

Public Firm Presence, Financial Reporting, and the Decline of U.S. Manufacturing

STEPHEN GLAESER* AND JAMES D. OMARTIAN†

Received 2 December 2019; accepted 11 August 2021

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,

*Kenan Flagler Business School, University of North Carolina at Chapel Hill; †Ross School of Business, University of Michigan.

Accepted by Rodrigo Verdi. We additionally thank an anonymous referee, Chris Armstrong, Phil Berger (discussant), Matthias Breuer, Bob Holthausen, Raffi Indjejikian, Junyoung Jeong, Zach Kaplan, Eva Labro, Mark Lang, Greg Miller, Gans Narayanamoorthy (discussant), Marcel Olbert, Nemit Shroff, DJ Stockbridge, and Gwen Yu for helpful comments and suggestions. We also thank participants at the 30th Annual Conference on Financial Economics and Accounting, the 32nd Accounting Research Conference in Memory of Nicholas Dopuch, and the Columbia Junior Accounting Faculty Conference, along with workshop participants at the Massachusetts Institute of Technology, the University of Michigan, and the University of Rochester for useful feedback. An online appendix to this paper can be downloaded at <http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements>.

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.

JEL codes: F14, F16, G18, G38, L60, M41

Keywords: competition; trade; private firms; public firms; financial reporting; proprietary costs; disclosure externalities

1. Introduction

We examine whether the information generated by the presence of publicly traded manufacturing firms facilitates foreign import competition in the United States. 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 U.S. market and discourage foreign imports. Publicly traded firms are one information source that may ameliorate these frictions.

The U.S. Securities and Exchange Commission (SEC) requires firms publicly traded in U.S. 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, Shroff, and White [2013], Kim [2019], Bernard, Blackburne, and Thornock [2020]). For example, foreign importers can draw on information about production schedules, investments, profitability, accruals, sales, and risk factors to understand U.S. market demand and consumer preferences, as well as the U.S. competitive landscape. This information can thus reduce foreign firms' uncertainty about the U.S. 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 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, Shroff, and White [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 because of 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 U.S. 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 of nonrelated 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, one-third 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

¹For example, 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].

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, Shroff, and White [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 interindustry differences in the expected costs of SOX as instruments for differences in public firm 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, Hayes, and Wang [2007], Leuz, Triantis, and Wang [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, Glaeser, and Huang [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

Although 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, Kling, and Mullainathan [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, Leuz, and Vanhaverbeke [2019] and Breuer [2021]). Although nominally required to disclose publicly, most German limited liability firms

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.

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.

did not do so until sweeping enforcement reforms increased compliance.⁴ Using the increase in financial reporting because of this plausibly exogenous increase in enforcement in a difference-in-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, Shroff, and White [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 United Kingdom, 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, and falsification test provides 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

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

Beyer et al. [2010] for a review). However, evidence consistent with this assumption is scarce. As Roychowdhury, Shroff, and Verdi [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 Badertscher, Shroff, and White [2013]. Badertscher, Shroff, and White [2013] show that increased public firm presence improves the investment efficiency of private domestic firms.

We build on Badertscher, Shroff, and White [2013] by documenting how public firm presence shifts industry production toward 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 because of potentially greater information processing costs (Blankespoor, deHaan, and Marinovic [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 policy makers seek to differentially influence foreign and domestic competition.⁹

⁵ For example, Bernard [2016], Berger et al. [2019], Christensen et al. [2020], Hann et al. [2020], and Breuer [2021].

⁶ For example, Badertscher et al. [2013], Shroff et al. [2017], Bernard et al. [2020], Kim and Olbert [2021], and Sani [2021].

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 U.S. segment disclosures increase import competition in the United States. Another concurrent working paper, Zhou [2021], finds evidence that improved financial reporting quality increases exporting from and importing into a country.

⁹ Consistent with policy makers seeking to differentially influence the two, government-set trade tariffs discourage foreign import competition whereas blocking anticompetitive mergers

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 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 [2014] 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. Alborno et al. [2012] use 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 explain 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 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

encourages domestic competition (U.S. Department of Justice and Federal Trade Commission, 2010).

¹⁰ See, for example, Rauch and Casella [2003], Rauch and Trindade (2003), Alborno 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.

United States granting Permanent Normal Trade Relations (PNTR) status to China, which reduced policy uncertainty about tariff rates, increased trade from China to the United States. 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 U.S. 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 U.S. firms. Public financial reports also contain a tremendous amount of nonfinancial information, including the existence of trade secrets (Glaeser [2018]), discussions about the material risks firms face (Heinle and Smith [2017]), material contracts (Costello [2013]), 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, because of the accruals system or by SEC mandate, or implicitly, because of 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, Guay, and Weber [2010] and Dechow, Ge, and Schrand [2010] for reviews of the literature on investor demand for information).¹²

¹¹ <https://www.sec.gov/Article/whatwedo.html>.

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

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 U.S. public firms is for the intended benefit of investors, competitors may also use it (Roychowdhury, Shroff, and Verdi [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 financially constrained German firms forced to disclose public financial information dispose of fixed assets and lose market share. Berger, Choi, and Tomar [2019] find that profitability dispersion is greater in Korean industries in which more firms do not disclose costs of goods information. Hann et al. [2020] find that profitability dispersion is greater in U.S. manufacturing industries in which financial reporting provides less information about the productivity of assets. Christensen, Liu, and Maffett [2020] find that increased public oversight of U.K. firms' financial reporting results in equity market price 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, Shroff, and White [2013], Shroff, Verdi, and Yost [2017]).¹⁵ Further, previous research also finds that investment opportunities cause firms to acquire accounting information about their rivals (Bernard, Blackburne, and Thornock [2020]). In total,

presence on average encourages import competition, even if some specific disclosures can discourage import competition.

¹³ For example, 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.

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.

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 understand market opportunities, U.S. firms' competitive positions and plans, and what has and has not worked for U.S. firms (e.g., financial reports may reveal why poorly performing U.S. firms did not succeed, helping foreign competitors enter the market despite U.S. firms' poor performance). Consequently, on the margin, foreign competitors should be more willing and able to compete in the markets in which U.S. 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 U.S. manufacturing setting. Although 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 products can be easily produced in one market and sold in another. The U.S. 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

TABLE 1
Sample Characteristics

Panel A. Descriptive statistics						
Variable	<i>N</i>	25%	Median	75%	Mean	Std. Dev.
<i>PublicPresence_{i,t}</i>	1634	0.237	0.556	0.980	0.844	1.271
<i>PublicPresence_{i,t}</i> (industry demeaned)	1634	-0.086	-0.011	0.065	0.000	0.581
<i>ImportComp_{i,t}</i>	1364	0.058	0.125	0.234	0.424	1.118
<i>Tariff_{i,t}</i>	1615	0.363	1.137	2.534	2.013	2.593
<i>NTRGap_i</i>	1615	0.224	0.324	0.384	0.315	0.120
<i>ValueAdd_{i,t}</i>	1634	0.411	0.495	0.557	0.484	0.115
<i>Payroll_{i,t}</i>	1634	0.103	0.155	0.197	0.153	0.063
<i>WageRate_{i,t}</i>	1634	14.412	17.708	21.495	17.854	6.739
<i>Concentration_{i,t}</i>	1634	38.980	54.240	68.515	54.291	20.476
<i>IndustryGrowth_{i,t}</i>	1634	-0.035	0.016	0.063	0.011	0.109
<i>ImportsOHIC_{i,t}</i>	1615	0.005	0.010	0.023	0.019	0.023
<i>CapEx_{i,t}</i>	1190	0.019	0.025	0.033	0.028	0.015
<i>TotalInv_{i,t}</i>	1190	0.087	0.118	0.143	0.119	0.047
<i>SOXBHAR_{i,t}</i>	412	-0.079	-0.037	0.001	-0.043	0.059
<i>DiscShif_t</i>	1341	0.116	0.306	0.727	0.912	1.826
<i>GermanPublicPresence_{i,t}</i>	1957	0.000	0.000	0.007	0.538	3.297
<i>GermanImportComp_{i,t}</i>	1961	0.232	0.496	1.147	1.860	4.193
<i>UKDisclosure_{i,t}</i>	1916	0.309	0.926	2.520	5.778	29.004
<i>UKPublicPresence_{i,t}</i>	1916	0.000	0.000	0.038	0.708	3.465
<i>UKImportComp_{i,t}</i>	1938	0.288	0.798	2.033	2.619	5.917
<i>ICScore1_{i,t}</i>	1394	0.002	0.008	0.023	0.025	0.047
<i>ICScore2_{i,t}</i>	1394	0.001	0.005	0.017	0.019	0.039
<i>ICScore3_{i,t}</i>	1394	0.003	0.008	0.022	0.021	0.038
<i>ICScore4_{i,t}</i>	1394	0.002	0.005	0.016	0.015	0.033
<i>Downloads_{i,j,t}</i>	193821	0	0	15	891	19962

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

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 the diagonal and Pearson correlations are below.

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 data sets, 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 because of 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:

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

where i indexes four-digit NAICS industries and t indexes calendar years. We follow Badertscher, Shroff, and White [2013] and measure public firm presence, or *PublicPresence*, as sales by U.S. firms in industry i as reported in the Compustat database, scaled by total U.S. production as reported by the Census Bureau. For multisegment 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 U.S. Census Bureau's Annual Survey of Manufactures and Census of Manufacturers (ASM/CMF) to measure U.S. production at the four-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 noncensus 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

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.

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 U.S. production from public firms. We adjust for geographic, instead of industry, segments to exclude non-U.S. 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>.

sampling error of the 2016 survey at the granular NAICS 6 industry code level was 2.08%, 2.85%, and 0%.¹⁹

Equation (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., 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 $NTRGap \times Post2001$, measured as in Pierce and Schott [2016].²⁰ Higher values of $NTRGap \times Post2001$ represent greater uncertainty resolution about trade policy after Congress granted China PNTR 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, Dorn, and Hanson [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., because of 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).

We control for the size and market control of major industry firms using *Concentration*, defined as the percentage of U.S. production by the top 20 in-

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 nonmanufacturing industries cannot rely on the ASM and hence may have to interpolate values from census years to noncensus 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.

²⁰ Specifically, we measure $NTRGap \times Post2001$ 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 PNTR 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]).

dustry 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., because of entry deterrence; Dixit [1980]). These measures are only available from 2003 onwards. We obtain *IndustryGrowth*, *ValueAdd*, *Payroll*, *WageRate*, *Concentration*, *CapEx*, and *TotalInv* from the U.S. Census Bureau ASM/CMF data.

In table 2, we present the results of estimating equation (1). Columns 1 and 2 show the results without industry fixed effects, whereas columns 3 and 4 include industry fixed effects. In columns 2 and 4, we include *CapEx* and *TotalInv*, which shortens the sample window because of 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 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:

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

where X includes the determinants of public firm presence from equation (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 U.S. production in

TABLE 2
Determinants of Public Presence

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

These 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 × 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.

industry *i* in year *t*, and it captures the competitive pressure foreign firms exert on U.S. manufacturers.²² We use U.S. Census Bureau import data

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

from Peter Schott's Web site 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 (four-digit) NAICS industry-year level. We use the U.S. Census Bureau's related party trade reports to remove related party trade 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 equation (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 reestimate equation (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*, in equation (2). We also re-estimate equation (2) after taking the inverse hyperbolic sine of all variables (e.g., *ImportComp* and *PublicPresence* become $\sinh^{-1}(\text{ImportComp})$ and $\sinh^{-1}(\text{PublicPresence})$).²⁴ The inverse hyperbolic sine transformation leads to similar interpretations, and similarly reduces the influence of outliers, as the natural logarithm transformation (Burbidge, Magee, and Robb [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, Magee, and Robb [1988]).

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

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

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.

TABLE 3
Association Tests of Public Firm Presence and Foreign Competition

Panel A. Main results			
	<i>ImportComp_{i,t}</i>		
	(1)	(2)	(3)
<i>PublicPresence</i> _{<i>i,t-1</i>}	0.230** (0.095)	0.175** (0.072)	0.242*** (0.084)
<i>PublicPresence</i> ² _{<i>i,t-1</i>}			-0.020* (0.012)
<i>Tariff</i> _{<i>i,t-1</i>}		0.012 (0.123)	0.012 (0.110)
<i>NTRGap_i</i> × <i>Post2001_t</i>		0.589* (0.320)	0.540* (0.322)
<i>ValueAdd</i> _{<i>i,t-1</i>}		1.281* (0.667)	1.079 (0.738)
<i>Payroll</i> _{<i>i,t-1</i>}		10.231** (4.125)	9.757** (3.933)
<i>WageRate</i> _{<i>i,t-1</i>}		-0.065** (0.029)	-0.058** (0.027)
<i>Concentration</i> _{<i>i,t-1</i>}		0.027** (0.013)	0.025** (0.012)
<i>IndustryGrowth</i> _{<i>i,t-1</i>}		0.224 (0.243)	0.219 (0.227)
<i>ImportsOHC</i> _{<i>i,t</i>}		6.128 (4.295)	5.850 (4.090)
<i>Fixed Effects:</i>			
Industry (<i>i</i>)	Yes	Yes	Yes
Year (<i>t</i>)	Yes	Yes	Yes
<i>Number of Clusters:</i>			
Industry	85	85	85
Year	17	17	17
<i>N</i>	1364	1364	1364
<i>R</i> ² Full Model	0.903	0.924	0.929
<i>R</i> ² Projected Model	0.254	0.417	0.450
Panel B. Elasticities			
	$\sinh^{-1}(\text{ImportComp}_{i,t})$ (1)		
$\sinh^{-1}(\text{PublicPresence}_{i,t-1})$			0.197*** (0.063)
$\sinh^{-1}(\text{Tariff}_{i,t-1})$			-0.030 (0.042)
$\sinh^{-1}(\text{NTRGap}_i \times \text{Post2001}_t)$			0.573*** (0.163)
$\sinh^{-1}(\text{ValueAdd}_{i,t-1})$			0.715*** (0.228)
$\sinh^{-1}(\text{Payroll}_{i,t-1})$			3.712*** (0.926)
$\sinh^{-1}(\text{WageRate}_{i,t-1})$			-0.268** (0.124)

(Continued)

TABLE 3—(Continued)

Panel B. Elasticities			
	$\sinh^{-1}(\text{ImportComp}_{i,t})$		
	(1)		
$\sinh^{-1}(\text{Concentration}_{i,t-1})$	0.289 (0.232)		
$\sinh^{-1}(\text{IndustryGrowth}_{i,t-1})$	0.054 (0.082)		
$\sinh^{-1}(\text{ImportsOHIC}_{i,t})$	1.424 (1.173)		
<i>Fixed Effects:</i>			
Industry (<i>i</i>)	Yes		
Year (<i>t</i>)	Yes		
<i>Number of Clusters:</i>			
Industry	85		
Year	17		
	1364		
R^2 Full Model	0.948		
R^2 Projected Model	0.352		
Panel C. Numerator and denominator effects			
	<i>NonRPIImports_{i,t}</i>	<i>USProduction_{i,t}</i>	<i>RPIImports_{i,t}</i>
	(1)	(2)	(3)
<i>PublicPresence_{i,t-1}</i>	0.092*** (0.032)	-0.082** (0.033)	0.008 (0.023)
<i>Tariff_{i,t-1}</i>	-0.012 (0.023)	-0.057** (0.025)	-0.022 (0.028)
<i>NTRGap_i × Post2001_i</i>	0.386*** (0.139)	-0.829*** (0.201)	0.236** (0.095)
<i>ValueAdd_{i,t-1}</i>	0.157 (0.302)	-0.547 (0.424)	-0.044 (0.274)
<i>Payroll_{i,t-1}</i>	-1.148 (1.350)	0.515 (1.841)	0.004 (1.105)
<i>WageRate_{i,t-1}</i>	0.032 (0.021)	0.069 (0.050)	0.040 (0.029)
<i>Concentration_{i,t-1}</i>	-0.0002 (0.007)	0.009 (0.007)	-0.002 (0.006)
<i>IndustryGrowth_{i,t-1}</i>	0.097 (0.143)	0.293** (0.146)	0.078 (0.123)
<i>ImportsOHIC_{i,t}</i>	13.988*** (3.967)	1.587 (5.800)	11.711** (5.913)
<i>Fixed Effects:</i>			
Industry (<i>i</i>)	Yes	Yes	Yes
Year (<i>t</i>)	Yes	Yes	Yes
<i>Number of Clusters:</i>			
Industry	85	85	85
Year	17	17	17
<i>N</i>	1364	1364	1364
R^2 Full Model	0.949	0.895	0.960
R^2 Projected Model	0.330	0.155	0.262

(Continued)

TABLE 3—(Continued)

Panel D. Changes and asymmetry	$\Delta \text{ImportComp}_{i,t}$	
	(1)	(2)
Tercile ($\text{PublicPresence}_{i,t-1} - \text{PublicPresence}_{i,t-2}$)	0.013* (0.007)	
Tercile ($\text{PublicPresence}_{i,t-2} - \text{PublicPresence}_{i,t-3}$)	0.017** (0.008)	
Tercile ($\text{PublicPresence}_{i,t-3} - \text{PublicPresence}_{i,t-4}$)	0.021** (0.009)	
I(Tercile ($\text{PublicPresence}_{i,t-1} - \text{PublicPresence}_{i,t-2}$) = 1)		-0.007 (0.010)
I(Tercile ($\text{PublicPresence}_{i,t-1} - \text{PublicPresence}_{i,t-2}$) = 3)		0.018 (0.013)
I(Tercile ($\text{PublicPresence}_{i,t-2} - \text{PublicPresence}_{i,t-3}$) = 1)		-0.009 (0.007)
I(Tercile ($\text{PublicPresence}_{i,t-2} - \text{PublicPresence}_{i,t-3}$) = 3)		0.020 (0.014)
I(Tercile ($\text{PublicPresence}_{i,t-3} - \text{PublicPresence}_{i,t-4}$) = 1)		-0.015* (0.009)
I(Tercile ($\text{PublicPresence}_{i,t-3} - \text{PublicPresence}_{i,t-4}$) = 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

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 (U.S. 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 table A1. In panels A and C, $\text{PublicPresence}_{i,t-1}$ 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.

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

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

Column 3 presents the results of estimating equation (1) after including *PublicPresence*². We find some evidence that the relation between *PublicPresence* and *ImportComp* is weakly concave (*PublicPresence*² *t*-statistic of 1.67).²⁶ Finally, panel B presents the results of estimating equation (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, one-third 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.

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., *NonRPIImports*), decreases in the denominator of *ImportComp* (i.e., *USProduction*), or both. Specifically, we separately reestimate equation (2) after replacing *ImportComp* with *NonRPIImports* 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 *RPIImports*, 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 because of public firm presence reducing information frictions, then related party imports should relate less to public firm presence than should nonrelated party imports. Consistent with public firm presence reducing information frictions driving our results, the coefficient estimate on *PublicPresence* when using *RPIImports* as the dependent

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 because of 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 because of U.S. granting PNTR 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.

variable in column 3 is one-tenth of the magnitude of the coefficient when using *NonRPIImports* 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 reestimate 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 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 D, 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 whereas 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, four of the six 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., the coefficient on increases at $t - 1$ is 157% greater in magnitude than the coefficient on decreases, whereas 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 equation (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, Shroff, and White [2013] and use the introduction of SOX as a natural experiment to obtain plausibly exogenous variation in public firm presence.

The U.S. 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 8, 2002, to July 20, 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 four-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,

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

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.

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, Glaeser, and Huang [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:

$$\begin{aligned} PublicPresence_{i,t-1} = & \theta_1 (SOXBHAR_i \times Year2003_t) + \dots + \\ & \theta_6 (SOXBHAR_i \times Year2008_t) \\ & + \rho' X_{i,t} + \lambda_i + \nu_t + \eta_{i,t}; \end{aligned} \quad (3a)$$

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

The first-stage equation, equation (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 although $PublicPresence$ may quickly reflect the effect of firms deregistering after SOX enactment, the effect of firms forgoing public listing may take more time to accumulate.

Equation (3) includes industry fixed effects (λ_i), which isolate variation in $PublicPresence$ across industries time and absorb the main effect of $SOXBHAR_i$. Equation (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 equation (1) that might affect $PublicPresence$. The second-stage equation, equation (3b), 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

³⁰ Although 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), Iliiev (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.

³¹ Subscripts are consistent for observations across equations (3a) and (3b)—that is, 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.

causal estimates. We examine the relevance condition by estimating equation (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 $PublicPresence$, 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, Triantis, and Wang [2008]) or going private (Engel, Hayes, and Wang [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 equation (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 nonmanufacturing 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 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 8, 2002 and July 20, 2002 is unlikely.³² We also estimate parallel trends falsification tests to examine this assumption.

³²It is possible that high costs because of SOX may put U.S. firms in that industry at a competitive disadvantage relative to foreign firms. If so, it would work against finding our results.

TABLE 4
SOX Instrument for Public Firm Presence

	<i>PublicPresence</i> _{<i>i,t-1</i>}	
	(1)	(2)
<i>SOXBHAR</i> _{<i>i</i>} × <i>Year2003</i> _{<i>i</i>}	-0.022 (0.057)	
<i>SOXBHAR</i> _{<i>i</i>} × <i>Year2004</i> _{<i>i</i>}	0.008 (0.058)	
<i>SOXBHAR</i> _{<i>i</i>} × <i>Year2005</i> _{<i>i</i>}	0.184** (0.081)	
<i>SOXBHAR</i> _{<i>i</i>} × <i>Year2006</i> _{<i>i</i>}	0.209** (0.093)	
<i>SOXBHAR</i> _{<i>i</i>} × <i>Year2007</i> _{<i>i</i>}	0.375*** (0.125)	
<i>SOXBHAR</i> _{<i>i</i>} × <i>Year2008</i> _{<i>i</i>}	0.297** (0.137)	
<i>SOXBHAR</i> _{<i>i</i>} × <i>YearGE2005</i> _{<i>i</i>}		0.265*** (0.102)
<i>Tariff</i> _{<i>i,t-1</i>}	-0.949 (1.230)	-0.932 (1.215)
<i>NTRGap</i> _{<i>i</i>} × <i>Post2001</i> _{<i>i</i>}	0.612 (0.799)	0.620 (0.783)
<i>ValueAdd</i> _{<i>i,t-1</i>}	0.514*** (0.195)	0.497** (0.195)
<i>Payroll</i> _{<i>i,t-1</i>}	1.069 (0.756)	1.053 (0.742)
<i>WageRate</i> _{<i>i,t-1</i>}	-0.415 (0.261)	-0.413 (0.256)
<i>Concentration</i> _{<i>i,t-1</i>}	1.473** (0.744)	1.468** (0.740)
<i>IndustryGrowth</i> _{<i>i,t-1</i>}	-0.038 (0.048)	-0.031 (0.048)
<i>ImportsOHIC</i> _{<i>i,t</i>}	2.225 (7.892)	2.770 (7.638)
<i>Fixed Effects:</i>		
Industry (<i>i</i>)	Yes	Yes
Year (<i>t</i>)	Yes	Yes
<i>N</i>	412	412
<i>R</i> ² Full Model	0.700	0.697
<i>R</i> ² Projected Model	0.275	0.267

	<i>ImportComp</i> _{<i>i,t</i>}	
	(1)	(2)
$\widehat{PublicPresence}_{i,t-1}$	0.262** (0.123)	0.228* (0.124)
<i>Tariff</i> _{<i>i,t-1</i>}	-0.658** (0.333)	-0.693* (0.378)

(Continued)

TABLE 4—(Continued)

Panel B. Second stage: Foreign competition and instrumented public firm presence

	<i>ImportComp_{i,t}</i>	
	(1)	(2)
<i>NTRGap_t</i> × <i>Post2001_t</i>	−0.046 (0.042)	−0.044 (0.045)
<i>ValueAdd_{i,t−1}</i>	0.118 (0.078)	0.132* (0.077)
<i>Payroll_{i,t−1}</i>	0.298 (0.214)	0.334 (0.241)
<i>WageRate_{i,t−1}</i>	−0.058 (0.048)	−0.072 (0.056)
<i>Concentration_{i,t−1}</i>	−0.086 (0.283)	−0.037 (0.297)
<i>IndustryGrowth_{i,t}</i>	−0.019 (0.024)	−0.020 (0.024)
<i>ImportsOHIC_{i,t}</i>	0.719 (1.815)	1.041 (1.874)
<i>Fixed Effects:</i>		
Industry (<i>i</i>)	Yes	Yes
Year (<i>t</i>)	Yes	Yes
<i>N</i>	412	412
<i>R</i> ² Full Model	0.955	0.953
<i>R</i> ² Projected Model	0.573	0.558

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

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

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

Panel B presents the second-stage regressions:

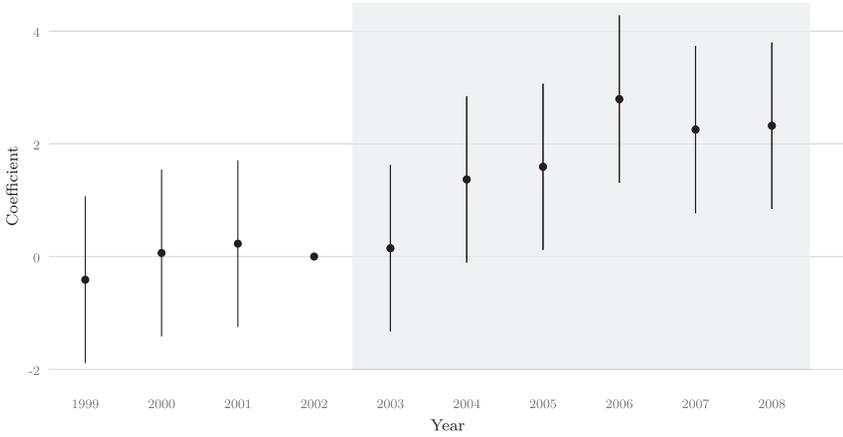
$$ImportComp_{i,t} = \alpha_1 \widehat{PublicPresence}_{i,t-1} + \beta' X_{i,t} + \gamma_i + \delta_t + \varepsilon_{i,t},$$

where *i* denotes four-digit NAICS industry and *t* is year. The sample extends from 2000 through 2008. All variables are defined in table A1. 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.

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), in Figure 1. We find no evidence that

³³ Similarly, we do not remove related party trade from this test because that data begins in 2000, limiting our ability to detect pretreatment 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).

Panel A. Changes in Public Presence from SOX Compliance Costs



Panel B. Changes in Import Competition from SOX Compliance Costs

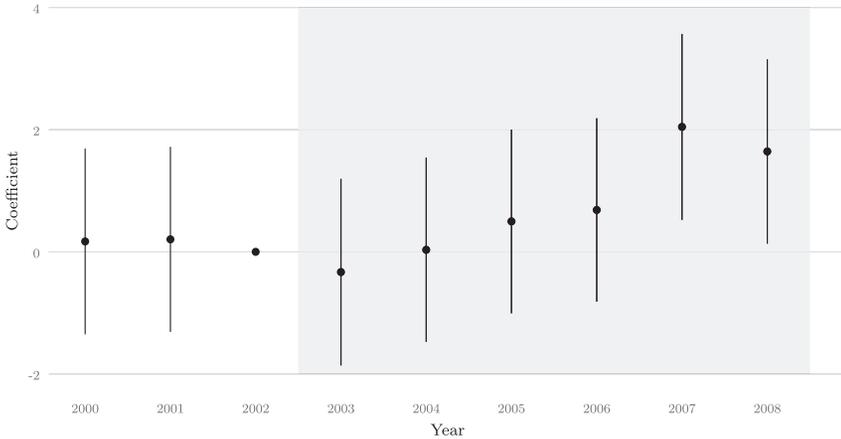


FIG 1.—SOX difference-in-difference trend analysis.

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: $PublicPresence_{i,t} = \alpha_1 Tariff_{i,t} + \alpha_2 (NTRGap_i \times Post2001_t) + \alpha_3 ValueAdd_{i,t} + \alpha_4 Payroll_{i,t} + \alpha_5 WageRate_{i,t} + \alpha_6 Concentration_{i,t} + \alpha_7 IndustryGrowth_{i,t-1} + \beta_1 (SOXBHAR_i \times Year1999_t) + \dots + \beta_3 (SOXBHAR_i \times Year2001_t) + \beta_4 (SOXBHAR_i \times Year2003_t) + \dots + \beta_5 (SOXBHAR_i \times Year2008_t) + \gamma_i + \delta_i + \varepsilon_{it}$. In Panel B we estimate: $ImportComp_{i,t} = \alpha_1 Tariff_{i,t-1} + \alpha_2 (NTRGap_i \times Post2001_t) + \alpha_3 ValueAdd_{i,t-1} + \alpha_4 Payroll_{i,t-1} + \alpha_5 WageRate_{i,t-1} + \alpha_6 Concentration_{i,t-1} + \alpha_7 IndustryGrowth_{i,t-1} + \beta_1 (SOXBHAR_i \times Year2000_t) + \beta_2 (SOXBHAR_i \times Year2001_t) + \beta_3 (SOXBHAR_i \times Year2003_t) + \dots + \beta_8 (SOXBHAR_i \times Year2008_t) + \gamma_i + \delta_i + \varepsilon_{it}$, i indexes industry and t indexes year. γ_i is a time-invariant fixed effect for each industry and δ_i is a year fixed effect. $YearXXXX_t$ is an indicator if $t = XXXX$. All other variables are defined in table A1. The reference period in both panels is the year 2002. The sample period is from 1999 to 2008 for panel A and 2000 to 2008 for panel B (1999 lost because of lagged variables). Standard errors are clustered by industry and year.

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 although they investigate the relation between *PublicPresence* and *ImportComp*, they do not isolate the mechanism(s) through which this relation arises. Consequently, we build on Bernard [2016] and examine a natural mechanism experiment around major financial reporting enforcement reforms in Germany (Ludwig, Kling, and Mullainathan [2011]). Since 1987, German limited liability firms are subject to EU 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 noncompliance, which caused most firms to ignore them (Henselmann and Kaya [2009], Bernard [2016]).

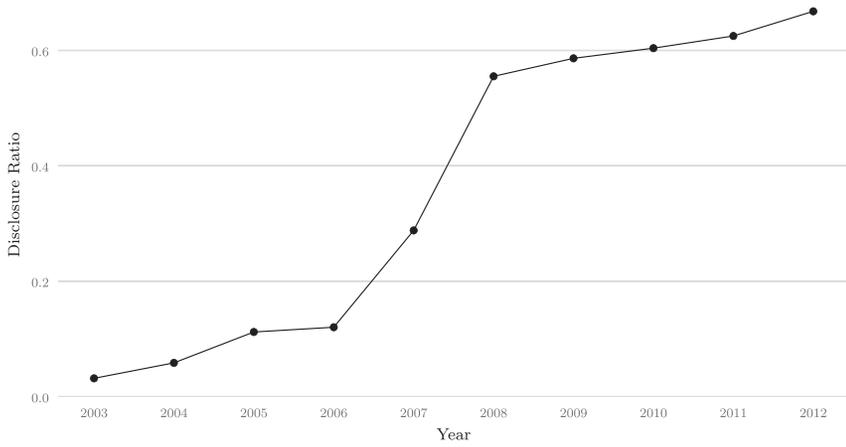
The low rate of compliance changed in November 2006 when a series of court cases and increasing pressure from the European Commission caused the German government to enact the Bill on the Electronic Registers for Commerce, Companies and Associations (Electronic Commercial and Company Registrar, Gesetz über elektronische Handelsregister und Genossenschaftsregister sowie das Unternehmensregister).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 noncompliance. Unsurprisingly, financial reporting compliance rapidly increased (Bernard [2016], Breuer, Leuz, and Vanhaverbeke [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:

$$GermanImportComp_{i,t} = \alpha_1 Post2007_t \times DiscShift_i + \gamma_i + \delta_t + \varepsilon_{i,t}. \quad (4)$$

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

Panel A. Disclosure Changes



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

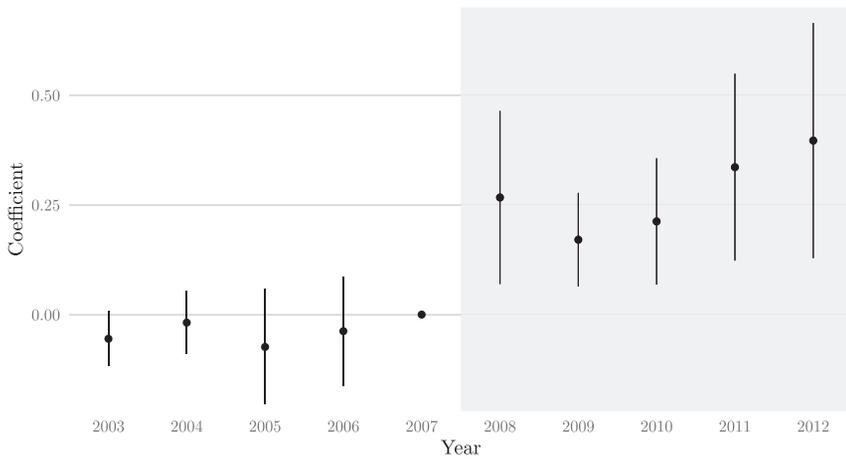


FIG 2.—German enforcement shock difference-in-difference trend analysis.

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: $GermanImportComp_{i,t} = \alpha_1(DiscShift_i \times Year2003_t) + \alpha_2(DiscShift_i \times Year2004_t) + \dots + \alpha_4(DiscShift_i \times Year2006_t) + \alpha_5(DiscShift_i \times Year2008_t) + \alpha_6(DiscShift_i \times Year2009_t) + \dots + \alpha_6(DiscShift_i \times Year2012_t) + \beta ImportComp_{i,t} + \gamma_i + \delta_t + \varepsilon_{i,t}$, 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 four-digit NAICS industry's disclosure ratio in 2008 minus the ratio in 2006. $ImportComp_{i,t}$ is U.S. import competition. All other variables are defined in table A1. The reference period is the year 2007. Standard errors are clustered by industry and year.

We measure the increase in public reporting because of increased enforcement using the difference in the sales-weighted proportion of firms reporting publicly between 2008 and 2006 in industry i , or *DiscShift*. *Post2007* is an indicator for the period after 2007 (Bernard [2016], Breuer, Leuz, and Vanhaverbeke [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 equation (4) is a generalized difference-in-differences specification, the main effect of *DiscShift* is absorbed by the industry indicators, γ_{is} , and the main effect of *Post2007* is absorbed by the year indicators, δ_t .

We modify equation (4) to include U.S. import competition (*ImportComp*) and its interaction with *Post2007* to control for any common features between the German market and U.S. market related to imports. We are also able to modify equation (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 *Post2007*. 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 equation (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 because of a decrease in magnitudes, as the coefficient estimate on *Post2007* \times *DiscShift* in column 5 actually becomes slightly larger in 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 U.S. 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

TABLE 5
German Disclosure Enforcement Change

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

This table presents estimates of regressing *GermanImportComp_{i,t}* on *Post2007_t × DiscShift_i*, *ImportComp_{i,t}* and *GermanPublicPresence_{i,t-1}*, and year and industry fixed effects where *i* denotes four-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 table A1. Standard errors (in parentheses) are clustered by industry and year. *, **, *** indicate two-sided *p*-values less than 10%, 5%, and 1%, respectively.

the U.S. 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 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 U.S. MECHANISM TESTS

In this section, we estimate or summarize a series of tests in the U.S. 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 U.S. Financial Reports. We first estimate equation (2) after including measures of the informativeness of U.S. financial reports and their interaction with *PublicPresence*. We predict that when competitors find U.S. 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, Ge, and Schrand [2010] for a review).³⁵ Specifically, we estimate four similar measures of financial report informativeness, *ICScore1-4*, 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 U.S. Financial Information. We next modify equation (2) to examine the potential moderating effect of different types of manager and analyst forecasts on the relation between *PublicPresence* and

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).

TABLE 6
Information Content of Earnings Cross-Sectional Analysis

<i>Information Content Measure (ICScoreX_{i,t-1}):</i>	<i>ImportComp_{i,t}</i>			
	<i>ICScore1_{i,t-1}</i> (1)	<i>ICScore2_{i,t-1}</i> (2)	<i>ICScore3_{i,t-1}</i> (3)	<i>ICScore4_{i,t-1}</i> (4)
<i>PublicPresence_{i,t-1}</i>	0.125 (0.086)	0.128 (0.086)	0.149* (0.089)	0.153* (0.088)
<i>ICScoreX_{i,t-1}</i>	0.527 (0.366)	0.519 (0.472)	0.071 (0.324)	0.013 (0.331)
<i>PublicPresence_{i,t-1} × ICScoreX_{i,t-1}</i>	2.638** (1.041)	2.806** (1.140)	1.348* (0.703)	1.361* (0.739)
<i>Additional Controls:</i>	Yes	Yes	Yes	Yes
<i>Fixed Effects:</i>				
<i>Industry (i)</i>	Yes	Yes	Yes	Yes
<i>Year (t)</i>	Yes	Yes	Yes	Yes
<i>Number of Clusters:</i>				
<i>Industry</i>	80	80	80	80
<i>Year</i>	17	17	17	17
<i>N</i>	1186	1186	1186	1186
<i>R² Full Model</i>	0.939	0.938	0.933	0.933
<i>R² Projected Model</i>	0.505	0.501	0.457	0.456

This table presents presents estimates of the model:

$$\begin{aligned}
 \text{ImportComp}_{i,t} = & \alpha_1 \text{PublicPresence}_{i,t-1} + \alpha_2 \text{ICScoreX}_{i,t-1} + \alpha_3 (\text{PublicPresence}_{i,t-1} \times \text{ICScoreX}_{i,t-1}) \\
 & + \beta \text{Controls}_{i,j,t-1} + \gamma_i + \delta_t + \varepsilon_{i,t},
 \end{aligned}$$

where *i* indexes four-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 × 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:

$$\frac{\text{TradingVolume}_{f,d}}{\text{SharesOutstanding}_{f,d}} = \beta_0^1 + \beta_1^1 \text{AnyRelease}_{f,d} + \beta_2^1 \text{AnyPeerRelease}_{f,d} + \eta_{f,d}^1,$$

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:

$$|\text{Return}_{f,d}| = \beta_0^3 + \beta_1^3 \text{AnyRelease}_{f,d} + \beta_2^3 \text{AnyPeerRelease}_{f,d} + \eta_{f,d}^3,$$

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.

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 reestimate equation (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 (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, Shroff, and White [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 macroeconomy and importers may care more about macroeconomic information (Hutton, Lee, and Shu [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. 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

TABLE 7
Forecasts

Panel A. Management guidance				
Guidance Type:	<i>ImportComp_{i,t}</i>			
	EPS (1)	Sales (2)	Gross Margin (3)	Cap Ex (4)
<i>GuidedPublicPresence_{i,t-1}</i>	0.327*** (0.122)	0.373*** (0.122)	0.473*** (0.153)	0.528*** (0.175)
<i>UnguidedPublicPresence_{i,t-1}</i>	0.353*** (0.086)	0.288*** (0.106)	0.220** (0.105)	0.199** (0.080)
<i>Guided – Unguided</i>	-0.027	0.085	0.253*	0.329**
χ^2 Test Statistic	0.163	0.764	2.999	4.593
<i>p</i> -value	0.686	0.382	0.083	0.032
<i>Controls:</i>	Yes	Yes	Yes	Yes
<i>Fixed Effects:</i>				
Industry (<i>i</i>)	Yes	Yes	Yes	Yes
Year (<i>t</i>)	Yes	Yes	Yes	Yes
<i>Number of Clusters:</i>				
Industry	85	85	85	85
Year	17	17	17	17
<i>N</i>	1354	1354	1354	1354
<i>R</i> ² Full Model	0.923	0.931	0.929	0.932
<i>R</i> ² Projected Model	0.393	0.455	0.440	0.463
Panel B. Analyst forecast type				
Type:	<i>ImportComp_{i,t}</i>			
	EPS (1)	Sales (2)	Gross Margin (3)	Cap Ex (4)
<i>PublicPresence_{i,t-1}</i>	0.117 (0.133)	0.024 (0.066)	0.118 (0.117)	0.125 (0.118)
<i>Analysts_{i,t-1}</i>	-0.006** (0.003)	-0.006* (0.003)	0.0003 (0.007)	0.001 (0.012)
<i>PublicPresence_{i,t-1} × Analysts_{i,t-1}</i>	0.113*** (0.023)	0.150*** (0.030)	0.119*** (0.038)	0.203*** (0.050)
<i>Controls</i>	Yes	Yes	Yes	Yes
<i>Fixed Effects:</i>				
Industry (<i>i</i>)	Yes	Yes	Yes	Yes
Year (<i>t</i>)	Yes	Yes	Yes	Yes
<i>Number of Clusters:</i>				
Industry	85	85	85	85
Year	17	17	17	17
<i>N</i>	1354	1354	1354	1354
<i>R</i> ² Full Model	0.941	0.947	0.939	0.938
<i>R</i> ² Projected Model	0.535	0.586	0.519	0.513

(Continued)

TABLE 7—(Continued)

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

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 tabulated 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.

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 U.S. 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 U.S. GAAP, as measured by Bradshaw, Bushee, and Miller [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 U.S. GAAP. We also examine whether foreign competitors appear to access public disclosures made by U.S. firms. Consistent with this, we find that increases in EDGAR downloads of U.S. financial statements in a given industry by users in a foreign country precede increases in import competition from that country and industry.

3.7 U.K. FALSIFICATION TEST

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

We focus on the United Kingdom because it is in many other ways culturally and economically similar to the United States (e.g., the Special Relationship; see Griffith, Harrison, and Van Reenen [2006]). For example, the United Kingdom received the highest score on the Brookings' institute's global manufacturing scorecard (78), whereas the United States 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 United Kingdom. We also anticipate that any nonfinancial reporting characteristic of public firms that causally affects import competition will also be present in the United Kingdom (i.e., we expect alternative mechanisms to also be present in the United Kingdom). 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 United States (i.e., as the ratio of imports to total U.K. production in industry *i* in year *t*).

We obtain U.K. import data from the BACI international trade database. We measure *UKPublicPresence* as sales by U.K. public firms, as inferred by nonmissing market capitalization divided by U.K. production, and obtained from the Orbis database and Eurostat. We measure *UKDisclosure* as disclosed sales divided by U.K. production, which are also from the Orbis database and Eurostat. Because the FRC historically enforced disclosure requirements, it may be comparatively more difficult to identify an incremental

³⁶ <https://www.brookings.edu/research/global-manufacturing-scorecard-how-the-us-compares-to-18-other-nations/>.

³⁷ <https://unctad.org/statistics>.

TABLE 8
U.K. Falsification Test

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

This table presents estimates of regressions in the form:

$$UKImportComp_{i,t} = \alpha_1 UKPublicPresence_{i,t-1} + \alpha_2 UKDisclosure_{i,t-1} + [\alpha_3 USImportComp_{i,t} + \alpha_4 USPublicPresence_{i,t-1}] + \beta_i + \gamma_t + \varepsilon_{i,t},$$

where *i* denotes four-digit NAICS industry and *t* denotes year. β_i are time-invariant industry fixed effects and γ_t are year fixed effects. All variables are defined in table A1. 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.

effect of disclosure in the United Kingdom, relative to Germany.³⁸ Finally, we again include both *USImportComp* and *USPublicPresence*.

Table 8 presents the results of estimating equation (6). Column 1 reports the baseline results and column 2 reports the results including the U.S. variables. Columns 3 and 4 present the results from repeating the sequence using the inverse hyperbolic sine transformation of the independent and

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

dependent variables of interest. Across all four columns, we find no evidence of a relation between public firm presence and import competition in the United Kingdom where both public and private firms report publicly (t -statistics of 0.16 to 0.78).

However, we note that although the United Kingdom is similar to the United States 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 United Kingdom is 0.798, whereas in the United States 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 United States at saturations of *ImportComp* that are equivalent to the mean level in the United Kingdom, 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 United Kingdom. Therefore, a difference between the United Kingdom and the United States is responsible for the differential relation between public firm presence and import competition in the two countries. Arguably, the most significant difference is that the SEC does not require U.S. private firms to publicly report, whereas the FRC requires that U.K. 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 United States.

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

³⁹ Similarly, the results suggest that the positive relation between import competition and public disclosure is present in Germany, but not in the United Kingdom. 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, whereas the FRC did.

intended beneficiaries of this information, foreign competitors may be able to use it to compete with U.S. firms. We find evidence that this is the case. Information spillovers from U.S. firms to foreign competitors represent an important externality that may be of interest to policy makers. 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.

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 four-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 U.S. production from the top 20 firms by shipments in four-digit NAICS industry i in year $t - 1$

DiscShift_t: 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-U.S. 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-U.S. countries during year $t - 1$

GermanDisclosure_{i,t-1}: Sum of sales from German firms in BvD's Orbis database for four-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 four-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:

$$\frac{\text{TradingVolume}_{f,d}}{\text{SharesOutstanding}_{f,d}} = \beta_0 + \beta_1 \text{AnyRelease}_{f,d} + \beta_2 \text{AnyPeerRelease}_{f,d} + \varepsilon_{f,d},$$

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:

$$\frac{\text{TradingVolume}_{f,d}}{\text{SharesOutstanding}_{f,d}} = \beta_0 + \beta_1 \text{EARelease}_{f,d} + \beta_2 \text{EAPeerRelease}_{f,d} + \varepsilon_{f,d},$$

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:

$$|\text{Return}_{f,d}| = \beta_0 + \beta_1 \text{AnyRelease}_{f,d} + \beta_2 \text{AnyPeerRelease}_{f,d} + \varepsilon_{f,d},$$

where *f* indexes firm, *d* indexes day, $|\text{Return}_{f,d}|$ 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:

$$|\text{Return}_{f,d}| = \beta_0 + \beta_1 \text{EARelease}_{f,d} + \beta_2 \text{EAPeerRelease}_{f,d} + \varepsilon_{f,d},$$

where *f* indexes firm, *d* indexes day, $|\text{Return}_{f,d}|$ 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 United States in four-digit NAICS i during year t scaled by U.S. production in the same four-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 United States in four-digit NAICS i during year t scaled by U.S. production in the same four-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 four-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 four-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 U.S. firms in four-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 four-digit NAICS industry i in year $t - 1$, scaled by U.S. 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 four-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 four-digit NAICS industry i for the 12 trading days following July 8, 2002. Calculated for all industries with 10 or more publicly traded firms

$Tariff_{i,t-1}$: Tariffs levied on imported goods in four-digit NAICS industry i worldwide divided by total value of imports the same 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 four-digit NAICS i in year $t - 1$

- TotalInv_{i,t}*: Total year end inventories for four-digit NAICS industry i scaled by industry sales in year t
- UKDisclosure_{i,t}*: Sum of sales from U.K. firms in BvD's Orbis database for four-digit NAICS industry i and year $t - 1$ scaled by U.K. production in the same industry and year from Eurostat. Winsorized at 1% and 99%
- UKImportComp_{i,t}*: Sum of U.K. imports in industry i and year $t - 1$ from the BACI database scaled by U.K. production in the same industry and year from Eurostat. Winsorized at 1% and 99%
- UKPublicPresence_{i,t-1}*: Sum of sales from U.K. firms with non-missing market capitalization in BvD's Orbis database for four-digit NAICS industry i and year $t - 1$ scaled by U.K. 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 rate in dollars for the average production worker in industry i in year $t - 1$
- ValueAdd_{i,t-1}*: Value added by U.S. manufacturing (shipments – raw materials and fuels) scaled by shipments in four-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

REFERENCES

- ACEMOGLU, D.; D. AUTOR; D. DORN; G. H. HANSON; and B. PRICE. "Import Competition and the Great US Employment SAG of the 2000s." *Journal of Labor Economics* 34 (2016): S141–98.
- ACHARYA, V., and Z. XU. "Financial Dependence and Innovation: The Case of Public Versus Private Firms." *Journal of Financial Economics* 124 (2017): 223–43.
- AGHION, P.; A. BERGEAUD; M. LEQUIEN; and M. J. MELITZ (2018). *The Impact of Exports on Innovation: Theory and Evidence (No. w24600)*. National Bureau of Economic Research.
- AGHION, P.; N. BLOOM; R. BLUNDELL; R. GRIFFITH; and P. HOWITT. "Competition and Innovation: An Inverted-U Relationship." *The Quarterly Journal of Economics* 120 (2005): 701–28.
- ALBORNOZ, F.; H. F. C. PARDO; G. CORCOS; and E. ORNELAS. "Sequential Exporting." *Journal of International Economics* 88 (2012): 17–31.
- ALLEN, T. "Information Frictions in Trade." *Econometrica* 82 (2014): 2041–83.
- ARMSTRONG, C.; S. GLAESER; and S. HUANG (2018). *Controllability of Risk and the Design of Incentive-Compensation Contracts*. Singapore Management University School of Accountancy Research Paper, (2017-55).
- ARMSTRONG, C. S., S. GLAESER; and J. D. KEPLER. "Accounting Quality and the Transmission of Monetary Policy." *Journal of Accounting and Economics* 68 (2019): 101265.
- ARMSTRONG, C. S.; W. R. GUAY; and J. P. WEBER. "The Role of Information and Financial Reporting in Corporate Governance and Debt Contracting." *Journal of Accounting and Economics* 50 (2010): 179–234.

- AUTOR, D. H.; D. DORN; and G. H. HANSON. "The China Syndrome: Local Labor Market Effects of Import Competition in the United States." *American Economic Review* 103 (2013): 2121–68.
- BADERTSCHER, B.; N. SHROFF; and H. D. WHITE. "Externalities of Public Firm Presence: Evidence from Private Firms' Investment Decisions." *Journal of Financial Economics* 109 (2013): 682–706.
- BALL, R., and P. BROWN. "An Empirical Evaluation of Accounting Income Numbers." *Journal of Accounting Research* 6 (1968): 159–78.
- BEATTY, A.; S. LIAO; and J. J. YU. "The Spillover Effect of Fraudulent Financial Reporting on Peer Firms' Investments." *Journal of Accounting and Economics* 55 (2013): 183–205.
- BEAVER, W. H. "The Information Content of Annual Earnings Announcements." *Journal of Accounting Research* 6 (1968): 67–92.
- BERGER, P. G.; J. H. CHOI; and S. TOMAR. "Breaking It Down: Competitive Costs of Cost Disclosures." Available at SSRN 3358435, 2019.
- BERNARD, D. "Is the Risk of Product Market Predation a Cost of Disclosure?" *Journal of Accounting and Economics* 62 (2016): 305–25.
- BERNARD, D.; T. BLACKBURN; and J. THORNOCK. "Information Flows Among Rivals and Corporate Investment." *Journal of Financial Economics* 136 (2020): 760–79.
- BEYER, A.; D. A. COHEN; T. Z. LYS; and B. R. WALTHER. "The Financial Reporting Environment: Review of the Recent Literature." *Journal of Accounting and Economics* 50 (2010): 296–343.
- BLANKESPOOR, E.; E. DEHAAN; and I. MARINOVIC. "Disclosure processing costs, investors' information choice, and equity market outcomes: A review." *Journal of Accounting and Economics* 70 (2020): 101344.
- BLOOMFIELD, M. J.; and M. TUIJN. "Do Firms Strategically Announce Capacity Expansions in Response to Heightened Entry Threats?" Available at SSRN 3195932, 2019.
- BRADSHAW, M. T., B. J. BUSHEE; and G. S. MILLER. "Accounting Choice, Home Bias, and US Investment in Non-US Firms." *Journal of Accounting Research* 42 (2004): 795–841.
- BREUER, M. "How Does Financial-Reporting Regulation Affect Market-Wide Resource Allocation?" *Journal of Accounting Research* 59 (2021): <https://doi.org/10.1111/1475-679X.12345>.
- BREUER, M.; C. LEUZ; and S. VANHAVERBEKE. *Mandated Financial Reporting and Corporate Innovation* (No. w26291). National Bureau of Economic Research. (2019).
- BURBIDGE, J. B.; L. MAGEE; and A. L. ROBB. "Alternative Transformations to Handle Extreme Values of the Dependent Variable." *Journal of the American Statistical Association* 83 (1988): 123–7.
- BUSHEE, B. J.; J. E. CORE; W. GUAY; and S. J. HAMM. "The Role of the Business Press as an Information Intermediary." *Journal of Accounting Research* 48 (2010): 1–19.
- CARHART, M. M. "On Persistence in Mutual Fund Performance." *The Journal of Finance* 52 (1997): 57–82.
- CHRISTENSEN, H. B.; E. FLOYD; L. Y. LIU; and M. MAFFETT. "The Real Effects of Mandated Information on Social Responsibility in Financial Reports: Evidence from Mine-Safety Records." *Journal of Accounting and Economics* 64 (2017): 284–304.
- CHRISTENSEN, H. B.; L. Y. LIU; and M. G. MAFFETT. "Proactive Financial Reporting Enforcement and Shareholder Wealth." *Journal of Accounting and Economics* 69 (2020): 101267.
- COSTELLO, A. M. "Mitigating Incentive Conflicts in Inter-Firm Relationships: Evidence from Long-Term Supply Contracts." *Journal of Accounting and Economics* 56 (2013): 19–39.
- DECHOW, P.; W. GE; and C. SCHRAND. "Understanding Earnings Quality: A Review of the Proxies, Their Determinants and Their Consequences." *Journal of Accounting and Economics* 50 (2010): 344–401.
- DISDIER, A., and K. HEAD. "The Puzzling Persistence of the Distance Effect on Bilateral Trade." *The Review of Economics and Statistics* 90 (2008): 37–48.
- DIXIT, A. "The Role of Investment in Entry-Deterrence." *The Economic Journal* 90 (1980): 95–106.
- DOUGAL, C.; J. ENGELBERG; D. GARCIA; and C. A. PARSONS. "Journalists and the Stock Market." *The Review of Financial Studies* 25 (2012): 639–79.
- ENGEL, E.; R. M. HAYES; and X. WANG. "The Sarbanes–Oxley Act and Firms' Going-Private Decisions." *Journal of Accounting and Economics* 44 (2007): 116–45.

- ENGELBERG, J. E., and C. A. PARSONS. "The Causal Impact of Media in Financial Markets." *The Journal of Finance* 66 (2011): 67–97.
- GAO, F.; J. S. WU; and J. ZIMMERMAN. "Unintended Consequences of Granting Small Firms Exemptions from Securities Regulation: Evidence from the Sarbanes-Oxley Act." *Journal of Accounting Research* 47 (2009): 459–506.
- GILJE, E. P., and J. P. TAILLARD. "Do Private Firms Invest Differently than Public Firms? Taking Cues from the Natural Gas Industry." *The Journal of Finance* 71 (2016): 1733–78.
- GLAESER, S. "The Effects of Proprietary Information on Corporate Disclosure and Transparency: Evidence from Trade Secrets." *Journal of Accounting and Economics* 66 (2018): 163–93.
- GLAESER, S., and W. R. GUAY. "Identification and Generalizability in Accounting Research: A Discussion of Christensen, Floyd, Liu, and Maffett (2017)." *Journal of Accounting and Economics* 64 (2017): 305–12.
- GLAESER, S. A., and W. R. LANDSMAN. "Deterrent Disclosure." *The Accounting Review* 96 (2021): 291–315.
- GLAESER, S.; J. MICHELS; and R. E. VERRECCHIA. "Discretionary Disclosure and Manager Horizon: Evidence from Patenting." *Review of Accounting Studies* 25 (2020): 597–635.
- GREENE, W. H. (2003). *Econometric Analysis*. Noida, India: Pearson Education India.
- GRIFFITH, R., R. HARRISON; and J. VAN REENEN. "How Special Is the Special Relationship? Using the Impact of US R&D Spillovers on UK Firms as a Test of Technology Sourcing." *American Economic Review* 96 (2006): 1859–75.
- GUISSO, L., and G. PARIGI. "Investment and Demand Uncertainty." *The Quarterly Journal of Economics* 114 (1999): 185–227.
- HANDLEY, K., and N. LIMÃO. "Policy Uncertainty, Trade, and Welfare: Theory and Evidence for China and the United States." *American Economic Review* 107 (2017): 2731–83.
- HANN, R. N.; H. KIM; W. WANG; and Y. ZHENG. "Information Frictions and Productivity Dispersion: The Role of Financial Reporting Quality." *The Accounting Review* 95 (2020): 223–50?
- HEAD, K., and T. MAYER. "Gravity equations: Workhorse, toolkit, and cookbook." In *The Handbook of International Economics*, Eds. G. Gopinath, E. Helpman, & K. Rogoff. (Vol. 4, (2014): pp. 131–95). Amsterdam: Elsevier.
- HEINLE, M. S., and K. C. SMITH. "A Theory of Risk Disclosure." *Review of Accounting Studies* 22 (2017): 1459–91.
- HENSELMANN, K., and D. KAYA. "Empirische Analyse des Offenlegungszeitpunkts von Jahresabschlüssen nach dem EHUG (No. 2008-1)." Working Papers in Accounting Valuation Auditing. 2009.
- HUANG, Y.; R. JENNINGS; and Y. YU. "Product Market Competition and Managerial Disclosure of Earnings Forecasts: Evidence from Import Tariff Rate Reductions." *The Accounting Review* 92 (2016): 185–207.
- HUTTON, A. P.; L. F. LEE; and S. Z. SHU. "Do Managers Always Know Better? The Relative Accuracy of Management and Analyst Forecasts." *Journal of Accounting Research* 50 (2012): 1217–44.
- ILIEV, P. "The Effect of SOX Section 404: Costs, Earnings Quality, and Stock Prices." *The Journal of Finance* 65 (2010): 1163–96.
- JOHNSON, N. L. "Systems of Frequency Curves Generated by Methods of Translation." *Biometrika* 36: (1949) 149–76.
- KELLY, B., and A. LJUNGQVIST. "Testing Asymmetric-Information Asset Pricing Models." *The Review of Financial Studies* 25 (2012): 1366–413.
- KIM, C. *Spillover Effects of Financial Reporting on Public Firms' Corporate Investment*. Working paper, University of Pennsylvania. 2019.
- KIM, J.; and M. OLBERT. "How Does Private Firm Disclosure Affect Demand for Public Firm Equity? Evidence from the Global Equity Market." *Evidence from the Global Equity Market* (April 30, 2021). 2021.
- KLETZER, L. G. (2001). *Job Loss from Imports: Measuring the Costs*. Washington, DC: Institute for International Economics.

- LEUZ, C.; A. TRIANTIS; and T. Y. WANG. "Why Do Firms Go Dark? Causes and Economic Consequences of Voluntary SEC Deregistrations." *Journal of Accounting and Economics* 45 (2008): 181–208.
- LUDWIG, J., J. R. KLING; and S. MULLAINATHAN. "Mechanism Experiments and Policy Evaluations." *Journal of Economic Perspectives* 25 (2011): 17–38.
- MAKSIMOVIC, V.; G. PHILLIPS; and L. YANG. "Private and Public Merger Waves." *The Journal of Finance* 68 (2013): 2177–217.
- MICHAELY, R., and M. R. ROBERTS. "Corporate Dividend Policies: Lessons from Private Firms." *The Review of Financial Studies* 25 (2011): 711–46.
- MUMMOLO, J., and E. PETERSON. "Improving the Interpretation of Fixed Effects Regression Results." *Political Science Research and Methods* 6 (2018): 829–35.
- OSTER, E. "Unobservable Selection and Coefficient Stability: Theory and Evidence." *Journal of Business & Economic Statistics* 37 (2019): 187–204.
- PIERCE, J. R., and P. K. SCHOTT. "The Surprisingly Swift Decline of US Manufacturing Employment." *American Economic Review* 106 (2016): 1632–62.
- RAJAN, R., and L. ZINGALES. "Financial Development and Growth." *American Economic Review* 88 (1998): 559–86.
- RAUCH, J. E., and A. CASELLA. "Overcoming Informational Barriers to International Resource Allocation: Prices and Ties." *The Economic Journal* 113: (2003) 21–42.
- RAUCH, J. E., and V. TRINDADE. "Information, International Substitutability, and Globalization." *American Economic Review* 93 (2003): 775–91.
- ROMANO, R. "The Sarbanes-Oxley Act and the Making of Quack Corporate Governance." *Yale Law Journal* 114 (2004): 1521.
- ROYCHOWDHURY, S.; N. SHROFF; and R. S. VERDI. "The Effects of Financial Reporting and Disclosure on Corporate Investment: A Review." *Journal of Accounting and Economics* 68 (2019): 101246.
- SADKA, G. "The Economic Consequences of Accounting Fraud in Product Markets: Theory and a Case from the US Telecommunications Industry (WorldCom)." *American Law and Economics Review* 8 (2006): 439–75.
- SAGER, E., and O. A. TIMOSHENKO. "Demand Uncertainty, Selection, and Trade." Working Paper. 2019.
- SANI, J. (2021). "Delegation of Decision Rights Within Firms and the Role of the Peer Information Environment." Working Paper.
- SCHOTT, P. K. "The Relative Sophistication of Chinese Exports." *Economic Policy* 23 (2008): 6–49.
- SHROFF, N.; R. S. VERDI; and B. P. YOST. "When Does the Peer Information Environment Matter?" *Journal of Accounting and Economics* 64 (2017): 183–214.
- SHROFF, N.; R. S. VERDI; and G. YU. "Information Environment and the Investment Decisions of Multinational Corporations." *The Accounting Review* 89 (2013): 759–90.
- STEINWENDER, C. "Real Effects of Information Frictions: When the States and the Kingdom Became United." *American Economic Review* 108 (2018): 657–96.
- UNITED STATES. DEPARTMENT OF JUSTICE, & UNITED STATES. FEDERAL TRADE COMMISSION. (2010). Horizontal Merger Guidelines. US Department of Justice. Available at: <https://www.justice.gov/atr/horizontal-merger-guidelines-08192010>.
- YANG, Y. J. "Real Effects of Disclosure Regulation: Evidence from US Import Competition." Available at SSRN 3456743, 2019.
- ZHANG, I. X. "Economic Consequences of the Sarbanes–Oxley Act of 2002." *Journal of Accounting and Economics* 44 (2007): 74–115.
- ZHOU, Y. "Financial Reporting Quality and International Trade." Available at SSRN 3832915, 2021.