ESG Shocks in Global Supply Chains*

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Abstract

We show that U.S. firms cut imports by 11.1% and are 4.2% more likely to terminate a trade relationship 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 have real effects in far-flung economies. Larger trade cuts around the scandal result in higher supplier E&S scores in subsequent years, and in the eventual resumption of trade. Our results highlight the role of customers' exit in ensuring suppliers' E&S compliance along global supply chains.

Keywords: ESG; environmental incidents; global supply chains; regulatory outsourcing; shareholder pressure

JEL Classifications: F14, F18, G34, G38

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1 Introduction

Corporations are facing increasing pressure by customers, workers, shareholders, and regulators to monitor and manage environmental and social (E&S) activities along their supply chains. In November 2021 and 2022, Amazon was subject to worldwide strikes against poor working conditions in its network of downstream distributors and upstream suppliers.[1] In January 2022, Costco's shareholders voted for tougher measures to be implemented on the company's indirect greenhouse gas emissions along the supply chain (so-called "Scope 3 Emissions").[2] On its end, the U.S. Securities and Exchange Commission (SEC) started discussing mandatory disclosure rules for publicly-listed U.S. companies' Scope 3 Emissions.[3] These recent anecdotes and policy discussions bear the question as to how firms manage E&S standards adherence along widespread and complex supply chain structures.

Firms have been active in engaging with their suppliers to ensure their adherence to the E&S standards (see, e.g., Schiller, 2018 and Dai, Duan, Liang, and Ng, 2021a; Dai, Liang, and Ng, 2021b). In addition to such *governance by engagement*, anecdotal evidence suggests that importers often cut their trade relationships when the suppliers do not abide to these standards.[4] However, we lack economic estimates of these trade cuts and an understanding of the economic incentives underlying them in a broad sample of firms. Additionally, we have no evidence on whether customers' *governance by exit* is an effective mechanism in improving global E&S standards.

In this paper, we study how U.S. customers change trade relationships after their international suppliers are involved in E&S-related controversies. For this purpose, we

[1] https://www.businessinsider.com/make-amazon-pay-warehouse-strike-protest-black-friday-2022-11.

[2] https://www.wsj.com/articles/costco-shareholder-vote-signals-focus-on-supply-chain-emissions-11643194803.

[3] See, e.g., The Economist, 2022.

[4] For example, the collapse of Dhaka's Rana Plaza building in 2013 led to trade cuts between Bangladeshi retailers and French importers (Koenig and Poncet, 2022). More recently, in September 2018 Nestlè and PepsiCo closed their joint ventures with Indofood Group, Indonesia's palm oil giant, citing environmental concerns, and multiple international retailers ended their relationships with Cambodian Hulu Garment Co. failed to pay its workers during the Covid-19 pandemic.

use trade data between foreign suppliers and U.S. customers over the 2007-20 period, sourced by S&P Global Panjiva from cargo declarations to U.S. Customs and Border Protection (CBP). This data, available at the shipment-level, captures the universe of direct maritime trade relationships between U.S. firms and their foreign suppliers above and beyond those that are disclosed in their regulatory filings or in public communications.

We study how imports by U.S. customers respond when their international suppliers (including small, privately-held ones) are associated with negative E&S events based on the RepRisk dataset, sources ESG-related events from media as well as regulatory and commercial documents.[5] We focus on *environmental incidents* such as those related to pollution, overuse and wasting of resources, and animal mistreatment, as well as *social incidents* such as those related to human rights abuses, forced or child labor, and occupational health and safety accidents.

Our granular cargo declaration and E&S scandal data allow us to get precise economic estimates of U.S. customers' supply chain adjustments after negative E&S incidents, and to explore the drivers of response heterogeneity. Our main sample consists of 1,038 supplier-year pairs and 1,301 relationship-year pairs hit by an E&S scandal over the period 2010-18. We first show that supplier scandals trigger negative stock price reactions for U.S. customers. U.S. customers experience an average -10 basis points cumulative abnormal return (CARs) in a [-1;+1] day window around the supplier incident, suggesting a material downstream economic impact.

In our main tests, we then follow a "stacked" difference-in-differences regression approach (e.g., Gormley and Matsa, 2011) to study the effect of supplier E&S incidents on the imports by U.S. customer firms. For each E&S incident, we build separate time cohorts that include the trade relationships between an E&S incident-stricken 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 suppli-

[5]We also refer to these negative E&S events as "incidents" or "scandals."

ers and customers ("control" relationships) three years before and three years after the event. Our estimates capture the change in trade between U.S. customers and their incident-stricken international suppliers three years before and three years after the incident, relative to the change in trade within other U.S. customers and international supplier relationships during the same time period. As most customers have multiple suppliers at the same time, our specifications allow us to control for time-varying customer demand for foreign suppliers (driven, for example, by the customer's economic conditions).

In our main specifications, we measure trade intensity by the number of containers shipped by the international supplier during the year. Our baseline findings show that over the three years following the supplier's incident, the annual number of containers imported by U.S. customers from that supplier decreases by 11.1%. Such drop appears in the first year following the scandal and persists for more than three years.

We then break down the relationship readjustments into the extensive margin (i.e., a complete disappearance of the trade relationship) and the intensive margin (i.e., a decrease in the container quantities traded). We find that the average relationship is 4.2% more likely to be terminated after a supplier experiences an E&S incident—a 50% increase relative to the baseline probability of a trade relationship termination. Conditional on trade continuation, container shipments drop by 9.5% on average, suggesting that even when customers continue trading with the incident-stricken supplier, they severely reduce their reliance on that supplier in subsequent periods.

To the best of our knowledge, our results are the first to document partial trade adjustments in response to E&S shocks. One possible explanation for these novel effects is U.S. customers' inability to fully switch out of the relationship (perhaps due to input specificity, e.g., Barrot and Sauvagnat, 2016), or the unavailability of competitive alternatives. A related explanation is that U.S. customers may be looking to diversify their supply chain risk, and to reduce their exposure to future E&S scandals from the original supplier.[6] With this, customers may also use trade cuts as costly governance actions to improve the supplier's E&S performance.

The granularity of our data allows us to perform additional cross-sectional tests and tease out the forces underlying the documented trade adjustments. We first validate our estimation methodology as well as our E&S incident measures by showing that our main results are stronger in the cross-section for incidents more likely to generate adverse downstream reputational effects, and in the time-series in periods of greater E&S awareness. Specifically, we find that our trade cuts are quantitatively larger for more severe scandals, when the scandal announcement triggers larger negative market reactions for the customer, and after the 2015 Paris Agreement. We also see slightly larger effects for social incidents as compared to environmental incidents, although the effect is the largest for incidents carrying *both* environmental and social implications.

We also perform additional heterogeneity tests to tease out the sources of pressure triggering U.S. customers' trade adjustments. In particular, these trade cuts could be driven by the preferences of their ESG-minded institutional investors. Alternatively, the trade cuts could result from U.S. customers' end-consumer preferences and pressure. To test these hypotheses, we perform a within-supplier analysis where we measure differential trade changes between the same supplier involved in the E&S scandal and its U.S. customers with different characteristics.

We show that, for the same supplier scandal, trade cuts are larger when U.S. customers are more likely subject to higher E&S investor pressure. First, we find that trade cuts are increasing in the customer's ESG rating.[7] Second, we find that trade cuts by publicly-listed U.S. customers are larger when the proportion of their shares held by E&S-conscious institutional investors (Gantchev, Giannetti, and Li, 2022) is higher. These

[6] See, e.g., https://www.ey.com/en_nl/supply-chain/how-diverse-sourcing-can-create-more-resilient-supply-chains.

[7] ESG ratings also partly reflect the customer's capability to manage financially-relevant ESG risks, and high-ESG customers could be more conscious in keeping only the relationships with high-E&S-performance suppliers.

investors might impose E&S pressure on firms via investor meetings, shareholder proposals, or voting, thus influencing the customer's supply chain structure. Third, we find that trade cuts by listed customers are larger after these customers receive shareholder proposals related to E&S issues—our most direct direct proxy for investors' engagement in E&S activities. Fourth, we expand the sample to include the universe of privately-held U.S. customers. We find evidence of trade cuts by these privately-held customers, suggesting that the E&S preferences of managers, private owners, and other stakeholders may also have real effects on supply chain networks. However, trade cuts implemented by publicly-listed customers are five times as large than those implemented by privately-held customers, highlighting supply chain adjustments due to investor preferences as a potential cost of being public.

Our within-supplier results thus suggest that investor E&S pressure plays an important role in driving the transmission of E&S shocks along the supply chain network. Additionally, differential reactions of different U.S. customers for the same supplier incident suggest that our main findings are unlikely driven by revised customer expectations about suppliers' product quality or financial position, as long as these expectations are independent of the customers' E&S preferences. As an alternative explanation, we ask whether customers react to potential pressure from their own end consumers (which may be implemented, for example, through product boycotts) using multiple proxies of industry end-consumer exposure. However, we do not find statistically-significant differences in the estimated effects between industries with high and low exposure to end consumers.

Next, we study supplier and industry characteristics that may affect the effectiveness of exit as a governance mechanism, and formally test for supply chain reallocation following a supplier E&S shock. First, we show that our results are stronger for smaller, privately-held suppliers, suggesting that U.S. customers' exit may impose a larger threat on these suppliers, and that international publicly-listed suppliers may already be exposed to direct E&S governance by capital markets.[8] Second, we show that our results are stronger when the industry of the supplier is more competitive, and when the inputs produced by the supplier are more substitutable. These results suggest that exit may not be an effective governance tool if customers' choice set of alternative suppliers is limited, and when inputs are specific to the customer's production process.

Third, we study how U.S. customers readjust their supply chain relationships following a supplier incident. Specifically, we ask whether U.S. customers switch to other international suppliers and, if so, whether the new suppliers are located in a different country than the original supplier and have high ESG scores. We find evidence of cross-country reallocation, suggesting within-country reputational spillovers. In addition, we find evidence of reallocation to high-ESG suppliers, confirming that U.S. customers actively adjust their supply chains to manage their E&S profiles.

In our final set of tests, we ask whether initial trade cuts are correlated with the incident-stricken supplier's future E&S performance and trade reversals. First, we find that larger trade cuts after the incident are associated with larger subsequent improvements in suppliers' RepRisk E&S performance ratings. On the other hand, no such improvements in E&S performance are observable among suppliers that do not experience a drop in trade after the incident. Second, we study how the interaction between initial trade cuts and subsequent changes in the supplier's RepRisk E&S rating is associated with the resumption of trade between the same customer and supplier, and we find that joint trade cuts and rating increases are associated with trade reversal. Overall, these results provide evidence of a customer governance by exit mechanism, whereby a temporary trade reduction may improve the environmental and social performance of smaller international suppliers.

Our results contribute to the literature on how environmental and social considera-

[8]This result also suggests that the supply chain relationship databases that only contain relationships between publicly-listed suppliers and publicly-listed customers are likely to underestimate the extent of trade cuts.

tions shape the structure of global supply chains. Dai, Liang, and Ng (2021b) documents positive assortative matching between customers and suppliers in terms of corporate social responsibility (CSR) ratings. Schiller (2018) finds that E&S policies, as measured by the components of ESG ratings, propagate from customers to suppliers. Ben-David, Jang, Kleimeier, and Viehs (2021) and Dai, Duan, Liang, and Ng (2021a) show that U.S. firms outsource part of their carbon emissions to foreign suppliers, and that this decision can be linked to investor, customer, and government pressure. We contribute to this literature by conducting the first large-sample study of trade cuts following supplier E&S incidents, and by proposing a governance by exit effect whereby customers' trade cuts can discipline suppliers' adherence to ESG standards.

In a related study, Koenig and Poncet (2022) documents a drop in exports to France by Bangladeshi retailers connected to the 2013 collapse of Dhaka's Rana Plaza building. Our paper generalizes this event study to a broader sample of E&S incidents, and establishes investor pressure as the main driver of the observed trade cuts. In another related study, Pankratz and Schiller (2021) documents customer responses and permanent relationship terminations following perceived changes in suppliers' climate risk exposure. Different from Pankratz and Schiller (2021), our paper focuses on actual E&S scandals rather than on perceived supplier risk. Additionally, we are able to study intensive-margin trade reductions not possible using other datasets.[9] Different from both studies, our paper establishes investor-induced customer exit as a disciplining threat for international suppliers.

Our paper also contributes to the literature on institutional investors' role in monitoring firms' E&S activities (e.g., Krueger, Sautner, and Starks, 2020; Atta-Darkua, Glossner, Krueger, and Matos, 2022, and Azar, Duro, Kadach, and Ormazabal, 2021). To the best of our knowledge, our paper is the first to study how institutional investors E&S prefer-

[9] For example, an the often-used FactSet Supply Chain Relationships (formerly, Revere) dataset only provides sales data for less than 10% of the sample (Pankratz and Schiller, 2021). Therefore, it is only possible to study the extensive margin of supply chain relationships using this data.

ences affect trade activity with suppliers and the structure of international supply chains. Our paper complements Gantchev, Giannetti, and Li (2022), which shows that E&S incidents are followed by (limited) investor divestitures and large greenhouse emission reductions when firms are owned by E&S-conscious investors. Rather than focusing on the direct disciplining role of exit by E&S-conscious investors, we document an indirect disciplining role of supply chain exit by customers owned by these investors.

More broadly, our paper shows how E&S-minded institutional investors can exert pressure on privately-held firms outside of their country and, possibly, their investment universe. In 2019, private firms' GHG (CO2-equivalent) emissions contributed to 59% of global corporate fossil fuel emissions (Atta-Darkua, Glossner, Krueger, and Matos, 2022).[10] Our results suggest that holding stakes in U.S. publicly-listed firms with a wide global supplier network can act as conduit to monitor and discipline private suppliers in far-flung countries.

2 Empirical Analysis

2.1 Data Sources and Matching

In this section, we describe our data sources on cross-border shipments and supplier E&S scandals, and how we use these sources to construct our main matched sample. In Appendix Table A1, we provide definitions for all the variables used in the paper.

2.1.1 Cross-border Shipments

We obtain shipment-level data on transactions between foreign suppliers and U.S. customers over the 2007-20 period from the S&P Global Panjiva database. Title 19 of the United States Code of Federal Regulation (CFR) requires U.S. firms to report shipment

[10]On the other hand, Shive and Forster (2020) finds that U.S. privately-held firms have lower greenhouse gas emissions, as compared to similar U.S. publicly-listed firms.

details in cargo declarations to the U.S. Customs and Border Protection (CBP). For each shipment transaction, Panjiva provides information about the sender, the consignee, the origin and destination of the shipment, the product codes and descriptions of the items contained in the shipment, and the shipment container specifications.

We link U.S. consignees in Panjiva to their ultimate parent in Compustat, and then aggregate the Panjiva data to the Panjiva supplier-Compustat customer-year level. In order 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. In building the panel, we also add two years before the first year in which a given supplier-customer relationship appears in our sample to account for relationships' ramp-up over time (Intintoli, Serfling, and Shaikh, 2017). Similarly, we extend the panel by two years after the last year in which the relationship is observed in the data to account for relationship deterioration. All transaction values are set to zero for these extended periods, as well as for all the years in which transaction values are missing between the first and the last relationship years.[11]

2.1.2 E&S Incidents

We gather the universe of negative ESG-related incidents for the period 2007-2021 from RepRisk, a leading business research provider which screens media, regulatory, and commercial documents searching for companies' ESG-related incidents (Gantchev et al., 2022).[12] RepRisk classifies each incident into environmental ("E"), social ("S"), and governance ("G") categories. Environmental incidents are incidents related to pollution, ecosystems and landscapes, overuse and wasting of resources, and animal mistreatment. Social incidents involve community relations (such as human rights abuses and social discrimination) and employee relations (such as forced or child labor and occupational

[11] Appendix Table A2 describes the sample selection process for the Panjiva data.

[12] According to RepRisk, a team of analysts manually verifies that each incident is indeed ESG-related, records the incident location and the firms involved in it, and ranks the severity of the incident.

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.

In this paper, we focus on incidents such as waste management and human rights abuses that are prone to negative externalities for local communities and thus could carry downstream reputational effects above and beyond pure business risk. While some governance-related incidents (such as bribery and extortion) resemble environmental and social incidents in this respect, other governance-related incidents (such as executive compensation or accounting fraud) are the result of failures in private contracting between suppliers' shareholders and their management, and are unlikely to have such direct externalities to local communities.[13] As a result, in what follows we focus on environmental and social ("E&S") incidents, and in the main analysis exclude governance related-incidents from the RepRisk sample.

We use a fuzzy name algorithm to link Panjiva foreign suppliers (both privately-held and publicly-listed) to their RepRisk E&S incidents. To ensure at least three years of cross-border shipment data before and after an incident, we focus on incidents occurring between 2010 and 2018. Panel A of Table 1 provides a description of the resulting matched sample, which consists of 1,049 (1,010) supplier-years (unique suppliers) and 1,319 (1,281) relationship-years (unique relationships) hit by an E&S incident.[14] We find that 158 incidents are related only to "E" issues, 629 to "S" issues, and 273 to both "E" and "S" issues. In Panel B of Table 1, we provide a breakdown of supplier incidents by the Fama-French 48 industry of the U.S. customer. We can see that industries that

[13]How such corporate governance incidents affect customer-supplier relationships has also been studied in the literature. For example, Karpoff, Lee, and Martin (2008) argue that accounting misconduct can reveal suppliers' inability to fulfil orders or support warranties. Johnson, Xie, and Yi (2014) show that fraud increases customers' wariness in dealing with dishonest management, thus reducing product market interactions.

[14]We start with 4,975 supplier-year E&S incidents over the 2010-2018 period, corresponding to 6,565 supplier-customer-years, and to 2,288 unique customer-years. After removing observations with confounding incidents in the three years before and after the incident, we are left with 1,049 supplier-year events, corresponding to 1,319 supplier-customer-years, and to 838 unique customer-years.

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, the distribution of supplier incidents is spread out 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 10 incidents.

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

Before moving to our main estimation exercises, we establish the economic relevance of supplier E&S incidents for U.S. importers by documenting customers' stock price reactions around supplier incidents' announcements, both unconditionally and within out matched sample. We start with all E&S incidents recorded by RepRisk, and remove incident observations with other confounding events 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 our CAR estimation results in the full sample. The first row documents an average -10 basis point CAR for customer stocks around the announcement of supplier incidents, significant at the 1% confidence level. The second and third rows respectively show that the results are statistically similar and economically larger when we increase the CAR estimation window to [-3, +3] and [-5, +5] days around the supplier incident announcement. In Panel B, we document results of similar magnitude but lower statistical significance in our baseline sample, perhaps due to smaller number of observations relative to the overall RepRisk data. Overall, the results of this event study analysis confirm that supplier incidents trigger negative customer stock price reactions, and are thus likely to have a material impact on customers.

2.1.4 Anecdotal Evidence

In this section, we present one anecdote from our sample to set the hypotheses that we study in the rest of the paper. On March 11, 2015, nearly 5,000 workers of a shoe factory in southern China started a strike over wage benefits.[15] On March 10, 2015, Reprisk reported an E&S incident for the owner of the factory, Stella International Holdings Ltd (Stella), and flagged the company for "poor employment conditions." The stock price of Deckers Outdoor Corporation (Deckers), one of the main U.S. customers of Stella, declined by 2.1% in a span of three days from USD 72.25 on March 10, 2015 to USD 70.69 on March 13, 2015.

In Appendix Figure A1, we display trade dynamics between Stella and Deckers around Stella's incident. The figure shows that almost immediately Deckers stopped sourcing from Stella and did not resume trade until Stella's RepRisk ESG rating improved in 2020.[16] In the rest of the paper, we investigate whether and how this anecdote generalizes to a broader sample of firms, and study the underlying incentives of customers and suppliers.

2.2 Panel Structure and Estimation Strategy

In our main analysis, we use a "stacked" difference-in-differences regression design (see, e.g., Cengiz, Dube, Lindner, and Zipperer, 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 denote by t the year of the incident, and we construct cohorts of treated and control trade relationships in an interval of $[t-3, t+3]$ years around 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 ex-

[15]See, e.g., https://www.reuters.com/article/us-china-strike-idUSKBN0M70EZ20150311 for media coverage of the case.

[16]Our data ends in 2020, which prevents us from studying long-run trade reversals in this example.

periencing an incident at time t) and their other suppliers not experiencing any incident in the same $[t-3, t+3]$ window; and ii) relationships in which none of the suppliers experience any E&S incident in the $[t-3, t+3]$ window. To mitigate potential confounding variation arising from repeated treatment over time (Baker, Larcker, and Wang, 2022), we also exclude any supplier E&S incident that follows or is followed by another incident involving the same supplier 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 our main regression model:

$$Y_{i,j,c,t} = \beta_1 Treat\ Supp_{j,c} \times Post_{c,t} + \beta_2 X_{i,t-1} + \gamma_{i,j,c} + \tau_{i,c,t} + \epsilon_{i,j,c,t}, \tag{1}$$

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; $X_{i,t-1}$ is a matrix of customer-specific lagged characteristics; $\gamma_{i,j,c}$ 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 as well as to capture time-varying customer characteristics such as demand shocks. In all our specifications, we cluster standard errors at the supplier-cohort level.

In our main specifications, we measure $Y_{i,j,c,t}$ as the natural logarithm of one plus the number of containers imported by customer i from supplier j in year t.[17] In these regressions, the main coefficient of interest is β_1, pinning down the percentage change in the number of containers imported by U.S. customers from treated suppliers after the

[17]We focus on containers due to their uniform measurement. We also show that our results are robust when we use the natural logarithm of the annual number of shipments from the supplier to the customer; the natural logarithm of the total weight of all annual shipments from the supplier to the customer; and the natural logarithm of the annual quantity of all shipments from the supplier to the customer as alternative measures of trade.

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, the coefficient β_1 identifies changes in the relative probability of trade between treated and control firms before and after the E&S incident.

2.3 Summary Statistics

Our final stacked panel consists of 1,000,950 supplier-customer-cohort-year observations for the period 2010-2018. In Panel C of Table 1, we report summary statistics for the main dependent and independent variables in our sample. The first two rows of Panel C show that around 0.7% of our supplier-cohort observations are treated with an E&S incident, and that around 71% of our sample consists of control observations where a U.S. customer is linked to the affected supplier but has at least one other international supplier. In other words, while the unconditional probability of an E&S incident is relatively low in our sample, U.S. customers have diversified supply chain structures that include many international suppliers. As a result, the probability that a U.S. customer in our sample is indirectly exposed to an E&S incident through one of its suppliers is large. As a comparison, Gantchev et al. (2022) find that the annual unconditional probability of a firm being *directly* hit by an E&S scandal is 22%, highlighting 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, i.e., the number of containers shipped from suppliers to customers in a given year, and the annual probability of a container shipment. The average supplier in our data 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 that we use in some of our empirical specifications. We define *Size* as the natural logarithm of the customer's total assets, *MTB* (market to book) as total assets plus market value of equity minus the book value of equity divided by total assets, *Lev* (the leverage ratio) as long-term debt plus short-term debt scaled by total assets, *R&D* as research and development expenditures scaled by total assets, *Capx* as the ratio of capital expenditure to total assets, and *Cash* as the ratio of cash and cash equivalents to total assets. All the control variables are lagged by one year, and are winsorized at the 1% and 99% levels.

3 Supplier E&S Incidents and Trade Relationships

In this section, we present our baseline results on how U.S. customers change trade after foreign suppliers' E&S incidents, followed by cross-sectional analyses based on incident characteristics.

3.1 Baseline Results

Table 3 reports our estimates of regression model (1), where we compare trade changes between incident-stricken international suppliers and their U.S. customers in a six-year window around the incident, and trade changes between other international suppliers and their U.S. customers during the same time window. We first focus on overall trade changes following the E&S incident, and later break down our estimates between the intensive and the extensive margins. Our initial baseline sample includes publicly-listed U.S. customers and both publicly-listed and privately-held international suppliers.

The first column of Table 3 reports our baseline result. In this column, we control for relationship (i.e., customer-supplier) pair-cohort fixed effects and for 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, and imports *by the same customers* from suppliers not directly involved in the incidents over the same time period. Column (1) shows that, over the three years following a supplier's E&S incident, imports by U.S. customers decline on average by 11.1% relative to the imports by the same U.S. customers from unaffected suppliers. These estimates are quantitatively large, and correspond to 0.105 containers per year (relative to the unconditional sample mean) and to 7.99% of a standard deviation. Together with the results of Table 2, this result suggests that E&S incidents have an effect not only on customers' stock market performance, but also on their supply chain sourcing.

Next, we 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 results 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 4.2% more likely to be terminated after the supplier is involved in an E&S incident. This estimate is quantitatively large, and it implies a nearly 50% increase relative to the 9% unconditional relationship termination rate in our sample. Column (3) similarly shows that, if we condition on trade continuation and study pure intensive margin effects, the average U.S. customer decreases its imports by 9.5% following a supplier's E&S incident, corresponding to a 0.0895 drop in annual container shipments relative to the unconditional mean and to 6.8% of a standard deviation.

Our intensive margin estimates of Table 3 show that even when customers continue their trade relationships, they severely reduce the shipments from suppliers involved in an E&S incident. Such partial trade cuts could imply that U.S. customers start diversifying their supply chains away from affected suppliers, but are unable to fully terminate the relationship (e.g., due to supplier specificity or the unavailability of competitive alternatives). A complementary hypothesis is that customers may be sending a costly signal to suppliers to improve their E&S performance. In Section 5.3, we document trade reversals when suppliers improve their E&S performance following initial trade cuts. This finding lends support for the interpretation of partial adjustments as an effective threat mechanism.[18]

3.2 Incident Characteristics

In Table 4, we provide the results of cross-sectional tests on our main result based on incident characteristics. In this table, we report cross-sectional tests on the main result from column (1) of Table 3, and we present the corresponding intensive margin results in Appendix Table A3. First, we ask whether trade cuts vary across environmental ("E") and social ("S") incidents.[19] Column (1) documents a slightly larger (but statistically not significant) effect for pure social incidents as compared to pure environmental incidents, and the largest effects for incidents that have both environmental and social implications, suggesting that such incidents carry the largest downstream reputational effects.

Second, we ask whether trade cuts increase with the severity of the scandal, using the definition of severity offered by RepRisk.[20] In column (2), we show that while imports shrink for both high-severity and low-severity scandals, trade cuts are larger for higher-severity scandals.

[18] In principle, relationship terminations could also be due to supplier "window-dressing" (e.g., by registering the supplier under a different company name, or by adding additional phantom suppliers along the supply chain to hide direct connections) or to ESG assortative matching (as in Dai et al., 2021b). However, we believe these interpretations to be less likely in light of our partial trade adjustment results.

[19] Environmental incidents are related to pollution, ecosystems and landscapes, overuse and wasting of resources, and animal mistreatment. Social incidents involve community relations (such as human rights abuses and social discrimination) and employee relations (such as forced or child labor and occupational health and safety accidents).

[20] RepRisk provides a proprietary coding of scandal 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 very few cases are actually coded as high-severity in the RepRisk data.

Third, we link the value losses documented in Table 2 with the trade cuts documented in our baseline tests. Column (3) shows that the trade cuts are larger in the sub-sample of customers that experience larger negative market reactions upon incident announcement. This result suggests that costly trade cuts and reallocation to different suppliers have a negative impact on customer value, and that the announcement returns documented in Table 2 are at least partly due to a negative cash flow effect. Finally, we look at whether the effects are stronger in the sample period after the 2015 Paris Agreement, which presumably triggered media and policy discussions on firms' ESG posture, as well as pressure from U.S. institutional investors. In column (4), we indeed see that our baseline effects are larger in the post-2015 period.

Overall, Table 4 and its extensive margin counterpart Appendix Table A3 show that our results are stronger in the cross-section for incidents more likely to generate adverse downstream reputational effects, and in the time-series in periods of greater awareness for E&S-related issues. Together, these results provide a first piece of evidence that the trade cuts we observe in the data are indeed driven by E&S incidents and not by other correlated shocks at the supplier level.

4 Investor vs. End-Customer Preferences

The results from the previous section suggest that E&S shocks have real transmission effects along the supply chain network by reducing trade between U.S. customers and suppliers affected by E&S incidents. In this section, we aim at separating the possible sources of pressure that trigger these trade adjustments. First, U.S. customers could respond to the preferences of their ESG-minded institutional investors. Alternatively, U.S. customers could react to supplier scandals due to pressure from their own end-consumers.

To test these hypotheses, we add supplier-cohort fixed effects to our main regression specification (1), thus comparing import responses to the *same* supplier incident by U.S. customers with different investor characteristics and different end-consumer exposure. For example, these tests allow us to compare trade changes between a supplier involved in an E&S incident and its U.S. customers subject to stronger institutional investor E&S preferences, and trade changes between the *same* supplier and its U.S. customers subject to weaker investor E&S preferences. In our tests, we also control for partitioning variable-year fixed effects to capture general trends that certain characteristics (such as, e.g., E&S salience) could have on international trade, irrespective of E&S incidents.

4.1 Investors' ESG Preferences

We start by analyzing investor preferences as a potential explanation for the trade cuts we observe in the data. We report results for the overall effects in Table 5, and the corresponding results for the extensive margin effects in Appendix Table A4. In column (1) of Table 5, we first use the customer's ESG rating to proxy for institutional investors' portfolio selection preferences. We use the Refinitiv ESG score of the customer when a supplier scandal hits, and define *High E&S* as a binary variable equal to one for customers with above-the-median ESG scores, and equal to zero otherwise. In column (1), we find a significantly negative interaction effect between $TreatSupp \times Post$ and $High\ ESG$, confirming that our results are driven by customers with better ESG profiles. Customer ESG ratings capture not only investor preferences, but also a firm's ability to manage its financially-relevant ESG risks. In this sense, the results from column (1) could be a reflection of high-ESG customers being more active in keeping relationships with suppliers with high E&S performance.

In columns (2) and (3) of Table 5, we use more direct proxies for shareholder E&S preferences. First, we follow the approach developed in Gantchev et al. (2022) and identify E&S-conscious investors based on the Refinitiv ESG ratings of their portfolio holdings.[21] We create an indicator variable, *High IO_ESG*, equal to one if the proportion of the customer's outstanding shares owned by E&S-conscious investors in the event year is greater than the sample median and equal to zero otherwise, and interact this indicator variable with the treatment effect indicator $TreatSupp \times Post$. Column (2) shows that the coefficient associated with $TreatSupp \times Post \times HighIO\_ESG$ is negative, suggesting that customers are more likely to reduce imports from treated suppliers when their shareholders have stronger E&S preferences. On the other hand, the coefficient associated with the baseline treatment effect $TreatSupp \times Post$ is economically small and statistically not significant at conventional levels, suggesting that customer firms do not adjust their supply chain structures in response to supplier incidents when they are not owned by E&S-conscious investors.

Second, we use shareholder proposals related to E&S issues as a direct proxy for investors' engagement in E&S activities. We obtain information about shareholder proposals from Institutional Shareholder Services (ISS), and categorize proposals on socially responsible investments (SRI) as E&S proposals. Due to ISS data availability, we restrict our stacked panel to U.S. customers in the S&P 1500 index. For each customer in the matched sample, we then construct a binary variable, *ESGProposal*, equal to one if the customer received at least one E&S (SRI) proposal from event year $t-3$ to event year $t-1$, included. Column (3) confirms that only the coefficient associated with the interaction term $TreatSupp \times Post \times ESGProposal$ is negative and statistically significant, suggesting that customers are more likely to reduce imports from treated suppliers when they have recently faced more active E&S engagement by shareholders. Similar to column (2), we find no evidence of a baseline treatment effect on customers not experiencing E&S proposals by their investors before the supplier incident.

[21] As in Gantchev et al. (2022), we classify investors with average portfolio ratings in the top tercile as E&S-conscious, and the remaining investors as non-E&S-conscious. Different from Gantchev et al. (2022), which uses the overall ESG rating provided by Refinitiv to measure a firm's E&S performance, we use the average environmental and social (E&S) ratings to construct our measures of investor E&S consciousness. We do not observe significant divestitures of customers' stocks by E&S-conscious investors after suppliers become involved in E&S incidents.

Finally, in column (4) we ask whether our trade exit results are only present in public firms, or whether private firms also experience trade reductions following E&S incidents by their suppliers. In order 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 (4) shows a baseline 2.3% reduction in imports following a supplier E&S incident. This coefficient is statistically significant at the 10% confidence level, and suggests that even privately-held U.S. customers reduce trade. However, the effect is almost four times as large for publicly-listed customers. The interaction coefficient between the baseline treatment effect indicator, $TreatSupp \times Post$, and the indicator for publicly-listed customers, *Public Cust*, is negative and statistically significant at the 1% level, and it implies an overall 13.2% reduction in trade following a supplier E&S incident. Overall, the results of column (1) show that, in response to the same E&S incident, public firms reorganize their supply chains more aggressively than privately-held firms, and provides an additional piece of evidence consistent with investor preferences being the main driver of the observed trade adjustments.[22]

The results of column (4) also add to the ongoing debate on the ESG-related costs and benefits of being publicly listed.[23] First, the results 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 below), and privately-held customers may be shielded from these costs relative to their publicly-held peers. Second, the current trend of public

[22] An alternative explanation for the results in column (4) is that privately-held firms are more constrained in replacing their existing suppliers. However, Appendix Table A5 shows that our results hold within the sample of financially-constrained publicly-listed customers, making this explanation less likely.

[23] For example, Jason Jay, director of the MIT Sustainability Initiative, argues that some companies will refrain from going public to avoid reporting complexities or sell their dirty assets if the SEC imposes Scope 3 Emission disclosure requirements: *"Companies might not choose to go public because [they think], 'I'm going to be subject to so much complexity of reporting, so I'm just going to stay in the private markets and be opaque to the world in terms of this kind of transparency'"* (Vereckey, 2022).

firms' delistings in the U.S. (e.g., Doidge, Karolyi, and Stulz, 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 adherence.

In Appendix Table A6, we also confirm that our results are statistically and economically robust when we include restrictive supplier-cohort-year fixed effects to the empirical specification, thus controlling for time-varying economic conditions affecting the supplier. Overall, the results of Table 5 and Appendix Table A6 support the hypothesis that investor pressure is an important determinant of the observed trade adjustments following suppliers' E&S incidents. Importantly, these within-supplier-cohort(-year) results also reduce potential concerns that the observed trade changes are reflective of changes in suppliers' business or financial risks orthogonal to E&S.[24] Given that U.S. customers facing stronger investor pressure are those implementing the largest trade cuts in response to the same supplier incident, we can infer that these customers are either more active in managing business risks correlated with E&S (such as regulatory actions, fines, or other restrictions), or that their trade cut decisions come purely from their E&S preferences.

4.2 End-Consumer Exposure

While our results so far suggest that investor preferences play an important role in driving supply chain adjustments to E&S shocks, an alternative explanation for these adjustments is the potential pressure faced by U.S. importers from their own end consumers. To test this hypothesis, we conduct two additional sets of tests, in which we study differential trade adjustments based on customer firms' cross-sectional exposure to end consumers. Our main assumption in these tests is that firms with higher end-consumer

[24]For example, one could have argued that an E&S incident may simply signal poor financial conditions of the supplier, or low product quality.

exposure (such as retail and apparel brands) face more (social and traditional) media coverage of their international supply chains. As a result, suppliers' E&S incidents may result in more widespread consumer boycotts in these firms, and lead to stronger supply chain adjustments.

Table 6 reports the results of our cross-sectional tests on importers' end-consumer exposure. The corresponding results for the extensive margin effects in Appendix Table A7. Our first proxy for importers' end-consumer exposure is the importer industry's share of final-user sales to total industry sales reported in the 2007 U.S. Bureau of Economic Analysis (BEA) input-output tables. In column (1), we interact a binary variable for industries with above-median final users sales' shares, *High %Final Users*, with our main treatment effect indicator to test for incremental trade changes by importers with high end-consumer exposure. In column (2), our second proxy compares business-to-customer (B2C) industries (where individual consumers are the predominant customers) with non-B2C industries.[25] We find no statistically significant evidence of an interaction effect between the E&S incident and the importer's end-consumer exposure, suggesting that firms with high and low end-consumer exposure implement similar supply chain adjustments following a supplier E&S scandal. In Appendix Table A8, we also show null interaction results when we include restrictive supplier-cohort-year fixed effects. Overall, the results of Table 6 and Appendix Table A8 do not provide support to the explanation that importers reshape their supply chains in response to (or anticipation of) end-consumer pressure.[26]

[25]We follow Lev, Petrovits, and Radhakrishnan (2010) and Flammer (2015) and identify B2C industries based on their four-digit SIC codes.

[26]Similarly, Liaukonytė, Tuchman, and Zhu (2022) document a very short-lived and limited effect of social-media generated consumer boycotts on individual goods' purchases and total firm sales.

5 Suppliers, Reallocation, and Trade Reversals

We now focus on the the long-term consequences of trade cuts following supplier E&S incidents. To do so, we perform tests along four dimensions. First, we confirm that our baseline results are stronger when customer switching costs are lower, suggesting that the ability to switch suppliers poses a natural constraint on customer supply chain readjustments. Second, we show that U.S. customers switch to suppliers located in different countries than the original supplier and to suppliers with good ESG performance. Third, we show that customers' initial trade cuts are correlated with how the incident-stricken supplier improves its future E&S performance. Fourth, similar to the anecdotal evidence on Stella and Deckers from Section 2.1.4, we show that if suppliers improve their E&S performance following an initial trade cut, they are able to re-establish trade with their U.S. customers in subsequent periods.

5.1 Supplier Characteristics and Switching Costs

In Table 7, we study cross-sectional variation in our baseline result based on suppliers' characteristics and different proxies for customers' costs of switching to other suppliers. As in the previous sections, in this table we present overall effects, and we relegate the extensive margin results to Appendix Table A9. First, while data on international supplier characteristics is scarce (most of the suppliers in our data are privately-held), we can study cross-sectional effects based on whether the supplier is privately-held or publicly-listed. Our hypothesis is that, when suppliers are publicly-listed, they already directly exposed to external governance of their E&S performance. Hence, customers may rely on this external governance rather than on trade cuts to discipline the supplier after an incident. In column (1) of Table 7, we confirm that our baseline effects are indeed stronger (both economically and statistically) when suppliers are privately-held. When combined with our results on investor preferences in Section 4, the result of column (1) suggests a special role for ESG-minded investors of U.S. public firms in improving the E&S performance of small, privately-held international suppliers. Our results suggest that, effectively, U.S. customers may end up exporting the E&S preferences of their own investors to foreign suppliers.

Second, we also hypothesize that large suppliers have stronger bargaining power with their customers than small suppliers, which may reduce the effectiveness of a governance threat. Column (2) presents results consistent with this hypothesis: the data shows a large negative effect on trade with small suppliers, and no statistically significant effects on trade with large suppliers.

Third, we ask whether the observed effects vary with the competitiveness of customers' input market, as well as with input specificity. In these tests, we hypothesize switching costs to be relatively low when suppliers operate in competitive markets and sell homogeneous goods, leading to larger trade cuts following an E&S incident. We measure the competitiveness of the supplier's two-digit HS product market based on the Herfindahl-Hirschman Index (HHI) in the event year. In column (3) we confirm that the effect is significantly larger when supplier HHI is low, i.e., when the customer's input market is more competitive.[27] Next, we measure how substitutable the supplier's two-digit HS product is based on the Rauch (1999) differentiation index. As shown in column (4), the effect is significantly larger when suppliers sell homogeneous products.[28] These results suggest that the threat of exit may be less credible if customers' choice set of alternative suppliers is limited, and that governance by exit may be less effective when supplier inputs are highly specific to the customer's production process.

[27] To calculate HHI, we take individual shares of trade of each international supplier to U.S. customers in this 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.

[28] If a supplier sells more than one product, we require all products to be homogeneous for indicator assignment. Our results 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).

5.2 Supplier Reallocation

Next, we formally test how 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 original supplier involved in the E&S incident.

To identify such reallocation effects, we borrow from Berg, Reisinger, and Streitz (2021) and estimate the regression model (2):

$$\begin{aligned} Y_{i,j,c,t} &= \beta_1 \mathit{Treat\ Supp}_{j,c} \times \mathit{Post}_{c,t} + \beta_2 \%\mathit{Treat\ Supp}_{i,c} \times \mathit{TreatSupp}_{j,c} \times \mathit{Post}_{c,t} \\ &\quad + \beta_3 \%\mathit{Treat\ Supp}_{i,c} \times \mathit{TreatCust},\ \mathit{ControlSupp}_{j,c} \times \mathit{Post}_{c,t} \\ &\quad + \beta_4 X_{i,t-1} + \gamma_{i,j,c} + \tau_{c,t} + \epsilon_{i,j,c,t}, \end{aligned} \tag{2}$$

where $\%\mathit{Treat\ Supp}_{i,c}$ denotes the fraction of suppliers hit by an E&S scandal in each customer-cohort, measured in the year before the shock; $\mathit{TreatCust},\ \mathit{ControlSupp}_{j,c}$ is an indicator for control suppliers of customers with at least one supplier hit by the E&S scandal; and the remaining variables are identical to those in specification (1). The coefficient of interest in specification (2) is β_3, which identifies the reallocation effects on 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). Similar to Berg et al. (2021), this coefficient 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.[29] We predict the sign of this coefficient to be positive if customers switch from suppliers with

[29] Different from 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 on indirect spillovers through the network, as we do in this section. In this sense, our estimation strategy also bears resemblance to the reallocation specifications of Giroud and Mueller (2019), again with the difference that U.S. customers in our sample are not affected by the treatment directly but only through their suppliers.

E&S incidents to other international suppliers.

Table 8 reports results consistent with these predictions. First, column (1) confirms a negative and statistically significant 11.1% drop in trade between treated suppliers and their customers after the treatment. Second, column (1) also documents a positive and statistically significant reallocation effect on control suppliers. The estimates suggest that a 1% increase in the share of treated suppliers in a given cohort increases trade between their linked customers and *control* suppliers by 0.8% after the treatment, on average. In other words, U.S. customers partially replace their scandal-hit suppliers with other international suppliers. Finally, column (1) shows no spillover effects on the treated group, suggesting 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 located in the same country as the treated suppliers, or to suppliers located in different countries. On the one hand, switching to suppliers from the same country may be less costly (due, e.g., to familiarity with the local institutional environment). On the other hand, the supplier's E&S scandal might hurt the reputation of all suppliers in its country, and thus motivate customers to search for new partners in other countries to diversify their risks. To test this hypothesis, we split the indicator $TreatCust,\ Control\ Supp_{j,c}$ into two indicator variables: $Treat\ Cust,\ Control\ Supp,\ Same\ Country_{j,c}$, indicating control suppliers (of customers linked to treated suppliers) located in 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 8 shows that the reallocation effects manifest themselves only in the sample of suppliers from other countries, suggesting that E&S scandals can have negative impacts not only on the affected firms, but also on the reputation of other suppliers in their countries.

In column (3), we also ask whether customers switch to suppliers with high ESG ratings by splitting the indicator $TreatCust,\ Control\ Supp_{j,c}$ into two indicator variables for control suppliers with average RepRisk rating before the scandal in the top quintile of the distribution ($Treat\ Cust,\ Control\ Supp,\ HighSupp\ E\&S_{j,c}$), and in the bottom four quintiles of the distribution ($Treat\ Cust,\ Control\ Supp,\ LowSupp\ E\&S_{j,c}$). Although our sample shrinks considerably due to lack of ESG rating data availability for international suppliers, column (3) shows a significantly negative baseline treatment effect and a positive spillover effect only on suppliers with high ESG ratings. The evidence from column (3) thus adds support to our hypothesis that U.S. customers actively manage their E&S risks by switching to other international suppliers once one of their suppliers is affected by an E&S incident.

In Appendix Table A10, we also provide evidence suggesting that re-optimizing supply chains is costly for customers. In particular, customers that implement trade cuts with suppliers hit by an E&S incident experience reductions in their profitability—as measured by their gross profit margins—in the years after the incident, suggestive of higher cost of goods sold (arising, e.g., from second-best supplier sourcing) or even constraints in selling products (arising, e.g., from lack of alternative inputs). On the other hand, customers that do not implement such trade cuts do not experience significant changes in their gross profit margins after the incident.

5.3 Supplier E&S Improvements and Trade Reversals

What happens to the suppliers themselves when their trade with U.S. customers decreases? We start by breaking down our main results from Table 3 into annual changes before and after the incident. In Panel A of Figure 1, we show the evolution of the baseline treatment effect (corresponding to column (1) of Table 3) between years $t-2$ to $t+3$ of the event window, taking year $t-3$ as a baseline. Panel A shows a large and statistically significant 10% drop in container shipments one year following the supplier incident, which persists throughout the entire event period.[30] In other words, for the average relationship in our sample, the data shows no significant reversal to the pre-incident trade levels even three years after the incident. In Panel B of Figure 1, we similarly show the evolution of the treatment effect on the intensive margin (corresponding to column (3) of Table 3). Similar to Panel A, Panel B documents a persistent 15% drop in the probability of a trade relationship in the three years following the incident.

While the full-sample results in Figure 1 show no unconditional evidence of trade recoveries after the initial supplier incident, one of our main hypotheses is that some customers may use trade cuts as a costly governance threat to ensure their suppliers' E&S adherence. In such cases, initial trade cuts may be followed by subsequent improvements in the supplier's E&S posture, and by the eventual resumption of trade.

To study whether E&S incidents and the associated import cuts by U.S. customers trigger adjustments in the supplier's E&S performance and 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 risk rating.[31] Second, we ask whether U.S. customers' trade cuts and international suppliers' ESG rating improvements are jointly associated with future trade reversals. We report our results in Table 9.

In Panel A of Table 9, we study the dynamic response of suppliers' RepRisk ESG ratings following trade cuts by U.S. customers. Specifically, we test whether a supplier's ESG risk rating after the scandal varies based on the size of customers' trade cuts in a

[30] Panel A of Figure 1 also shows a small decrease in trade in year $t-1$ relative to year $t-3$, possibly due to customers' early knowledge of suppliers' E&S-related issues. This decrease is not statistically significant at conventional levels.

[31] Similar to RepRisk ESG incidents, RepRisk ESG ratings are updated daily based on negative news in the media. These ratings are measured on a AAA to D scale, with D being the worst, and are widely used by asset managers to monitor the ESG performance of their portfolio (see, e.g., https://corpgov.law.harvard.edu/2017/07/27/esg-reports-and-ratings-what-they-are-why-they-matter/). Not all suppliers have a RepRisk ESG rating, and thus we limit the sample to suppliers for which RepRisk ESG ratings are available around the initial incident.

window of three years (i.e., from year $t-1$ to year $t+1$) around the E&S incident. For each foreign supplier, we aggregate export changes around the E&S scandal across all U.S. customers, and then split the sample based on the percentile distribution of aggregate trade changes. Column (1) of Panel A corresponds to the sub-sample of suppliers experiencing the largest negative trade changes (the 25% percentile of the aggregate distribution, corresponding to an overall trade change of -29% over the three years around the incident); column (2) corresponds to the sub-sample of suppliers experiencing a trade change within the interquartile range; and column (3) corresponds to the sub-sample of suppliers experiencing the smallest drop in trade in our sample (i.e., trade changes above the 75% percentile).

Panel A of Table 9 shows that, on average, RepRisk ESG risk ratings decrease after the E&S incident, and that this pattern persists over time. This result is expected, as initial E&S incidents are often followed by negative media mentions, which increase the supplier's ESG risk. However, as shown in column (1), the negative effect of the incident on ESG risk ratings is statistically and economically short-lived (as compared to the pre-incident benchmark) when U.S. customers significantly cut trade with affected suppliers. Indeed, column (1) shows a rating recovery 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)).[32]

Next, we ask whether improved ESG ratings can be related to trade reversals. We group treated and control relationships into cohorts of $[t+1, t+6]$ years from the supplier's initial E&S incident. We classify observations in years $[t+1, t+3]$ from the incident as "post-incident" observations in which suppliers may adjust their E&S poli-

[32] In related tests, we also investigate whether import cuts by a customer result in ESG rating improvements by the customer's other suppliers not directly involved in the incident. We do not find evidence of such spillovers, suggesting either that the other suppliers operate at the level of E&S desired by the customer, or that trade cuts with one supplier do not change the (perceived) probability of trade cuts with other suppliers following an incident.

cies, and observations in years $[t+4, t+6]$ from the incident as "post-adjustment" observations. Next, we split treated relationship cohorts into sub-samples 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. 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. Since on average trade with control group suppliers increases after the incident (as documented in Section 5.1), absolute trade cuts are smaller than relative trade cuts.

The independent variables thus include four mutually-exclusive interaction terms between indicator variables for customer trade cuts between the pre- and post-incident periods ($CutTrade = 1$), and supplier rating increases in the post-incident period ($IncRating = 1$). We set the indicator variable $CutTrade$ equal to one if $\Delta Trade$ is negative (column (1)), if $\Delta Trade$ is lower than the 25^{th} percentile of the trade cut distribution (-29%, column (2)), and if $\Delta Trade$ is less than 50% (column (3)).[33] As before, the dependent variable is the natural logarithm of one plus the number of annual container shipments.

We report the results in Panel B of Table 9. Two sets of results emerge. First, the joint presence of customer trade cuts and supplier ESG rating improvements leads to subsequent trade reversals, and these trade reversals are increasing in the original trade cut. Relative to the control group, trade cuts, cuts below the 25^{th} percentile, and cuts lower than 50% are associated with relative increases between the post-incident and the post-adjustment period of 37.7%, 44.9%, and 54.9%, respectively.[34]

[33] This test is similar in nature to a quadruple difference-in-differences test with cross-sectional cuts based on initial trade cuts and subsequent trade reversals. Due to lack of data on control suppliers' ESG ratings, however, we cannot perform such test, and thus causal inference from the results of Panel B is limited.

[34] These estimates come from different sub-samples of treated firms, and thus are not directly comparable to our baseline estimates from Table 3. However, the estimates are comparable if we condition the treated sample on the initial trade cut. For example, if we estimate specification (1) only keeping treated relationships where $\Delta\text{Trade} < 0$, we find a treatment effect of -70.4%, which combined with the point estimate in column (1) of Table 9, Panel B, implies an overall trade change of -59.24% between the pre-incident and the post-adjustment periods.

Second, *only* the joint presence of trade cuts and ESG rating improvements leads to subsequent reversals: we find no 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 results of Table 9 lend support to our hypothesis that U.S. customers may use real trade activity as an effective mechanism to discipline their suppliers' E&S performance.

6 Robustness Tests

In Table 10, we report robustness tests for our baseline specifications from Table 3. In Panel A, we show that our results are robust to alternative measures of trade intensity, namely the number of individual shipments (column (1)), the total shipment weight (in tonnes, column (2)), and the total shipment quantity (in terms of individual units in a shipment, column (3)). Our results are consistent across different measurement choices, and columns (2) and (3) show even larger effects when we measure trade using shipment weights and quantities.

In Panel B, we report the results based on alternative matching samples. In column (1), we report our results when we match treated and control samples based on customer firms' four-digit SIC industries. That is, for each cohort, we only include control customers operating in the same industry as treated customers. In column (2), we similarly report our results when we match on customer firms' four-digit SIC industry and size deciles. In column (3), we report our results when we match on customer firms' four-digit SIC industry and size deciles, as well as on supplier country. That is, we only include control suppliers from the same country as treated suppliers. The results are economically and statistically robust to these alternative choices, confirming that the control group choice does not systematically affect our main results. Additionally, the estimated coefficient in column (3) is slightly smaller in magnitude than those in the first two columns of the panel, providing additional support for our international reallocation results in Table 8. Finally, column (4) shows that our results are also robust if we restrict the sample to customer-country pairs with at least one treated and one control supplier in the same country.

In Panel C, we loosen the restriction of excluding suppliers with confounding (and distinct) E&S incidents in the $[t-3, t+3]$ year window around the scandal. In column (1), we only include suppliers that do not have such confounding scandals in a narrower $[t-2, t+2]$ year window. In column (2), we only include suppliers that do not have such scandals in an even narrower $[t-1, t+1]$ year window. In both cases, we follow the most restrictive specification and match on customer firms' SIC industry, size deciles, and supplier country. Even in this case, we obtain results consistent with our baseline estimates.

In Panel D, we show that our results are economically and statistically robust to alternative and less-stringent combinations of fixed effects than in our main specification (1). In particular, we include cohort-year (column (1)), cohort-year and customer-year (column (2)), cohort-year, customer-year, and supplier-cohort (column (3)), and cohort-year and pair-cohort fixed effects (column (4)). The economic estimates magnitudes of our coefficient of interest show limited variation across these specifications.

Finally, in Panel E we show that the results are also robust to alternative specification choices. First, in column (1), we expand the sample beyond E&S incidents to include those (G)overnance-related incidents that have possible downstream reputational externalities—bribery and fraud incidents. Second, since cargo shipments are measured at a high frequency, in column (2) we confirm that our results hold even when we use quarterly instead of annual data. Third, in columns (3) and (4) we show that our results hold under alternative scaling choices for the outcome variable. In column (3), we show that relative container imports (i.e., containers scaled by the total size of the customer's annual imports) decrease by 0.007 for treated suppliers after the treatment, a 21.27%

drop relative to the sample mean. In column (4), we document a quantitatively similar 26.4% reduction in relative container imports using Poisson regressions. The results of column (4) also reduce concerns that some of our results may be driven by zeros or quasi-zeros in the data (Cohn, Liu, and Wardlaw, 2022).

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 scandals affect their future trade relationships with U.S. customers.

We document partial trade adjustments. In terms of shipments, the imports from affected suppliers decrease by 11.1% compared to those from suppliers not involved in any scandal. Customer switch to other suppliers, especially to those in other countries, but do not always fully terminate their relationships. The average trade relationship is only 4.2% more likely to be terminated following an E&S incident. Additionally, we find evidence of trade reversals over the long run if U.S. customers' initial trade cuts are followed by improved E&S performance by the supplier, suggesting that partial trade adjustments could act as an effective mechanism of governance by exit.

In the cross-section, the effects are stronger when the institutional investors of publicly-listed customers have stronger E&S preferences, and much smaller effects for privately-held than for publicly-listed firms. This finding adds to the ongoing debate on the ESG-related benefits and costs of being public: If privately-held U.S. customers face lower pressure from financial markets to reorganize their supply chains following an E&S incident, they retain more flexibility in building their supply chain networks, which may reduce their incentives to go public. If this is the case, the current trend of delistings in the U.S. and abroad could lead to lower E&S standards' adherence in countries where the suppliers are located.

The option to cut (rather than engage with) the supplier also suggests previously-unstudied benefits from having suppliers outside of the boundaries of the firm. First, customers have the option of picking an alternative supplier rather than fixing the underlying issue with the current supplier. Second, the option of quitting the relationship creates an actionable threat that can improve the supplier's performance. Another aspect of the theory of the firm suggested by this paper is that a publicly-listed U.S. firm might be an attractive investment for E&S-minded shareholders who want to monitor private foreign suppliers otherwise outside of their investment universe.

Our results also speak to the current policy debate on regulatory outsourcing of global supply chain monitoring activities. International suppliers' E&S activities are beyond the reach of domestic governments. However, these governments can impose domestic supply chain regulations to gain extraterritorial reach. One recent example of such "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).[35] Since compliance by U.S. companies is linked to compliance by their suppliers, U.S. companies are responsible for implementing and enforcing regulatory standards on firms abroad.

In this paper, we show that U.S. firms' governance (by exit) of their suppliers' E&S activities is effective beyond the specific case of conflict minerals, especially when firms face stronger investor pressure. In this respect, the currently-discussed Scope 3 emis-

[35] 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 and with 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. A similar UK Modern Slavery Act also applies to all companies around the world with turnover over £36 million that operate in the UK market.

sions' reporting requirements could help investors gather more knowledge on firms' supply chain environmental performance, put necessary pressure when needed, and thus effectively assist governments that adopt Scope 3 regulations to achieve extraterritorial reach. Future work could study whether the loss of international customers and reputation due to high-profile E&S scandals could also induce new regulations by exporting countries.

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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 scandals on international trade. To estimate the dynamic effects of E&S scandal 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. In this figure, we plot the estimated interaction coefficients and their associated 90% confidence intervals.

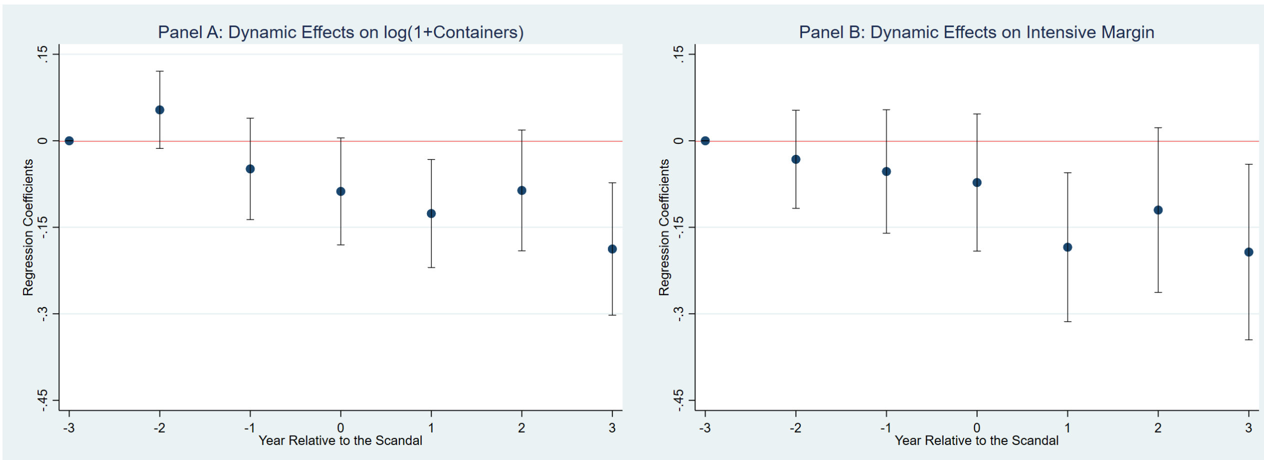

Table 1: Summary Statistics

Panel A reports the sample distribution across cohorts (i.e., event years of supplier scandals). Panel B reports the distribution of treated relationships across the Fama-French 48 industry of the customer. Panel C reports descriptive statistics for the variables used in our main analyses.

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	172	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 Minin	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	1,000,950	0.007	0.084	0.000	0.000	0.000
Treat Cust, Control Supp	1,000,950	0.711	0.453	0.000	1.000	1.000
Post	1,000,950	0.559	0.496	0.000	1.000	1.000
Container	1,000,950	0.942	1.308	0.000	0.000	1.609
1 (Trade>0)	1,000,950	0.471	0.499	0.000	0.000	1.000
Size	1,000,950	8.418	2.251	6.846	8.272	9.813
MTB	1,000,950	1.350	1.147	0.515	1.075	1.741
Lev	1,000,950	0.221	0.166	0.088	0.225	0.308
R&D	1,000,950	0.020	0.040	0.000	0.000	0.026
Capx	1,000,950	0.045	0.031	0.020	0.038	0.063
Cash	1,000,950	0.128	0.113	0.041	0.095	0.182

Table 2: Customers' Stock Market Reactions Around Supplier Scandals

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 results for all incidents covered by the RepRisk data. Panel B reports results for incidents in our baseline sample. CAR $[-\tau, +\tau]$ is the cumulative abnormal return for customer firms from day $-\tau$ to day $+\tau$, and day 0 is the incident announcement date. Abnormal returns are estimated using the market model in a $[-200, -60]$ trading day window 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%	-2.79
CAR [-5,+5]	9,957	-0.19%	-0.07%	-2.47

Panel B: Within-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 *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. The dependent variables in columns (2) and (3) are *1(Trade>0)* and *Log(1+Containers)*, respectively. Column (3) requires a relationship-cohort-year to have a positive amount of trade to be included in the regression sample. All columns control for relationship×cohort and customer firm×year×cohort fixed effects. All the variables are defined in Table A1. 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)	1(Trade>0)	Log(1+Containers)
		Extensive Margin	Intensive Margin
	(1)	(2)	(3)
Treat Supp×Post	-0.111***	-0.042***	-0.095*
	(0.039)	(0.014)	(0.054)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	990,439	990,439	410,322
Adj. R^2	0.392	0.160	0.640

Table 4: Cross-sectional Tests: Incident Characteristics

This table shows cross-sectional results based on incident characteristics. The dependent variable is *Log(1+Containers)*. Column (1) partitions incidents into incidents related to environmental issues only (*Treat Supp, E only*), social issues only (*Treat Supp, S only*), and both environmental and social issues (*Treat Supp, E & S*). 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 the supplier incidents (*High Reaction*) and a group with low negative market reaction to the supplier incidents (*Low Reaction*). Column (4) partitions incidents into incidents that occurred on or before 2015 (*Pre2016*), and incidents that occurred on or after 2016 (*Post2016*). All columns control for relationship×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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)			
	(1)	(2)	(3)	(4)
Treat Supp, E only×Post	-0.044			
	(0.110)			
Treat Supp, S only×Post	-0.096*			
	(0.051)			
Treat Supp, E & S×Post	-0.180***			
	(0.069)			
Treat Supp, High Severity×Post		-0.140**		
		(0.059)		
Treat Supp, Low Severity×Post		-0.086*		
		(0.051)		
Treat Supp, High Reaction×Post			-0.178***	
			(0.062)	
Treat Supp, Low Reaction×Post			-0.069	
			(0.058)	
Treat Supp, Pre2016×Post				-0.056
				(0.049)
Treat Supp, Post2016×Post				-0.200***
				(0.064)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	990,439	990,439	990,439	990,439
Adj. R^2	0.392	0.392	0.392	0.392

Table 5: Cross-sectional Tests: Investor E&S Preferences

This table shows the differential effects of the same supplier scandal for trade with customers with different investor characteristics. The dependent variable is *Log(1+Containers)*. Columns (1) to (3) of the table use the same sample as in Table 3. *High CustESG* is a binary variable indicating customers with above-median Refinitiv ESG ratings in the event year. *High IO_ESG* is a binary variable indicating customers with above-median outstanding shares' ownership by E&S-conscious investors at the beginning of the event year. E&S-conscious investors are defined similar to Gantchev et al. (2022) as investors with average portfolio E&S ratings in the top tercile of the distribution. *ESGProposal* 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 (4) 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 customers, and equal to zero otherwise. The data comes from CRSP. All columns include supplier×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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)			
	(1)	(2)	(3)	(4)
Treat Supp×Post	-0.054	-0.030	0.017	-0.023*
	(0.050)	(0.055)	(0.067)	(0.013)
Treat×Post×High CustESG	-0.138*			
	(0.079)			
Treat×Post×High IO_ESG		-0.151**		
		(0.077)		
Treat×Post×ESG Proposal			-0.235**	
			(0.100)	
Treat×Post×Public Cust				-0.109***
				(0.041)
Partition Var.×Treat	Yes	Yes	Yes	Yes
Supplier×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	990,439	990,439	559,468	28,005,984
Adj. R^2	0.353	0.353	0.364	0.279

Table 6: Cross-sectional Tests: End Consumer Exposure

This table shows the differential effects of the same supplier scandal on trade with customers with different end-consumer exposure. The dependent variable is *Log(1+Containers)*. *High %Final User* is a binary variable that equals one if the customer industry's final-user sales to total sales ratio is above the sample median. *B2C* is a binary variable that equals one if the customer industry is categorized as a business-to-consumer industry (Lev et al., 2010, Flammer, 2015). All columns control for supplier×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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)	
	(1)	(2)
Treat Supp×Post	-0.095**	-0.108**
	(0.044)	(0.053)
Treat×Post×High %Final User	-0.147	
	(0.114)	
Treat×Post×B2C		-0.014
		(0.073)
Partition Var.×Treat	Yes	Yes
Supplier×Cohort FE	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes
Obs.	830,537	990,439
Adj. R^2	0.371	0.353

Table 7: Relationship with Suppliers and Switching Costs

This table shows cross-sectional results based on supplier characteristics and switching costs. The dependent variable is *Log(1+Containers)*. Column (1) partitions suppliers into public suppliers (*Treat Supp, Public*) and private suppliers (*Treat Supp, Private*). Column (2) partitions suppliers into large suppliers (*Treat Supp, Large*) and Small suppliers (*Treat Supp, Small*). Column (3) partitions suppliers into a group with high HS product Herfindahl-Hirschman Index (HHI) (*High HHI*) and a group with low HS product HHI (*Low HHI*). Column (4) partitions suppliers into a group with high product differentiation (*High Differentiation*) and a group with low product differentiation (*Low Differentiation*). All columns control for relationship×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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)			
	(1)	(2)	(3)	(4)
Treat Supp, Public×Post	-0.088			
	(0.059)			
Treat Supp, Private×Post	-0.124**			
	(0.050)			
Treat Supp, Large×Post		-0.088		
		(0.057)		
Treat Supp, Small×Post		-0.147***		
		(0.041)		
Treat Supp, High HHI×Post			-0.036	
			(0.048)	
Treat Supp, Low HHI×Post			-0.217***	
			(0.064)	
Treat Supp, High Differentiation×Post				-0.086**
				(0.042)
Treat Supp, Low Differentiation×Post				-0.294***
				(0.103)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	990,439	990,439	990,439	990,439
Adj. R^2	0.392	0.392	0.392	0.392

Table 8: International Supply Chain Reallocation

This table documents trade reallocation along the supply chain network. The dependent variable is *Log(1+Containers)*. *%Treat Supp* is the fraction of suppliers hit by an E&S scandal 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 hit by an E&S scandal). *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 columns control for relationship×cohort and customer firm×cohort fixed effects. Variable definitions are in Table A1. 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)		
	(1)	(2)	(3)
Treat Supp×Post	-0.119**	-0.119**	-0.086*
	(0.048)	(0.048)	(0.052)
%Treat×Treat Supp×Post	0.673	0.673	0.663
	(0.418)	(0.418)	(0.418)
%Treat×Treat Cust, Control Supp×Post	0.857***		
	(0.177)		
%Treat×Treat Cust, Control Supp, Same Country×Post		0.046	
		(0.336)	
%Treat×Treat Cust, Control Supp, Diff Country×Post		1.080***	
		(0.202)	
%Treat×Treat Cust, Control Supp, High SuppE&S×Post			10.345***
			(3.953)
%Treat×Treat Cust, Control Supp, Low SuppE&S×Post			-1.044
			(0.942)
Size	0.186***	0.186***	0.089*
	(0.008)	(0.008)	(0.046)
Leverage	-0.607***	-0.607***	-0.258*
	(0.027)	(0.027)	(0.141)
R&D	2.535***	2.537***	0.094
	(0.217)	(0.217)	(0.955)
Capx	-0.238**	-0.238**	-0.353
	(0.093)	(0.093)	(0.497)
Cash	0.149***	0.149***	0.373**
	(0.034)	(0.034)	(0.172)
Pair×Cohort FE	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes
Obs.	990,439	990,439	39,182
Adj. R^2	0.266	0.266	0.239

Table 9: Trade Cuts, E&S Improvements, and Trade Reversal

This table studies supplier E&S 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 $[t-3, t+6]$ years window around the incident year t. The dependent variable is the supplier's RepRisk ESG risk rating. *Treat* is a binary variable indicating whether the supplier is hit 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. 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 *Log(1+Containers)*. *Treat* is a binary variable indicating suppliers hit 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 indicated on the top of the table (0, -29%, and -50%, 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 risk rating between year $t-1$ and year $t+3$, and zero otherwise. All columns controls for relationship-cohort and firm-year-cohort fixed effects. The variables are defined as in Table A1. 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 RepRisk ESG Score		
	< P25	P25-P75	>P75
	(1)	(2)	(3)
Treat×Post(0)	-0.918***	-0.984***	-1.010***
	(0.060)	(0.045)	(0.066)
Treat×Post(+1)	-0.934***	-0.932***	-1.038***
	(0.069)	(0.053)	(0.072)
Treat×Post(+2)	-0.301***	-0.350***	-0.442***
	(0.095)	(0.054)	(0.082)
Treat×Post(+3)	-0.053	-0.265***	-0.430***
	(0.105)	(0.061)	(0.105)
Treat×Post(+4)	0.063	-0.125*	-0.337**
	(0.112)	(0.075)	(0.134)
Treat×Post(+5)	-0.053	-0.046	-0.252*
	(0.150)	(0.087)	(0.150)
Treat×Post(+6)	-0.160	0.050	-0.306*
	(0.182)	(0.105)	(0.177)
Supplier×Cohort FE	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes
Obs.	17,871	37,634	15,936
Adj. R^2	0.866	0.860	0.857

Table 9: Trade Cuts, E&S Improvements, and Trade Reversal (Continued)

Panel B: Trade Reversal

Dep. Var. =	Log(1+Containers)		
	where CutTrade=1 is defined if		
	ΔTrade < 0	ΔTrade < -0.29	ΔTrade < -0.5
	(1)	(2)	(3)
Treat×Post4 (CutTrade=1, Inc_Rating=1)	0.377***	0.449***	0.549***
	(0.106)	(0.109)	(0.119)
Treat×Post4 (CutTrade=1, Inc_Rating=0)	0.219	0.296	0.477
	(0.241)	(0.252)	(0.319)
Treat×Post4 (CutTrade=0, Inc_Rating=1)	-0.126	-0.160	-0.142
	(0.156)	(0.146)	(0.127)
Treat×Post4 (CutTrade=0, Inc_Rating=0)	-0.298	-0.313	-0.303*
	(0.207)	(0.199)	(0.177)
Relationship×Cohort FE	Yes	Yes	Yes
Firm×Cohort×Year FE	Yes	Yes	Yes
Obs.	233,442	233,442	233,442
Adj. R^2	0.566	0.566	0.566

Table 10: Additional Robustness

This table shows the results of robustness tests on our main results from Table 3. Panel A reports results using alternative measures of trade. The dependent variables in columns (1) to (3) are *#Ship*, *Weight*, and *Quantity*, respectively. Panel B reports results using alternative matching samples. The dependent variable is *Log(1+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 results using alternative approaches to deal with confounding incidents. The dependent variable is *Log(1+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 results using alternative fixed effects. The dependent variable is *Log(1+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, column (4) controls for year-cohort and relationship-cohort fixed effects. Panel E reports results using alternative samples and specifications. The dependent variable is *Log(1+Containers)* in columns (1) and (2), and *Containers* in columns (3) and (4). Column (1) includes supplier incidents related to corruption, bribery, and fraud in addition to E&S incidents used in our main analysis. Column (2) expands the main sample to the quarterly level. Column (3) 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. Column (4) estimates a Poisson regression model using the number of containers divided by the total number of containers imported by the firm as the dependent variable. Variable definitions are in Table A1. 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. =	#Shipments	Weight	Quantity
	(1)	(2)	(3)
Treat Supp×Post	-0.092***	-0.462***	-0.237**
	(0.036)	(0.151)	(0.099)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Cohort×Year FE	Yes	Yes	Yes
Obs.	990,439	990,439	990,439
Adj. R^2	0.393	0.246	0.315

Panel B: Matching Sample

	Log(1+Containers)			
	Industry	Industry, Size	Industry, Size, Supplier Country	Firm-countries with both treated and control suppliers
	(1)	(2)	(3)	(4)
Treat Supp×Post	-0.110***	-0.110***	-0.103**	-0.090*
	(0.039)	(0.038)	(0.044)	(0.047)
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.	788,608	735,878	163,495	161,095
Adj. R^2	0.393	0.393	0.434	0.262

Table 10: Additional Robustness (Continued)

Panel C: Alternative Restrictions on Confounding Incidents

Dep. Var. =	Log(1+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	-0.105***	-0.057**
	(0.034)	(0.027)
Firm×Cohort FE	Yes	Yes
Firm×Cohort×Year FE	Yes	Yes
Obs.	811,101	1,093,221
Adj. R^2	0.394	0.393

Panel D: Alternative Fixed Effects

Dep. Var. =	Log(1+Containers)			
	(1)	(2)	(3)	(4)
Treat Supp	0.137***	0.164***		
	(0.035)	(0.035)		
Treat Supp×Post	-0.091**	-0.101***	-0.098**	-0.091**
	(0.039)	(0.039)	(0.040)	(0.041)
Size	-0.001	0.119***	0.175***	0.186***
	(0.001)	(0.007)	(0.008)	(0.008)
Leverage	0.133***	-0.398***	-0.599***	-0.606***
	(0.013)	(0.024)	(0.027)	(0.027)
R&D	-0.344***	1.842***	2.537***	2.546***
	(0.054)	(0.195)	(0.216)	(0.217)
Capx	-0.921***	-0.134	-0.263***	-0.243***
	(0.059)	(0.088)	(0.093)	(0.093)
Cash	-0.054***	0.200***	0.147***	0.150***
	(0.020)	(0.031)	(0.034)	(0.034)
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.	990,439	990,439	990,439	990,439
Adj. R^2	0.016	0.057	0.230	0.266

Table 10: Additional Robustness (Continued)

Panel E: Alternative Sample and Specification

Dep. Var. =	Log(1+Containers)		Containers	Containers
	Including corruption, bribery, fraud	Quarterly data	Scaled by Size	Poisson Regression
	(1)	(2)	(3)	(4)
Treat Supp×Post	-0.095***	-0.063***	-0.007***	-0.264***
	(0.035)	(0.023)	(0.002)	(0.081)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	1,027,861	4,080,488	990,439	936,179
Adj. R^2	0.392	0.398	0.481	
Pseudo R^2				0.721

Appendix: Intended for Online Publication

Figure A1: Anecdotal Evidence: Supplier E&S Incidents and International Trade

This figure displays RepRisk ESG rating dynamics for Stella International Holding Ltd (Stella) and trade dynamics between Stella and Deckers Outdoor Corporation (Deckers) around Stella's factory worker strike in March 2015. The solid line displays the dynamics of Stella's ESG ratings. The bar chart displays the number of containers shipped from Stella to Deckers available from Panjiva.

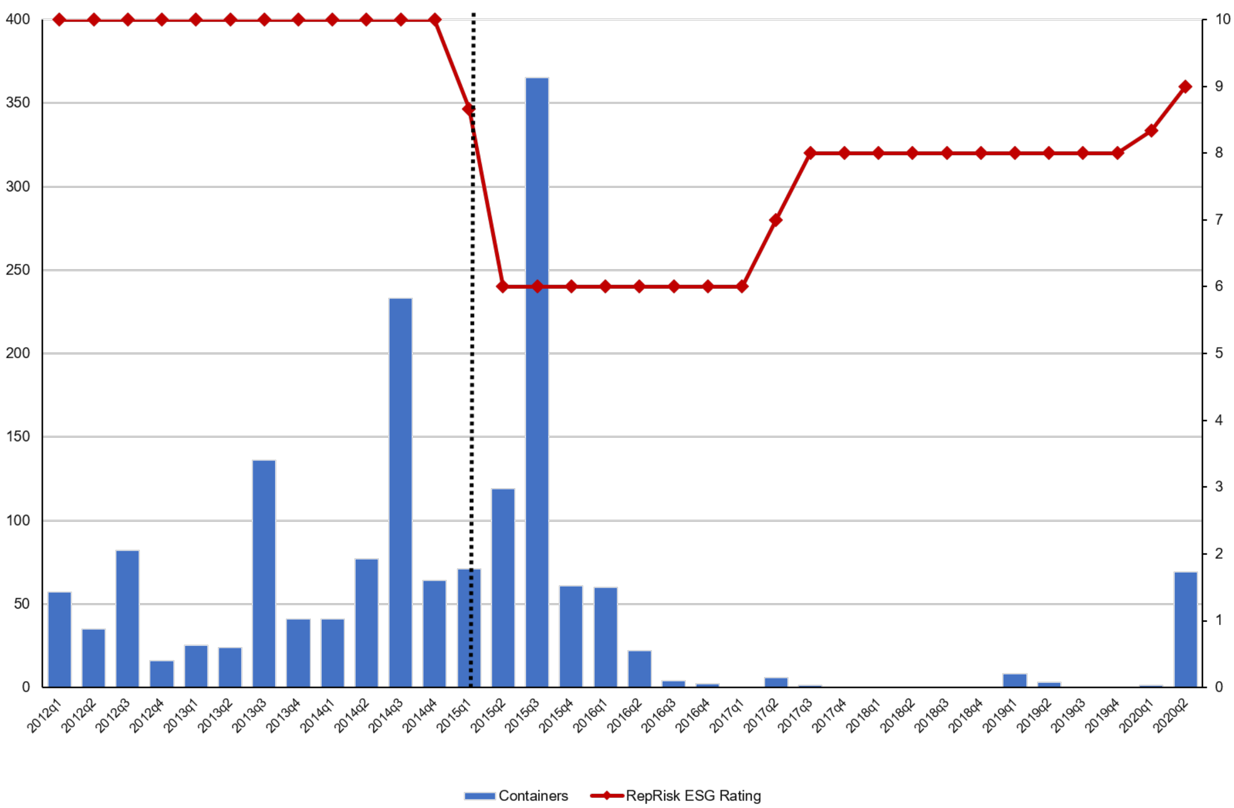

Table A1: Variable Definitions

Variable	Definition	Data Source
Containers	The natural logarithm of 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
Ship	The natural logarithm of the number of shipments from the supplier to the customer in the year.	
Weight	The natural logarithm of the total weight of all shipments from the supplier to the customer in the year.	Panjiva
Quantity	The natural logarithm of the number of individual items shipped from the supplier to the customer in the year.	Panjiva
Treat Supp	A binary variable that equals one if the supplier 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 only	The product of *Treat Supp* and a binary variable that equals one if the incident is coded as related to environmental but not social issues.	RepRisk
Treat Supp, S only	The product of *Treat Supp* and a binary variable that equals one if the incident is coded as related to social but not environmental issues.	RepRisk
Treat Supp, E & S	The product of *Treat Supp* and a binary variable that equals one if the incident is coded as related to both environmental and 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 [-5,+5] day window around the supplier incident is above the sample median.	RepRisk, CRSP
Treat Supp, Low Reaction	The product of *Treat Supp* and a binary variable that equals one if the customer's market reaction over a [-5,+5] day window around the supplier incident is below the sample median.	RepRisk, CRSP
Treat Supp, Pre2016	The product of *Treat Supp* and a binary variable that equals one if the incident occurred on or before 2015.	RepRisk
Treat Supp, Post2016	The product of *Treat Supp* and a binary variable that equals one if the incident occurred after 2015.	RepRisk

Table A1: Variable Definitions (Continued)

Variable	Definitions	Data Source
Treat Supp, Public	The product of *Treat Supp* and a binary variable that equals one if the supplier is a public firm.	RepRisk
Treat Supp, Private	The product of *Treat Supp* and a binary variable that equals one if the supplier is a private firm.	RepRisk
Treat Supp, Large	The product of *Treat Supp* and a binary variable that equals one if the supplier's annual container shipments are greater than the sample median.	Panjiva
Treat Supp, Small	The product of *Treat Supp* and a binary variable that equals one if the supplier's annual container shipments are smaller than the sample median.	Panjiva
Treat Supp, High HHI	The product of *Treat Supp* and a binary variable that equals one if the HHI of the supplier's two-digit HS product is above the sample median.	Panjiva
Treat Supp, Low HHI	The product of *Treat Supp* and a binary variable that equals one if the HHI of the supplier's two-digit HS product is below the sample median.	Panjiva
Treat Supp, High Differentiation	The product of *Treat Supp* and a binary variable that equals one if the supplier's HS products are classified as differentiated goods according to Rauch (1999).	Rauch (1999)
Treat Supp, Low Differentiation	The product of *Treat Supp* and a binary variable that equals one if the supplier's HS products are not classified as differentiated goods according to Rauch (1999).	Rauch (1999)
High ESG	A binary variable that equals one if the customer firm's Refinitiv ESG score in the event year is above the sample median.	Refinitiv
High IO_ESG	A binary variable that equals one if the fraction of outstanding shared owned by E&S-conscious investors at the beginning of the event year is above the sample median.	Thomson Reuters
ESG 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
B2C	A binary variable that equals one if the customer firm operates in business-to-customer industries.	Lev et al. (2010)
High %Final User	A binary variable that equals one if fraction of industry final-user sales to total sales is greater than the sample median.	Bureau of Economic Analysis

Table A2: Panjiva Sample Selection

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 A3: Incident Characteristics: Robustness Tests on Extensive Margin

This table shows cross-sectional results based on incident characteristics. The dependent variable is *1(Trade>0)*. Column (1) partitions incidents into incidents related to environmental issues only (*Treat Supp, E only*), social issues only (*Treat Supp, S only*), and both environmental and social issues (*Treat Supp, E & S*). 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 the supplier incidents (*High Reaction*) and a group with low negative market reaction to the supplier incidents (*Low Reaction*). Column (4) partitions incidents into incidents that occurred on or before 2015 (*Pre2016*), and incidents that occurred on or after 2016 (*Post2016*). All columns control for relationship×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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. =	1(Trade>0)			
	(1)	(2)	(3)	4
Treat Supp, E only×Post	-0.033			
	(0.038)			
Treat Supp, S only×Post	-0.019			
	(0.018)			
Treat Supp, E & S×Post	-0.099***			
	(0.028)			
Treat Supp, High Severity×Post		-0.042**		
		(0.021)		
Treat Supp, Low Severity×Post		-0.043**		
		(0.019)		
Treat Supp, High Reaction×Post			-0.051**	
			(0.021)	
Treat Supp, Low Reaction×Post			-0.033	
			(0.021)	
Treat Supp, Pre2016×Post				-0.021
				(0.018)
Treat Supp, Post2016×Post				-0.077***
				(0.023)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	990,439	990,439	990,439	990,439
Adj. R^2	0.160	0.160	0.160	0.160

Table A4: Investor E&S Preferences: Robustness Tests on Extensive Margin

This table shows the differential effects of the same supplier incident on trade with customers with different investor characteristics. The dependent variable is *1(Trade>0)*. Columns (1) to (3) of the table use the same sample as in Table 3. *High CustESG* is a binary variable indicating customers with above-median Refinitiv ESG ratings in the event year. *High IO_ESG* is a binary variable indicating customers with above-median outstanding shares' ownership by E&S-conscious investors at the beginning of the event year. E&S-conscious investors are defined similar to Gantchev et al. (2022) as investors with average portfolio E&S ratings in the top tercile of the distribution. *ESGProposal* 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 (4) 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 customers, and equal to zero otherwise. The data comes from CRSP. All columns include supplier×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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. =	1(Trade>0)			
	(1)	(2)	(3)	(4)
Treat Supp×Post	-0.021	-0.011	-0.002	-0.017***
	(0.019)	(0.021)	(0.026)	(0.005)
Treat×Post×High CustESG	-0.047*			
	(0.027)			
Treat×Post×High IO_ESG		-0.056**		
		(0.028)		
Treat×Post×ESG Proposal			-0.070**	
			(0.035)	
Treat×Post×ESG Proposal				-0.028*
				(0.015)
Partition Var.×Treat	Yes	Yes	Yes	Yes
Supplier×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	990,439	990,439	559,468	28,005,984
Adj. R^2	0.160	0.160	0.173	0.105

Table A5: Cross-sectional Tests: Financial Constraints

This table shows the differential effects of the same supplier incident on trade with customers with different end-consumer exposure. The dependent variable is *Log(1+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. All columns control for supplier×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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)	
	(1)	(2)
Treat Supp×Post	-0.107**	-0.151***
	(0.053)	(0.052)
Treat×Post×High KZindex	-0.020	
	(0.082)	
Treat×Post×High WWindex		0.085
		(0.080)
Partition Var.×Treat	Yes	Yes
Supplier×Cohort FE	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes
Obs.	940,259	942,722
Adj. R^2	0.352	0.352

Table A6: Investor E&S Preferences: Controlling for Supplier-cohort-year FE

This table shows the differential effects of the same supplier incident on trade with customers with different investor characteristics. The dependent variable is *Log Container*. Columns (1) to (3) of the table use the same sample as in Table 3. *High CustESG* is a binary variable indicating customers with above-median Refinitiv ESG ratings in the event year. *High IO_ESG* is a binary variable indicating customers with above-median outstanding shares' ownership by E&S-conscious investors at the beginning of the event year. E&S-conscious investors are defined similar to Gantchev et al. (2022) as investors with average portfolio E&S ratings in the top tercile of the distribution. *ESGProposal* 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 (4) 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 customers, and equal to zero otherwise. The data comes from CRSP. All columns include supplier×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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. =	Log(1+Containers)			
	(1)	(2)	(3)	(4)
Treat×Post×High CustESG	-0.393**			
	(0.176)			
Treat×Post×High IO_ESG		-0.364**		
		(0.177)		
Treat×Post×ESG Proposal			-0.663**	
			(0.225)	
Treat×Post×Public Cust				-0.080*
				(0.047)
Partition Var.×Treat	Yes	Yes	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	168,116	168,116	72,081	16,823,743
Adj. R^2	0.265	0.265	0.285	0.233

Table A7: End Consumer Exposure: Robustness Tests on Extensive Margin

This table shows the differential effects of the same supplier incident on trade with customers with different end-consumer exposure. The dependent variable is *1(Trade>0)*. *High %Final User* is a binary variable that equals one if the customer industry's final-user sales to total sales ratio is above the sample median. *B2C* is a binary variable that equals one if the customer industry is categorized as a business-to-consumer industry (Lev et al., 2010, Flammer, 2015). All columns control for supplier×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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. =	1(Trade>0)	
	(1)	(2)
Treat Supp×Post	-0.051***	-0.033
	(0.017)	(0.021)
Treat×Post×High %Final User	0.003	
	(0.039)	
Treat×Post×B2C		-0.015
		(0.026)
Partition Var.×Treat	Yes	Yes
Supplier×Cohort FE	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes
Obs.	830,537	990,439
Adj. R^2	0.175	0.160

Table A8: End Consumer Exposure: Controlling for Supplier-cohort-year FE

This table shows the differential effects of the same supplier incident on trade with customers with different end-consumer exposure. The dependent variable is *Log(1+Containers)*. *High %Final User* is a binary variable that equals one if the customer industry's final-user sales to total sales ratio is above the sample median. *B2C* is a binary variable that equals one if the customer industry is categorized as a business-to-consumer industry (Lev et al., 2010, Flammer, 2015). All columns control for supplier×year×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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. =	Log(1+Containers)	
	(1)	(2)
Treat×Post×High %Final User	0.023	
	(0.346)	
Treat×Post×B2C		0.017
		(0.159)
Partition Var.×Treat	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes
Obs.	127,625	168,116
Adj. R^2	0.293	0.265

Table A9: Relationship with Suppliers and Switching Costs: Robustness Tests on Extensive Margin

This table shows cross-sectional results based on supplier characteristics and switching costs. The dependent variable is *1(Trade>0)*. Column (1) partitions suppliers into public suppliers (*Treat Supp, Public*) and private suppliers (*Treat Supp, Private*). Column (2) partitions suppliers into large suppliers (*Treat Supp, Large*) and Small suppliers (*Treat Supp, Small*). Column (3) partitions suppliers into a group with high HS product Herfindahl-Hirschman Index (HHI) (*High HHI*) and a group with low HS product HHI (*Low HHI*). Column (4) partitions suppliers into a group with high product differentiation (*High Differentiation*) and a group with low product differentiation (*Low Differentiation*). All columns control for relationship×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. 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. =	1(Trade>0)			
	(1)	(2)	(3)	(4)
Treat Supp, Public×Post	-0.032			
	(0.024)			
Treat Supp, Private×Post	-0.048***			
	(0.017)			
Treat Supp, Large×Post		-0.015		
		(0.018)		
Treat Supp, Small×Post		-0.086***		
		(0.022)		
Treat Supp, High HHI×Post			-0.027	
			(0.018)	
Treat Supp, Low HHI×Post			-0.065***	
			(0.023)	
Treat Supp, High Differentiation×Post				-0.037**
				(0.015)
Treat Supp, Low Differentiation×Post				-0.090**
				(0.043)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	990,439	990,439	990,439	990,439
Adj. R^2	0.160	0.160	0.160	0.160

Table A10: Trade Cut 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 variables in columns (1) to (4) are gross margins measured in years t, $t+1$, $t+2$, and $t+3$, respectively. Gross margins are 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 hit 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 columns controls for year-cohort and firm-cohort fixed effects. The variables are defined as in Table A1. 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	-0.003	-0.006*	-0.010**	-0.010**
	(0.003)	(0.004)	(0.004)	(0.004)
Treat Cust (CutTrade=0)×Post	0.004	0.003	0.004	0.000
	(0.004)	(0.004)	(0.004)	(0.004)
Size	0.002	-0.009***	-0.017***	-0.017***
	(0.003)	(0.003)	(0.003)	(0.003)
Leverage	0.016*	0.030***	0.036***	0.047***
	(0.008)	(0.009)	(0.013)	(0.013)
R&D	-0.009**	-0.005***	0.002	0.006*
	(0.004)	(0.002)	(0.002)	(0.003)
Capx	0.185***	0.087***	0.082**	0.005
	(0.027)	(0.027)	(0.032)	(0.043)
Cash	0.016	0.013	0.008	0.023*
	(0.011)	(0.008)	(0.012)	(0.013)
Year×Cohort FE	Yes	Yes	Yes	Yes
Firm×Cohort FE	Yes	Yes	Yes	Yes
Obs.	24,434	22,996	21,147	19,105
Adj. R^2	0.928	0.934	0.930	0.930