ESG Shocks in Global Supply Chains*

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Abstract

We show that U.S. firms cut imports by 29.9% when their international suppliers experience environmental and social (E&S) incidents. These trade cuts are larger for publicly listed U.S. importers facing high E&S investor pressure and lead to cross-country supplier reallocation, suggesting that E&S preferences in capital markets can be privately costly but have real effects in far-flung economies. Larger trade cuts around the incident result in higher supplier E&S performance in subsequent years, and in the eventual resumption of trade. Our results highlight the role of investors in ensuring suppliers' E&S compliance along global supply chains.

Keywords: ESG; ESG risk management; global supply chains; investor pressure; reputation

JEL Classifications: F14, F18, G34, G38

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1 Introduction

Corporate managers face unprecedented pressure by stakeholders and regulators to monitor and manage environmental, social, and governance (ESG) risks along their supply chains.[1] Anecdotes from the media, surveys, and corporate disclosures suggest that, in response to such heightened pressure, managers have increasingly been adopting "de-risking" strategies – relationship restrictions and terminations – when suppliers violate ESG standards.[2] Outside of such anecdotes, however, we lack systematic evidence on these ESG-related trade adjustments and an understanding of their associated economic incentives. Do these adjustments follow changes in operational and regulatory risks, or do managers respond to the ESG-related preferences of stakeholders such as consumers and investors? Do firms reallocate trade, and are there costs of and limits to reallocation? Can de-risking help to improve foreign suppliers' ESG standards?

This paper studies trade adjustments between U.S. importers and international suppliers involved in ESG-related controversies. We find that importers either completely terminate or often partially adjust their trade relationships to diversify their sourcing risks, even when these controversies are unlikely to affect the supplier's operations or result in regulatory penalties. Such active (but costly) de-risking provides incentives for suppliers to improve their ESG performance, leading to subsequent trade resumption. After the same incident, importers with ESG-sensitive institutional investors adjust trade more actively, shedding light on how institutional investor engagement can have far-reaching effects on public and private firms worldwide.

Our data include shipment-level information on trade between foreign suppliers and

[1] As a prominent example, the Corporate Sustainability Due Diligence Directive recently signed by the European Union (EU) mandates corporations to monitor their suppliers' adverse impacts on human rights and the environment. The directive targets large EU firms and non-EU firms with substantial presence in the EU, making their directors legally liable for supplier noncompliance.

[2] The term de-risking, originally used in the context of financial institutions (`www.state.gov`), is now commonly used to define trade reallocation aimed at decreasing corporate supply chain risks (see, e.g., `www.forbes.com`). A recent survey finds that, in 2021 alone, one out of five UK retailers cut relationships with their suppliers over ESG concerns. See `www.barclayscorporate.com`.

U.S. customers over the 2007-20 period, sourced by S&P Global Panjiva from cargo declarations to the U.S. Customs and Border Protection (CBP). These data capture the universe of direct maritime imports (the largest trade mode for U.S. firms), and thus include relationships between U.S. firms and their foreign suppliers beyond those recorded in regulatory filings and public communications. We study how the imports of U.S. customers respond when their international suppliers (including small, privately held ones) are associated with ESG incidents recorded in the RepRisk dataset, which sources ESG-related events from the media as well as regulatory and commercial documents. In our main analysis, we follow the literature and focus on incidents likely to generate externalities on local communities (E&S incidents hereafter; see, e.g., Gantchev et al., 2022 and Starks, 2023): *environmental* ("E") incidents such as those related to pollution and overuse and wasting of resources, as well as *social* ("S") such as those related to human rights abuses and health and safety accidents.

Our sample consists of 1,049 supplier-year pairs and 1,319 relationship-year pairs affected by an E&S incident over 2010-18. We start by showing that supplier incidents trigger negative stock price reactions for U.S. customers in this sample: we document an average -10 basis points cumulative abnormal return (CAR) in a [-1,+1] day window around the supplier incident. This preliminary finding suggests a material downstream economic impact for the customer, which we later find to be consistent with costly reallocation and increased sourcing costs.

Our main tests use a stacked difference-in-differences regression approach to study the effect of supplier E&S incidents on imports by U.S. customer firms. For each E&S incident, we build separate time cohorts that include trade relationships between an E&S incident-affected supplier and its U.S. customers ("treated" relationships), as well as relationships between the same U.S. customers and their other suppliers and relationships between unaffected suppliers and other U.S. customers ("control" relationships) three years before and three years after the event. Our estimates capture the change in

trade intensity between U.S. customers and their incident-affected international suppliers three years before and three years after the incident, relative to the change in trade between other U.S. customers and international suppliers during the same time period. As most U.S. customers simultaneously have multiple suppliers, our tests allow us to control for time-varying customer demand.

We measure trade intensity by the number of containers shipped annually between international suppliers and U.S. customers. We find that, following a supplier's incident, the annual number of containers imported by the average U.S. customer from that supplier decreases by 29.9%. This decline in trade appears in the year immediately following the incident (but not before), and on average lasts for more than three years. We also find no pre-existing differences between treated and control suppliers across several other characteristics (including their E&S profiles), confirming the identifying assumption that supplier E&S incidents are generally unanticipated by corporate customers.

When we break down trade cuts into the extensive margin (i.e., a complete disappearance of the trade relationship) and the intensive margin (i.e., a decrease in the number of containers traded, conditional on relationship continuation), we find that the average relationship is 4.3% more likely to be terminated after a supplier's E&S incident—a 50% increase relative to the baseline probability of a termination. Conditional on continuation, container shipments drop by 18.3% on average, suggesting that even when customers continue trading with an incident-affected supplier the extent of the relationship decreases significantly. While many trade cuts in our sample are complete terminations, partial trade cuts are extremely frequent: around 44% of the trade cuts in our sample involve reduced trade but not a complete termination.

To the best of our knowledge, we are the first to provide large-sample evidence of trade adjustments in response to E&S shocks. On the supply side, these adjustments could be motivated by operational disruptions around the incident. On the demand side, customers may be looking to diversify correlated *operational* risks (e.g., risk of disruptions

in suppliers' product quality and regulatory penalties) by reducing their exposure to the supplier. Alternatively, these trade cuts could reflect the incentives of the customers' stakeholders such as consumers and investors, who may have reputational concerns (e.g., Krueger et al., 2020) or intrinsic non-pecuniary preferences (e.g., Riedl and Smeets, 2017; Landier and Lovo, 2023; Houston et al., 2022; Meier et al., 2023).[3]

We perform additional tests to tease out the economic incentives underlying the observed trade adjustments. We start by showing that the documented trade cuts are unlikely motivated by supplier business risk but rather are correlated with potential externalities to local communities.[4] First, while the trade cuts are quantitatively similar for E-related and S-related incidents, we do not observe them for the governance ("G")-related incidents that are less likely to generate externalities (e.g., incidents related to compensation issues or tax avoidance). Second, we manually collect the news corresponding to individual RepRisk incidents, and categorize them based on their likely impact on suppliers' operating performance, product quality, and risk of regulatory penalties. We find quantitatively similar estimates to the baseline effects in sub-samples of incidents with low impact on suppliers' operations, product quality, and regulatory risk, suggesting that these sources of business risk are not the first-order determinants of the trade cuts in our sample.

While these findings suggest that de-risking in our sample does not systematically occur because of supplier business risk, we instead find that it is correlated with the E&S incentives of customers' stakeholders, institutional investors in particular. To disentangle these incentives, we perform a within-incident analysis, reminiscent of the Khwaja and Mian (2008) methodology, where we measure differential trade changes between *the same incident-affected supplier* and U.S. customers with different characteristics. Do-

[3]In what follows, we refer to such E&S incentives of stakeholders as "E&S sensitivity." We abstract from whether it is driven by their own reputational concerns or by intrinsic pro-E&S preferences.

[4]Anecdotal examples on this are ubiquitous. For example, Nestlé's recent trade cut with the Brazilian supplier Marfrig is likely not the result of the concerns about product quality or other operational risks, but rather by Marfrig's failure to comply with Nestlé's E&S standards and Nestlé's concern about the associated reputational losses. See Appendix A.1 for more details and other examples.

ing so allows us to control for average variation stemming from general deterioration in supplier exports, and to study how trade cuts vary in the cross-section of customer characteristics (such as relative bargaining power and ownership by E&S-sensitive investors) while holding supplier and incident characteristics constant.

For the same supplier incident, we find stronger evidence of de-risking when the U.S. customer's *investor base* is more likely to be sensitive to E&S issues. First, trade cuts are increasing in the customer's overall E&S rating. Second, trade cuts are increasing in the customer's stock ownership by E&S-sensitive institutional investors, who might impose E&S pressure via changes in their holdings, investor meetings, shareholder proposals, or voting.[5] Third, trade cuts are larger if the customer receives shareholder proposals related to E&S issues before the incident. Fourth, when we expand our sample to also include privately held U.S. customers we find that trade cuts of publicly listed customers are on average 19.1% larger than those of privately held ones, suggesting that the flexibility needed to maintain sustainable supply chains is a potential cost of being public.

A natural concern is that our cross-sectional estimates based on measures of investor E&S sensitivity may be due to systematic correlation with other customer characteristics, such as profitability, bargaining power, or operational risk management ability. However, these various financial and risk-management characteristics do not explain cross-sectional variation in trade cuts. Conversely, the cross-sectional findings on investor E&S composition hold even *conditional on these characteristics*.

To consider an alternative set of relevant corporate stakeholders (Bénabou and Tirole, 2010), we also ask whether de-risking is reflective of pressure by retail consumers. Based on granular scanner data on U.S. importers' retail sales from Nielsen, we do observe a decline in the quantity of products sold locally by U.S. retailers around supplier incidents. However, product prices increase around these incidents. Thus, the observed

[5]For example, in January 2022 Costco's shareholders voted in favor of activist investor's Green Century Funds proposal to implement tough measures on the company's indirect greenhouse gas emissions along the supply chain (so-called "Scope 3 Emissions"). See `www.sec.gov` and `www.wsj.com`. See also Appendix A.1 for additional anecdotes on investor E&S pressure on firms' supply chains.

quantity changes are more likely related to changes in input supply rather than by lower end-consumer demand, for which we would expect product price discounts. In sum, our evidence shows that (likely due to information or coordination frictions), consumers have limited effects on supply chain E&S policies relative to investors.

In line with an E&S risk management interpretation, affected customers reallocate trade, and more so to suppliers outside of the country of the original supplier and to suppliers with high E&S profiles. Such reallocation appears to be costly for the customer: gross profit margins decrease by 0.9% for customers that cut trade with incident-affected suppliers, but do not change for other customers that do not cut trade. To the best of our knowledge, these results are among the first in the literature to document the monetary costs of maintaining sustainable supply chains.

Incidentally and perhaps most importantly, de-risking also seems to act as a disciplining force following poor supplier E&S performance.[6] First, we show that trade cuts after the incident are associated with subsequent improvements in suppliers' E&S performance, and these improvements are increasing in the extent of the initial trade cut. No such improvements are observable absent post-incident trade cuts. Second, we find that only relationships with both initial trade cuts *and* post-incident improvements in supplier E&S performance are associated with future trade reversals. Overall, these findings provide evidence of a novel mechanism whereby de-risking (likely motivated by investors' incentives) ensures supplier E&S adherence in production networks.

Our findings contribute to the literature on corporate risk management (e.g. Froot et al., 1993; Stulz, 2008; Rampini et al., 2014), and in particular environmental (e.g., Sautner et al., 2023; Li et al., 2024; Pankratz and Schiller, 2024) and reputational risk management (e.g., Lins et al., 2017; Albuquerque et al., 2020; Akey et al., 2023). We contribute to this literature by documenting what we believe is the first systematic evidence on corporate supply chain de-risking following ESG-related violations, including viola-

[6]See, for example, Appendix Figure A1 on Nestlés approach to deforestation management and trade suspensions within its supply chain.

tions that carry little operational and regulatory risks but are likely to generate negative downstream reputational effects. In this sense, we show that active reputational risk management can generate positive externalities outside the firm's boundaries, which is also a novel contribution to this literature.

Our findings also contribute to the literature on how firms' environmental and social postures are transmitted through supply chains. Recent work (Schiller, 2018; Dai et al., 2021b) shows that the pro-E&S policies of suppliers are linked to those of their corporate customers, with financial benefits for both parties. In the same strand of literature, Koenig and Poncet (2022) document a decline in French imports from Bangladeshi retailers connected to the 2013 collapse of Dhaka's Rana Plaza building. We complement these studies by conducting the first large-sample study of trade changes and customer de-risking following supplier E&S incidents. We show that the E&S incentives of stakeholders (as opposed to regulation, as in Schiller, 2018; concerns for the quality of the supplier, as in Dai et al., 2021b; or correlated operational risks, as in Koenig and Poncet, 2022) are important drivers of E&S preference transmission from customers to suppliers. Different from the previous literature, we also highlight substantial costs to maintaining sustainable supply chains, and provide new evidence that de-risking and termination threats constitute powerful disciplining tools in global production networks.[7]

A fast-growing body of work also studies ESG issues in capital markets, including investors' E&S preferences (Riedl and Smeets, 2017; Krueger et al., 2020) and their impact on firm activities (e.g., Azar et al., 2021; Atta-Darkua et al., 2022; and Barko et al., 2022).[8] We contribute to it with novel evidence on the effects of institutional investors' E&S preferences on trade and on the structure of international supply chains. Prior literature

[7]Two strands of the literature have separately studied how production networks affect customer and supplier policies (e.g., Titman, 1984; Banerjee et al., 2008; Ahern and Harford, 2014; Barrot and Sauvagnat, 2016) and how institutional investors use termination to improve corporate governance in the firms they invest in (e.g., Admati and Pfleiderer, 2009; Edmans, 2009; Edmans and Manso, 2011; Bharath et al., 2013). To the best of our knowledge, there is limited empirical evidence on how corporate customers' exit may act as a disciplining device to influence supplier policies, especially those with negative externalities.

[8]Our findings also complement those on mandatory supplier ESG disclosure (e.g., Dai et al., 2021b; Darendeli et al., 2022; and She, 2022). Instead, we look at cases where incident disclosure is not mandatory.

(Gantchev et al., 2022, von Beschwitz et al., 2022, and Derrien et al., 2022) also studies investor and analyst reactions to publicly listed firms' E&S incidents but focuses on the direct role of capital market participants on corporate E&S behavior. We contribute to it by documenting an *indirect* disciplining role of capital markets on the suppliers when corporate customers are owned by E&S-sensitive investors.

More broadly, the stakeholder capitalism literature (Bénabou and Tirole, 2010; Hart and Zingales, 2017) highlights lack of extra-territorial reach as a potential reason why governments may fail to affect corporate externalities. Consistent with the arguments in Bénabou and Tirole (2010), our paper suggests that stakeholders such as investors can overcome these limitations and exert pressure on firms (even privately held ones) outside of their country and, possibly, their investment universe. Complementing theoretical work (Landier and Lovo, 2023), our findings suggest that holding stakes in U.S. publicly listed firms with a wide global supplier network can provide a conduit for E&S-sensitive institutional investors to monitor and discipline suppliers in far-flung countries.

2 Data Sources and Matching

2.1 Cross-border Shipments

Maritime imports constitute the vast majority of U.S. imports, both in terms of tonnage and in terms of value.[9] Title 19 of the United States Code of Federal Regulation (CFR) requires U.S. firms to report shipment details in cargo declarations to the U.S. Customs and Border Protection (CBP). We obtain shipment-level data on these maritime transactions between foreign suppliers and U.S. customers over the 2007-20 period from the S&P Global Panjiva database (henceforth Panjiva). For each shipment transaction, Panjiva provides information about the sender, the consignee, the origin and destination, the

[9] `www.trade.gov`.

product codes, and the shipment container specifications.[10] The information included in Panjiva is required by U.S. customs law, which reduces potential selection concerns based on suppliers' and customers' disclosure incentives around E&S incidents.[11]

We link U.S. consignees in Panjiva to their ultimate parent in Compustat and aggregate the Panjiva data to the Panjiva supplier-Compustat customer-year level.[12] To track within-relationship variation over time, we require the supplier-customer relationship to appear in at least two distinct years during our sample period. We also add two years before the first year in which a given supplier-customer relationship appears in our sample to account for the ramp-up of relationships over time (Intintoli et al., 2017). Similarly, we extend the panel by two years after the last year in which the relationship appears in the data to account for possible relationship deterioration and termination. All transaction values are set to zero for these extended periods, and for all the years in which transaction values are missing between the first and the last relationship years. Appendix Table B1 provides definitions for all the variables used in the paper. Appendix Table B2 describes the sample selection process for the Panjiva data.

2.2 E&S Incidents

We gather the universe of ESG-related incidents for 2007-2021 from RepRisk, a leading business research provider that searches media, regulatory, and commercial documents for companies' ESG-related incidents (Gantchev et al., 2022). RepRisk classifies incidents into environmental ("E"), social ("S"), and governance ("G") categories. Environmental incidents involve pollution; overuse and wasting of resources; and animal mistreatment. Social incidents involve community relations (such as human rights abuses and social

[10]Importantly, this data allow studying intensive margin adjustments of supply chain relationships. For example, the often-used FactSet Supply Chain Relationships (formerly Revere) dataset provides sales data for less than 10% of the sample (Pankratz and Schiller, 2024) and thus is only used for an extensive margin analysis on whether customer retains the supplier.

[11]Financial analysts are one of the main users of Panjiva, suggesting that institutional investors use this dataset to monitor the supply chains of portfolio firms. See `www.spglobal.com`.

[12]Around 32.4% of non-financial firms in Compustat appear as importers in Panjiva in any given year.

discrimination) and employee relations (such as forced or child labor and occupational health and safety accidents). Governance incidents include corruption, bribery, extortion, money laundering, executive compensation issues, misleading communication, fraud, tax evasion, tax optimization, and anti-competitive practices.

We focus on incidents likely to create negative externalities for local communities and thus downstream reputational effects beyond pure business risks. While some G incidents (such as bribery and extortion) resemble E&S incidents in this respect, other G incidents (such as issues with executive compensation and tax avoidance) result from failures in private contracting between suppliers' shareholders and managers and are unlikely to trigger downstream reputational effects. Following the literature (Krüger, 2015; Dai et al., 2021b; Dyck et al., 2019; Gantchev et al., 2022; Starks, 2023) in our main analysis we focus on E&S incidents and leave G incidents to robustness tests.

RepRisk discloses the name and the identifier of the company involved in the incident, the date and topic(s) of the incident, and additional information such as the incident location, its severity and novelty, and whether the incident news has international, national, or local reach. According to RepRisk, a team of analysts manually verifies that each incident is indeed ESG-related. However, RepRisk does not provide the content of the incident. To overcome this potential limitation, we manually search traditional and social media as well as publicly available regulatory filings for information on the incidents in our sample. We are able to recover news for around 46% of the incidents in our sample, mostly in local news (only 2.3% of the incidents in our sample are covered by media outlets with international reach) and languages other than English. This additional dataset allows us to investigate whether E&S incidents systematically affect suppliers' operations, product quality, and regulatory enforcement risk. The details on the data collection and categorization process are available in Appendix A.2.

2.3 Matching and Final Dataset

We use a fuzzy name algorithm to link Panjiva foreign suppliers (both privately held and publicly listed) to their RepRisk E&S incidents. By construction, the resulting sample only contains supply chain relationships between U.S. customers and their first-degree foreign suppliers. To ensure at least three years of cross-border shipment data before and after an incident, we study incidents occurring between 2010 and 2018. Panel A of Table 1 describes the resulting matched sample, which consists of 1,049 (1,010) supplier-years (unique suppliers) and 1,319 (1,281) relationship-years (unique relationships) affected by an E&S incident.[13] In the matched sample, 645 suppliers are affected by incidents primarily related to environmental issues, and 404 are affected by incidents primarily related to social issues. Incident-affected suppliers are economically material for U.S. customers: 4.7% of the pre-incident container imports for the average customer in our sample come from incident-affected suppliers. Turning to the main focus of our analysis, 74.4% of these incident-affected suppliers face trade cuts after the incident.

In Panel B of Table 1, we break down supplier incidents by the U.S. customer Fama-French 48 industry. Industries that heavily rely on intermediate goods, such as Retail, Apparel, and Machinery, have the largest number of cases in our sample period (231, 100, and 96, respectively). However, supplier incidents are distributed across many industries: 42 out of the 48 Fama-French industries experience at least one E&S incident in our sample, and 25 industries experience more than ten incidents. The geographic span of incidents in our final sample is also diverse. Treated suppliers are located in 84 different jurisdictions, and top five jurisdictions (Mainland China, the United Kingdom, Hong Kong, Germany, and Japan) constitute only 37.1% of the full sample.

[13] We start with 4,975 supplier-year E&S incidents over the 2010-2018 period, which correspond to 6,565 supplier-customer-years and 2,288 unique customer-years. We focus on *novel* events following RepRisk's classification, and we remove repeated incidents from the sample to avoid confounding variation arising from slow news dissemination over time. After removing observations with other confounding incidents in the three years before and after the incident, we have 1,049 supplier-year events corresponding to 1,319 supplier-customer-years and 845 unique customer-years.

2.4 International Suppliers' E&S Incidents and U.S. Customers' Value

Before providing evidence on trade changes, we establish the economic relevance of supplier E&S incidents for U.S. importers by documenting stock price reactions around supplier incident announcements. We start with all E&S incidents recorded by RepRisk and remove incident observations with other confounding RepRisk incidents in the week before the incident. We then compute cumulative abnormal returns (CARs) in a [-1, +1] day window around the supplier incident for publicly listed customers that had positive trade with the affected supplier in the year before the incident.

Table 2, Panel A presents CAR estimates for the full sample of incidents (i.e., before implementing the restrictions that the incidents are novel and not confounded by other incidents in a window of $[t-3, t+3]$ years around the incident). The first row documents an average -10 basis point three-day CAR for customer stocks around the announcement of supplier incidents, statistically significant at the 1% confidence level. The second and third rows show, respectively, that the findings are statistically similar and economically larger with CAR estimation windows of [-3, +3] and [-5, +5] days around the supplier incident announcement. In Panel B, we document CARs of similar magnitude but lower statistical significance in the sample where we remove confounding incidents (i.e., the sample we use for our stacked difference-in-differences analysis). Overall, supplier incidents trigger negative customer stock price reactions and thus likely have a material downstream financial impact. In Section 5.1, we provide evidence suggesting that the negative stock price changes can be explained by upward revisions in customer firms' expected sourcing costs around supplier incidents.

2.5 Panel Structure and Estimation Strategy

In our main analysis, we use a stacked difference-in-differences regression design (e.g., Cengiz et al., 2019) to study how the imports of U.S. customers change around foreign

suppliers' E&S incidents. For each supplier incident in our sample, we construct cohorts of treated and control trade relationships in an interval of $[t-3, t+3]$ years around the incident, where t is the year of the incident. The treated sample in any given cohort consists of supplier-customer relationships in which the supplier experiences an E&S incident in year t. The control sample consists of i) relationships between affected customers (i.e., U.S. firms with at least one supplier experiencing an incident at time t) and their other suppliers not experiencing an incident in our sample period; and ii) never-treated relationships in which none of the customers' suppliers experience any E&S incident in our sample period. To mitigate potential confounding variation arising from repeated treatment over time (e.g., Baker et al., 2022), the treated group also excludes trade relationships in which the same supplier experiences other incidents in the $[t-3, t+3]$ estimation window.

Our main stacked panel contains trade observations at the customer-supplier-cohort-year level. In this stacked panel, we estimate the following regression model:

$$Y_{i,j,c,t} = \beta_1 Treat\ Supp_{j,c} \times Post_{c,t} + \gamma_{i,j,c} + \tau_{i,c,t} + \epsilon_{i,j,c,t}, \tag{1}$$

where i, j, c, and t denote customers, suppliers, cohorts, and years, respectively; $Y_{i,j,c,t}$ is a measure of trade between customer i and supplier j in year t; $Treat\ Supp_{j,c}$ indicates suppliers with an E&S incident in cohort c; $Post_{c,t}$ indicates years following the event year t in cohort c; $\gamma_{i,j,c}$ is a relationship-cohort fixed effect, which allows us to identify trade variation between the same supplier and the same customer over time; and $\tau_{i,c,t}$ is a customer-cohort-time fixed effect, which allows us to identify cross-sectional variation between treated and control groups in the same cohort and capture time-varying customer characteristics such as demand shocks.

In our main specifications, we measure $Y_{i,j,c,t}$ as the number of containers imported by customer i from supplier j in year t. We focus on containers due to their uniform

measurement, but our findings are robust to using the annual number of shipments from the supplier to the customer, the total weight of all annual shipments from the supplier to the customer, and the annual quantity of all shipments from the supplier to the customer as alternative ways to capture trade relationships.

Due to the discrete nature and zero values of the container data, we estimate model (1) with Poisson regressions (e.g., Cohn et al., 2022).[14] The main coefficient of interest is β_1, which pins down the percentage change in the number of containers imported by U.S. customers from treated suppliers after the incident, relative to those imported by either the same customers or by other customers from suppliers not experiencing any incident. To identify complete trade cuts on the extensive margin, we also measure $Y_{i,j,c,t}$ as an indicator variable for whether any container is imported by customer i from supplier j in year t. In these cases, we estimate model (1) with linear OLS regressions.

2.5.1 Identification and Control Group Choices

The identifying assumption for the coefficient β_1 to have a causal interpretation is that the incident does not affect customer-supplier trade behavior before the incident news is released, either because the incident is completely unanticipated, or because news of the incident increase its salience to U.S. customers' stakeholders who in turn induce pressure on the management. This assumption is supported by our focus on novel supplier incidents (i.e., incidents not related to previous supplier incidents in RepRisk), by our own incident search (see Appendix A.2), and by anecdotal evidence on incident opacity in global supply chains: multinational firms hire due diligence experts and data providers such as RepRisk precisely to gauge their suppliers' E&S behavior, which is otherwise difficult to monitor directly.

A related concern is that E&S incidents and trade may be systematically correlated to deteriorating supplier's operating or financial performance. In Appendix Figure B1,

[14] As shown in Appendix Table B3, our main findings are qualitatively similar when we perform log-transformations of a constant plus the dependent variable (e.g., log(1+containers)).

we plot public suppliers' operating and financial performance around the incident year using Compustat Global data. The data shows no evidence of pre-existing trends in suppliers' ROA, leverage, and investment before the incident, supporting our identifying assumption that the E&S incidents in our sample are unanticipated by customers based on observable financial and operating characteristics. In Section 5.2, we similarly provide evidence of no pre-existing trends in suppliers' E&S performance. Additionally, supplier profitability (as measured by ROA) decreases only after the incident, providing preliminary evidence that suppliers affected by an incident may not perfectly substitute away from U.S. customers after a trade cut, resulting in lower profitability. We expand on this hypothesis in Section 5.2. We also find no evidence of changes in leverage and investment around the incident, suggesting that neither the incident nor the subsequent trade cuts systematically alter suppliers' financing or investing activities.

Finally, in our baseline tests the control group in any given cohort includes the other suppliers of the affected U.S. customer as well as never-treated supplier-customer relationships. In both cases, within-customer reallocation and within-industry reputational spillovers might lead to concerns about the stability of our estimates of the coefficient β_1. In Section 5.1, we address this concern by showing that our results are statistically and economically unchanged when we explicitly control for spillovers (Berg et al., 2021). In Section 3.2, we show that our estimates are also similar when we restrict the sample to control suppliers of never treated customers, thus completely removing confounding variation arising from within-customer reallocation; and to suppliers operating in different industries than the affected suppliers, thus removing confounding variation arising from potential within-industry spillovers.

2.6 Summary Statistics

Our final stacked panel consists of 994,962 supplier-customer-cohort-year observations for 2010-2018. Panel C of Table 1 reports summary statistics for the main dependent and

independent variables. While the unconditional probability of an E&S incident is only 0.7% in terms of supplier-cohort observations treated with an E&S incident, U.S. customers have diversified supply chain structures that include many international suppliers. As a result, the average U.S. customer in our sample has a high probability of being exposed to a supplier E&S incident: only 29% of U.S. customers in our sample do not have any relationships with incident-affected international suppliers. As a comparison to these averages in our sample, Gantchev et al. (2022) find that the annual unconditional probability of a firm being directly affected by an E&S incident is lower (around 22%), which highlights the importance of indirect exposures for E&S risk management.

The next two rows of Table 1, Panel C show summary statistics for our main dependent variables: the number of containers shipped from suppliers to customers in a given year and the annual probability of a container shipment. The average supplier ships 0.942 containers to the average customer in our data, with a standard deviation of 1.308 containers per year. Similarly, the probability of any container shipment between the average supplier and the average customer in any given year is equal to 0.471, with a standard deviation of 0.499. The remainder of Table 1, Panel C provides summary statistics for the control variables of some of our empirical specifications. All control variables are lagged by one year and winsorized at the 1% and 99% levels.

3 Supplier E&S Incidents and Trade Relationships

3.1 Baseline Estimates

Table 3 reports the estimates of the regression model (1). Our baseline sample includes publicly listed U.S. customers and both publicly listed and privately held international suppliers. The first column of Table 3 reports Poisson regression estimates where the dependent variable is the number of containers shipped from an international supplier to a U.S. customer. This specification controls for relationship (i.e., customer-supplier)

pair-cohort fixed effects and customer firm-year-cohort fixed effects. In this way, we can control for time-varying customer characteristics and compare imports from suppliers directly affected by incidents to imports *by the same U.S. customers* from suppliers not directly involved in an incident.

Column (1) shows that in the three years following a supplier's E&S incident, imports by U.S. customers decline on average by 29.9% relative to imports by the same U.S. customers from unaffected suppliers.[15] These estimates are quantitatively large, and correspond to 0.282 containers per year (relative to the unconditional sample mean) and to 21.53% of a sample standard deviation. Figure 1 breaks down the estimate from column (1) into dynamic trade changes from years $t-2$ to $t+3$ of the event window, taking year $t-3$ as a baseline. This figure does not show evidence of pre-existing differences in trade across treated and control suppliers, and confirms a large and statistically significant 30% to 40% decline in container shipments following the supplier incident.

Next, we separately focus on the extensive and intensive margins of trade. On the extensive margin, we construct a binary variable equal to one if the customer has non-zero imports from the supplier in a given year. On the intensive margin, we condition on positive trade observations before estimating specification (1). We report our findings in columns (2) and (3) of Table 3, respectively. Column (2) shows that the average relationship between U.S. customers and their international suppliers is 4.3% more likely to be terminated after the supplier is involved in an E&S incident. This quantitatively large estimate implies a nearly 50% increase relative to the 9% unconditional relationship termination rate in our sample. Similarly, column (3) shows that if we condition on trade continuation and study intensive margin effects, the imports of the average U.S. customer decrease by 18.3% after a supplier's E&S incident—a 0.172 drop in annual shipments relative to the unconditional mean and to 13.18% of a standard deviation.

The intensive margin estimates in Table 3, column (3), suggest that even when a

[15]The effective sample size in our Poisson regression estimates shrinks to 939,578 observations due to the inclusion of various fixed effects.

trade relationships continues, shipments from suppliers involved in E&S incidents significantly decrease. Such partial trade cuts are rather frequent: around 44% of the trade cuts in our sample involve reduced trade but not a complete relationship termination. On the supply side, these partial trade cuts could imply a reduction in suppliers' ability to fulfil international customers' orders following an incident, a hypothesis that we examine below and find little empirical support for. On the demand side, these partial trade cuts could mean that U.S. customers start diversifying their supply chains away from affected suppliers but stop when diversification costs (related, for example, to supplier specificity or to the unavailability of competitive alternatives) exceed the benefits of reducing E&S risk exposure to the original supplier. The partial trade cuts also suggest that customers may give themselves the option of reverting to the original supplier, should the supplier's E&S performance improve.[16]

Partial trade adjustments also help us rule out the interpretation of supplier "window-dressing" (e.g., registering the supplier under a different company name, adding phantom intermediary suppliers to hide direct connections, or redacting the supplier name to customs), as in such cases it is likely that *all* imports would be redirected via the third party. Section 5.1 also shows that trade cuts and the associated supply chain adjustments have large negative effects on U.S. customers' profitability, which makes the supplier window-dressing hypothesis even less likely.

3.2 Robustness Tests

In this section we present the results of robustness tests on our baseline specifications from Table 3, column (1). For the sake of exposition, we report estimates of the coefficient β_1 in equation (1) and their associated standard errors in Table 4, and provide the full specifications and additional details in Appendix Table B4. Panel A of Table 4 shows

[16]Legal issues are unlikely to limit the ability to switch suppliers after an incident, as many supplier contracts include E&S covenants such as, for example, those from The Chancery Lane Project.

that our findings are robust to alternative measures of trade intensity: the number of individual shipments (row (1)), the total shipment weight (in tonnes, row (2)), and the total shipment quantity (the individual units in a shipment, row (3)). The estimates are consistent across different measurement choices, and row (3) shows even larger effects when we measure trade using shipment quantities.

Panel B reports estimates based on alternative matching samples. We require matching of treated and control samples based on customers' four-digit SIC industries (row (4)), on customers' four-digit SIC industries and size deciles (row (5)), and on customers' four-digit SIC industries, size deciles, and suppliers' countries (row (6)). The findings are economically and statistically robust across matching samples, confirming that the control group choice does not systematically affect our main estimates. Finally, row (7) shows that the findings are robust to restricting the sample to customer-country pairs with at least one treated and one control supplier in the same country.

Our baseline sample excludes trade relationships in which suppliers have confounding (and distinct) E&S incidents in the $[t-3, t+3]$ year window around the incident. In Panel C, we loosen this restriction and exclude suppliers without confounding incidents in a narrower $[t-2, t+2]$ year window (row (8)) and $[t-1, t+1]$ year window (row (9)). In both cases, we follow the most restrictive specification and match on customer firms' SIC industry, size deciles, and supplier country. The estimates are consistent with the previous ones.

In Panel D, we show that our findings are economically and statistically robust to alternative and less-stringent combinations of fixed effects than in our main specification (1). We include cohort-year (row (10)), cohort-year and customer-year (row (11)), cohort-year, customer-year, and supplier-cohort (row (12)), and cohort-year and pair-cohort (row (13)) fixed effects. The economic estimates of our coefficient of interest show limited variation across these specifications.

Panel E shows that our findings are robust to alternative control group choices. In

row (14), we report estimates based on a sample that only keeps the control group of *never treated* suppliers and customers, further mitigating concerns of repeated treatment over time outside the incident window (Baker et al., 2022). Since our findings are robust to removing control suppliers of the same customer, this specification further mitigates bias concerns arising from possible stable unit treatment value assumption (SUTVA) violations arising from spillovers (see also Section 5.1.1). Row (15) similarly reports estimates based on a sample in which control suppliers operate in *different industries* than treated suppliers. We additionally remove control suppliers that share the same customers with suppliers affected by E&S incidents (row (16)), and find similar effects.

Panel F shows that the findings are robust to alternative sample selection choices and empirical specifications. Row (17) shows economically larger trade cuts when we weigh each observation by pre-incident trade volume between the same customer and supplier, reducing potential concerns that our estimates may be driven by small and economically negligible suppliers. Row (18) shows that the findings are also robust to removing cohort-year $t + 1$ from the sample, which mitigates potential concerns that the partial trade cuts may reflect import timing or pre-existing contractual agreements between customers and suppliers (e.g., if the shipments are already on their way when the incident occurs). Specification (19) reports similar findings in a larger sample beyond E&S incidents that includes governance-related ("G") incidents with potential downstream reputational externalities—bribery and fraud incidents. Since cargo shipments are measured at a high frequency, in row (20) we also confirm that our findings are consistent even when we use quarterly instead of annual data. Row (21) shows that the findings are consistent if we use containers scaled by the total size of the customer's annual imports as the outcome variable. Indeed, *relative* container imports decrease by 0.006 for treated suppliers after the treatment, a 21.27% drop relative to the sample mean.

3.3 Incident Characteristics

Next, we start our analysis of customers' and suppliers' economic incentives by asking which incident characteristics are systematically correlated with the observed trade cuts. Table 5 presents estimates of cross-sectional tests on incident characteristics based on the specification reported in column (1) of Table 3. Since only treated suppliers experience incidents in any given cohort, this table separately reports estimated coefficients and χ^2-statistics for their differences in different sub-samples of treated suppliers.

Table 5 provides three sets of results. First, column (1) documents similar trade cuts across environmental ("E") and social ("S") incidents, which suggests that both incident types carry similar downstream reputational effects. Second, column (2) shows larger and statistically significant trade cuts for higher-severity incidents.[17] Third, column (3) shows larger trade cuts in the sub-sample of customers with worse stock market reactions after the incident announcement, confirming a close relationship between incidents, investor reactions, and trade adjustments.[18]

Overall, our baseline trade cut estimates are stronger for incidents more likely to generate adverse downstream reputational effects and investor reactions. While these findings are consistent with E&S motives on the customer's side, E&S incidents and trade changes could also be correlated with general business disruptions on the supplier side. For example, affected suppliers may be more likely subject to penalties and operational disruptions, leading to lower trade. Our results could also be driven by revised customer

[17] RepRisk provides a proprietary coding of incident severity. Severity is determined as a function of three dimensions: i) the consequences of the incident (e.g., health and safety incidents are ranked based on whether they have no further health consequences or whether they results in injuries or deaths); ii) the incident impact (e.g., if one person, a group of people, or a large number of people are involved in the incident); and iii) whether the incident is caused by an accident, negligence, intent, or by systematic issues. We group high-severity and medium-severity incidents into the high-severity group since RepRisk codes very few cases as high-severity.

[18] Later, we provide evidence on increased sourcing costs following trade cuts, suggesting that the announcement returns documented in Table 2 may reflect lower cash flow expectations. Arguably, customers' management could also cut trade *after* observing stock price declines and learning about investors' expectations and their E&S sensitivity (e.g., Goldstein, 2023). This distinction does not affect our conclusions and, as a result, we abstract from it in this paper.

expectations about product quality after an E&S-related incident.[19]

In Table 6, we conduct two additional sets of tests aimed at disentangling supplier business risk from U.S. customers' E&S motives. Panel A reports trade changes after G-related incidents such as compensation issues and tax avoidance, which are usually associated with regulatory penalties and operational disruptions (such as, e.g., executive turnover) but have less impact on local communities and thus arguably carry lower reputational risk for downstream customers than E&S incidents (e.g., Starks, 2023). Panel A shows no trade changes following supplier G-related incidents.

Second, we hand-collect data on suppliers' RepRisk incidents. We are able to find news for 45% of our sample cases and we sort these incidents in terms of their potential effects for the supplier's i) regulatory risk, ii) operations (e.g., labor strike or interruption in productive process), and iii) product quality (e.g., dangerous chemicals are used in production). Further details on sample construction are provided in Appendix A.2. We then study trade changes following supplier E&S incidents with low perceived impact on regulatory risk, operations, and product quality. In Panel B of Table 6, we show that the trade cuts following these incidents are statistically and economically similar to those documented in our baseline tests, suggesting that supplier business risk is unlikely to be the main driver of our findings.

3.4 Supplier Characteristics

Next, Table 7 reports how the observed trade cuts vary in the cross-section based on suppliers' characteristics. Column (1) shows that our baseline effects are similar (both economically and statistically) when suppliers are privately held and publicly listed, suggesting that foreign capital markets provide limited direct E&S pressure on suppliers. Next, we hypothesize that large suppliers have access to a larger pool of customers with

[19]Nevertheless, our sample excludes RepRisk incidents cross-cutting all ESG-related issues including those related to product deficiency, which decreases the likelihood of potential confounding variation from customers' quality concerns (Houston et al., 2022).

different E&S preferences, which may reduce the effectiveness of trade cuts as a threat mechanism. Column (2) supports this hypothesis: U.S. customers' percentage trade cuts with small suppliers in Panjiva are around three times as large as the trade cuts with large suppliers.

We also ask whether the observed effects vary with the competitiveness of customers' input market, as well as with input specificity. We hypothesize that switching costs may be relatively low when suppliers operate in competitive markets and sell homogeneous goods, resulting in stronger incentives to cut trade following an E&S incident.[20] Consistent with this hypothesis, column (3) shows that our baseline effect is significantly larger when supplier HHI is low and when suppliers sell homogeneous products. These findings suggest that exiting a trade relationship may be more difficult if customers have a limited choice set of alternative suppliers, and that the threat of a trade cut may be less effective to discipline suppliers when supplier inputs are highly specific to the customer's production process.

4 U.S. Importers and their Investors

The results from the previous section suggest that E&S shocks have real transmission effects along the supply chain network. Next, we ask whether the baseline effects documented in Table 3 vary with U.S. customers' financial characteristics, and particularly with the E&S sensitivity of their investors.

[20]We measure the competitiveness of the U.S. market from the perspective of foreign suppliers with the shipment Herfindahl-Hirschman Index (HHI) in each two-digit HS code category and event year. To calculate HHI, we take the individual shares of shipments of each international supplier to U.S. customers in each two-digit HS product category, as recorded in Panjiva. If a supplier ships more than one product category, we use the shipment-weighted average HHI of each product category. We measure a supplier's two-digit product substitutability based based on the Rauch (1999) differentiation index. If a supplier sells more than one product, we require all products to be homogeneous for indicator assignment. Our findings are robust if we instead require at least one of the products sold by the supplier to be categorized as homogeneous according to Rauch (1999).

4.1 Methodology and Financial Characteristics

To test whether the baseline effects vary with customers' characteristics and with the E&S pressure of their stakeholders, we add supplier-year-cohort fixed effects to specification (1), thus comparing responses to the *same* supplier incident by U.S. customers with different characteristics (similar to Khwaja and Mian, 2008). For example, these tests allow us to compare trade changes between a supplier involved in an E&S incident and its more profitable customers, and trade changes between the same supplier and its less profitable customers. By focusing on differential trade reactions to the same supplier incident, these tests also complement those in Section 3.3 by allowing us to test whether trade cuts are purely driven by suppliers' business risk, or whether they also vary in the cross-section based on the E&S motives of some customers and their stakeholders.[21]

We first apply this methodology to financial and operating characteristics of the customer such as customers' market-to-book, ROA, gross profit margins. We report our estimates in Table 8. The first three columns of Panel A show no cross-sectional differences in the estimates based on customers' high market-to-book (*High MTB*), high ROA (*High ROA*), or high gross profit margins (*High GrossMargin*), suggesting that average trade cuts in our sample are not systematically initiated by customers with sounder financial conditions or other characteristics correlated with profitability such as, for example, bargaining power or product quality. Appendix Table B5 similarly shows that the baseline effect does not reflect differences in financial constraints and supply chain risk diversification, which may affect supplier reallocation and its associated costs. That is, we find no systematic cross-sectional variation in customers' reactions *to the same incident* across financially unconstrained and constrained customers, nor across customers with different levels of supply chain risk diversification, which makes explanations related to general risk management capabilities of the customer less likely.

[21]The sample size in this sample drops substantially mainly because these regressions with high-dimensional fixed effects require a supplier to have at least two customers in a year. In the Appendix, we show that the main effects reported in this table hold unconditionally in the full sample.

Next, we study how the estimates vary by customers' E&S profiles. To do so, we interact the baseline treatment indicator with *High CustE&S*, a variable equal to one for customers with above-the-median E&S scores (the average of individual E and S scores from Refinitiv), and equal to zero otherwise. Column (4) of Panel A shows a significantly negative interaction effect between *Treat Supp* $\times$ *Post* and *High CustE&S*. These findings, together with those reported in the first three columns of Panel A (and in Appendix Table B5), suggest that while customers that cut trade do not systematically differ in their observable financial characteristics from others, the identified effects are concentrated among customers with better E&S performance.

4.2 Investors

The finding that better E&S scores are associated with larger trade cuts could reflect differences in holdings by E&S-sensitive investors, who may have reputational concerns and high sensitivity to E&S risks (e.g., Krueger et al., 2020) or intrinsic non-pecuniary preferences (e.g., Riedl and Smeets, 2017; Landier and Lovo, 2023). Next, we study whether our estimates vary with the E&S characteristics of the shareholder base.

We present our findings in Table 8, Panel B. First, similar to Gantchev et al. (2022) we classify E&S-sensitive investors based on the Refinitiv E&S ratings of their portfolio holdings.[22] We create an indicator variable, *High IO_E&S*, equal to one if the average proportion of the customer's outstanding shares owned by E&S-sensitive investors in the pre-incident period is greater than the sample median and equal to zero otherwise, and we interact this indicator variable with the indicator *Treat Supp* $\times$ *Post*. Column (1) of Panel B shows that the coefficient associated with *Treat Supp* $\times$ *Post* $\times$ *High IO_E&S* is negative, suggesting that U.S. firms are more likely to reduce imports from treated

[22]Like Gantchev et al. (2022), we classify investors with average portfolio ratings in the top tercile as E&S-sensitive, and the remaining investors as non-E&S-sensitive. Unlike Gantchev et al. (2022), who use the overall ESG rating provided by Refinitiv to measure a firm's E&S performance, we only use the average environmental and social (E&S) ratings to construct our measures of investor E&S sensitivity.

suppliers when their shareholders invest in firms with better E&S performance.

Second, we use shareholder proposals related to E&S issues as a direct proxy for investors' sensitivity to E&S activities. We obtain information about shareholder proposals from Institutional Shareholder Services (ISS), and we categorize proposals on socially responsible investments (SRI) as E&S proposals. For each customer in the matched sample, we then construct a binary variable, *E&S Proposal*, equal to one if the customer received at least one E&S (SRI) proposal from event year $t-3$ to event year $t-1$. Column (2) of Panel B shows that the coefficient associated with the incremental interaction term $Treat\ Supp \times Post \times E\&SProposal$ is negative and statistically significant, suggesting that customers are more likely to reduce imports from treated suppliers when their E&S-sensitive shareholders engage with portfolio firms on E&S issues, thus complementing the findings in Dimson et al. (2015). These cross-sectional findings based on shareholder proposals also suggests that the trade cuts observed in our sample unlikely the result of managerial preferences, and if anything reflect opposing incentives of shareholders and managers.

Figure 2 provides additional evidence on the dynamic effects across firms facing institutional investors with different levels of E&S sensitivity. In Panel A, we report the same trends as in Figure 1, split among customers that either receive an E&S proposal or have above-median holdings by E&S-sensitive investors in the pre-incident period. In Panel B, we report these trends for all other U.S. customers with incident-affected suppliers. Figure 2 shows that only U.S. customers with E&S-sensitive investors reduce trade around the supplier incident, and no evidence of trade cuts for customers without E&S-sensitive investors.

Finally, we test whether U.S. customer firms that are publicly listed (and hence more likely exposed to E&S-sensitive investors) experience larger trade reductions compared to privately held firms. To perform this test, we expand our stacked Panjiva-RepRisk panel to include the universe of Panjiva U.S. customers that are not publicly traded, and

we create a customer firm-year indicator variable, *Public Cust*, equal to one if the stocks of the customer's ultimate parent are publicly traded in the incident year, and equal to zero otherwise. Column (3) of Panel B shows that the interaction coefficient between the baseline treatment effect indicator, $Treat\ Supp \times Post$, and the indicator for publicly listed customers, *Public Cust*, is negative, statistically significant at the conventional levels, and implies an incremental 19.1% post-incident trade cut by publicly listed firms. These findings suggest that in response to the same E&S incident, public firms reorganize their supply chains more actively than privately held firms, and provide further evidence that institutional investor preferences likely play an important role in trade adjustments.

The findings of column (3) also add to the ongoing debate on the E&S-related costs and benefits of being publicly listed. Specifically, they highlight one of the potential benefits of being private: reorganizing supply chains after an E&S incident can be costly for U.S. customers (as we confirm in our tests below), and privately held customers may be more shielded from these costs than their publicly held peers. Our findings also imply that the current trend of public firms' delistings in the U.S. (e.g., Doidge et al., 2017; Ewens and Farre-Mensa, 2020) could result in an overall decrease in E&S performance around the globe if these delistings are accompanied by lower pressure to discipline international suppliers' E&S performance.

In Appendix Table B6, we provide a series of robustness tests. First, a possible concern with our interpretation of the findings in Panel B of Table 8 is that suppliers with multiple customers and their incidents might be inherently different from those with a single customer, and that, as a result, the estimated effects might be different in our baseline sample (where we do not condition on suppliers having more than one customer prior to the incident). However, Appendix Table B6 shows that our estimates also hold in the baseline sample. Second, by sorting customers based on their investors' E&S sensitivity we may also be sorting on other correlated customer characteristics. For example, E&S-sensitive investors could invest in firms with better financial performance,

and hence less constrained to search for new suppliers. However, our triple-interaction estimates also hold when controlling for market-to-book ratio, ROA, gross margins, size, and their interactions with the treatment indicator.[23] This finding confirms that cross-sectional variation in investor preferences *orthogonal to operating and financial performance* explains a large fraction of the documented trade cuts.

Overall, Table 8 and Appendix Tables B5 and B6 suggest that trade adjustments following suppliers' E&S incidents are correlated with investors' sensitivity to E&S above and beyond customers' operating and financial performance. These within-supplier-cohort-year findings provide additional evidence against the interpretation that the observed trade cuts only reflect changes in suppliers' business or financial risks orthogonal to E&S considerations. For example, suppose that the documented trade cuts arise only from a change in (perceived) product quality following an incident. For this to be the main economic driver of our findings, it would need to be the case that customers with E&S-sensitive investors systematically care about product quality more than customers with less E&S-sensitive investors, but that, at the same time, customers with high profitability and market-to-book ratios do not perceive such quality effects differently than customers with low profitability and market-to-book ratios. We view this potential scenario as unlikely. With these findings, we can also rule out government-imposed trade cuts (e.g., withhold-and-release orders by customs over forced labor) as a potential explanation for our findings as those should also hold uniformly across U.S. customers.

5 Costly Reallocation and Trade Reversals

We conclude our analysis by focusing on the long-term effects of supplier E&S incidents on trade relationships. First, we study costly reallocation to other international suppliers. Second, we show that customer exit can discipline supplier E&S performance.

[23] Financial data is unavailable for private customers in our sample, making this robustness test unfeasible for column (3) of Panel B.

5.1 Supplier Reallocation and the Costs of Cutting Trade

How do U.S. customers readjust their supply chains following a supplier E&S incident? We ask whether U.S. customers switch to other international suppliers and, if so, whether the new suppliers are from the same country as the supplier involved in the E&S incident. We also study whether supplier cuts and switches are associated with changes in U.S. customers' cost structure.

5.1.1 Reallocation and New Suppliers

To identify reallocation effects, we follow Berg et al. (2021) and estimate the model:

$$\begin{aligned} Y_{i,j,c,t} \;=\; & \beta_1 Treat\ Supp_{j,c} \times Post_{c,t} + \beta_2 \%Treat\ Supp_{i,c} \times TreatSupp_{j,c} \times Post_{c,t} \\ & + \beta_3 \%Treat\ Supp_{i,c} \times TreatCust,\ Control\ Supp_{j,c} \times Post_{c,t} \\ & + \beta_4 X_{i,t-1} + \gamma_{i,j,c} + \tau_{c,t} + \epsilon_{i,j,c,t}, \end{aligned} \tag{2}$$

where $\%Treat\ Supp_{i,c}$ denotes the fraction of suppliers affected by an E&S incident in each customer-cohort, measured in the year before the shock; $Treat\ Cust,\ Control\ Supp_{j,c}$ is an indicator for control suppliers of customers with at least one supplier affected by the E&S incident; $X_{i,t-1}$ is a matrix of lagged control variables; and the remaining variables are identical to those in specification (1).

The coefficient of interest in specification (2) is β_3. This coefficient identifies reallocation to control suppliers that share a customer link with at least one treated supplier, while also controlling for potential spillover effects on other treated suppliers (pinned down by the coefficient β_2). As in Berg et al. (2021), β_3 identifies marginal post-treatment changes in trade between control suppliers and customers linked to treated suppliers for a marginal increase in the fraction of treated suppliers in the cohort.[24] We expect the sign

[24]Unlike specification (1), specification (2) includes less-restrictive sets of fixed effects, which allow us to estimate β_2 and β_3 separately (see Berg et al., 2021). Berg et al. (2021) focus on direct treatment spillovers to control and treated groups rather than indirect spillovers through the network, as we do here. In this

of this coefficient to be positive if customers switch from suppliers with E&S incidents to other international suppliers (rather than to North American suppliers not covered by the Panjiva maritime customs data or to in-house production).

The findings in Table 10 support these predictions. Column (1) documents a negative and statistically significant 29% post-treatment decline in trade after a supplier incident, confirming the stability of our baseline treatment effect estimates after explicitly controlling for spillover effects (Berg et al., 2021). The third row of column (1) also documents a positive and statistically significant reallocation effect on control suppliers: for example, the average fraction of treated suppliers in a given cohort is 0.73% (standard deviation 1.8%), such that a one-standard-deviation increase in the share of treated suppliers in the cohort raises trade between their linked customers and *control* suppliers by $1.8\% \times 1.596 = 2.87\%$. In other words, U.S. customers partially replace their incident-affected suppliers with other international suppliers. Finally, column (1) shows no spillover effects within the treated group, suggesting that the extent of trade cuts with treated suppliers is independent of other treated suppliers' incidents.

Next, we ask whether U.S. customers switch to suppliers from the same country as treated suppliers, or to suppliers from other countries. On the one hand, switching to suppliers from the same country may be less costly. On the other hand, the supplier's E&S incident might hurt the reputation of all suppliers in the country and motivate customers to find new partners in other countries to diversify their supply chain risks. We thus split the indicator *Treat Cust*, *Control Supp*$_{j,c}$ into two indicator variables: *Treat Cust*, *Control Supp*, *Same Country*$_{j,c}$, indicating control suppliers (of customers linked to treated suppliers) from the same country as the treated supplier, and *Treat Cust*, *Control Supp*, *Diff Country*$_{j,c}$, indicating control suppliers located in other countries. Column (2) of Table 10 shows that the reallocation effects appear only in the sample of suppliers from other countries, suggesting that E&S incidents can have

sense, our estimation strategy also resembles the reallocation specifications of Giroud and Mueller (2019).

negative reputational spillovers within a country and providing a first piece of evidence that reallocation may be costly.

Finally, we ask whether customers switch to suppliers with high RepRisk ESG ratings by splitting the indicator *Treat Cust*, *Control Supp*$_{j,c}$ into two indicator variables for control suppliers with average ESG rating before the incident in the top quintile of the distribution (*Treat Cust*, *Control Supp*, *High Supp ESG*$_{j,c}$), and in the bottom four quintiles of the distribution (*Treat Cust*, *Control Supp*, *Low Supp ESG*$_{j,c}$). Although our sample shrinks considerably due to the lack of ESG rating data for all international suppliers, column (3) shows a statistically significant negative baseline treatment effect and a positive spillover effect only on suppliers with high ESG ratings.[25] Thus, U.S. customers seem to switch to international suppliers with low expected future E&S incident exposure after one of their suppliers is affected by an E&S incident.

Table 10 focuses on existing supplier-customer relationships. Appendix Table B7 shows that, relative to the control group, U.S. customers with incident-exposed suppliers also increase the number of *new suppliers* by around 20.6% and the number of *new sourcing countries* by around 16.2% after the incident. Collectively, our findings suggest that customers readjust supply chains on both the intensive and the extensive margins.

5.1.2 The Costs of Cutting Trade

Table 11 provides evidence suggesting that re-optimizing supply chains after a supplier's E&S incident is costly for U.S. importers. Customers that cut trade with suppliers affected by an E&S incident show 0.9% lower profitability (as measured by their gross profit margins) after the incident. Meanwhile, customers that do not implement trade cuts do not experience statistically significant changes in their gross profit margins after

[25] We do not use Refinitiv data as we do in our customer-level tests as Refinitiv only covers publicly listed firms. RepRisk provides a unique ESG rating rather than individual E&S ratings, updated daily based on negative news in the media. The ESG rating ranges from AAA (best) to D (worst), and is widely used by asset managers to monitor the ESG performance of their portfolio (see, e.g., `corpgov.law.harvard.edu`). Since the RepRisk rating is typically a function of past incidents, we can interpret our evidence as customers switching to suppliers with low historical incident risk.

the incident. These findings are consistent with increased costs of goods sold (arising, e.g., from more expensive supplier sourcing) or constraints in production (arising, e.g., from lack of alternative inputs), and complement our event study results by suggesting that the negative stock market reactions documented in Section 2.4 may be partly due to revised cash flow expectations. By showing that E&S risk management is costly, these findings also contribute to policy debates and recent academic work (e.g., Schiller, 2018; Koenig and Poncet, 2022; and Dai et al., 2021a,b) mostly highlighting the benefits of sustainable supply chains.

5.2 Supplier E&S Improvements and Trade Reversals

Our results so far suggest that cross-sectional variation in investor E&S composition is strongly correlated with trade cuts. However, trade cuts, especially partial ones, do not seem an obvious response to E&S reputation. On the one hand, if investors with reputational concerns and high aversion to E&S risks (e.g., Krueger et al., 2020; Hoepner et al., 2024) want to disassociate portfolio firms from problematic suppliers, why are the trade cuts only *partial* for around half of the supplier incidents in our sample? On the other hand, if importers and their stakeholders intrinsically care about E&S issues (e.g., Riedl and Smeets, 2017; Landier and Lovo, 2023), why do they diversify away from the supplier rather than fix the underlying issue? We conclude our long-run analysis by studying adjustments on the supplier side.

Table 12, Panel A, reports the dynamics of treated and control suppliers' average RepRisk ESG ratings around E&S incidents. The RepRisk ratings of suppliers affected by E&S incidents show large and statistically significant decreases after the incident relative to the control group, which last for three years on average. Since the ESG ratings of treated and control suppliers are economically and statistically similar before the incident, Panel A alleviates concerns that supplier E&S characteristics are already deteriorating before the incident, thus complementing the findings reported in Section 3.4

and further supporting our identifying assumptions.

Next, we study whether import cuts by U.S. customers trigger adjustments in suppliers' E&S performance and subsequent trade. We proceed in two steps. First, we restrict the sample to customer-supplier relationships in which the supplier experienced an E&S incident (i.e., the treated relationships in our main sample), and we study whether large trade cuts are followed by changes in the supplier's RepRisk ESG rating. In these tests, we limit the sample to suppliers that have RepRisk ESG ratings around the initial incident. Second, we ask whether U.S. customers' trade cuts and international suppliers' ESG rating improvements are jointly associated with future trade reversals.

Table 12, Panel B, reports the dynamic response of suppliers' RepRisk ESG ratings following trade cuts by U.S. customers. In this panel, we test whether a supplier's post-incident ESG rating varies with the extent of customers' trade cuts in a window of three years (year $t-1$ to year $t+1$) around the E&S incident. For each foreign supplier, we aggregate export changes around the E&S incident across all U.S. customers and then split the sample based on the distribution of aggregate trade changes. Column (1) corresponds to the subsample of suppliers experiencing the largest negative trade changes (the 25th percentile of the aggregate distribution, corresponding to an overall trade change of -67% over the three years around the incident); column (2) corresponds to the subsample of suppliers experiencing a trade change within the interquartile range; and column (3) corresponds to the subsample of suppliers experiencing the smallest drop in trade in our sample (i.e., trade changes above the 75th percentile).

Consistent with Panel A, Panel B shows that on average, RepRisk ESG ratings decrease after the E&S incident, and this pattern persists over time. However, the negative effect of the incident on ESG ratings is statistically and economically short-lived (column (1)) when U.S. customers significantly cut trade with affected suppliers. Indeed, ratings recover after year $t+2$, suggesting that significant losses in foreign revenues may force international suppliers to improve their E&S performance. Such effects are more delayed

and generally weaker for smaller trade cuts (columns (2)-(3)).

Next, we ask whether improved ESG ratings are associated with trade reversals. We group treated and control relationships into cohorts of $[t+3, t+6]$ years from the supplier's initial E&S incident, classifying observations in years $[t+1, t+3]$ relative to the incident as "post-incident" observations in which suppliers may adjust their E&S policies, and observations in years $[t+4, t+6]$ from the incident as "post-adjustment" observations. We then split treated relationship cohorts into subsamples based on i) different distributional cuts of total trade changes ($\Delta Trade$) between the "pre-incident" ($[t-3, t-1]$) and post-incident ($[t+1, t+3]$) periods, and ii) changes of affected suppliers' ESG ratings during the post-incident period.[26] The independent variables thus include four mutually exclusive interaction terms between indicator variables for customer trade cuts between the pre- and post-incident periods ($Cut\ Trade = 1$), and supplier rating improvements in the post-incident period ($Inc\ Rating = 1$). *Cut Trade* is equal to one if $\Delta Trade$ is negative (column (1)), if $\Delta Trade$ is less than -25% (column (2)), and if $\Delta Trade$ is less than 67% (column (3)).[27]

Panel C of Table 12 presents our findings. First, the joint presence of customer trade cuts and supplier ESG rating improvements is associated with subsequent trade reversals, and these trade reversals are increasing in the original trade cut. Relative to the control group, trade cuts, cuts below the 25th percentile, and cuts lower than 67% are associated with relative increases between the *post-incident* and the *post-adjustment* period of 49.9%, 83.4%, and 115.3%, respectively.[28] Second, *only* the joint presence of trade cuts and ESG rating improvements is associated with subsequent reversals: we find no

[26] To simplify the analysis, we focus on absolute trade cuts within the same relationship relative to the pre-incident period, as opposed to trade cuts relative to relationships in the control group. On average, trade with control group suppliers increases after the incident (as documented in Section 3.4), so absolute trade cuts are smaller than relative trade cuts.

[27] This test is similar to a quadruple difference-in-differences test with cross-sectional cuts based on initial trade cuts and subsequent trade reversals, which would require data on control suppliers' ESG ratings and thus would further limit the sample. In this respect, a causal interpretation of the findings of Panel C is limited.

[28] These estimates are based on a subsample of treated firms with different average treatment effects than those reported in Table 3, and thus might not be directly comparable to our main estimates.

evidence of a trade increase in the post-adjustment period if customers' trade cuts are not followed by supplier ESG rating improvements, nor if trade was not cut after the E&S incident to begin with. Collectively, the findings in Table 12 support the interpretation that U.S. customers may use real trade activity as a mechanism to discipline their suppliers' E&S performance.[29]

6 Retail Consumers

We conclude by examining a likely and complementary alternative to investors' E&S sensitivity as the main driver of the observed trade cuts, namely, that U.S. importers face pressure to readjust their supply chains from their own end consumers (Bénabou and Tirole, 2010). To test this hypothesis, we use Nielsen scanner data (henceforth Nielsen) to study whether the quantities and prices of retail products sold by U.S. importers change after their international suppliers are exposed to an E&S incident.

We first match Nielsen manufacturers to their Compustat ultimate holder using a combination of fuzzy and manual name-matching procedures, and we keep Compustat firms that appear as a match. Second, we expand the resulting sample to include information on average prices and quantities of retail products sold by the U.S. importers in each Zip-3 code, product category (also known as module), and quarter. Third, we merge the resulting dataset with our main sample, collapsed at the customer firm-year-cohort level. In this collapsed panel, *customer events* are years in which at least one of the U.S. customer's suppliers is affected by an E&S incident, and the control group consists of U.S. customers with no suppliers affected by E&S incidents. Fourth, we perform stacked difference-in-differences tests around customer events to study the effect of sup-

[29] One potential concern is that RepRisk's ESG ratings are based on RepRisk's incidents (`www.reprisk.com`) and their reversal may be mechanically driven by the exclusion of repeated incidents from our sample. However, only a subset of all the incident-exposed suppliers in our sample faces rating reversals, which makes this explanation unlikely. Additionally, Appendix Table B8 shows similar trade reversals for a subset of suppliers based on their Sustainalytics E&S ratings, further mitigating this concern.

plier incidents on total quantities and average prices of products sold by the importers.

Table 9 reports our findings. In the first two columns, we use the natural logarithm of the total quantities sold by the U.S. importer in a given product module and Zip-3 area as the dependent variable, controlling for firm $\times$ Zip-3 $\times$ product module $\times$ cohort fixed effects and Zip-3 $\times$ product module $\times$ quarter $\times$ cohort fixed effects, so that we achieve identification from both time-series and cross-sectional variation in the data. In column (1), we report the average treatment effect, and in column (2) we report its dynamics. Our findings show that local quantities sold by U.S. importers decrease by around 8% after the supplier incident, and the effect persists for around two years, suggesting that the incident may negatively affect U.S. customers' sales.

The granularity of the Nielsen data allows us to ask whether the estimates of the first two columns of Table 9 are more likely due to changes in customer demand (due, e.g., to consumer boycotts) or to changes in importers' supply (due, e.g., to lower imports from the incident-affected supplier and imperfect reallocation to other suppliers). In the former case, we might expect importers' product prices to stay constant or even decrease if U.S. customers have to lower product prices to attract the marginal retail consumer. In the latter case, we might expect prices to increase if the new suppliers are more expensive and some of this increased cost is passed through to the consumer. Columns (3) and (4) of Table 9 provide evidence consistent with the second interpretation: the price of goods sold by U.S. customers whose suppliers are affected by an E&S incident increases by around 4.5% on average after the incident, and the effect persists for around three years.

Overall, the findings reported in this section provide limited support to the interpretation that retail consumers directly react to supplier E&S incidents and, as a result, it is unlikely that U.S. customers reduce reliance on these suppliers in response to retail pressure. These findings contribute to the current debate on whether end consumers respond to and can affect retailers' E&S posture (e.g., Houston et al., 2022; Liaukonytė et al., 2022; Handziuk and Lovo, 2023; Meier et al., 2023). Consistent with Christensen et al. (2023),

our estimates suggest that information salience (which is very low in our sample, where less than less than 3% of the RepRisk incidents reach the international news) is necessary for end-consumers' actions to have an impact on corporate E&S profiles.

7 Discussion and Conclusions

We provide empirical evidence on how U.S. firms adapt their global supply chains after their international suppliers become involved in E&S incidents. We use data on the universe of cargo imports by U.S. firms based on declarations to the U.S. Customs and Border Protection over 2007-2020 to study how international suppliers' E&S incidents affect their future trade relationships with U.S. customers.

We document partial trade adjustments. Shipments from affected suppliers decrease by 29.9% compared to those from unaffected suppliers. Customers switch to other suppliers, especially to those in other countries, but do not always terminate their relationships. Trade reverses over the long run if the supplier's E&S performance improves after trade is cut, suggesting that partial trade adjustments can be an effective mechanism to discipline suppliers. In the cross-section, the effects are stronger when the institutional investors of publicly listed customers are more sensitive to E&S, and they are smaller for privately held than for publicly listed customers. This finding adds to the debate on the ESG-related benefits and costs of being public.

Our paper offers three implications. First, the option to (partially) cut trade also suggests benefits of having suppliers outside the firm's boundaries. While customers could in theory pick an alternative supplier rather than engage with the current supplier to fix the underlying issue, we find that the option of quitting the relationship creates an actionable threat that can improve the supplier's performance. Such option is more costly and may even be impossible to implement if the firm is vertically integrated.

Second, an aspect of the theory of the firm suggested by this paper is that publicly

listed U.S. firms with foreign suppliers might be attractive for impact investors interested in affecting the E&S performance of foreign firms. Such suppliers might be outside of U.S. investors' investment universe (e.g., be privately held or thinly traded), and indirect influence through U.S. customers could be more effective than direct investor engagement with individual foreign suppliers (Landier and Lovo, 2023).

Third, our findings speak to the policy debate on regulatory outsourcing of global supply chain monitoring activities. International suppliers' E&S activities are beyond the reach of domestic governments. However, governments can impose domestic supply chain regulations to gain extraterritorial reach. One recent example of "regulatory outsourcing" is the Dodd-Frank Wall Street Reform and Consumer Protection Act's section 1502 on conflict minerals, with which the U.S. government forces multinationals to indirectly regulate firms along their supply chains (Sarfaty, 2015).[30] Consistent with the arguments in Bénabou and Tirole (2010), we show that the pressure of some stakeholders induces firms to actively manage their suppliers' E&S profiles even outside government-regulated sectors such as minerals. If firms face sufficient E&S pressure by these stakeholders, such private regulation has the potential to complement government regulation in improving global E&S standards.

[30]See Christensen (2022) and Baik et al. (2022) for a discussion on the effectiveness of this legislation. A related regulation is the California Transparency in Supply Chains Act 2010, which requires businesses to disclose whether and to what extent they proactively address slavery and human trafficking in their supply chains. This act applies to retail sellers and manufacturers of goods doing business in California that have worldwide gross receipts of USD $100 million or more, irrespective of their domicile. See She (2022) for a study of the real effects of this act.

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Figure 1: Dynamic Effects of Supplier E&S Incidents on International Trade

This figure displays the dynamic effects of supplier E&S incidents on international trade. To estimate the dynamic effects of E&S incident exposure, we replace the *Treat Supp*$\times$*Post* indicator from specification (1) with interaction terms between the *Treat Supp* indicator and event year indicators from $t-2$ to $t+3$ around event year t, taking event year $t-3$ as our baseline. We focus on trade relationships starting on or before year $t-3$, and we remove the cohort of 2018 to ensure that all cohorts have three years of observations in the post-incident period. In this figure, we plot the estimated interaction coefficients and their associated 90% confidence intervals.

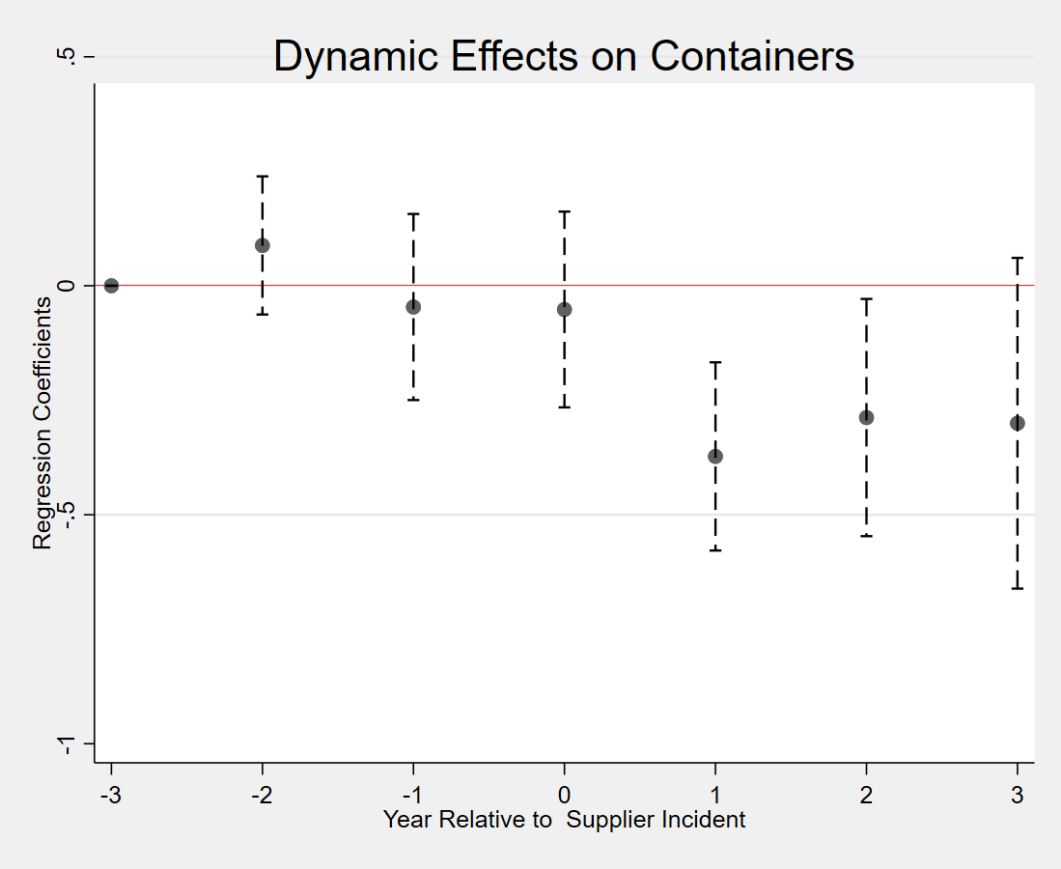


Figure 2: Investor E&S Sensitivity and Trade Dynamics

This figure displays the dynamic effects of supplier E&S incidents on trade in two sub-samples of U.S. customers with different levels of investor E&S sensitivity. To estimate the dynamic effects of E&S incident exposure, we replace the *Treat Supp*$\times$*Post* indicator from Specification (1) with interaction terms between the *Treat Supp* indicator and event year indicators from $t-2$ to $t+3$ around event year t, taking event year $t-3$ as our baseline. As in Figure 1, we focus on trade relationships starting on or before year $t-3$, and we remove the cohort of 2018 to ensure that all cohorts have three years of observations in the post-incident period. Panel A plots the dynamic effects in the sub-sample of U.S. customers that either received an E&S-related proposal or have above-median E&S-sensitive investor holdings in the pre-incident period. Panel B plots the dynamic effects in the remaining sub-set of U.S. customers with incident-affected suppliers. In this figure, we plot the estimated interaction coefficients and their associated 90% confidence intervals.

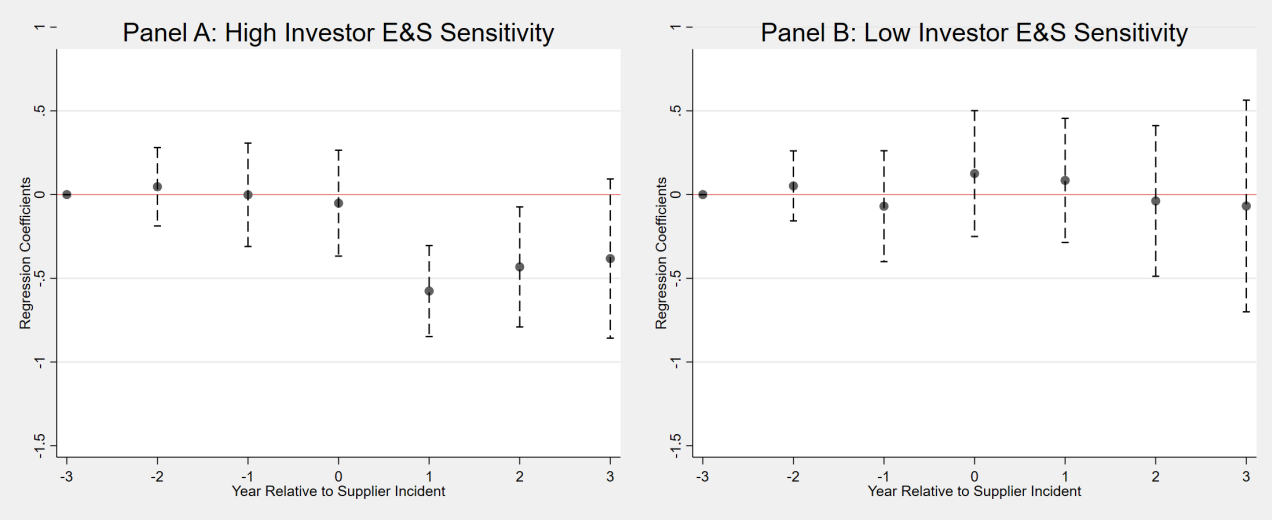


Table 1: Summary Statistics

Panel A reports the sample distribution across cohorts (i.e., event years of supplier incidents). Panel B reports the distribution of treated relationships across the Fama-French 48 industries of customers. Panel C reports descriptive statistics for the variables used in our main analyses. The definitions of the variables are provided in Appendix Table B1.

Panel A: Sample Distribution

Cohort	#Relationships	#Treated Suppliers	#Treated Relationships	#Customers	#Affected Customers
2010	19,586	76	88	848	57
2011	18,470	74	84	799	56
2012	27,524	129	166	802	107
2013	21,215	103	133	789	83
2014	23,945	131	175	794	106
2015	26,217	135	180	786	109
2016	29,536	142	173	771	112
2017	24,702	121	149	772	112
2018	22,213	138	171	697	103
All	60,305	1,010	1,281	1,515	434

Panel B: Distribution of Treated Relationships by Customer Industry

FF48 Industry	Freq.	FF48 Industry	Freq.
Agriculture	4	Aircraft	20
Food Products	28	Defense	1
Candy & Soda	1	Precious Metals	1
Tobacco Products	1	Non-Metallic and Industrial Metal Mining	1
Recreation	25	Petroleum and Natural Gas	47
Printing and Publishing	13	Personal Services	2
Consumer Goods	55	Business Services	26
Apparel	100	Computers	56
Healthcare	1	Electronic Equipment	75
Medical Equipment	8	Measuring and Control Equipment	22
Pharmaceutical Products	37	Business Supplies	31
Chemicals	78	Shipping Containers	3
Rubber and Plastic Products	5	Transportation	35
Textiles	16	Wholesale	65
Construction Materials	13	Retail	231
Construction	3	Restaraunts, Hotels, Motels	9
Steel Works Etc	33	Banking	15
Fabricated Products	2	Insurance	1
Machinery	96	Trading	1
Electrical Equipment	23	Other	18
Automobiles and Trucks	79		

Table 1: Summary Statistics (Continued)

Panel C: Summary Statistics of Variables

Variable	Obs.	Mean	Std. Dev.	P25	P50	P75
Treat Supp	994,962	0.007	0.084	0.000	0.000	0.000
Treat Cust, Control Supp	994,962	0.712	0.453	0.000	1.000	1.000
Post	994,962	0.560	0.496	0.000	1.000	1.000
Container	994,962	8.543	27.061	0.000	0.000	4.000
1 (Trade>0)	994962	0.472	0.499	0.000	0.000	1.000
Size	994,962	8.421	2.251	6.848	8.296	9.813
MTB	994,962	1.362	1.199	0.516	1.075	1.741
Lev	994,962	0.221	0.166	0.088	0.225	0.308
R&D	994,962	0.020	0.040	0.000	0.000	0.026
Capx	994,962	0.045	0.031	0.020	0.038	0.063
Cash	994,962	0.128	0.113	0.041	0.095	0.182

Table 2: Customers' Stock Market Reactions Around Supplier Incidents

This table shows U.S. customers' stock market reactions around international suppliers' E&S incidents. We start with all E&S incidents recorded in the RepRisk data, and remove incidents with confounding events in the week before the incident. Panel A reports the estimates for all incidents covered by the RepRisk data. Panel B reports estimates for incidents in our main sample. CAR $[-\tau, +\tau]$ is the cumulative abnormal return for customers' stocks from day $-\tau$ to day $+\tau$, taking day 0 as the incident announcement date. Abnormal returns are estimated using the market model in $[-200, -60]$ trading day windows before the event (e.g., Chen et al., 2007; Qiu and Wang, 2018). We require a minimum of 60 days in the estimation window, and winsorize all variables at the 1% and 99% levels. Standard errors for the *t*-test of the null hypothesis that the average CAR is equal to zero are clustered at the supplier-level.

Panel A: Entire RepRisk Sample

	Obs.	Mean (%)	Median (%)	*t*-stat: Mean = 0
CAR [-1,+1]	9,957	-0.10%	-0.08%	-2.79
CAR [-3,+3]	9,957	-0.19%	-0.08%	-2.78
CAR [-5,+5]	9,957	-0.19%	-0.07%	-2.47

Panel B: In-sample Incidents

	Obs.	Mean (%)	Median (%)	*t*-stat: Mean = 0
CAR [-1,+1]	1,057	-0.15%	-0.02%	-1.38
CAR [-3,+3]	1,057	-0.27%	-0.01%	-1.71
CAR [-5,+5]	1,057	-0.46%	-0.20%	-2.39

Table 3: The Effect of Supplier E&S Incidents on Trade

This table shows the effect of supplier E&S incidents on trade relationships. The dependent variable in column (1) is *Containers*, defined as the number of containers received by a U.S. customer from a given supplier over the year. The dependent variables in columns (2) and (3) are *1(Trade>0)* and *Containers*, respectively. Column (3) requires a relationship-cohort-year to have a positive amount of trade to be included in the regression sample. Columns (1) and (3) are estimated using Poisson regressions. Column (2) is estimated using OLS regressions. All specifications control for relationship×cohort and customer firm×year×cohort fixed effects. All the variables are defined as in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Containers	1(Trade>0)	Containers
		Extensive Margin	Intensive Margin
	(1)	(2)	(3)
Treat Supp×Post	-0.299***	-0.043***	-0.183**
	(0.083)	(0.014)	(0.079)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	939,578	939,578	412,184
Pseudo. R^2	0.719		0.787
Adjusted R^2		0.134	

Table 4: Summary of Robustness Tests on Baseline Results

This table shows the estimates of robustness tests for the main estimates of the coefficient β_1 in Table 3, column (1). Full specifications and additional details are provided in Appendix Table B4. Panel A reports Poisson regressions using alternative measures of trade as dependent variables. Panel B reports results based on alternative matching samples of treated and control observations. Panel C reports the findings under alternative approaches to deal with confounding incidents. Panel D reports the findings using alternative fixed effects. Panel E reports the findings using alternative control groups. Panel F reports the findings using alternative samples and specifications. The dependent variable in all rows but (1) to (3) and (21) is *Containers*. All specifications (except specification (21), which is estimated using OLS regression) are estimated using Poisson regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

	$\hat{\beta}_1$	Std. Err.
Panel A: Alternative Trade Measures		
1. Shipment	-0.241***	(0.081)
2. Weight	-0.260***	(0.080)
3. Item	-0.333***	(0.117)
Panel B: Matching Sample		
4. Industry	-0.301***	(0.084)
5. Industry, size	-0.304***	(0.084)
6. Industry, size, supplier country	-0.294***	(0.105)
7. Firm-countries with both treated and control suppliers	-0.329***	(0.097)
Panel C: Alternative Restrictions on Confounding Incidents		
8. Two-year window	-0.231***	(0.071)
9. One-year window	-0.115**	(0.056)
Panel D: Alternative Fixed Effects		
10. Year-cohort	-0.184**	(0.083)
11. Year-cohort, firm-cohort	-0.227***	(0.083)
12. Year-cohort, firm-cohort, supplier-cohort	-0.309***	(0.083)
13. Year-cohort, pair-cohort	-0.276***	(0.083)
Panel E: Alternative Control Groups		
14. Control suppliers of never treated customers in different industries	-0.180**	(0.083)
15. Treated customers' unaffected suppliers selling different products and control customers' suppliers selling different products	-0.475***	(0.108)
16. Control customers' suppliers selling different products	-0.272***	(0.093)
Panel F: Alternative Samples and Specifications		
17. Weighted regression	-0.345**	(0.129)
18. Remove year t+1	-0.272***	(0.085)
19. Including corruption, bribery, fraud	-0.217***	(0.077)
20. Quarterly data	-0.286***	(0.080)
21. Scaled by size	-0.006***	(0.002)

Table 5: Cross-sectional Tests: Incident Characteristics

This table shows cross-sectional estimates based on incident characteristics. The dependent variable is *Containers*. Column (1) partitions incidents as primarily related to environmental issues (*Treat Supp, E*) and social issues (*Treat Supp, S*), and reports the difference in the two estimated coefficients as well as its χ^2-statistics. Column (2) partitions incidents into high-severity (*Treat Supp, High Severity*) and low-severity (*Treat Supp, Low Severity*). Column (3) partitions customers into a group with high negative market reaction to supplier incidents (*High Reaction*) and a group with low negative market reaction to supplier incidents (*Low Reaction*). All specifications control for relationship×cohort and customer firm×year×cohort fixed effects. All columns are estimated using Poisson regressions. Variable definitions are in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Containers		
	(1)	(2)	(3)
Treat Supp, E×Post ($\hat{\beta}_1$)	-0.319***		
	(0.105)		
Treat Supp, S×Post ($\hat{\beta}_2$)	-0.262*		
	(0.136)		
Treat Supp, High Severity×Post ($\hat{\beta}_1$)		-0.435***	
		(0.120)	
Treat Supp, Low Severity×Post ($\hat{\beta}_2$)		-0.168	
		(0.109)	
Treat Supp, High Reaction×Post ($\hat{\beta}_1$)			-0.537***
			(0.139)
Treat Supp, Low Reaction×Post ($\hat{\beta}_2$)			-0.226**
			(0.101)
$\hat{\beta}_1 - \hat{\beta}_2$	-0.057	-0.267*	-0.311*
(χ^2)	(0.11)	(2.72)	(3.18)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	939,578	939,578	939,578
Pseudo R^2	0.719	0.719	0.719

Table 6: Supplier Business Risk and Trade

In this table, we ask whether our baseline effects are purely driven by supplier business risk. Panel A shows the effect of supplier governance incidents (other than those with potential reputational externalities, i.e., corruption, bribery and fraud) on trade relationships. The dependent variable in column (1) is *Containers*. The dependent variables in columns (2) and (3) are *1(Trade>0)* and *Containers*, respectively. Column (3) requires a relationship-cohort-year to have a positive amount of trade to be included in the regression sample. Columns (1) and (3) are estimated using Poisson regressions. Column (2) is estimated using OLS regressions. Panel B shows the effect of supplier E&S incidents on trade relationships after excluding incidents with operational impacts. The dependent variable is *Containers*. We exclude relationships in which at least one of the incidents likely leads to penalties for the supplier (column (1)), disrupts the supplier's operations (column (2)), and is related to product quality issues for the supplier (column (3)), as described in Appendix A.2. All specifications control for relationship×cohort and customer firm×year×cohort fixed effects. All columns are estimated using Poisson regressions. Variable definitions are in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Panel A: Supplier Governance-related Incidents and Trade

Dep. Var. =	Containers	1(Trade>0)	Containers
		Extensive Margin	Intensive Margin
	(1)	(2)	(3)
Treat Supp, G×Post	0.091	-0.015	0.058
	(0.110)	(0.022)	(0.092)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	939,578	939,578	412,184
Pseudo. R^2	0.719		0.787
Adjusted R^2		0.134	

Panel B: E&S Incidents without Operational Impact

Dep. Var. =	Containers		
	No Penalty	No Operational Disruption	No Product Quality Impact
	(1)	(2)	(3)
Treat Supp×Post	-0.255**	-0.257**	-0.350***
	(0.125)	(0.122)	(0.113)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	602,638	557,168	619,464
Pseudo R^2	0.727	0.728	0.729

Table 7: Supplier Characteristics and Switching Costs

This table shows cross-sectional estimates based on supplier characteristics and switching costs. The dependent variable is *Containers*. Column (1) includes the interaction term *Public Supplier*, which partitions the sample into publicly listed and privately held suppliers. Column (2) include the interaction term *Small Supplier*, which partitions the sample into small and large suppliers. Column (3) includes the interaction term *High Substitutability*, which partitions suppliers into a group with high substitutability (i.e., with below-median HS product Herfindahl-Hirschman Index (HHI) and low product differentiation) and a group with low substitutability. All specifications control for relationship×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Appendix Table B1. All columns are estimated using Poisson regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Containers		
	(1)	(2)	(3)
Treat×Post	-0.315***	-0.214**	-0.208**
	(0.090)	(0.093)	(0.104)
Treat×Post×Public Supplier	0.161		
	(0.243)		
Treat×Post×Small Supplier		-0.422**	
		(0.204)	
Treat×Post×High Substitutability			-0.370**
			(0.183)
Partition Var.×Year×Cohort FE	Yes	Yes	Yes
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Yes			
Obs.	939,578	939,578	733,780
Pseudo R^2	0.719	0.728	0.728

Table 8: Customer Characteristics and Investor E&S Sensitivity

This table shows the differential effects of the same supplier incident on trade with customers with different financial and investor characteristics. The dependent variable is *Containers*. In Panel A, *High MTB* is a binary variable indicating customers with above-median market-to-book ratios at the beginning of the event year. *High ROA* is a binary variable indicating customers with above-median returns on assets at the beginning of the event year. *High GrossMargin* is a binary variable indicating customers with above-median gross margins at the beginning of the event year. *High CustE&S* is a binary variable indicating customers with above-median Refinitiv E&S ratings in the pre-period. In Panel B, columns (1) and (2) use the same sample as in Table 3. *High IO_E&S* is a binary variable indicating customers with above-median outstanding shares' ownership by E&S-sensitive investors in the pre-period. E&S-sensitive investors are defined similar to Gantchev et al. (2022) as investors with average portfolio E&S ratings in the top tercile of the distribution. *E&S Proposal* is a binary variable indicating publicly-listed customers receiving at least one E&S-related shareholder proposal in the three-year window preceding the event year. Column (3) expands the stacked panel to include relationships with privately-held customers. *Public Cust* is a dummy variable equal to one if the customer's shares are publicly-traded, and equal to zero otherwise. All specifications include supplier×year×cohort FE and customer firm×year×cohort fixed effects. All columns are estimated using Poisson regressions. Variable definitions are in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-year-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Panel A: Firm Characteristics

Dep. Var. =	Containers			
	(1)	(2)	(3)	(4)
Treat×Post×High MTB	-0.287			
	(0.429)			
Treat×Post×High ROA		-0.011		
		(0.355)		
Treat×Post×High GrossMargin			-0.466	
			(0.541)	
Treat×Post×High CustE&S				-0.812*
				(0.477)
Partition Var.×Treat	Yes	Yes	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	119,880	119,880	119,880	119,880
Pseudo R^2	0.715	0.715	0.715	0.715

Table 8: Customer Characteristics and Investor E&S Sensitivity(continued)

Panel B: Investor E&S Sensitivity

Dep. Var. =	Containers		
	(1)	(2)	(3)
Treat×Post×High IO_E&S	-0.828**		
	(0.364)		
Treat×Post×E&S Proposal		-0.790**	
		(0.353)	
Treat×Post×Public Cust			-0.191**
			(0.081)
Partition Var.×Treat	Yes	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	119,880	119,880	14,319,501
Pseudo R^2	0.715	0.715	0.623

Table 9: Retail Sales Volume and Prices

This table shows the effects of supplier E&S incidents on retail sales using transaction-level data from Nielsen scanner data (henceforth Nielsen). We first match Nielsen manufacturers to their Compustat ultimate holder using a combination of fuzzy and manual name-matching procedures, and we only keep Compustat firms that appear as a match. Second, we expand the resulting panel to include information on average prices and quantities of retail products sold by U.S. importers in each Zip-3 code, product module, and quarter, requiring the revenue for a product module-quarter to be at least $5,000. Third, we collapse our main stacked panel into a customer firm-year-cohort sample, and merge it with the Compustat-Nielsen matched data. In this collapsed panel, *customer events* are years in which at least one of the U.S. importer's suppliers is affected by an E&S incident, and the control group consists of U.S. customers with no suppliers affected by E&S incidents. Fourth, we perform stacked difference-in-differences tests around customer events to study the effect of supplier incidents on average prices and quantities sold by U.S. importers. The dependent variables are *Log(Quantity)* in column (1) and *Log(Price)* in column (2). *Treat Cust* is a binary variable indicating customers with at least one supplier affected by E&S incidents in a cohort. *Post* is a binary variable indicating observations after the incident. *Post (Year τ)* is a binary variable indicating year τ relative to the incident. All columns control for *Size*, *Lev*, *R&D*, *Cpax*, and *Cash*. All specifications control for firm×3-digit zip code×module×cohort and 3-digit zip code×module×quarter×cohort fixed effects. Variable definitions are in Appendix Table B1. All columns are estimated using OLS regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

	Log(Quantity)		Log(Price)	
	(1)	(2)	(3)	(4)
Treat Cust×Post	-0.079**		0.045***	
	(-2.066)		(2.772)	
Treat Cust×Post(Year -2)		-0.023		0.001
		(-1.147)		(0.099)
Treat Cust×Post(Year -1)		-0.027		0.013
		(-0.866)		(0.808)
Treat Cust×Post(Year 0)		-0.107***		0.035*
		(-2.920)		(1.696)
Treat Cust×Post(Year +1)		-0.118**		0.047*
		(-2.549)		(1.824)
Treat Cust×Post(Year +2)		-0.088		0.061**
		(-1.564)		(2.373)
Treat Cust×Post(Year +3)		-0.065		0.062**
		(-1.038)		(2.224)
Controls	Yes	Yes	Yes	Yes
Firm×Zip3×Module×Cohort FE	Yes	Yes	Yes	Yes
Zip3×Module×Quarter×Cohort FE	Yes	Yes	Yes	Yes
Obs.	5,632,856	5,632,856	5,632,856	5,632,856
Adjusted R^2	0.982	0.982	0.991	0.991

Table 10: Supply Chain Reallocation

This table documents trade reallocation along the supply chain network. The dependent variable is *Containers*. *%Treat Supp* is the fraction of suppliers affected by an E&S incident in any given cohort. *Treat Cust, Control Supp* is a binary variable indicating control suppliers of "treated" customers (i.e., customers with at least one supplier affected by an E&S incident). *Treat Cust, Control Supp, Same Country* is a binary variable indicating control suppliers of "treated" customers located in the same country of the treated supplier. *Treat Cust, Control Supp, Diff Country* indicates control suppliers in other countries. *Treat Cust, Control Supp, High SuppE&S* is a binary variable indicating control suppliers of "treated" customers with average pre-incident RepRisk ESG rating above the top quintile of the sample distribution. *Treat Cust, Control Supp, Low SuppE&S* indicates control suppliers of "treated" customers with average pre-incident RepRisk ESG rating below the top quintile of the sample distribution. All specifications control for relationship×cohort and customer firm×cohort fixed effects. Variable definitions are in Appendix Table B1. All columns are estimated using Poisson regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Containers		
	(1)	(2)	(3)
Treat Supp×Post	-0.290***	-0.291***	-0.318***
	(0.094)	(0.094)	(0.106)
%Treat×Treat Supp×Post	1.154	1.154	1.113
	(0.844)	(0.845)	(0.828)
%Treat×Treat Cust, Control Supp×Post	1.596***		
	(0.518)		
%Treat×Treat Cust, Control Supp, Same Country×Post		0.799	
		(0.929)	
%Treat×Treat Cust, Control Supp, Diff Country×Post		1.787***	
		(0.586)	
%Treat×Treat Cust, Control Supp, High SuppE&S×Post			22.941***
			(7.591)
%Treat×Treat Cust, Control Supp, Low SuppE&S×Post			-3.674
			(3.020)
Controls	Yes	Yes	Yes
Pair×Cohort FE	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes
Obs.	939,578	939,578	37,286
Pseudo R^2	0.625	0.625	0.619

Table 11: The Costs of Cutting Trade: Trade Cuts and Gross Profit Margins

This table shows the effect of trade cuts following supplier incidents on future gross margins. We collapse our main sample into a cohort-customer firm-year sample over a $[t-3, t+3]$ years window around the incident year t. The dependent variable in columns (1) to (4) is *Gross Margin* measured in years t, $t+1$, $t+2$, and $t+3$, respectively. *Gross Margin* is the difference between sales and cost of goods sold, scaled by sales. We require both sales and cost of goods sold to be greater than \$5 million to avoid the impact of extreme values. *Treat Cust* is a binary variable indicating customers with at least one supplier affected by E&S incidents in a cohort. *Post* is a binary variable indicating observations after the incident. *CutTrade* is a customer-specific indicator that equals one if trade growth between the pre-incident and the post-incident period is below the sample median, and zero otherwise. Trade growth for each customer is computed as the weighted average trade growth across all its suppliers, weighted by the pre-incident trade level. All specifications control for year-cohort and firm-cohort fixed effects. All columns are estimated using OLS regressions. The variables are defined as in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Gross Margin t	Gross Margin $t+1$	Gross Margin $t+2$	Gross Margin $t+3$
	(1)	(2)	(3)	(4)
Treat Cust (CutTrade=1)×Post	-0.002	-0.006*	-0.008**	-0.009**
	(0.003)	(0.004)	(0.004)	(0.004)
Treat Cust (CutTrade=0)×Post	0.005	0.003	0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Size	0.006**	-0.003	-0.015***	-0.015***
	(0.003)	(0.003)	(0.003)	(0.003)
Leverage	0.009	0.025***	0.037***	0.046***
	(0.008)	(0.009)	(0.014)	(0.013)
R&D	-0.009**	-0.007***	-0.000	0.003
	(0.004)	(0.003)	(0.003)	(0.004)
Capx	0.239***	0.158***	0.136***	0.014
	(0.028)	(0.031)	(0.037)	(0.042)
Cash	0.034***	0.029***	0.018*	0.028**
	(0.010)	(0.008)	(0.011)	(0.012)
Year×Cohort FE	Yes	Yes	Yes	Yes
Firm×Cohort FE	Yes	Yes	Yes	Yes
Obs.	29,187	27,432	24,698	21,830
Adjusted R^2	0.922	0.925	0.923	0.923

Table 12: Trade Cuts, E&S Improvements, and Trade Reversal

This table studies supplier ESG rating changes and trade reversals after initial import cuts by U.S. customers. In Panel A, we report the average RepRisk ESG rating of treated and control suppliers over a $[t-3, t+3]$ years window around the incident year t. t-statistics of the difference between the ratings of treated and control suppliers are displayed in parentheses. In Panel B, we construct a cohort-supplier-year panel over a window of $[t-3, t+6]$ years around the incident year t. The dependent variable is the supplier's RepRisk ESG rating. *Treat* is a binary variable indicating whether the supplier is affected by a scandal in year t, and *Post (n)* is a binary variable indicating the n-th year after the incident. For each supplier, we aggregate trade changes between years $t-1$ and $t+1$ across all U.S. customers, and we partition the sample based on distributional cuts of these trade changes. Columns (1) to (3) correspond to trade cuts below the bottom quartile (i.e., the largest trade cuts), within the interquartile range (i.e., moderate trade cuts), and in the top quartile (i.e., small trade cuts) of the trade cut distribution, respectively. All columns control for supplier-cohort and year-cohort fixed effects. All columns are estimated using Poisson regressions. In Panel C, we construct a cohort-relationship-year sample over a $[t-3, t+6]$ years window around the incident year t. The dependent variable is *Containers*. *Treat* is a binary variable indicating suppliers affected by incidents. *Post4* is a binary variable indicating observations in the interval $[t+4, t+6]$ after the incident. *CutTrade* is a relationship-specific indicator equal to one if average trade growth from the $[t-3, t-1]$ period to the $[t+1, t+3]$ period falls below the threshold specified in each column (0, -25%, and -67%, in columns (1) to (3), respectively), and zero otherwise. *Inc_Rating* is a supplier-specific indicator equal to one if the supplier improved its RepRisk ESG rating between year $t-1$ and year $t+3$, and zero otherwise. All specifications control for relationship-cohort and firm-year-cohort fixed effects. All columns are estimated using Poisson regressions. The variables are defined as in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Panel A: Dynamic of Supplier RepRisk Rating

	Supplier RepRisk ESG Rating			
	Control (1)	Treated (2)	Diff. (1)-(2)	T-value
-3	7.641	7.639	0.002	0.030
-2	7.661	7.599	0.062	0.799
-1	7.680	7.607	0.073	0.964
0	7.677	6.627	1.050***	14.015
+1	7.631	6.613	1.018***	12.616
+2	7.699	7.442	0.257***	3.049
+3	7.688	7.604	0.084	0.858

Table 12: Trade Cuts, E&S Improvements, and Trade Reversal (Continued)

Panel B: Future Supplier Risk

Dep. Var. =	Supplier RepRisk ESG Rating		
	< P25	P25-P75	>P75
	(1)	(2)	(3)
Treat×Post(0)	-0.132***	-0.143***	-0.138***
	(0.012)	(0.007)	(0.011)
Treat×Post(+1)	-0.135***	-0.145***	-0.143***
	(0.013)	(0.008)	(0.013)
Treat×Post(+2)	-0.043**	-0.047***	-0.068***
	(0.019)	(0.008)	(0.013)
Treat×Post(+3)	-0.028	-0.033***	-0.051***
	(0.024)	(0.010)	(0.015)
Treat×Post(+4)	-0.020	-0.010	-0.029
	(0.023)	(0.013)	(0.018)
Treat×Post(+5)	-0.029	-0.003	-0.044*
	(0.029)	(0.014)	(0.023)
Treat×Post(+6)	-0.036	-0.017	-0.059**
	(0.041)	(0.016)	(0.028)
Supplier×Cohort FE	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes
Obs.	12,178	26,413	12,837
Pseudo R^2	0.105	0.103	0.108

Panel C: Trade Reversal

Dep. Var. =	Containers		
	where CutTrade=1 is defined if		
	ΔTrade < 0	ΔTrade < -0.25	ΔTrade < -0.67
	(1)	(2)	(3)
Treat×Post (CutTrade=1,Inc_Rating=1)	0.499	0.834**	1.153**
	(0.331)	(0.362)	(0.465)
Treat×Post (CutTrade=1,Inc_Rating=0)	0.051	0.050	0.197
	(0.519)	(0.589)	(0.725)
Treat×Post (CutTrade=0,Inc_Rating=1)	-0.030	-0.063	-0.074
	(0.164)	(0.160)	(0.158)
Treat×Post (CutTrade=0,Inc_Rating=0)	-0.052	-0.050	-0.067
	(0.165)	(0.161)	(0.157)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Cohort×Year FE	Yes	Yes	Yes
Obs.	63,430	63,430	63,430
Pseudo R^2	0.822	0.822	0.822

Supplemental Material to "ESG Shocks in Global Supply Chains"

Appendix Intended for Online Publication

A E&S Incidents

In this Appendix Section, we provide anecdotal evidence on trade cuts following supplier E&S incidents. We also detail the process we follow to collect the news associated with E&S incidents in our sample, as well as to categorize these news according to their likely impact on the supplier's operating performance, product quality, and risk of regulatory penalties.

A.1 Anecdotal Evidence

This section provides three pieces of recent anecdotal evidence about trade cuts following supplier E&S incidents.

E&S Reputation Risk: Nestlé and Marfrig. In November 2023, Nestlé cut ties with the Brazilian beef producer Marfrig, which is accused of sourcing some of its livestock from an abattoir linked to contested indigenous land.[A1] Nestlé's trade cut is arguably motivated by E&S reputation risks, as it is unlikely that this incident affects Marfrig's operating performance or the quality of the products it supplies to Nestlé. It is also unlikely that this incident affects Marfrig's risk of regulatory penalties, since i) Marfrig is not directly responsible for the incident but only indirectly through some intermediary farmers; ii) the indigenous lands are contested and it is not clear whether the farmers are in violation of any law; and iii) Marfrig is one of the largest companies in Brazil, and possible regulatory penalties associated with this incident are likely to have a small impact on its profitability and operations.

Investor E&S Sensitivity: Hyundai, SMART, and SL Alabama. In early 2023, Hyundai cuts ties with its suppliers SMART and SL Alabama over child labor issues.[A2] This cut

[A1] See, e.g., `www.guardian.com`

[A2] See `www.reuters.com` and `www.al.com`.

is partly a response to mounting investor pressure, as testified by the letter issued by the investor SOC Investment Group shortly after the incident news is released.[A3]

Disciplining Suppliers with Exit: Dyson and ATA. In November 2021, Dyson cuts ties with the Malaysian electronics manufacturer ATA over forced overtime labor allegations.[A4] The statement of Dyson's representatives suggests that customers' exit may prompt suppliers to improve their E&S profiles:

> *"We have now terminated our relationship with six months' of contractual notice. We hope this gives ATA the impetus to improve [. . .]."*

A.2 Dissecting RepRisk E&S Incidents

RepRisk does not disclose the incident news source. The data contains information on the name of the company affected by the incident, the incident date, the countries involved, the incident topic (e.g., overuse of resources or human rights violations), the countries involved in the incident, and the reach, novelty and language of the news. With the help of a team of students, we collect the news relative to the RepRisk incidents in our sample from various public sources, including traditional media outlets available through internet searches, social media plaftorms such as Facebook and Weibo, Bloomberg, and Factiva. We are able to recover 482 news sources (about 46% of all incidents in our final sample), out of which 79% are identified with a high degree of confidence.

We then ask the students to rate the news on a one to three scale based on their likelihood of being associated with regulatory penalties, operational disruptions, and changes in product quality. Specifically, we ask them to answer the following questions:

1. Did the incident result or was it likely to result in regulatory penalties (e.g., the

[A3] `www.reuters.com`.
[A4] `www.reuters.com`.

firm needs to pay a fine or shut down a plant)? Please provide the answer on a 1-3 scale, where 1 is no penalty, 2 is a small penalty, and 3 is a large penalty.

2. Did the incident have material implications for the supplier's operating activities (e.g., labor strike or interruption in productive process)? Please provide a rating from 1 to 3, where 1 means that the incident had little implications for the firm's operating activities, and 3 means that the incident had large implications.

3. Did the incident have material implications for the supplier's operating activities (e.g., dangerous chemicals were used in production)? Please provide a rating from 1 to 3, where 1 means that the incident had little implications for the firm's product quality, and 3 means that the incident had large implications.

At least two students are asked to rate the same news, and the resulting scores are averaged across students. Figure A2 reports the distribution of the responses for each question. We find that the majority of the RepRisk news in our sample has little perceived impact on penalty risk, operating risk, and product quality risk: around 110 out of 482 news are associated with a "low" score in all questions. On the other hand, very few incidents have a large perceived impact on penalty risk, operating risk, and product quality risk, with only 3 news receiving a "high" score in all questions.

Figure A1: Nestlé's Approach to Deforestation Management

This example demonstrates Nestlé's approach to deforestation management in its supply chain. Suppliers identified with high deforestation risk are temporarily suspended from trade and given a timeframe to address the associated issues. If the supplier successfully addresses the concerns within the stipulated period, it re-enters into Nestlé's supply chain. Otherwise, it remains excluded.

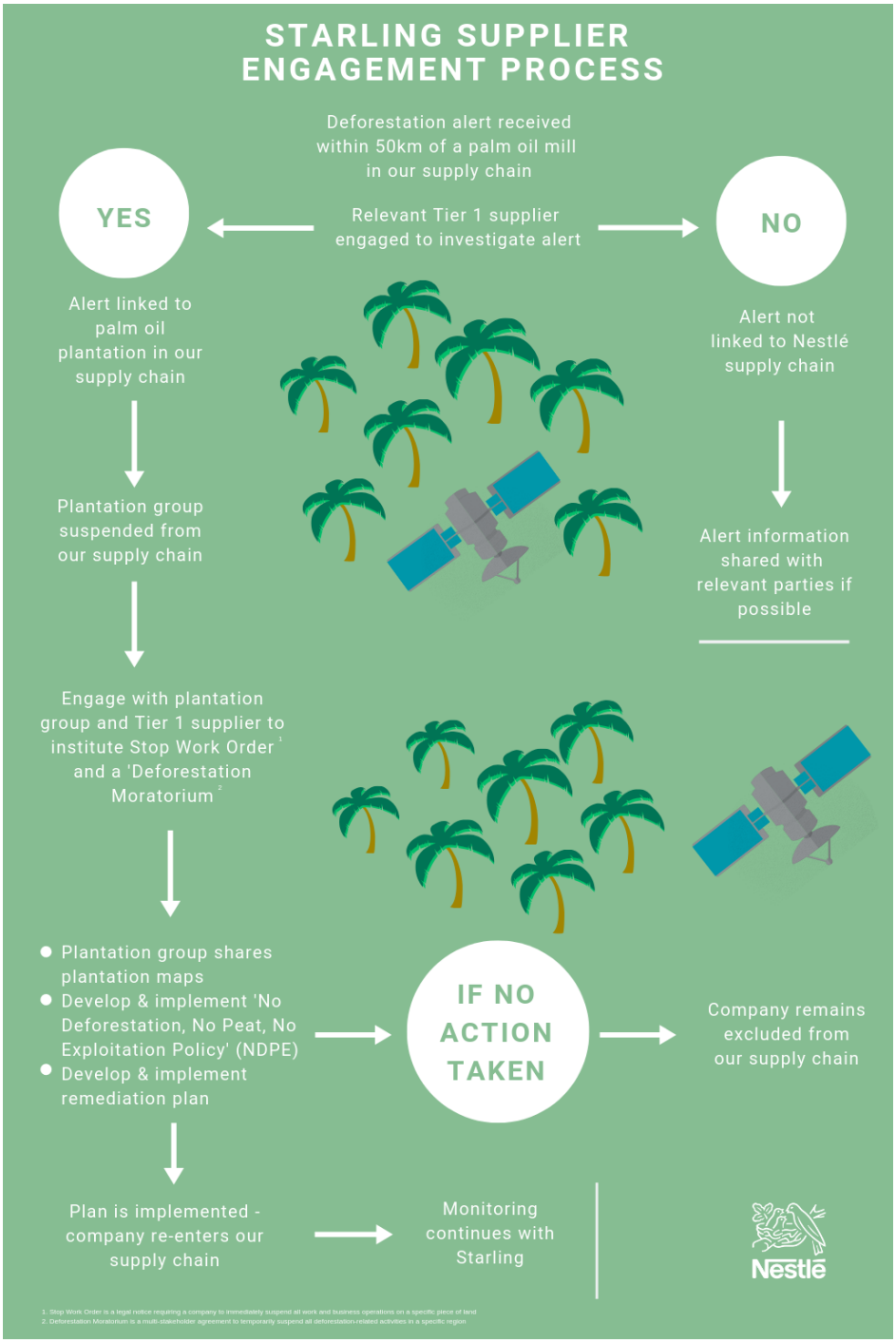
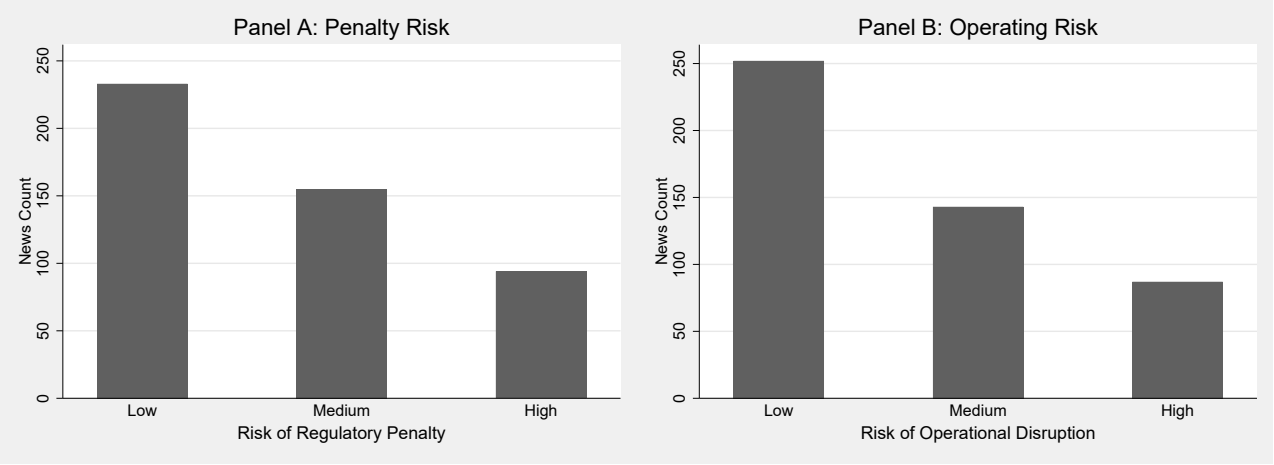
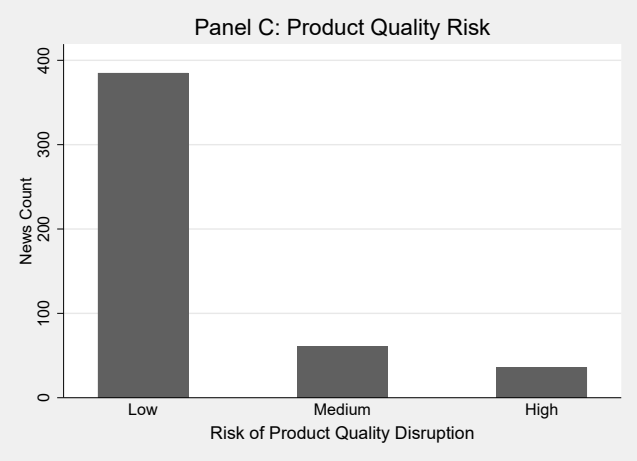


Figure A2: Dissecting RepRisk E&S Incidents

This figure displays the distribution of responses to the questions related to incident characteristics in Appendix Section A.2. Panel A reports the responses on the likely effects of RepRisk incidents on suppliers' regulatory penalty risk. Panel B reports the responses on the effects on supplier operating risk. Panel C reports the responses on supplier product quality.





B Additional Figures and Tables

Figure B1: Dynamic Effects on Supplier Performance

This figure displays the dynamic effects of supplier E&S incidents on the operating and financial performance of the supplier. To estimate the dynamic effects of E&S incident exposure, we collapse our cohort-supplier-customer-year panel to a cohort-supplier-year panel and estimate

$$Y_{c,i,t} = \Sigma_{k=-2}^{3} \beta_k \times Treat_{c,i} \times 1\{t = k\} + v_{c,i} + \tau_{c,t} + e_{c,i,t},$$

where c, i, and t denote cohort, supplier, and year, respectively. *Treat* is an indicator variable for treated suppliers with an E&S incident at time $t = 0$ in the cohort. $1\{t{=}k\}$ is an indicator variable for year k relative to the event year of the cohort. Panels A to D use the ratio of earnings to assets (ROA), the ratio of short-term debt to assets, the ratio of long-term debt to assets, and the ratio of capital expenditure to fixed assets as the dependent variable, respectively. All the specifications include cohort-supplier and cohort-year fixed effects, and the sample is restricted to suppliers of treated customers. In this figure, we plot the estimated interaction coefficients ($\hat{\beta}_k$) and their associated 90% confidence intervals.

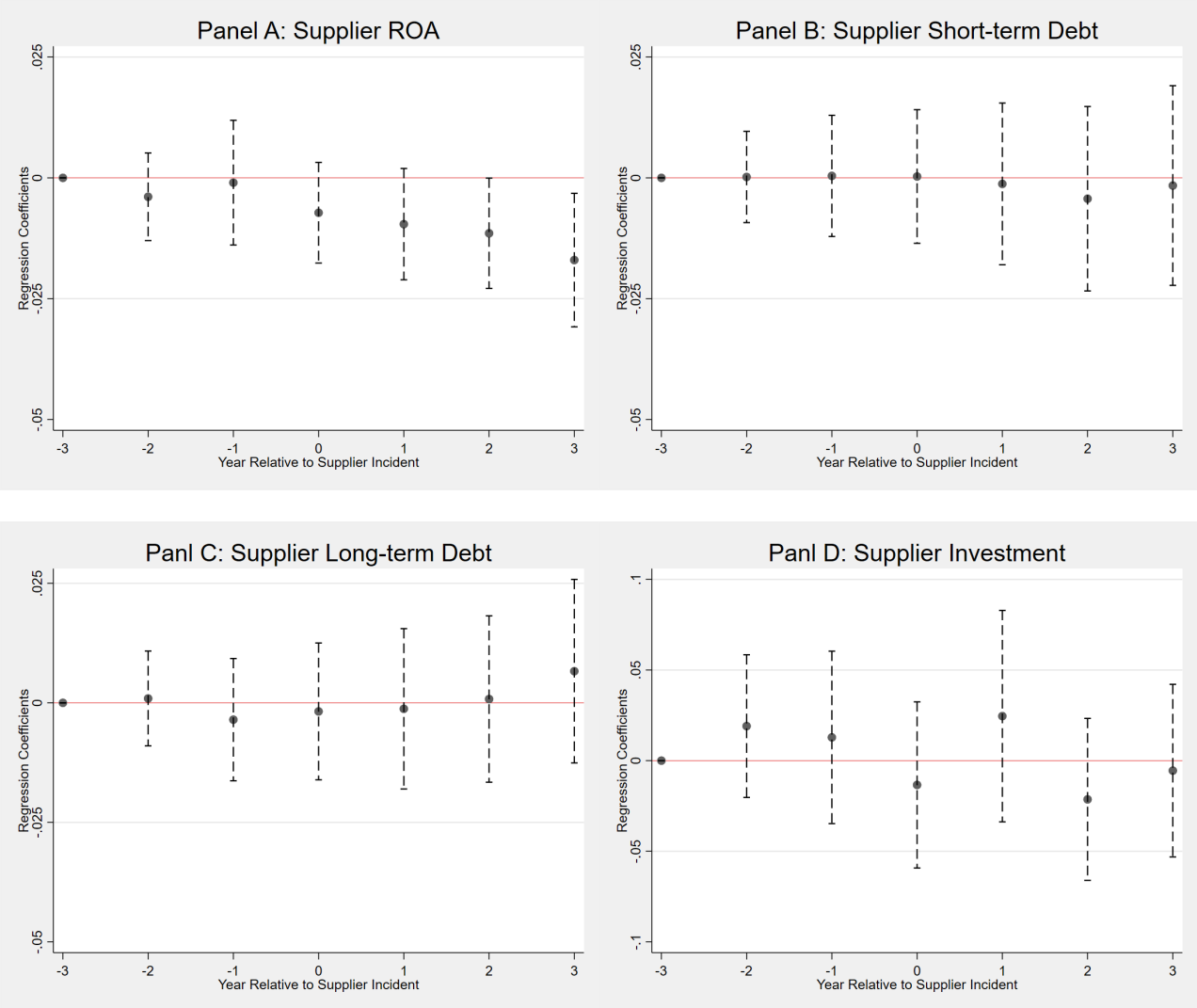


Table B1: Variable Definitions

Variable	Definition	Data Source
Containers	The number of containers shipped from the supplier to the customer in the year.	Panjiva
1(Trade>0)	A binary variable that equals one if the customer has non-zero container imports from the supplier in the year.	Panjiva
Shipment	The number of shipments from the supplier to the customer in the year.	
Weight	The total weight of all shipments from the supplier to the customer in the year.	Panjiva
Item	The number of individual items shipped from the supplier to the customer in the year.	Panjiva
Quantity	The total quantity sold by the firm in a given product module and Zip-3 area in a quarter.	Nielsen
Price	The value-weighted price of all products sold by the firm in a given product module and Zip-3 area in a quarter.	Nielsen
Gross Margin	The difference between sales and cost of goods sold, scaled by sales.	Compustat
Treat Supp	A binary variable that equals one if the supplier is subject to an E&S incident.	RepRisk
Treat Cust	A binary variable that equals one if any of the firm's suppliers is subject to an E&S incident.	RepRisk
Post	A binary variable that equals one for the periods following the supplier's E&S incident.	RepRisk
Size	The natural logarithm of the asset size of the customer firm.	Compustat
Leverage	The sum of short-term and long-term debt scaled by total assets.	Compustat
R&D	The ratio of R&D expenditure to total assets. Missing values are replaced with zero.	Compustat
CAPX	The ratio of capital expenditure to total assets.	Compustat
Cash	The ratio of cash and cash equivalents to total assets.	Compustat
Treat Supp, E	The product of *Treat Supp* and a binary variable that equals one if the incident is primarily related to environmental issues.	RepRisk
Treat Supp, S	The product of *Treat Supp* and a binary variable that equals one if the incident is primarily related to social issues.	RepRisk
Treat Supp, High Severity	The product of *Treat Supp* and a binary variable that equals one if the supplier incident is coded as a high- or medium-severity incident.	RepRisk
Treat Supp, Low Severity	The product of *Treat Supp* and a binary variable that equals one if the supplier incident is not coded as *High Severity*.	RepRisk
Treat Supp, High Reaction	The product of *Treat Supp* and a binary variable that equals one if the customer's market reaction over a [-1,+1] day window around the supplier incident is above the sample median.	RepRisk, CRSP

Table B1: Variable Definitions (Continued)

Variable	Definitions	Data Source
Treat Supp, Low Reaction	The product of *Treat Supp* and a binary variable that equals one if the customer's market reaction over a [-1,+1] day window around the supplier incident is below the sample median.	RepRisk, CRSP
Public Supplier	A binary variable that equals one if the supplier is a public firm.	RepRisk
Small Supplier	A binary variable that equals one if the supplier's annual container shipments relative to the aggregate container shipments to the SIC industry are smaller than the sample median.	Panjiva
High Substitutability	A binary variable that equals one if the HHI of the supplier's two-digit HS product is below the sample median, and the supplier's HS products are not classified as differentiated goods according to Rauch (1999).	Panjiva, Rauch (1999)
High MTB	A binary variable that equals one if the customer firm's ratio of market value of equity to book value of equity at the beginning of the event year is above the sample median.	Compustat
High ROA	A binary variable that equals one if the customer firm's ratio of operating income before depreciation and amortization to total assets at the beginning of the event year is above the sample median.	Compustat
High GrossMargin	A binary variable that equals one if the customer firm's ratio of gross margins at the beginning of the event year is above the sample median.	Compustat
High CustE&S	A binary variable that equals one if the average of the customer's Refinitiv E and S scores at the beginning of the event year is above the sample median.	Refinitiv
High IO_E&S	A binary variable that equals one if the fraction of outstanding shared owned by E&S-conscious investors in the pre-period is above the sample median.	Thomson Reuters
E&S Proposal	A binary variable that equals one if the customer firm received at least one ES-related shareholder proposal in the three-year window before the event year.	Institutional Shareholder Services
Public Cust	A binary variable that equals one if the customer firm is publicly listed in the event year.	CRSP

Table B2: Panjiva Sample Selection

This table shows the procedure we follow to construct the final supplier-customer relationship-year panel. We start with the overall Panjiva sample and remove (a) privately-held customers; (b) relationships that appear only once and do not allow us to estimate changes; and (c) customers that have missing financial data in the year before the incident. We then create a relationship-year panel based on the earliest and latest years a relationship appears on the Panjiva database.

Step	#Suppliers	#Customers	#Supplier-Customers	#Relationship-years
Panjiva Sample	1,598,415	382,215	4,322,747	-
(-) Private Customer	222,279	7,032	331,516	-
(-) Relationship Appearing Only Once	90,074	4,537	12,3081	-
(-) Missing $t-1$ Financial Data	58,298	1,937	73,916	-
Create a Relationship-year Panel	58,298	1,937	73,916	497,397

Table B3: Log(1+Containers) as the Dependent Variable

This table shows the effect of supplier E&S incidents on trade relationships. The dependent variable is *Log(1+Containers)*, defined as the natural logarithm of one plus the number of containers received by a U.S. customer from a given supplier over the year. Column (2) requires a relationship-cohort-year to have a positive amount of trade to be included in the regression sample. Both columns are estimated using OLS regressions. All specifications control for relationship×cohort and customer firm×year×cohort fixed effects. All the variables are defined in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Log(1+Containers)	Log(1+Containers)
		Intensive Margin
	(1)	(2)
Treat Supp×Post	-0.112***	-0.091*
	(0.039)	(0.054)
Pair×Cohort FE	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes
Obs.	939,578	412,184
Adjusted R^2	0.387	0.641

Table B4: Robustness on Baseline Results

This table shows the estimates of robustness tests for our main findings reported in Table 3. Panel A reports Poisson regressions using alternative measures of trade as dependent variables. The dependent variables in columns (1) to (3) are *Shipment*, *Weight*, and *Item*, respectively. Panel B reports the alternative matching samples. The dependent variable is *Containers*. Column (1) matches treatment and control relationships based on the customer's four-digit SIC industry. Column (2) matches treatment and control relationships based on the customer's four-digit SIC industry and asset size decile. Column (3) matches treatment and control relationships based on the customer's industry, the customer's asset size decile, and the supplier's country. Column (4) restricts the sample to customer firm-countries with at least one treatment and control suppliers. Panel C reports findings under alternative approaches to deal with confounding incidents. The dependent variable is *Containers*. Column (1) requires no confounding incidents two years before and two years after the focal incident. Column (2) requires no confounding incidents one year before and after the focal incident. We match treatment and control relationships based on customer industry, customer size decile, and supplier country. Panel D reports the findings using alternative fixed effects. The dependent variable is *Containers*. Column (1) controls for year-cohort fixed effects, column (2) controls for year-cohort and firm-cohort fixed effects, column (3) controls for year-cohort, cohort-firm, and supplier-cohort fixed effects, and column (4) controls for year-cohort and relationship-cohort fixed effects. Panel E reports the findings using alternative control groups. Column (1) removes from the control group suppliers that share the same customers with suppliers affected by E&S incidents, and control customers that are in the same SIC industry with treated customers. Column (2) removes from the control group suppliers selling the same products (i.e., same four-digit HS code) as treated suppliers in the same event cohort. Column (3) uses the same supplier control group as column (2), but also removes from the control group suppliers that share the same customers with suppliers affected by E&S incidents. Panel F reports findings using alternative samples and specifications. The dependent variable is *Containers*. Column (1) re-estimates the regression model (1) using trade volume between supplier and customer in the pre-incident period as a weight for each observation. Column (2) removes the observations in year $t+1$ (i.e., the year immediately following the incident-year t). Column (3) includes supplier incidents related to corruption, bribery, and fraud in addition to the E&S incidents used in our main analysis. Column (4) expands the main sample to the quarterly observation frequency. Column (5) estimates the regression model (1) using the number of containers divided by the total number of containers imported by the firm as the dependent variable. All columns except column (5) of Panel F are estimated using Poisson regressions. Column (5) of Panel F is estimated using OLS regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Panel A: Alternative Trade Measures

Dep. Var. =	Shipment	Weight	Item
	(1)	(2)	(3)
Treat Supp×Post	-0.241***	-0.260***	-0.333***
	(0.081)	(0.080)	(0.117)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Cohort×Year FE	Yes	Yes	Yes
Obs.	939,578	939,578	939,578
Pseudo R^2	0.678	0.800	0.814

Table B4: Additional Robustness (Continued)

Panel B: Matching Sample

	Containers			
	Industry	Industry, size	Industry, size, supplier country	Firm-countries with both treated and control suppliers
	(1)	(2)	(3)	(4)
Treat Supp×Post	-0.301***	-0.304***	-0.294***	-0.329***
	(0.084)	(0.084)	(0.105)	(0.097)
Controls	No	No	No	Yes
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	No
Year×Cohort FE	No	No	No	Yes
Obs.	750,569	699,135	151,353	154,087
Pseudo R^2	0.715	0.716	0.758	0.622

Panel C: Alternative Restrictions on Confounding Incidents

Dep. Var. =	Containers	
	No confounding incidents two years before and after the event	No confounding incidents one year before and after the event
	(1)	(2)
Treat Supp×Post	-0.231***	-0.115**
	(0.071)	(0.056)
Firm×Cohort FE	Yes	Yes
Firm×Cohort×Year FE	Yes	Yes
Obs.	1,337,007	1,740,226
Pseudo R^2	0.699	0.700

Table B4: Additional Robustness (Continued)

Panel D: Alternative Fixed Effects

Dep. Var. =	Containers			
	(1)	(2)	(3)	(4)
Treat Supp	0.267***	0.305***		
	(0.082)	(0.079)		
Treat Supp×Post	-0.184**	-0.227***	-0.309***	-0.276***
	(0.083)	(0.083)	(0.083)	(0.083)
Size	0.056***	0.194***	0.225***	0.231***
	(0.002)	(0.015)	(0.016)	(0.016)
Leverage	0.399***	-0.387***	-0.651***	-0.627***
	(0.036)	(0.052)	(0.053)	(0.053)
R&D	-2.627***	3.350***	4.253***	4.231***
	(0.171)	(0.458)	(0.497)	(0.498)
Capx	0.014	-0.762***	-1.267***	-1.297***
	(0.170)	(0.209)	(0.214)	(0.215)
Cash	0.218***	0.274***	0.237***	0.236***
	(0.058)	(0.070)	(0.071)	(0.071)
Year×Cohort FE	Yes	Yes	Yes	Yes
Firm×Cohort FE	No	Yes	Yes	No
Supplier×Cohort FE	No	No	Yes	No
Pair×Cohort FE	No	No	No	Yes
Obs.	936,676	936,676	936,249	936,183
Pseudo R^2	0.024	0.138	0.583	0.626

Table B4: Additional Robustness (Continued)

Panel E: Alternative Control Groups

Dep. Var. =	Containers		
Control Group =	Control suppliers of never treated customers in different industries	(i) Treated customers' unaffected suppliers selling products different from treated suppliers of the same cohort; (ii) Control customers' suppliers selling products different from treated suppliers of the same cohort	Control customers' suppliers selling products different from treated suppliers of the same cohort
	(1)	(2)	(3)
Treat Supp×Post	-0.180**	-0.475***	-0.272***
	(0.083)	(0.108)	(0.093)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	No	Yes	No
Year×Cohort FE	Yes	No	Yes
Obs.	180,534	191,774	68,405
Pseudo R^2	0.663	0.729	0.639

Panel F: Alternative Samples and Specifications

Dep. Var. =	Containers				
	Weighted Regression	Remove year $t+1$	Including corruption, bribery, fraud	Quarterly data	Scaled by size
	(1)	(2)	(3)	(4)	(5)
Treat Supp×Post	-0.345***	-0.272***	-0.217***	-0.286***	-0.006***
	(0.129)	(0.085)	(0.077)	(0.080)	(0.002)
Pair×Cohort FE	Yes	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes	Yes
Obs.	607,646	779,080	970,184	3,581,751	939,578
Pseudo R^2	0.709	0.727	0.718	0.598	
Adjusted R^2					0.498

Table B5: Cross-sectional Tests: Financial Constraints and Hedging

This table shows the differential effects of the same supplier incident on trade with customers with different degrees of financial constraints and supply chain risk hedging. As our main tests, these tests are performed using Poisson regressions and the dependent variable is *Containers*. *High KZindex* is a binary variable that equals one if the customer firm's KZ Index is above the sample median. *High WWindex* is a binary variable that equals one if the customer firm's WW Index is above the sample median and zero otherwise. *High Purchase Commitment* is a binary variable that equals one if the customer firm's ratio of purchase obligations to cost of goods sold is greater than the sample median and zero otherwise. All specifications control for supplier×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Appendix Table B1. All columns are estimated using Poisson regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Containers		
	(1)	(2)	(3)
Treat×Post×High KZindex	-0.468		
	(0.469)		
Treat×Post×High WWindex		0.521	
		(0.435)	
Treat×Post×High Purchase Commitment			-0.063
			(0.414)
Partition Var.×Treat	Yes	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	111,257	111,627	119,880
Pseudo R^2	0.712	0.712	0.715

Table B6: Cross-sectional Tests on Investor E&S Sensitivity: Robustness Test

This table shows the differential effects of the same supplier incident on trade with customers with different investor characteristics. *High IO_E&S* is a binary variable indicating customers with above-median outstanding shares' ownership by E&S-conscious investors in the pre-period. E&S-sensitive investors are defined similar to Gantchev et al. (2022) as investors with average portfolio E&S ratings in the top tercile of the distribution. *E&S Proposal* is a binary variable indicating publicly-listed customers receiving at least one E&S-related shareholder proposal in the three-year window preceding the event year. Columns (1) to (4) include pair×cohort and customer firm×year×cohort fixed effects, and columns (5) and (6) include supplier×year×cohort and customer firm×year×cohort fixed effects. Columns (3) to (6) also include the interaction terms among *Treat*, *Post*, and *High Size*, *High MTB*, *High ROA*, and *High GrossMargin*. *High Size* is a binary variable indicating customers with above-median market capitalization at the beginning of the event year. *High MTB* is a binary variable indicating customers with above-median market-to-book ratios at the beginning of the event year. *High ROA* is a binary variable indicating customers with above-median returns on assets at the beginning of the event year. *High GrossMargin* is a binary variable indicating customers with above-median gross margins at the beginning of the event year. All columns are estimated using Poisson regressions. Variable definitions are in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-year-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Containers					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-0.128	-0.187**	-0.029	-0.086		
	(0.109)	(0.087)	(0.133)	(0.119)		
Treat×Post×High IO_E&S	-0.270*		-0.370**		-0.686*	
	(0.392)		(0.159)		(0.360)	
Treat×Post×E&S Proposal		-0.310*		-0.391**		-0.846**
		(0.171)		(0.171)		(0.387)
Partition Var.×Treat	Yes	Yes	Yes	Yes	Yes	Yes
Pair×Cohort FE	Yes	Yes	Yes	Yes	No	No
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Supplier×Year×Cohort FE	No	No	No	No	Yes	Yes
High Size×Treat	No	No	Yes	Yes	Yes	Yes
High MTB×Treat	No	No	Yes	Yes	Yes	Yes
High ROA×Treat	No	No	Yes	Yes	Yes	Yes
High GrossMargin×Treat	No	No	Yes	Yes	Yes	Yes
Treat×Post×High MTB	No	No	Yes	Yes	Yes	Yes
Treat×Post×High ROA	No	No	Yes	Yes	Yes	Yes
Treat×Post×High GrossMargin	No	No	Yes	Yes	Yes	Yes
Obs.	939,578	939,578	939,578	939,578	119,880	119,880
Pseudo R^2	0.719	0.719	0.719	0.719	0.716	0.716

Table B7: Switching to New Suppliers

This table shows the effect of supplier E&S incidents on the number of new suppliers established by treated U.S. importers. We collapse our main sample into a cohort-customer firm-year sample over a $[t-3, t+3]$ years window around the incident year t. As in our main tests, we estimate Poisson regression models where the dependent variable is the number of new suppliers established in a year (columns (1) and (2)), and the number of new countries the U.S. customer sources from in a year (columns (3) and (4)). *Treat Cust* is a binary variable indicating customers with at least one supplier affected by E&S incidents in a cohort. *Post* is a binary variable indicating observations after the incident. All specifications control for year-cohort and firm-cohort fixed effects. All columns are estimated using Poisson regressions. The variables are defined as in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the firm-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	New suppliers		New supplier Countries	
	(1)	(2)	(3)	(4)
Treat×Post	0.205***	0.207***	0.147***	0.145***
	(0.037)	(0.037)	(0.039)	(0.039)
Controls	No	Yes	No	Yes
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	26,410	26,391	24,852	24,833
Pseudo R^2	0.774	0.773	0.249	0.249

Table B8: Robustness: Trade Cuts, E&S Improvements, and Trade Reversal

This table studies supplier ESG rating changes and trade reversals after initial import cuts by U.S. customers. In Panel A, we construct a cohort-supplier-year panel over a window of $[t-3, t+6]$ years around the incident year t. The dependent variable is the supplier's Sustainalytics E&S score, defined as the average of the firm's environmental and social scores from Sustainalytics. *Treat* is a binary variable indicating whether the supplier is affected by a incident in year t, and *Post (n)* is a binary variable indicating the n-th year after the incident. For each supplier, we aggregate trade changes between years $t-1$ and $t+1$ across all U.S. customers, and we partition the sample based on distributional cuts of these trade changes. Columns (1) and (2) correspond to firms with and without trade cut, respectively. All specifications control for supplier-cohort and year-cohort fixed effects. All columns are estimated using Poisson regressions. In Panel B, we construct a cohort-relationship-year sample over a $[t-3, t+6]$ years window around the incident year t. The dependent variable is *Containers*. *Treat* is a binary variable indicating suppliers affected by incidents. *Post4* is a binary variable indicating observations in the interval $[t+4, t+6]$ after the incident. *CutTrade* is a relationship-specific indicator equal to one if average trade growth from the $[t-3, t-1]$ period to the $[t+1, t+3]$ period falls below 0, and zero otherwise. *Inc_Rating* is a supplier-specific indicator equal to one if the supplier improved its Sustainalytics E&S score between year $t-1$ and year $t+3$, and zero otherwise. All specifications control for relationship-cohort and firm-year-cohort fixed effects. All columns are estimated using Poisson regressions. The variables are defined as in Appendix Table B1. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Panel A: Future Supplier Risk

Dep. Var. =	Supplier Sustainalytics ES Score	
	ΔTrade < 0	ΔTrade > 0
	(1)	(2)
Treat×Post(0)	-0.023**	-0.054***
	(0.010)	(0.010)
Treat×Post(+1)	-0.028**	-0.053***
	(0.012)	(0.013)
Treat×Post(+2)	-0.029*	-0.055***
	(0.015)	(0.015)
Treat×Post(+3)	-0.026	-0.082***
	(0.020)	(0.018)
Treat×Post(+4)	-0.042	-0.103***
	(0.027)	(0.024)
Treat×Post(+5)	0.015	-0.099***
	(0.036)	(0.030)
Treat×Post(+6)	0.056	-0.069**
	(0.043)	(0.033)
Supplier×Cohort FE	Yes	Yes
Year×Cohort FE	Yes	Yes
Obs.	2,109	2,121
Adjusted R^2	0.252	0.226

Table B8: Robustness: Trade Cuts, E&S Improvements, and Trade Reversal (Continued)

Panel B: Trade Reversal

Dep. Var. =	Containers
	where CutTrade=1 is defined if ΔTrade < 0
	(1)
Treat×Post (CutTrade=1,Inc_Rating=1)	1.278***
	(0.384)
Treat×Post (CutTrade=1,Inc_Rating=0)	0.122
	(0.893)
Treat×Post (CutTrade=0,Inc_Rating=1)	0.261
	(0.210)
Treat×Post (CutTrade=0,Inc_Rating=0)	0.463
	(0.365)
Pair×Cohort FE	Yes
Firm×Cohort×Year FE	Yes
Obs.	62,893
Pseudo R^2	0.822