements, and not some other characteristic of public firm presence, is responsible for the positive relation between public firm presence and import competition in the US.

4. Conclusion

We examine how public firm presence affects import competition. Public firms generate a tremendous amount of information, both directly from mandatory financial reports and voluntary manager forecasts, and indirectly from analysts and the business press. Although investors are the intended beneficiaries of this information, foreign competitors may be able to use it to compete with US firms. We find evidence that this is the case. Information spillovers from US firms to foreign competitors represent an important externality that may be of interest to policymakers. Our evidence that the information spills over to import competition contributes to the international trade literature by providing direct evidence of information frictions affecting trade. Our finding that financial reports help create these information spillovers also contributes to the financial reporting literature by providing evidence of foreign competitors using the proprietary information revealed in financial reports.

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[39] Similarly, the results suggest that the positive relation between import competition and public disclosure is present in Germany, but not in the UK. Therefore, a difference between public reporting and import competition between the two countries is likely responsible for the differential relation. Arguably, the most significant difference is that German regulators historically did not enforce public reporting requirements, while the FRC did.

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Appendix A: Variable Definitions

$AnyPublicFirms_{i,t}$ Indicator equaling 1 if industry i has any public firms, 0 otherwise

$Analysts_{i,t}$ Analyst following of each firm in the industry, aggregated to the industry-year level

$CapEx_{i,t}$ Total capital expenditures for 4-digit NAICS industry i scaled by industry sales in year t

$China_j$ Indicator equaling 1 if country j is China and 0 otherwise

$NotChina_j$ Indicator equaling 0 if country j is China and 1 otherwise

$Concentration_{i,t-1}$ Percentage of US production from the top 20 firms by shipments in 4-digit NAICS industry i in year $t-1$

$DiscShift_i$ Shift in fraction of German production publicly disclosed in financial reports in from 2006 to 2008

$Downloads_{i,j,t-1}$ Number of 10-K and 10-Q downloads per country, per industry, per year from EDGAR server logs where crawler = 0. Country information obtained from the first three octets of the downloading IP address, with country ranges obtained from `lite.ip2location.com`

$Downloads_{i,t-1}$ Sum of $Downloads_{i,j,t-1}$ for all non-US countries in industry i during year $t-1$

$Downloads_{j,t-1}$ Sum of $Downloads_{i,j,t-1}$ for all manufacturing industries in country j during year $t-1$

$Downloads_{t-1}$ Sum of $Downloads_{i,j,t-1}$ for all manufacturing industries from all non-US countries during year $t-1$

$GermanDisclosure_{i,t-1}$ Sum of sales from German firms in BvD's Orbis database for 4-digit NAICS industry i and year $t-1$ scaled by German production in the same industry and year from Eurostat. Winsorized at 1% and 99%

$GermanImportComp_{i,t}$ Sum of German imports in industry i and year $t-1$ from the BACI database scaled by German production in the same industry and year from Eurostat. Winsorized at 1% and 99%

$GermanPublicPresence_{i,t-1}$ Sum of sales from German firms with non-missing market capitalization in BvD's Orbis database for 4-digit NAICS industry i and year $t-1$ scaled by German production in the same industry and year from Eurostat. Winsorized at 1% and 99%

$GuidedPublicPresence_{i,t}$ Public presence but only counting firms issuing guidance in the numerator

$ICScore1_{i,t}$ R^2 from an industry year-regression:

$$\frac{TradingVolume_{f,d}}{SharesOutstanding_{f,d}} = \beta_0 + \beta_1 AnyRelease_{f,d} + \beta_2 AnyPeerRelease_{f,d} + \varepsilon_{f,d}$$

where f indexes firm, d indexes day, $TradingVolume_{f,d}$ is the number of firm f's shares traded on day d, $SharesOutstanding_{f,d}$ is the number of shares outstanding for firm f on day d, $AnyRelease_{f,d}$ is an indicator if d is a day that firm f releases its 10-K, 10-Q, or announces its earnings. $AnyPeerRelease_{f,d}$ is an indicator for the focal firm's industry peers' EDGAR 10-Q release dates, 10-K date, and earnings release dates from Compustat times each peer's sales weight in the industry in the prior year

$ICScore2_{i,t}$ R^2 from an industry year-regression:

$$\frac{TradingVolume_{f,d}}{SharesOutstanding_{f,d}} = \beta_0 + \beta_1 EARelease_{f,d} + \beta_2 EAPeerRelease_{f,d} + \varepsilon_{f,d}$$

where f indexes firm, d indexes day, $TradingVolume_{f,d}$ is the number of firm f's shares traded on day d, $SharesOutstanding_{f,d}$ is the number of shares outstanding for firm f on day d, $EARelease_{f,d}$ is an

indicator if d is a day that firm f announces its earnings. $EAPeerRelease_{f,d}$ is an indicator for the focal firm's industry peers' earnings release dates from Compustat times each peer's sales weight in the industry in the prior year

$ICScore3_{i,t}$ R^2 from an industry year-regression:

$$|Return_{f,d}| = \beta_0 + \beta_1 AnyRelease_{f,d} + \beta_2 AnyPeerRelease_{f,d} + \varepsilon_{f,d}$$

where f indexes firm, d indexes day, $|Return_{f,d}|$ is the absolute value of the stock return for firm f on day d. $AnyRelease_{f,d}$ is an indicator if d is a day that firm f releases its 10-K, 10-Q, or announces its earnings. $AnyPeerRelease_{f,d}$ is an indicator for the focal firm's industry peers' EDGAR 10-Q release dates, 10-K date, and earnings release dates from Compustat times each peer's sales weight in the industry in the prior year

$ICScore4_{i,t}$ R^2 from an industry year-regression:

$$|Return_{f,d}| = \beta_0 + \beta_1 EARelease_{f,d} + \beta_2 EAPeerRelease_{f,d} + \varepsilon_{f,d}$$

where f indexes firm, d indexes day, $|Return_{f,d}|$ is the absolute value of the stock return for firm f on day d. $EARelease_{f,d}$ is an indicator if d is a day that firm f announces its earnings. $EAPeerRelease_{f,d}$ is an indicator for the focal firm's industry peers' earnings release dates from Compustat times each peer's sales weight in the industry in the prior year

$ImportComp_{i,t}$ Worldwide imports to the US in 4-digit NAICS i during year t scaled by US production in the same 4-digit NAICS and year. Winsorized at 1% and 99%. Adjusted to remove related-party trade

$ImportComp_{i,j,t}$ Imports from country j to the US in 4-digit NAICS i during year t scaled by US production in the same 4-digit NAICS and year. Winsorized at 1% and 99%. Adjusted to remove related-party trade

$ImportComp98_{i,t}$ Same as $ImportComp_{i,t}$ but scaled by industry production in 1998 rather than in year t

$ImportCompInclRP_{i,t}$ Same as $ImportComp_{i,t}$ but without adjusting for related party trade

$ImportsOHIC_{i,t}$ Worldwide imports (in billions of USD) in 4-digit NAICS i and year t to eight high-income countries from Autor et al. (2003): Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. Data obtained from the BACI database

$IndustryGrowth_{i,t-1}$ Change in 4-digit NAICS industry sales from $t-2$ to $t-1$, scaled by industry sales in $t-2$

$NTRGap_i$ Difference between the normal trade relations (NTR) tariff rate and the higher non-market economies tariff rate in 1999 per industry from Pierce and Schott (2016)

$Payroll_{i,t-1}$ Payroll expenses for US firms in 4-digit NAICS industry i in year $t-1$, scaled by lagged total value of shipments for the same industry and year

$PublicPresence_{i,t-1}$ Sales from Compustat firms in a given 4-digit NAICS industry i in year $t-1$, scaled by US production in the same industry and year. Sales are allocated to industries using business segments for multi-industry firms. Winsorized at 1% and 99%

$Post2001_t$ Indicator equaling 1 if t is greater than 2001 and 0 otherwise

$PostSOX_t$ Indicator equaling 1 if t is greater than 2002, missing if 2002, and 0 less than 2002

$SOXBHAR_i$ Buy-and-hold return for an equal-weighted portfolio of stocks in 4-digit NAICS industry i for the 12 trading days following Jul 8, 2002. Calculated for all industries with 10 or more publicly traded firms

SOXBHARRank$_i$ Industry rank of buy-and-hold return for an equal-weighted portfolio of stocks in 4-digit NAICS industry i for the 12 trading days following Jul 8, 2002. Calculated for all industries with 10 or more publicly traded firms

Tariff$_{i,t-1}$ Tariffs levied on imported goods in 4-digit NAICS industry i worldwide divided by total value of imports the dame industry and year

Tariff$_{i,j,t-1}$ Tariffs levied on imported goods divided by total value of imports for consumption from country j in 4-digit NAICS i in year $t-1$

TotalInv$_{i,t}$ Total year end inventories for 4-digit NAICS industry i scaled by industry sales in year t

UKDisclosure$_{i,t}$ Sum of sales from UK firms in BvD's Orbis database for 4-digit NAICS industry i and year $t-1$ scaled by UK production in the same industry and year from Eurostat. Winsorized at 1% and 99%

UKImportComp$_{i,t}$ Sum of UK imports in industry i and year $t-1$ from the BACI database scaled by UK production in the same industry and year from Eurostat. Winsorized at 1% and 99%

UKPublicPresence$_{i,t-1}$ Sum of sales from UK firms with non-missing market capitalization in BvD's Orbis database for 4-digit NAICS industry i and year $t-1$ scaled by UK production in the same industry and year from Eurostat. Winsorized at 1% and 99%

UnguidedPublicPresence$_{i,t}$ Public presence but only counting firms not issuing guidance in the numerator

USImportComp$_{i,t}$ Same as $ImportComp_{i,t}$

USPublicPresence$_{i,t-1}$ Same as $PublicPresence_{i,t-1}$

WageRate$_{i,t-1}$ Hourly wage rage in dollars for the average production worker in industry i in year $t-1$

ValueAdd$_{i,t-1}$ Value added by US manufacturing (shipments - raw materials and fuels) scaled by shipments in 4-digit NAICS industry i in year $t-1$

Year2003$_t$ Indicator equaling 1 if t is 2003, 0 otherwise

Year2004$_t$ Indicator equaling 1 if t is 2004, 0 otherwise

YearGE2005$_t$ Indicator equaling 1 if t is 2005 or greater, 0 otherwise

Figure 1

SOX Difference-in-Difference Trend Analysis

This figure plots the β coefficients and associated 90% confidence intervals from estimating public presence and import competition as a function of the industry buy-and-hold returns associated with key Sarbanes-Oxley dates. Specifically, in Panel A we estimate:

$$
\begin{aligned}
PublicPresence_{i,t} = \; & \alpha_1\, Tariff_{i,t} + \alpha_2(NTRGap_i \times Post2001_t) + \alpha_3\, ValueAdd_{i,t} + \\
& \alpha_4\, Payroll_{i,t} + \alpha_5\, WageRate_{i,t} + \alpha_6\, Concentration_{i,t} + \alpha_7\, IndustryGrowth_{i,t-1} + \\
& \beta_1(SOXBHAR_i \times Year1999_t) + \cdots + \\
& \beta_3(SOXBHAR_i \times Year2001_t) + \\
& \beta_4(SOXBHAR_i \times Year2003_t) + \cdots + \\
& \beta_9(SOXBHAR_i \times Year2008_t) + \gamma_i + \delta_t + \varepsilon_{it}
\end{aligned}
$$

In Panel B we estimate:

$$
\begin{aligned}
ImportComp_{i,t} = \; & \alpha_1\, Tariff_{i,t-1} + \alpha_2(NTRGap_i \times Post2001_t) + \alpha_3\, ValueAdd_{i,t-1} + \\
& \alpha_4\, Payroll_{i,t-1} + \alpha_5\, WageRate_{i,t-1} + \alpha_6\, Concentration_{i,t-1} + \alpha_7\, IndustryGrowth_{i,t-1} + \\
& \beta_1(SOXBHAR_i \times Year2000_t) + \\
& \beta_2(SOXBHAR_i \times Year2001_t) + \\
& \beta_3(SOXBHAR_i \times Year2003_t) + \cdots + \\
& \beta_8(SOXBHAR_i \times Year2008_t) + \gamma_i + \delta_t + \varepsilon_{it}
\end{aligned}
$$

i indexes industry and t indexes year. γ_i is a time-invariant fixed effect for each industry and δ_t is a year fixed effect. $YearXXXX_t$ is an indicator if $t = XXXX$. All other variables are defined in Appendix A. The reference period in both panels is the year 2002. The sample period is from 1999-2008 for Panel A and 2000-2008 for Panel B (1999 lost because of lagged variables). Standard errors are clustered by industry and year.

Panel A. Changes in Public Presence from SOX Compliance Costs
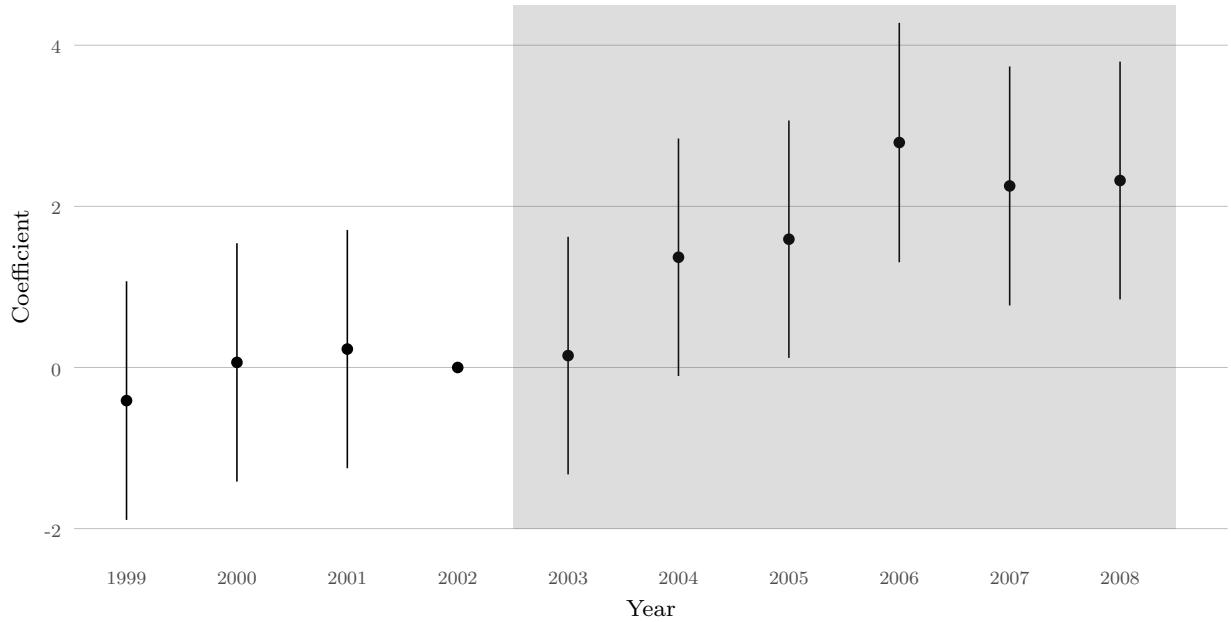
Panel B. Changes in Import Competition from SOX Compliance Costs
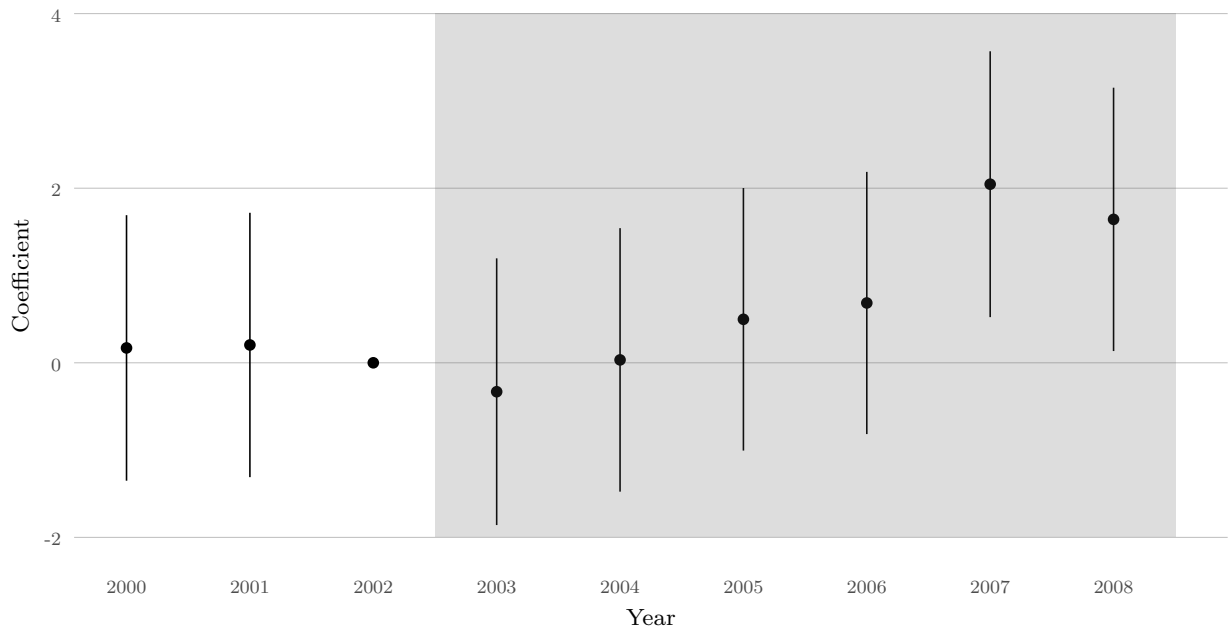
Figure 2

German Enforcement Shock Difference-in-Difference Trend Analysis

Panel A plots yearly changes in the fraction of German production disclosed in firm financial statements, highlighting the enforcement change in 2007. Panel B This figure plots the α coefficients and associated 90% confidence intervals from an estimation of the model:

$$
\begin{aligned}
GermanImportComp_{i,t} = \ & \alpha_1(DiscShift_i \times Year2003_t)+ \\
& \alpha_2(DiscShift_i \times Year2004_t) + \cdots + \\
& \alpha_4(DiscShift_i \times Year2006_t)+ \\
& \alpha_5(DiscShift_i \times Year2008_t)+ \\
& \alpha_2(DiscShift_i \times Year2009_t) + \cdots + \\
& \alpha_6(DiscShift_i \times Year2012_t)+ \\
& \beta ImportComp_{i,t} + \gamma_i + \delta_t + \varepsilon_{i,t}
\end{aligned}
$$

where i indexes industry and t indexes year. γ_i is a time-invariant fixed effect for each industry and δ_t is a year fixed effect. $YearXXXX_t$ is an indicator if $t = XXXX$. $DiscShift_i$ is the 4-digit NAICS industry's disclosure ratio in 2008 minus the ratio in 2006. $ImportComp_{i,t}$ is US import compeition. All other variables are defined in Appendix A. The reference period is the year 2007. Standard errors are clustered by industry and year.

Panel A. Disclosure Changes

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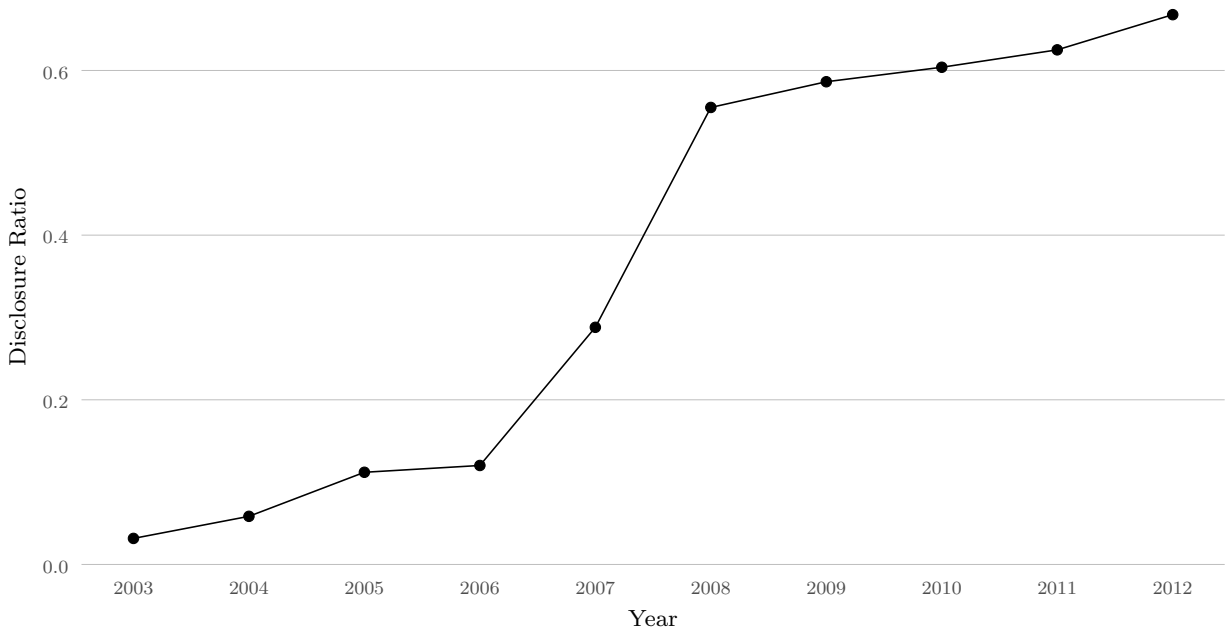
Panel B. Event-time Difference-in-difference Analysis

50
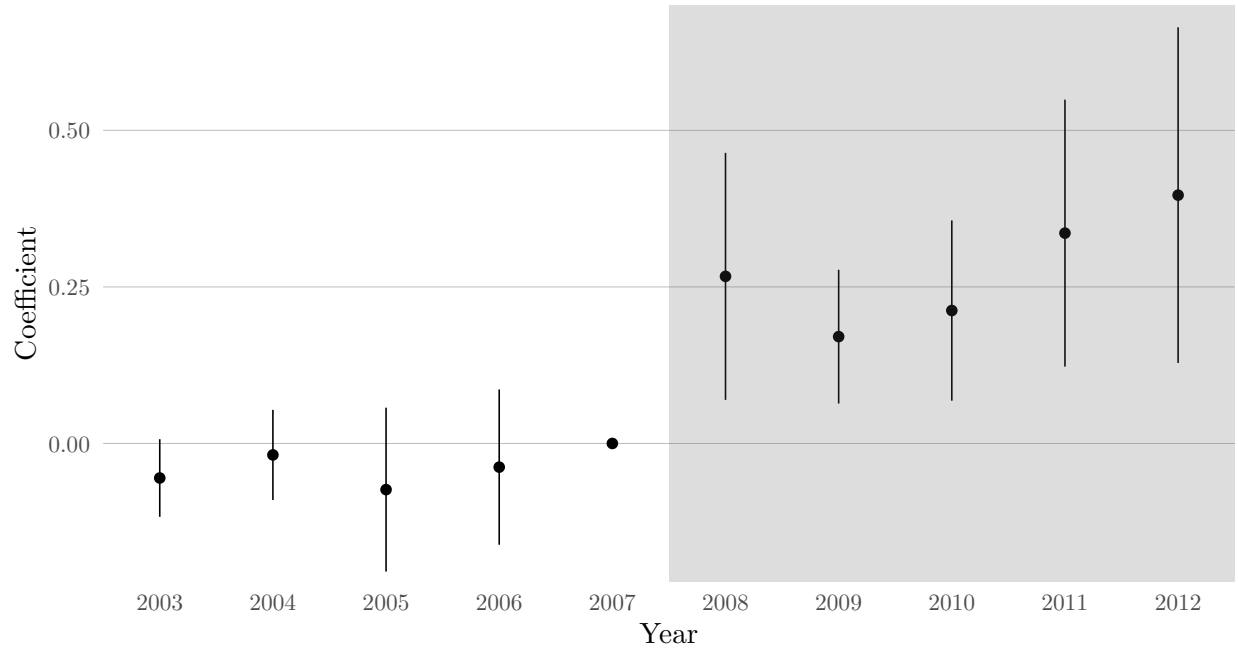
Table 1

Sample Characteristics

Panel A presents means, standard deviations, and quartiles of the sample variables. Panel B tabulates pairwise correlations of the key variables. Spearman correlations are above he diagonal and Pearson correlations are below.

Panel A. Descriptive Statistics

Variable	N	25%	Median	75%	Mean	Std. Dev.
$PublicPresence_{i,t}$	1634	0.237	0.556	0.980	0.844	1.271
$PublicPresence_{i,t}$ (industry demeaned)	1634	−0.086	−0.011	0.065	0.000	0.581
$ImportComp_{i,t}$	1364	0.058	0.125	0.234	0.424	1.118
$Tariff_{i,t}$	1615	0.363	1.137	2.534	2.013	2.593
$NTRGap_i$	1615	0.224	0.324	0.384	0.315	0.120
$ValueAdd_{i,t}$	1634	0.411	0.495	0.557	0.484	0.115
$Payroll_{i,t}$	1634	0.103	0.155	0.197	0.153	0.063
$WageRate_{i,t}$	1634	14.412	17.708	21.495	17.854	6.739
$Concentration_{i,t}$	1634	38.980	54.240	68.515	54.291	20.476
$IndustryGrowth_{i,t}$	1634	−0.035	0.016	0.063	0.011	0.109
$ImportsOHIC_{i,t}$	1615	0.005	0.010	0.023	0.019	0.023
$CapEx_{i,t}$	1190	0.019	0.025	0.033	0.028	0.015
$TotalInv_{i,t}$	1190	0.087	0.118	0.143	0.119	0.047
$SOXBHAR_{i,t}$	412	−0.079	−0.037	0.001	−0.043	0.059
$DiscShift_{i,t}$	1341	0.116	0.306	0.727	0.912	1.826
$GermanPublicPresence_{i,t}$	1957	0.000	0.000	0.007	0.538	3.297
$GermanImportComp_{i,t}$	1961	0.232	0.496	1.147	1.860	4.193
$UKDisclosure_{i,t}$	1916	0.309	0.926	2.520	5.778	29.004
$UKPublicPresence_{i,t}$	1916	0.000	0.000	0.038	0.708	3.465
$UKImportComp_{i,t}$	1938	0.288	0.798	2.033	2.619	5.917
$ICScore1_{i,t}$	1394	0.002	0.008	0.023	0.025	0.047
$ICScore2_{i,t}$	1394	0.001	0.005	0.017	0.019	0.039
$ICScore3_{i,t}$	1394	0.003	0.008	0.022	0.021	0.038
$ICScore4_{i,t}$	1394	0.002	0.005	0.016	0.015	0.033
$Downloads_{i,j,t}$	193,821	0	0	15	891	19,962

Panel B. Correlations

Variable		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$PublicPresence_{i,t}$	(1)		0.116	−0.087	0.098	−0.247	0.298	0.432	−0.001	0.488
$ImportComp_{i,t}$	(2)	0.644		0.236	0.145	0.341	−0.210	−0.047	−0.197	0.271
$Tariff_{i,t}$	(3)	0.256	0.635		0.009	0.145	−0.342	−0.086	−0.100	−0.137
$ValueAdd_{i,t}$	(4)	0.073	0.064	0.052		0.569	−0.061	−0.294	0.017	−0.065
$Payroll_{i,t}$	(5)	−0.039	0.217	0.210	0.505		−0.373	−0.591	−0.145	−0.182
$WageRate_{i,t}$	(6)	0.105	−0.184	−0.338	−0.102	−0.347		0.268	0.121	0.396
$Concentration_{i,t}$	(7)	0.286	0.058	−0.052	−0.264	−0.610	0.287		−0.006	0.063
$IndustryGrowth_{i,t}$	(8)	−0.077	−0.126	−0.155	−0.015	−0.162	0.123	0.030		0.047
$ImportsOHIC_{i,t}$	(9)	0.291	0.110	−0.086	−0.045	−0.200	0.402	0.077	0.025	

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Table 2

Determinants of Public Presence

Following are estimates of $PublicPresence_{i,t}$ regressed on industry characteristics. Each column includes year fixed effects, whereas columns (3) and (4) also include industry fixed effects. Columns (2) and (4) include the variables $CapEx_{i,t}$ and $TotalInv_{i,t}$ which are only available from 2003 onward; $NTRGap_i \times Post2001_t$ is omitted from these columns as, with the smaller sample, becomes co-linear with $NTRGap_i$. Standard errors are clustered by industry and year. *, **, *** indicate two-sided p-values less than 10%, 5%, and 1%, respectively.

	$PublicPresence_{i,t}$			
	(1)	(2)	(3)	(4)
$Tariff_{i,t}$	0.297	0.340*	0.070	0.252
	(0.194)	(0.199)	(0.135)	(0.197)
$NTRGap_i$	−0.188	−0.252		
	(0.153)	(0.226)		
$NTRGap_i \times Post2001_t$	−0.003		0.065	
	(0.073)		(0.064)	
$ImportsOHIC_{i,t}$	0.332***	0.336***	0.150	0.061
	(0.112)	(0.116)	(0.097)	(0.090)
$ValueAdd_{i,t}$	0.135**	0.159**	−0.004	−0.040
	(0.062)	(0.067)	(0.072)	(0.049)
$Payroll_{i,t}$	0.214	0.270	0.806***	0.693**
	(0.181)	(0.190)	(0.264)	(0.272)
$WageRate_{i,t}$	−0.011	0.024	0.084	0.256
	(0.153)	(0.187)	(0.188)	(0.310)
$Concentration_{i,t}$	0.396**	0.418**	−0.007	−0.507
	(0.159)	(0.167)	(0.286)	(0.322)
$IndustryGrowth_{i,t}$	−0.054	−0.057	0.058**	0.047*
	(0.055)	(0.062)	(0.026)	(0.028)
$CapEx_{i,t}$		−0.075*		0.020
		(0.043)		(0.028)
$TotalInv_{i,t}$		0.022		−0.169
		(0.082)		(0.144)
Fixed Effects:				
Industry (i)	No	No	Yes	Yes
Year (t)	Yes	Yes	Yes	Yes
Number of Clusters:				
Industry	85	85	85	85
Year	19	14	19	14
N	1615	1190	1615	1190
R^2 Full Model	0.294	0.312	0.848	0.906
R^2 Projected Model	0.288	0.309	0.190	0.238

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Table 3

Association Tests of Public Firm Presence and Foreign Competition

    This table presents regressions of various measures of import competition on lagged public presence. Panel A presents main results; Panel B splits the import competition into numerator (imports) and denominator (US production) components, and presents a related-party placebo test; Panel C presents a changes design. The sample extends from 2000 through 2016. All variables are defined in Appendix A. In Panels A and C, $PublicPresence_{i,t-1}$ is demeaned by industry and then standardized to unit variance. In Panel B, all variables are transformed using the inverse hyperbolic sine function. In Panel C, the dependent variables are all scaled to mean 0 and unit variance. Panel D, column (1) includes terciles of the change in public presence, lagged 1, 2, and 3 years. Column (2) includes the terciles as indicators (with the middle tercile as the base case). The regressions in panel D include lagged changes in the control variables, but coefficients are suppressed for brevity. Standard errors (in parentheses) are clustered by industry and year. *, **, *** indicate two-sided p-values less than 10%, 5%, and 1%, respectively.

Panel A. Main Results

| | $ImportComp_{i,t}$ | | |
	(1)	(2)	(3)
$PublicPresence_{i,t-1}$	0.230**	0.175**	0.242***
	(0.095)	(0.072)	(0.084)
$PublicPresence_{i,t-1}^2$			−0.020*
			(0.012)
$Tariff_{i,t-1}$		0.012	0.012
		(0.123)	(0.110)
$NTRGap_i \times Post2001_t$		0.589*	0.540*
		(0.320)	(0.322)
$ValueAdd_{i,t-1}$		1.281*	1.079
		(0.667)	(0.738)
$Payroll_{i,t-1}$		10.231**	9.757**
		(4.125)	(3.933)
$WageRate_{i,t-1}$		−0.065**	−0.058**
		(0.029)	(0.027)
$Concentration_{i,t-1}$		0.027**	0.025**
		(0.013)	(0.012)
$IndustryGrowth_{i,t-1}$		0.224	0.219
		(0.243)	(0.227)
$ImportsOHIC_{i,t}$		6.128	5.850
		(4.295)	(4.090)
Fixed Effects:			
Industry (i)	Yes	Yes	Yes
Year (t)	Yes	Yes	Yes
Number of Clusters:			
Industry	85	85	85
Year	17	17	17
N	1364	1364	1364
R^2 Full Model	0.903	0.924	0.929
R^2 Projected Model	0.254	0.417	0.450

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Panel B. Elasticities

	sinh⁻¹(ImportComp_{i,t})
	$\sinh^{-1}(ImportComp_{i,t})$ (1)
$\sinh^{-1}(PublicPresence_{i,t-1})$	0.197***
	(0.063)
$\sinh^{-1}(Tariff_{i,t-1})$	−0.030
	(0.042)
$\sinh^{-1}(NTRGap_i \times Post2001_t)$	0.573***
	(0.163)
$\sinh^{-1}(ValueAdd_{i,t-1})$	0.715***
	(0.228)
$\sinh^{-1}(Payroll_{i,t-1})$	3.712***
	(0.926)
$\sinh^{-1}(WageRate_{i,t-1})$	−0.268**
	(0.124)
$\sinh^{-1}(Concentration_{i,t-1})$	0.289
	(0.232)
$\sinh^{-1}(IndustryGrowth_{i,t-1})$	0.054
	(0.082)
$\sinh^{-1}(ImportsOHIC_{i,t})$	1.424
	(1.173)
Fixed Effects:	
Industry (i)	Yes
Year (t)	Yes
Number of Clusters:	
Industry	85
Year	17
	1364
R^2 Full Model	0.948
R^2 Projected Model	0.352

Panel C. Numerator and Denominator Effects

	$NonRPImports_{i,t}$ (1)	$USProduction_{i,t}$ (2)	$RPImports_{i,t}$ (3)
$PublicPresence_{i,t-1}$	0.092***	−0.082**	0.008
	(0.032)	(0.033)	(0.023)
$Tariff_{i,t-1}$	−0.012	−0.057**	−0.022
	(0.023)	(0.025)	(0.028)
$NTRGap_i \times Post2001_t$	0.386***	−0.829***	0.236**
	(0.139)	(0.201)	(0.095)
$ValueAdd_{i,t-1}$	0.157	−0.547	−0.044
	(0.302)	(0.424)	(0.274)
$Payroll_{i,t-1}$	−1.148	0.515	0.004
	(1.350)	(1.841)	(1.105)
$WageRate_{i,t-1}$	0.032	0.069	0.040
	(0.021)	(0.050)	(0.029)
$Concentration_{i,t-1}$	−0.0002	0.009	−0.002
	(0.007)	(0.007)	(0.006)
$IndustryGrowth_{i,t-1}$	0.097	0.293**	0.078
	(0.143)	(0.146)	(0.123)
$ImportsOHIC_{i,t}$	13.988***	1.587	11.711**
	(3.967)	(5.800)	(5.913)
Fixed Effects:			
Industry (i)	Yes	Yes	Yes
Year (t)	Yes	Yes	Yes
Number of Clusters:			
Industry	85	85	85
Year	17	17	17
N	1364	1364	1364
R^2 Full Model	0.949	0.895	0.960
R^2 Projected Model	0.330	0.155	0.262

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Panel D. Changes and Asymmetry

	$\Delta ImportComp_{i,t}$	
	(1)	(2)
Tercile($PublicPresence_{i,t-1} - PublicPresence_{i,t-2}$)	0.013*	
	(0.007)	
Tercile($PublicPresence_{i,t-2} - PublicPresence_{i,t-3}$)	0.017**	
	(0.008)	
Tercile($PublicPresence_{i,t-3} - PublicPresence_{i,t-4}$)	0.021**	
	(0.009)	
$\mathbb{I}$(Tercile($PublicPresence_{i,t-1} - PublicPresence_{i,t-2}$) = 1)		−0.007
		(0.010)
$\mathbb{I}$(Tercile($PublicPresence_{i,t-1} - PublicPresence_{i,t-2}$) = 3)		0.018
		(0.013)
$\mathbb{I}$(Tercile($PublicPresence_{i,t-2} - PublicPresence_{i,t-3}$) = 1)		−0.009
		(0.007)
$\mathbb{I}$(Tercile($PublicPresence_{i,t-2} - PublicPresence_{i,t-3}$) = 3)		0.020
		(0.014)
$\mathbb{I}$(Tercile($PublicPresence_{i,t-3} - PublicPresence_{i,t-4}$) = 1)		−0.015*
		(0.009)
$\mathbb{I}$(Tercile($PublicPresence_{i,t-3} - PublicPresence_{i,t-4}$) = 3)		0.023**
		(0.010)
Controls (Changes)	Yes	Yes
Fixed Effects:		
Year (t)	Yes	Yes
Number of Clusters:		
Industry	85	85
Year	16	16
N	1275	1275
R^2 Full Model	0.054	0.059
R^2 Projected Model	0.043	0.048

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Table 4

SOX Instrument for Public Firm Presence

Panel A tabulates first-stage regressions of an instrumental variable analysis, using the models:

$$PublicPresence_{i,t-1} = \theta_1(SOXBHAR_i \times Year2003_t) + \cdots +$$
$$\theta_6(SOXBHAR_i \times Year2008_t) + \rho'X_{i,t} + \lambda_i + \nu_t + \eta_{i,t}$$

$$PublicPresence_{i,t-1} = \theta^a(SOXBHAR_i \times YearGE2005_t) + \rho^{a'}X_{i,t} + \lambda_i^a + \nu_t^a + \eta_{i,t}^a$$

Panel B presents the second-stage regressions:

$$ImportComp_{i,t} = \alpha_1 \widehat{PublicPresence}_{i,t-1} + \beta'X_{i,t} + \gamma_i + \delta_t + \varepsilon_{i,t}$$

where i denotes 4-digit NAICS industry and t is year. The sample extends from 2000 through 2008. All variables are defined in Appendix A. All variables are standardized to mean 0 and unit variance. $PublicPresence_{i,t-1}$ is demeaned by industry. $\widehat{PublicPresence}_{i,t-1}$ are the fitted values from first stage models in Panel A, and the models correspond by column across panels. Standard errors (in parentheses) are clustered by industry and year. *, **, *** indicate two-sided p-values less than 10%, 5%, and 1%, respectively.

Panel A. First Stage: Public firm presence and SOX industry returns.

	$PublicPresence_{i,t-1}$	
	(1)	(2)
$SOXBHAR_i \times Year2003_t$	−0.022	
	(0.057)	
$SOXBHAR_i \times Year2004_t$	0.008	
	(0.058)	
$SOXBHAR_i \times Year2005_t$	0.184**	
	(0.081)	
$SOXBHAR_i \times Year2006_t$	0.209**	
	(0.093)	
$SOXBHAR_i \times Year2007_t$	0.375***	
	(0.125)	
$SOXBHAR_i \times Year2008_t$	0.297**	
	(0.137)	
$SOXBHAR_i \times YearGE2005_t$		0.265***
		(0.102)
$Tariff_{i,t-1}$	−0.949	−0.932
	(1.230)	(1.215)
$NTRGap_i \times Post2001_t$	0.612	0.620
	(0.799)	(0.783)
$ValueAdd_{i,t-1}$	0.514***	0.497**
	(0.195)	(0.195)
$Payroll_{i,t-1}$	1.069	1.053
	(0.756)	(0.742)
$WageRate_{i,t-1}$	−0.415	−0.413
	(0.261)	(0.256)
$Concentration_{i,t-1}$	1.473**	1.468**
	(0.744)	(0.740)
$IndustryGrowth_{i,t-1}$	−0.038	−0.031
	(0.048)	(0.048)
$ImportsOHIC_{i,t}$	2.225	2.770
	(7.892)	(7.638)
Fixed Effects:		
Industry (i)	Yes	Yes
Year (t)	Yes	Yes
N	412	412
R^2 Full Model	0.700	0.697
R^2 Projected Model	0.275	0.267

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Panel B. Second Stage: Foreign Competition and Instrumented Public Firm Presence

	$ImportComp_{i,t}$	
	(1)	(2)
$\widehat{PublicPresence}_{i,t-1}$	0.262**	0.228*
	(0.123)	(0.124)
$Tariff_{i,t-1}$	−0.658**	−0.693*
	(0.333)	(0.378)
$NTRGap_i \times Post2001_t$	−0.046	−0.044
	(0.042)	(0.045)
$ValueAdd_{i,t-1}$	0.118	0.132*
	(0.078)	(0.077)
$Payroll_{i,t-1}$	0.298	0.334
	(0.214)	(0.241)
$WageRate_{i,t-1}$	−0.058	−0.072
	(0.048)	(0.056)
$Concentration_{i,t-1}$	−0.086	−0.037
	(0.283)	(0.297)
$IndustryGrowth_{i,t}$	−0.019	−0.020
	(0.024)	(0.024)
$ImportsOHIC_{i,t}$	0.719	1.041
	(1.815)	(1.874)
Fixed Effects:		
Industry (i)	Yes	Yes
Year (t)	Yes	Yes
N	412	412
R^2 Full Model	0.955	0.953
R^2 Projected Model	0.573	0.558

Table 5

German Disclosure Enforcement Change

This table presents estimates of regressing $GermanImportComp_{i,t}$ on $Post2007_t \times DiscShift_i$, $ImportComp_{i,t}$ and $GermanPublicPresence_{i,t-1}$, and year and industry fixed effects where i denotes 4-digit NAICS industry and t is year. The sample extends from 2003 through 2012. $Post2007_t$ is an indicator if $t > 2007$. $DiscShift_i$ is the change disclosure ratio for industry i from 2006 to 2008. All other variables are defined in Appendix A. Standard errors (in parentheses) are clustered by industry and year. *, **, *** indicate two-sided p-values less than 10%, 5%, and 1%, respectively.

	$GermanImportComp_{i,t}$				
	(1)	(2)	(3)	(4)	(5)
$Post2007_t \times DiscShift_i$	0.315*	0.313**	0.332**	0.297*	0.313
	(0.168)	(0.145)	(0.155)	(0.177)	(0.267)
$ImportComp_{i,t}$		0.674***	0.902*	0.671**	0.917*
		(0.257)	(0.516)	(0.263)	(0.511)
$Post2007_t \times ImportComp_{i,t}$			−0.443		−0.484
			(0.531)		(0.487)
$GermanPublicPresence_{i,t-1}$				0.037	0.032
				(0.115)	(0.441)
$Post2007_t \times GermanPublicPresence_{i,t-1}$					0.061
					(0.450)
Fixed Effects:					
Industry (i)	Yes	Yes	Yes	Yes	Yes
Year (t)	Yes	Yes	Yes	Yes	Yes
Number of Clusters:					
Industry	84	84	84	84	84
Year	10	10	10	10	10
N	805	805	805	805	805
R^2 Full Model	0.864	0.890	0.891	0.890	0.891
R^2 Projected Model	0.039	0.222	0.229	0.222	0.231

Table 6

Information Content of Earnings Cross-sectional Analysis

This table presents presents estimates of the model:

$$ImportComp_{i,t} = \alpha_1 PublicPresence_{i,t-1} + \alpha_2 ICScoreX_{i-t-1} + \alpha_3(PublicPresence_{i,t-1} \times ICScoreX_{i-t-1})$$
$$+ \beta Controls_{i,j,t-1} + \gamma_i + \delta_t + \varepsilon_{i,t}$$

where i indexes 4-digit NAICS industry and t indexes year. γ_i is an industry fixed effect and δ_t is a year fixed effect. $PublicPresence_{i,t-1}$ is demeaned by industry and scaled to unit variance. In each column we include the control variables $Tariff_{i,t-1}$, $NTRGap_i \times Post2001_t$, $ValueAdd_{i,t-1}$, $Payroll_{i,t-1}$, $WageRate_{i,t-1}$, $Concentration_{i,t-1}$, $IndustryGrowth_{i,t-1}$, and $ImportsOHIC_{i,t}$, but do not tabulate their coefficients for brevity. $ICScore1_{i,t}$ is the R^2 value from an industry-year regression:

$$\frac{TradingVolume_{f,d}}{SharesOutstanding_{f,d}} = \beta_0^1 + \beta_1^1 AnyRelease_{f,d} + \beta_2^1 AnyPeerRelease_{f,d} + \eta_{f,d}^1$$

where f indexes firm, d indexes day, $TradingVolume_{f,d}$ is the number of firm f's shares traded on day d, $SharesOutstanding_{f,d}$ is the number of shares outstanding for firm f on day d, $AnyRelease_{f,d}$ is an indicator if d is a day that firm f releases its 10-K, 10-Q, or announces its earnings. $AnyPeerRelease_{f,d}$ is an indicator for the focal firm's industry peers' EDGAR 10-Q release dates, 10-K date, and earnings release dates from Compustat times each peer's sales weight in the industry in the prior year. $ICScore2_{i,t}$ is the R^2 value from an analogous industry-year regression replacing $EARelease_{f,d}$ and $EAPeerRelease_{f,d}$ for $AnyRelease_{f,d}$ and $AnyPeerRelease_{f,d}$ respectively, which only consider earnings announcement days. $ICScore3_{i,t}$ is the R^2 value from an industry-year regression:

$$|Return_{f,d}| = \beta_0^3 + \beta_1^3 AnyRelease_{f,d} + \beta_2^3 AnyPeerRelease_{f,d} + \eta_{f,d}^3$$

Where $|Return_{f,d}|$ is the absolute value of the stock return for firm f on day d. $ICScore4_{i,t}$ is calculated analogously to $ICScore3_{i,t}$ but replaces $EARelease_{f,d}$ and $EAPeerRelease_{f,d}$ for $AnyRelease_{f,d}$ and $AnyPeerRelease_{f,d}$ respectively. Standard errors are clustered by industry and year. *, **, *** indicate two-sided p-values less than 10%, 5%, and 1%, respectively.

Information Content Measure ($ICScoreX_{i,t-1}$):	$ImportComp_{i,t}$			
	$ICScore1_{i,t-1}$ (1)	$ICScore2_{i,t-1}$ (2)	$ICScore3_{i,t-1}$ (3)	$ICScore4_{i,t-1}$ (4)
$PublicPresence_{i,t-1}$	0.125 (0.086)	0.128 (0.086)	0.149* (0.089)	0.153* (0.088)
$ICScoreX_{i,t-1}$	0.527 (0.366)	0.519 (0.472)	0.071 (0.324)	0.013 (0.331)
$PublicPresence_{i,t-1} \times ICScoreX_{i,t-1}$	2.638** (1.041)	2.806** (1.140)	1.348* (0.703)	1.361* (0.739)
Additional Controls:	Yes	Yes	Yes	Yes
Fixed Effects:				
Industry(i)	Yes	Yes	Yes	Yes
Year(t)	Yes	Yes	Yes	Yes
Number of Clusters:				
Industry	80	80	80	80
Year	17	17	17	17
N	1186	1186	1186	1186
R^2 Full Model	0.939	0.938	0.933	0.933
R^2 Projected Model	0.505	0.501	0.457	0.456

Table 7

Forecasts

This table conducts cross-sectional tests on the relation between $PublicPresence_{i,t}$ and $ImportComp_{i,t}$. Panel A splits the Public Presence variable into two components: the presence of public firms issuing management guidance and the presence of public firms not issuing management guidance. Column (1) partitions based on the issuance of EPS guidance, Column (2) based on management's issuance of sales forecasts, column (3) on gross margin forecasts, and column (4) on capital expenditures forecasts. The difference between $GuidedPublicPresence_{i,t-1}$ and $UnguidedPublicPresence_{i,t-1}$ is tabluated below the coefficients, along with a test of statistical significance. Panel B and Panel C fully interact an analyst coverage variable (number of analysts in the industry issuing forecasts scaled by \$1 million in industry public firm sales) with $PublicPresence_{i,t}$. Panel B has four different definitions of $Analysts_{i,t}$ based on the type of forecast, and Panel B has five different definitions of $Analysts_{i,t}$ based on the horizon of the EPS forecast. Standard errors are clustered by industry and year. *, **, *** indicate two-sided p-values less than 10%, 5%, and 1%, respectively.

Panel A. Management Guidance

	$ImportComp_{i,t}$			
Guidance Type:	EPS (1)	Sales (2)	Gross Margin (3)	Cap Ex (4)
$GuidedPublicPresence_{i,t-1}$	0.327***	0.373***	0.473***	0.528***
	(0.122)	(0.122)	(0.153)	(0.175)
$UnguidedPublicPresence_{i,t-1}$	0.353***	0.288***	0.220**	0.199**
	(0.086)	(0.106)	(0.105)	(0.080)
$Guided - Unguided$	-0.027	0.085	0.253*	0.329**
χ^2 Test Statistic	0.163	0.764	2.999	4.593
p-value	0.686	0.382	0.083	0.032
Controls:	Yes	Yes	Yes	Yes
Fixed Effects:				
Industry (i)	Yes	Yes	Yes	Yes
Year (t)	Yes	Yes	Yes	Yes
Number of Clusters:				
Industry	85	85	85	85
Year	17	17	17	17
N	1354	1354	1354	1354
R^2 Full Model	0.923	0.931	0.929	0.932
R^2 Projected Model	0.393	0.455	0.440	0.463

Panel B. Analyst Forecast Type

	$ImportComp_{i,t}$			
Type:	EPS (1)	Sales (2)	Gross Margin (3)	Cap Ex (4)
$PublicPresence_{i,t-1}$	0.117	0.024	0.118	0.125
	(0.133)	(0.066)	(0.117)	(0.118)
$Analysts_{i,t-1}$	-0.006**	-0.006*	0.0003	0.001
	(0.003)	(0.003)	(0.007)	(0.012)
$PublicPresence_{i,t-1} \times Analysts_{i,t-1}$	0.113***	0.150***	0.119***	0.203***
	(0.023)	(0.030)	(0.038)	(0.050)
Controls	Yes	Yes	Yes	Yes
Fixed Effects:				
Industry (i)	Yes	Yes	Yes	Yes
Year (t)	Yes	Yes	Yes	Yes
Number of Clusters:				
Industry	85	85	85	85
Year	17	17	17	17
N	1354	1354	1354	1354
R^2 Full Model	0.941	0.947	0.939	0.938
R^2 Projected Model	0.535	0.586	0.519	0.513

Panel C. Analyst Forecast Horizon

	$ImportComp_{i,t}$				
Horizon (Years):	1	2	3	4	5
	(1)	(2)	(3)	(4)	(5)
$PublicPresence_{i,t-1}$	0.117	0.104	0.082	0.257**	0.277**
	(0.133)	(0.122)	(0.094)	(0.122)	(0.121)
$Analysts_{i,t-1}$	−0.006**	−0.007***	−0.019*	−0.102	−0.168*
	(0.003)	(0.003)	(0.011)	(0.063)	(0.086)
$PublicPresence_{i,t-1} \times Analysts_{i,t-1}$	0.113***	0.116***	0.237***	0.453***	0.526**
	(0.023)	(0.021)	(0.066)	(0.158)	(0.206)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects:					
Industry (i)	Yes	Yes	Yes	Yes	Yes
Year (t)	Yes	Yes	Yes	Yes	Yes
Number of Clusters:					
Industry	85	85	85	85	85
Year	17	17	17	17	17
N	1354	1354	1354	1354	1354
R^2 Full Model	0.941	0.942	0.939	0.932	0.931
R^2 Projected Model	0.535	0.541	0.520	0.464	0.460

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Table 8

UK Falsification Test

This table presents estimates of regressions in the form:

$$UKImportComp_{i,t} = \alpha_1 UKPublicPresence_{i,t-1} + \alpha_2 UKDisclosure_{i,t-1}$$

$$[+ \alpha_3 USImportComp_{i,t} + USPublicPresence_{i,t-1}] + \beta_i + \gamma_t + \varepsilon_{i,t}$$

where i denotes 4-digit NAICS industry and t denotes year. β_i are time-invariant industry fixed effects and γ_i are year fixed effects. All variables are defined in Appendix A. In Columns (1) and (2) each variable is demeaned by industry and then standardized to unit variance. In Columns (3) and (4), each variable is transformed using the inverse hyperbolic sine function. Standard errors (in parentheses) are clustered by industry and year. *, **, *** indicate two-sided p-values less than 10%, 5%, and 1%, respectively.

	$UKImportComp_{i,t}$		$\sinh^{-1}(UKImportComp_{i,t})$	
	(1)	(2)	(3)	(4)
$UKPublicPresence_{i,t-1}$	0.014	0.023		
	(0.087)	(0.079)		
$UKDisclosure_{i,t-1}$	0.053	0.045		
	(0.068)	(0.069)		
$USImportComp_{i,t}$		0.253***		
		(0.092)		
$USPublicPresence_{i,t-1}$		0.067		
		(0.062)		
$\sinh^{-1}(UKPublicPresence_{i,t-1})$			0.023	0.030
			(0.090)	(0.081)
$\sinh^{-1}(UKDisclosure_{i,t-1})$			0.095	0.096
			(0.080)	(0.077)
$\sinh^{-1}(USImportComp_{i,t})$				0.692***
				(0.208)
$\sinh^{-1}(USPublicPresence_{i,t-1})$				0.051
				(0.101)
Observation Level:	i,t	i,t	i,t	i,t
Fixed Effects:				
Industry(i)	Yes	Yes	Yes	Yes
Year(t)	Yes	Yes	Yes	Yes
Number of Clusters:				
Industry	86	85	86	85
Year	16	16	16	16
N	1340	1262	1340	1262
R^2 Full Model	0.093	0.227	0.850	0.871
R^2 Projected Model	0.004	0.106	0.022	0.093