ESG Shocks in Global Supply Chains*

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Abstract

We show that U.S. firms cut imports by 29.9% when their international suppliers experience environmental and social (E&S) incidents. These trade cuts are larger for publicly listed U.S. importers facing high E&S investor pressure and lead to cross-country supplier reallocation, suggesting that E&S preferences in capital markets can have real effects in far-flung economies. Larger trade cuts around the incident result in higher supplier E&S performance in subsequent years, and in the eventual resumption of trade. Our results highlight the role of 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 face increasing pressure by customers, workers, shareholders, and regulators to monitor and manage environmental and social (E&S) activities along their supply chains. For example, over the past three years Amazon has been subject to repeated labor strikes against poor working conditions at upstream suppliers.[1] Small and large institutional investors are increasingly active in the management of portfolio firms' E&S risks, including those present in corporate supply chains.[2] On their end, regulators such as the U.S. Securities and Exchange Commission (SEC) and the European Commission are also discussing mandatory disclosure rules for publicly listed companies' downstream and upstream emissions ("Scope 3 emissons").[3] These recent developments raise the question of how firms ensure supplier adherence to E&S standards along geographically widespread and complex supply chain structures.

Media articles, industry reports, and corporate disclosures highlight customers' engagement as one mechanism to manage suppliers' E&S standards.[4] Recent academic work also highlights the benefits of customer-supplier collaborative efforts (e.g., Dai et al., 2021b). At the same time, frequent anecdotes also show that customers alter their behavior and adjust their relationships with suppliers if they do not comply with E&S standards.[5] However, we lack systematic evidence of the extent such trade adjustment in a broad sample of firms, as well as an understanding of customers' underlying incentives to break relationships rather than engage to improve suppliers' E&S standards. More generally, we lack evidence on whether customers cut relationships to distance

[1] `www.businessinsider.com`.

[2] See, e.g., Costco's recent shareholder vote on indirect greenhouse gas emissions (`www.wsj.com`), initially proposed by the activist Green Century Funds (`www.sec.gov`). Also see, e.g., `www.blackrock.com` and `www.unpri.org` for additional evidence from large institutional investors.

[3] See, e.g., `www.economist.com` and `commission.europa.eu`.

[4] See, e.g., `www.forbes.com` and `www.forbes.com`.

[5] 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). In 2018, Nestlè and PepsiCo closed their joint ventures with Indofood Group, Indonesia's palm oil giant, citing environmental concerns. Multiple international retailers ended their relationships with Cambodian Hulu Garment Co after it failed to pay its workers during the Covid-19 pandemic.

themselves from problematic suppliers, or whether the threat of exit is a means to ensure counterparty E&S performance in production networks.

In this paper, we study how U.S. customers change trade relationships after their international suppliers are involved in E&S-related controversies. We use shipment-level 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). These data capture the universe of direct maritime imports (the largest trade mode for U.S. firms), and thus include relationships between U.S. firms and their foreign suppliers beyond those recorded in regulatory filings and public communications.

We study how 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, which sources ESG-related events from the media as well as regulatory and commercial documents. In our main analyses, 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 health and safety accidents (e.g., Gantchev et al., 2022).

The granular cargo declaration and E&S incident data allow us to establish economic estimates of U.S. customers' supply chain adjustments after negative E&S incidents, and to explore the economic drivers of response heterogeneity. Our sample consists of 1,038 supplier-year pairs and 1,301 relationship-year pairs affected by an E&S incident over 2010-18. We start by showing that supplier incidents trigger negative stock price reactions for U.S. customers: we document an average -10 basis points cumulative abnormal return (CAR) in a [-1,+1] day window around the supplier incident, which suggests a material downstream economic impact.

In our main tests, we then use a stacked difference-in-differences regression approach to study the effect of supplier E&S incidents on imports by U.S. customer firms. For each E&S incident, we build separate time cohorts that include trade relationships between an E&S incident-affected supplier and its U.S. customers ("treated" relationships), as well as relationships between the same U.S. customers and their other suppliers, and relationships between unaffected suppliers and 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-affected 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 U.S. customers simultaneously have multiple suppliers, our specifications allow us to control for time-varying customer demand for foreign suppliers.

Our main specifications measure trade intensity by the number of containers the international supplier ships annually. We find that, following a supplier's incident, the annual number of containers imported by U.S. customers from that supplier decreases by 29.9%. This drop appears in the year immediately following the incident, and on average it lasts for more than three years.

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

To the best of our knowledge, we are the first to document partial trade adjustments in response to E&S shocks. One possible explanation for these effects is U.S. customers' inability to fully terminate the relationship (perhaps due to input specificity, e.g., Barrot and Sauvagnat, 2016, or the unavailability of competitive alternatives). Relatedly, customers may be looking to diversify their supply chain risk and to reduce their exposure to the original supplier's E&S incident.[6] Customers may also use trade cuts as costly disciplining actions to improve the supplier's E&S performance.

The granularity of the incident and trade data allows us to perform additional analyses to tease out the forces underlying the observed trade adjustments. We first validate our estimation methodology and E&S incident measures by showing that the main findings are stronger in the cross-section for incidents more likely to generate adverse downstream reputational effects. We find that trade cuts are quantitatively larger for more severe incidents, when the incident announcement triggers larger negative market reactions for the customer, and when the general environmental awareness is high (Ardia et al., 2022).

We perform additional heterogeneity tests to ask whether the observed trade cuts reflect only monetary incentives and business risk, or they can also be attributed to the non-monetary preferences of some stakeholders such as ESG-minded institutional investors and retail consumers (Bénabou and Tirole, 2010). To test these hypotheses, we perform a within-incident analysis measuring differential trade changes between the same incident-affected supplier and U.S. customers with different characteristics.

For the same supplier incident, we find larger trade cuts when the U.S. customer is more likely subject to E&S investor pressure. First, trade cuts are increasing in the customer's ESG rating. Second, trade cuts are increasing in the customer's stock ownership by E&S-conscious institutional investors (Gantchev et al., 2022). As suggested by the anecdotal evidence from Costco, these stakeholders might impose E&S pressure via investor meetings, shareholder proposals, or voting. Third, trade cuts by a listed customer are indeed larger after the customer receives shareholder proposals related to E&S issues. Fourth, when we expand our baseline sample to also include privately held

[6] See, e.g., www.ey.com.

U.S. customers, we find that trade cuts of a publicly listed customer are on average 20 p.p. larger than those of a privately held customer, suggesting that trade adjustments to maintain sustainable supply chains are a potential cost of being public.

Our within-supplier estimates suggest that investor preferences play an important role in determining the real effects of E&S shocks along supply chain networks. Additionally, the differential reactions of different U.S. customers to the same supplier incident imply that our main findings are unlikely to be explained by increased business risks (e.g., revised customer expectations about suppliers' product quality and financial position) as long as these expectations are independent of customers' E&S preferences. We also find no evidence of differential trade cuts based on financial and risk management characteristics of the customer, suggesting that these characteristics are unlikely to be confounding the investor-related cross-sectional estimates.

To consider an alternative explanation, we also ask whether customers cut trade due to the preferences of their own end consumers (e.g., due to product boycotts). Using granular scanner data on U.S. importers' retail sales from Nielsen, we do find a drop in the number of products sold locally by U.S. retailers around supplier incidents. However, we also find that product prices *increase* around these incidents; thus, these quantity changes are more likely related to a decrease in importer supply rather than by lower end consumer demand, for which we would expect product price declines.

Next, we study supplier and industry characteristics that may limit customers' ability to cut trade following an incident, and affect the effectiveness of exit as a disciplining mechanism. Our estimates are larger for smaller suppliers, for which U.S. customers' exit may have a larger revenue impact. The estimates are also larger when the supplier's industry is more competitive and the goods produced by the supplier are more substitutable. Thus, exit may be an effective disciplining tool as long as customers have a wide pool of suppliers, and supplier inputs are not too specific to the production process.

We also formally test how customers readjust their supply chains following a supplier incident: whether they switch to new suppliers, to suppliers from other countries, and to suppliers with better ESG profiles. We find evidence of cross-country reallocation, suggestive of within-country reputational spillovers. We also find evidence of reallocation to better-ESG-rated suppliers, confirming that U.S. customers actively adjust their supply chains to manage their E&S profiles. Supplier reallocation following an incident is costly: gross profit margins decrease by 0.9% for customers that cut trade with incident-affected suppliers (but do not change for other customers), highlighting substantial monetary costs of maintaining sustainable supply chains.

Finally, we ask whether exit is an effective disciplining mechanism in production networks. That is, we ask whether customers' initial trade cuts are correlated with the incident-affected supplier's subsequent E&S performance and with trade reversals. First, we show that trade cuts after the incident are associated with subsequent improvements in suppliers' E&S performance, and these improvements are increasing in the extent of the initial trade cut. No such improvements are observable absent post-incident trade cuts. Second, we study whether initial trade cuts and post-incident improvements in supplier E&S performance are jointly associated with trade resumptions, and find that only joint trade cuts *and* E&S performance improvements are associated with trade reversals. Overall, these findings provide novel evidence of discipline by exit in production networks.

Our findings contribute to the literature on how firms' environmental and social postures are transmitted through supply chains. Dai et al. (2021b) document 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 et al. (2021) and Dai et al. (2021a) show that U.S. firms outsource part of their carbon emissions to foreign suppliers in response to investor, customer, and government pressure. We complement this literature by conducting the first large-sample study of trade cuts following supplier E&S incidents. While the literature has largely studied the propagation of E&S policies in fixed supply chain structures, our paper shows that the structure of the supply chain can itself be affected by the E&S preferences of some stakeholders via direct trade cuts and reallocation.

In related studies, Koenig and Poncet (2022) document a drop in exports to France by Bangladeshi retailers connected to the 2013 collapse of Dhaka's Rana Plaza building, while Amengual and Distelhorst (2020) study supplier compliance after the Gap, Inc changed its management of suppliers' social behavior. Our paper generalizes these event studies to a broad sample of E&S incidents, establishes investor pressure as the main driver of customer behavior and trade cuts, and quantifies the associated costs. In another related study, Pankratz and Schiller (2021) document customer responses and permanent relationship terminations following perceived changes in suppliers' climate risk exposure. Our paper focuses on actual E&S incidents rather than on perceived climate risk exposure, isolates investor preferences from supplier business risk, and documents intensive-margin trade reductions which cannot be estimated using other datasets.[7] Unlike these studies, our paper establishes customer exit as a disciplinary threat for international suppliers.

Our paper also contributes to the literature on institutional investors' role in monitoring firms' E&S activities (e.g., Krueger et al., 2020; Atta-Darkua et al., 2022, and Azar et al., 2021). We believe our paper is the first to study how institutional investors' E&S preferences affect trade activity with suppliers and the structure of international supply chains. Our paper complements Gantchev et al. (2022) and von Beschwitz et al. (2022), who study investor reactions around portfolio firms' E&S incidents, and Derrien et al. (2022), who show that analysts reduce profit forecasts after E&S incidents. Rather than focusing on the direct disciplining role of capital market participants, we document an

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

indirect disciplining role along the supply chain by customers owned by E&S-conscious investors (as in Landier and Lovo, 2020).[8]

More broadly, the stakeholder capitalism literature (Bénabou and Tirole, 2010; Hart and Zingales, 2017) highlights lack of extra-territorial reach as a potential reason why governments may fail to curb corporate externalities. Consistent with the arguments in Bénabou and Tirole (2010), our paper suggests that corporate stakeholders such as investors can overcome these limitations and exert pressure on firms (even privately held ones) outside of their country and, possibly, their investment universe. Indeed, in 2019, private firms' GHG (CO2-equivalent) emissions contributed to 59% of global corporate fossil fuel emissions (Atta-Darkua et al., 2022).[9] Complementing recent theoretical evidence (Landier and Lovo, 2020), our findings suggest that holding stakes in U.S. publicly listed firms with a wide global supplier network can provide a 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 incidents, and explain how we use these sources to construct our main matched sample. In Appendix Table A1, we provide definitions for all variables used in the paper.

[8]The literature has also studied how production networks affect customer and supplier policies (e.g., Titman, 1984; Banerjee et al., 2008; Ahern and Harford, 2014; Barrot and Sauvagnat, 2016) and how institutional investors use exit to improve corporate governance in the firms they invest in (e.g., Admati and Pfleiderer, 2009; Edmans, 2009; Edmans and Manso, 2011; Bharath et al., 2013). To the best of our knowledge, there is limited empirical evidence on whether and how customers' trade cuts affect supplier policies, especially those with negative externalities.

[9]On the other hand, Shive and Forster (2020) find that privately held U.S. firms have lower greenhouse gas emissions than similar publicly listed U.S. firms.

2.1.1 Cross-border Shipments

Maritime imports constitute the vast majority of U.S. imports, both in terms of tonnage and in terms of value.[10] Title 19 of the United States Code of Federal Regulation (CFR) requires U.S. firms to report shipment details in cargo declarations to the U.S. Customs and Border Protection (CBP). We obtain shipment-level data on these maritime transactions between foreign suppliers and U.S. customers over the 2007-20 period from the S&P Global Panjiva database. For each shipment transaction, Panjiva provides information about the sender, the consignee, the origin and destination, the product codes and descriptions of the items, and the shipment container specifications. The information included in Panjiva is required by U.S. customs law, which reduces potential selection concerns based on suppliers' and customers' disclosure incentives around E&S incidents.[11]

We link U.S. consignees in Panjiva to their ultimate parent in Compustat and aggregate the Panjiva data to the Panjiva supplier-Compustat customer-year level.[12] To track within-relationship variation over time, we require the supplier-customer relationship to appear in at least two distinct years during our sample period. We also add two years before the first year in which a given supplier-customer relationship appears in our sample to account for the ramp-up of relationships over time (Intintoli et al., 2017). Similarly, we extend the panel by two years after the last year in which the relationship appears in the data to account for relationship deterioration. All transaction values are set to zero for these extended periods and for all the years in which transaction values are missing between the first and the last relationship years. Appendix Table A2 describes the sample selection process for the Panjiva data.

[10] `www.trade.gov`.

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

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

2.1.2 E&S Incidents

We gather the universe of negative ESG-related incidents for 2007-2021 from RepRisk, a leading business research provider that searches media, regulatory, and commercial documents for companies' ESG-related incidents (Gantchev et al., 2022).[13] RepRisk classifies incidents into environmental ("E"), social ("S"), and governance ("G") categories. Environmental incidents involve pollution; overuse and wasting of resources; and animal mistreatment. Social incidents involve community relations (such as human rights abuses and social discrimination) and employee relations (such as forced or child labor and occupational health and safety accidents). Governance incidents include corruption, bribery, extortion, money laundering, executive compensation issues, misleading communication, fraud, tax evasion, tax optimization, and anti-competitive practices.

We focus on incidents such as waste management and human rights abuses that are likely to create negative externalities for local communities and thus could carry downstream reputational effects above and beyond pure business risks. 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 and accounting fraud) result from failures in private contracting between suppliers' shareholders and managers, and their downstream reputational effects are unclear.[14] As a result, following recent work (Krüger, 2015; Dai et al., 2021b; Dyck et al., 2019; Gantchev et al., 2022) we focus on environmental and social ("E&S") incidents and exclude governance related-incidents from our main analysis. We study governance related-incidents in robustness tests.

[13]RepRisk does not disclose the source(s) of each individual incident entry. According to RepRisk, a team of analysts manually verifies that each incident is indeed ESG-related, records the incident location and the firms involved, and ranks the severity of the incident.

[14]Prior research has looked at how such corporate governance incidents affect customer-supplier relationships. For example, Karpoff et al. (2008) argue that accounting misconduct can reveal suppliers' inability to fulfil orders or support warranties. Johnson et al. (2014) show that fraud increases customers' wariness in dealing with dishonest management, thereby reducing product market interactions.

2.1.3 Matching and Final Dataset

We use a fuzzy name algorithm to link Panjiva foreign suppliers (both privately held and publicly listed) to their RepRisk E&S incidents. To ensure at least three years of cross-border shipment data before and after an incident, we study incidents occurring between 2010 and 2018. Panel A of Table 1 describes the resulting matched sample, which consists of 1,049 (1,010) supplier-years (unique suppliers) and 1,319 (1,281) relationship-years (unique relationships) affected by an E&S incident.[15] In the matched sample, we find that 158 incidents are related only to "E" issues, 629 only to "S" issues, and 273 to both "E" and "S" issues. Incident-affected suppliers are economically material for U.S. customers: we find that around 4.7% of the pre-incident container imports for the average customer in our sample come from incident-affected suppliers. Around 74.4% of these incident-affected suppliers face trade cuts after the incident, while the remaining 25.6% does not face any trade cut.

In Panel B of Table 1, we also break down supplier incidents by the U.S. customer Fama-French 48 industry. Industries that heavily rely on intermediate goods, such as Retail, Apparel, and Machinery, have the largest number of cases in our sample period (231, 100, and 96, respectively). However, supplier incidents are distributed across many industries: 42 out of the 48 Fama-French industries experience at least one E&S incident in our sample, and 25 industries experience more than 10 incidents.[16]

The combined RepRisk-Panjiva dataset gives us a unique picture of U.S. firms' importing behavior around the E&S incidents of foreign suppliers, and allows us to make use of more detailed information than that available from media coverage of the cus-

[15]We start with 4,975 supplier-year E&S incidents over the 2010-2018 period, which correspond to 6,565 supplier-customer-years and 2,288 unique customer-years. We focus on *novel* events that appear in RepRisk for the first time, and we remove repeated incidents from the sample to avoid confounding variation arising from slow news dissemination over time. After removing observations with other confounding incidents in the three years before and after the incident, we have 1,049 supplier-year events corresponding to 1,319 supplier-customer-years and 838 unique customer-years.

[16]The geographic footprint of incidents in our final sample is also diverse. Treated suppliers are located in 84 different jurisdictions, and incidents in the top 5 jurisdictions (Mainland China, the United Kingdom, Hong Kong, Germany, and Japan) constitute only 37.1% of the full sample.

tomer and from supply chain self-disclosure. For example, out of 1,674 RepRisk supplier incidents in our sample, only 13.9% are associated with RepRisk *customer* incidents in the same week and only 2.3% are covered by non-local media outlets such as CNN. In turn, this suggests that the U.S. public might not be aware of the incident, of the supply chain connections between incident-affected suppliers and their U.S. customers, or both. At the same time, the customers' value losses around supplier incident announcements suggest that *investors* may use Panjiva or other private sources to identify links with incident-affected suppliers when public information is not directly available.

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

Before describing the 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 incident announcements. 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 CAR estimates for 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 show, respectively, that the findings are statistically similar and economically larger when CAR estimation window is [-3, +3] and [-5, +5] days around the supplier incident announcement. In Panel B, we document that in the sample that we later use for our baseline estimates, CARs are of similar magnitude but lower statistical significance, perhaps due to the smaller number of observations relative to the overall RepRisk data. Overall, the estimates of this event study analysis confirm that supplier incidents trigger negative customer stock price reactions and thus likely have a material impact on customers.

2.2 Panel Structure and Estimation Strategy

In our main analysis, we use a stacked difference-in-differences regression design (e.g., Cengiz et al., 2019) to study how the imports of U.S. customers change around foreign suppliers' E&S incidents. For each supplier incident in our sample, we construct cohorts of treated and control trade relationships in an interval of $[t-3, t+3]$ years around the incident, where t is the year of the incident. The treated sample in any given cohort consists of supplier-customer relationships in which the supplier experiences an E&S incident in year t. The control sample consists of i) relationships between affected customers (i.e., U.S. firms with at least one supplier experiencing an incident at time t) and their other suppliers not experiencing an incident in our sample period; and ii) never-treated relationships in which none of the customers' suppliers experience any E&S incident in our sample period. To mitigate potential confounding variation arising from repeated treatment over time (e.g., Baker et al., 2022), the treated group also excludes supplier incidents that follow or are followed by other incidents involving the same supplier in the $[t-3, t+3]$ estimation window.

2.2.1 Empirical Specification

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

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

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 and capture time-varying customer characteristics such as demand shocks. We cluster standard errors at the supplier-cohort level.

In our main specifications, we measure $Y_{i,j,c,t}$ as the number of containers imported by customer i from supplier j in year t.[17] Due to the discrete nature and zero values of the container data, we estimate the model (1) with Poisson regressions (e.g., Cohn et al., 2022).[18] In these regressions, the main coefficient of interest is β_1, which pins down the percentage change in the number of containers imported by U.S. customers from treated suppliers after the incident, relative to those imported by either the same customers or by other customers from suppliers not experiencing any incident. To identify complete trade cuts on the extensive margin, we also measure $Y_{i,j,c,t}$ as an indicator variable for whether any container is imported by customer i from supplier j in year t. In these cases, we estimate the model (1) with linear OLS regressions.

2.2.2 Identification and Control Group Choices

The identifying assumption for the coefficient β_1 to have a causal interpretation is that U.S. customers do not start reacting to supplier incidents before the incident news is released—either the incident is completely unanticipated by customers, or reporting increases salience to U.S. customers' stakeholders such as investors and end consumers. This assumption is supported by our focus on novel supplier incidents (i.e., incidents that are not related to previous supplier incidents in RepRisk), as well as by anecdotal evidence on information opacity in global supply chains.[19] As we report below, the data

[17]We focus on containers due to their uniform measurement, but our findings are robust to using the annual number of shipments from the supplier to the customer, the total weight of all annual shipments from the supplier to the customer, and the annual quantity of all shipments from the supplier to the customer as alternative measures of trade.

[18]As shown in Appendix Table A3, our main findings are qualitatively similar when we perform log-transformations of a constant plus the dependent variable (e.g., log(1+containers)).

[19]For example, discussions with industry participants reveal that U.S. customers often hire foreign due diligence experts to search local news and social media for information about suppliers' E&S behavior.

also shows no evidence of pre-existing differences in either ESG metrics or trade between treated and control suppliers, further supporting the common trends assumption.

In our tests, the control group in any given cohort includes the other suppliers of the affected U.S. customer as well as never-treated supplier-customer relationships. In both cases, within-customer reallocation and within-industry reputational spillovers might lead to concerns about the stability of our estimates of the coefficient β_1. In Section 7, we show that our estimates are economically and statistically similar when we restrict the sample to control suppliers of never treated customers, thus controlling for potential within-customer reallocation; and to suppliers operating in different industries than the affected suppliers, thus controlling for potential within-industry spillovers.

2.3 Summary Statistics

Our final stacked panel consists of 1,000,950 supplier-customer-cohort-year observations for 2010-2018. Panel C of Table 1 reports summary statistics for the main dependent and independent variables. 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 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. 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, a U.S. customer in our sample has a high probability of being indirectly exposed to an E&S incident through one of its suppliers. As a comparison to these averages in our sample, Gantchev et al. (2022) find that the annual unconditional probability of a firm being *directly* affected by an E&S incident is 22%, which highlights the importance of *indirect* exposures for E&S risk management.

The next two rows of Table 1, Panel C show summary statistics for our main dependent variables: the number of containers shipped from suppliers to customers in a given year and the annual probability of a container shipment. The average supplier ships 0.942 containers to the average customer in our data, with a standard deviation of 1.308 containers per year. Similarly, the probability of any container shipment between the average supplier and the average customer in any given year is equal to 0.471, with a standard deviation of 0.499.

The remainder of Table 1, Panel C provides summary statistics for the control variables of some of our empirical specifications. 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 control variables are lagged by one year and winsorized at the 1% and 99% levels.

3 Supplier E&S Incidents and Trade Relationships

We first present the findings on trade changes around foreign suppliers' E&S incidents. We then explore cross-sectional effects based on incident characteristics.

3.1 Baseline Estimates

Table 3 reports the estimates of the regression model (1), where we compare trade changes between incident-affected international suppliers and their U.S. customers in a seven-year window around the incident, and trade changes between other international suppliers and their U.S. customers during the same time window. Our baseline sample includes publicly listed U.S. customers and both publicly listed and privately held international suppliers. The first column of Table 3 reports our baseline Poisson regression estimates. In this specification, we control for relationship (i.e., customer-supplier) pair-cohort fixed effects and customer firm-year-cohort fixed effects. In this way, we can control for time-varying customer characteristics and compare imports from suppliers directly affected by incidents to imports *by the same customers* from suppliers not directly involved in the incidents over the same time period.

Column (1) shows that in the three years following a supplier's E&S incident, imports by U.S. customers decline on average by 29.9% relative to imports by the same U.S. customers from unaffected suppliers. These estimates are quantitatively large, and correspond to 0.282 containers per year (relative to the unconditional sample mean) and to 21.53% of a standard deviation. Together with the findings of Table 2, these findings suggest that E&S incidents affect not only customers' stock performance but also 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 findings in columns (2) and (3) of Table 3, respectively. Column (2) shows that the average relationship between U.S. customers and their international suppliers is 4.3% more likely to be terminated after the supplier is involved in an E&S incident. This estimate is quantitatively large, and it implies a nearly 50% increase relative to the 9% unconditional relationship termination rate in our sample. Similarly, column (3) shows that if we condition on trade continuation and study pure intensive margin effects, the average U.S. customer reduces its imports by 18.3% following a supplier's E&S incident, which corresponds to a 0.172 drop in annual container shipments relative to the unconditional mean and to 13.18% of a standard deviation.

Figure 1 breaks down our estimates into dynamic changes around the incident. Panel A shows the evolution of the baseline treatment effect (corresponding to column (1) of Table 3) from years $t-2$ to $t+3$ of the event window, taking year $t-3$ as a baseline. This panel documents a large and statistically significant 30-50% drop in container shipments following the supplier incident, which partially reverts in the last year of the cohort. In Section 6.3, we study the economic incentives underlying these reversals. Panel B shows 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 20-30% drop in the probability of a trade relationship after the incident.

The intensive margin estimates in Table 3 and Figure 1 show that even when customers continue their trade relationships, they severely reduce shipments from suppliers involved in an E&S incident. Such partial trade cuts are extremely frequent: around 44% of the trade cuts in our sample involve reduced trade but not a complete termination. These partial trade cuts could imply that U.S. customers start diversifying their supply chains away from affected suppliers but cannot fully terminate the relationship (e.g., due to supplier specificity or the unavailability of competitive alternatives).[20] A complementary hypothesis is that customers may be implementing a costly threat to restore suppliers' E&S performance. Section 6.3 documents trade reversals when suppliers improve their E&S performance following initial trade cuts. This finding supports the interpretation that partial adjustments are an effective threat mechanism.

Partial trade adjustments also help us rule out supplier "window-dressing" (e.g., registering the supplier under a different company name or adding phantom suppliers to hide direct connections). Section 6.2 also shows that trade cuts and the associated supply chain adjustments have large negative effects on U.S. customers' profitability, which makes the supplier window-dressing hypothesis even less likely.

[20]On the other hand, legal issues are unlikely to limit the ability to switch suppliers, as many supplier contracts include E&S covenants such as, for example, those from The Chancery Lane Project.

3.2 Incident Characteristics

In Table 4, we provide the estimates of cross-sectional tests based on the specification reported in column (1) of Table 3. Appendix Table A4 reports the corresponding effects on the extensive margin. First, we ask whether trade cuts vary across incidents related to environmental ("E") and social ("S") issues. Column (1) of Table 4 shows no statistically and economically meaningful difference in trade cuts across "E" and "S" incidents, which suggests that these incident types carry similar downstream reputational effects.

Second, we investigate whether trade cuts increase with the incident's severity.[21] Column (2) shows that while imports shrink for both high- and low-severity incidents, trade cuts are larger and statistically significant only for higher-severity incidents. Third, we link the market 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 subsample of customers that experience larger negative market reactions after the incident announcement. This suggests that costly trade cuts and reallocation to different suppliers negatively affect customer value, and that the announcement returns documented in Table 2 at least partly reflect a negative cash flow effect. Finally, in column (4) we document that the effects are stronger when media and policy attention to firms' ESG posture is high, using the Media Climate Change Concerns Index of Ardia et al. (2022).

Overall, our estimates are stronger in the cross-section of incidents more likely to generate adverse downstream reputational effects, and in the time-series in periods of greater awareness of E&S-related issues, thus providing initial evidence that the trade cuts are best explained by E&S incidents, and not by other correlated shocks at the supplier level.

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

4 Customer Characteristics and Investor Preferences

Next, we ask whether the main effects documented in Table 3 vary with U.S. customers' characteristics, and particularly with their investor base. To test our hypotheses, we add supplier-year-cohort fixed effects to the regression specification (1), thereby comparing import responses to the same supplier incident by U.S. customers with different characteristics. For example, these tests allow us to compare changes in trade between a supplier involved in an E&S incident and its U.S. customers with high pre-incident return on assets (ROA) and changes in trade between the *same* supplier and its U.S. customers with low pre-incident ROA.

We report the overall effects in Table 5 and the corresponding effects for the extensive margin effects in Appendix Table A5. The first three columns of Panel A show no cross-sectional differences in our baseline estimates based on customers' market-to-book, ROA, or gross profit margins, suggesting that average trade cuts in our sample are not systematically driven by customers with sounder financial conditions or other characteristics correlated with profitability such as, for example, bargaining power. Appendix Table A6 also shows that the baseline effect does not reflect differences in firms' financial constraints and supply chain risk diversification, which may affect supplier reallocation and its associated costs. That is, we find no systematic cross-sectional variation across financially unconstrained and constrained customers, nor across customers with different levels of supply chain risk diversification, which makes these explanations less likely.

Next, we study whether the estimates vary in the cross-section for customers with different ESG profiles. To do so, we interact the baseline treatment effect indicator with *High E&S*, a binary variable equal to one for customers with above-the-median Refinitiv ESG scores, and equal to zero otherwise. Column (4) of Panel A shows a significantly negative interaction effect between *Treat Supp* $\times$ *Post* and *High ESG*, which confirms that the identified effects are concentrated among customers with better ESG profiles. The findings reported in this column and those reported in the first three columns of Panel

A and in Appendix Table A6 suggest that customers that cut trade do not systematically differ in their observable financial characteristics and supply chain risk diversification. In contrast, these customers differ in their Refinitiv ESG scores.

The observation that customers that cut trade have better ESG scores could reflect differences in holdings by ESG-friendly investors, as theoretically predicted by Landier and Lovo (2020). We investigate this hypothesis further in Panel B of Table 5. First, we follow Gantchev et al. (2022) and identify E&S-conscious investors using the Refinitiv ESG ratings of their portfolio holdings.[22] 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 we interact this indicator variable with the treatment effect indicator $Treat\ Supp \times Post$. Column (1) of Panel B shows that the coefficient associated with $Treat\ Supp \times Post \times High\ IO\_ESG$ is negative, suggesting that customers are more likely to reduce imports from treated suppliers when their shareholders invest in firms with better E&S performance.

Second, we use shareholder proposals related to E&S issues as a direct proxy for investors' engagement in E&S activities. We obtain information about shareholder proposals from Institutional Shareholder Services (ISS), and we categorize proposals on socially responsible investments (SRI) as E&S proposals. Due to ISS data availability, in this test our stacked panel of U.S. customers is restricted to the S&P 1500 index constituents. 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 $t-1$. Column (2) of Panel B shows that the coefficient associated with the incremental interaction term $Treat\ Supp \times Post \times ESGProposal$ is negative and statistically significant, suggesting that customers are more likely to reduce imports

[22] Like 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. Unlike Gantchev et al. (2022), who use 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.

from treated suppliers after facing active E&S engagement by their shareholders. These cross-sectional findings based on shareholder proposals also suggests that the trade cuts observed in our sample are unlikely driven by managerial preferences, and if anything reflect opposing E&S preferences of shareholders and managers.[23]

Figure 2 provides additional dynamic evidence across firms facing different pressure by institutional investors. In Panel A, we report the same trends as in Figure 1, Panel A, in the sub-sample of U.S. customers that either receive an ESG proposal or have above-median holdings by E&S-conscious investors in the pre-incident period.[24] In Panel B, we report these trends for all other U.S. customers with incident-affected suppliers. In summary, Figure 2 shows that only U.S. customers facing investor pressure cut trade around the supplier incident, and no evidence of trade cuts for customers without investor pressure.

Finally, we ask whether privately held firms also experience trade reductions following E&S incidents by their suppliers. To perform this test, we expand our stacked Panjiva-RepRisk panel to include the universe of Panjiva U.S. customers that are not publicly traded, and we create a customer firm-year indicator variable, *Public Cust*, equal to one if the stocks of the customer's ultimate parent are publicly traded in the incident year, and equal to zero otherwise. Column (3) of Panel B shows that the interaction coefficient between the baseline treatment effect indicator, $\textit{Treat Supp} \times \textit{Post}$, and the indicator for publicly listed customers, *Public Cust*, is negative, statistically significant at the conventional levels, and implies an incremental 19.1% post-incident trade cut by publicly listed firms. These findings then suggest that in response to the same E&S incident, public firms reorganize their supply chains more actively than privately held firms, and provide further evidence that institutional investor preferences are likely the main driver of the trade adjustments.

[23]See, e.g., `www.wsj.com`.

[24]The trends are qualitatively similar if we split the sample based on ESG proposals and on holdings by E&S-conscious investors separately. However, the statistical significance of the estimates decreases due to the smaller sample size.

The findings of column (3) also add to the ongoing debate on the ESG-related costs and benefits of being publicly listed.[25] Specifically, these findings highlight one of the potential benefits of being private: reorganizing supply chains after an E&S incident can be costly for U.S. customers (as we confirm in our tests below), and privately held customers may be more shielded from these costs than their publicly held peers. Our findings also imply that the current trend of public firms' delistings in the U.S. (e.g., Doidge et al., 2017; Ewens and Farre-Mensa, 2020) could result in an overall decrease in E&S performance around the globe if these delistings are accompanied by lower pressure to discipline international suppliers' E&S performance.

Overall, Table 5 and Appendix Tables A5 and A6 suggest investor pressure as the most likely determinant of the trade adjustments following suppliers' E&S incidents. These within-supplier-cohort-year findings reduce potential concerns that the trade cuts reflect changes in suppliers' business or financial risks orthogonal to E&S considerations.[26] With these findings, we also rule out government-imposed trade cuts such as withhold-and-release orders by the CBP over forced labor allegations as a potential explanation for our findings.[27]

5 Retail Consumer Response

While our findings so far suggest that investor preferences play an important role in supply chain adjustments to E&S shocks, an alternative explanation for these adjustments is the potential pressure U.S. importers face from their own end consumers. To test this hypothesis, we use Nielsen scanner data (henceforth Nielsen) to study whether

[25]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 (Vereckey, 2022).

[26]For example, one could argue that trade cuts following E&S incidents may simply reflect poor financial conditions of the supplier or low product quality. However, the within-supplier estimates show that these alternative mechanisms would need to hold for customers with E&S investors but not for other customers.

[27]`www.cbp.gov`.

the quantities and prices of retail products sold by U.S. importers change after their international suppliers are exposed in an E&S incident.

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

Table 6 reports our findings. In the first two columns, we use the natural logarithm of the total quantities sold by the U.S. importer in a given product module and Zip-3 area as the dependent variable, controlling for firm $\times$ Zip-3 $\times$ product module $\times$ cohort fixed effects and Zip-3 $\times$ product module $\times$ quarter $\times$ cohort fixed effects, so that we achieve identification from both time-series and cross-sectional variation in the data. In column (1), we report the average treatment effect, and in column (2) we report its dynamics. Our findings show that local sales of U.S. importers decrease by around 8% after the supplier incident, and the effect persists for around two years, suggesting that the incident may negatively affect U.S. customers' sales.

The granularity of the Nielsen data allows us to ask whether the estimates of the first two columns of Table 6 are more likely due to changes in customer demand (due, e.g., to consumer boycotts) or to changes in importers' supply (due to lower imports from the incident-affected supplier and imperfect reallocation to other suppliers). In the former case, we might expect importers' product prices to stay constant or even decrease if U.S. customers have to lower product prices to attract more retail consumer demand. In the latter case, we might expect prices to increase. Columns (3) and (4) of Table 6 provide evidence consistent with the second hypothesis: the price of goods sold by U.S. customers whose suppliers are affected by an E&S incident increases by around 4.5% on average after the incident, and the effect persists for around three years.

Overall, the findings reported in this section provide limited support to the interpretation that consumers directly react to supplier E&S incidents, perhaps due to limited information on the incident (less than 3% of the RepRisk incidents in our sample are covered by international media) or on the supply chain connection between suppliers and retailers. As a result, it is unlikely that U.S. customers pursue trade cuts in response to end consumer pressure. This is consistent with other recent findings that consumers have limited reactions to social and environmental issues (e.g., Liaukonytė et al. (2022); Handziuk and Lovo (2023)).

6 Suppliers, Reallocation, and Trade Reversals

We now focus on 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 findings are stronger when customer switching costs are lower, suggesting that the ability to switch suppliers imposes 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 whether the incident-affected supplier improves its future E&S performance. Fourth, we show that if suppliers improve their E&S performance following an initial trade cut, they can re-establish trade with their U.S. customers in subsequent periods.

6.1 Supplier Characteristics and Switching Costs

Table 7 reports cross-sectional variation in the main estimates based on suppliers' characteristics.[28] The corresponding extensive margin findings are relegated to Appendix Table A7. First, we provide cross-sectional estimates based on whether the supplier is privately held or publicly listed. Column (1) shows that our baseline effects are similar (both economically and statistically) when suppliers are privately held and publicly listed, suggesting that local capital markets provides limited direct E&S pressure, and that customer reactions to incidents by publicly listed and privately held suppliers are similar.

Second, we hypothesize that large suppliers have access to a larger pool of customers with different E&S preferences, which may reduce the effectiveness of a trade cut threat. Column (2) supports this hypothesis: the data show that U.S. customers' percentage trade cuts with small suppliers in Panjiva are around three times as large as the trade cuts with large suppliers following an incident.

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 that switching costs are relatively low when suppliers operate in competitive markets and sell homogeneous goods, which leads to larger trade cuts following an E&S incident. We measure the competitiveness of the U.S. market from the perspective of foreign suppliers with the shipment Herfindahl-Hirschman Index (HHI) in each two-digit HS code category and event year.[29] Column (3) shows that the effect is significantly larger when supplier HHI is low and the customer's input market is more competitive.

Fourth, we measure how substitutable the supplier's two-digit HS product is based on the Rauch (1999) differentiation index. As column (4) shows, the trade cuts are

[28]The data on international supplier characteristics are scarce as many of the suppliers in our sample are privately held.

[29]To calculate HHI, we take the individual shares of shipments of each international supplier to U.S. customers in each two-digit HS product category, as recorded in Panjiva. If a supplier ships more than one product category, we use the shipment-weighted average HHI of each product category.

significantly larger when suppliers sell homogeneous products.[30] These findings suggest that the threat of exiting a trade relationship may be less credible if customers have a limited choice set of alternative suppliers, and that exit may be less effective to discipline suppliers when supplier inputs are highly specific to the customer's production process.

6.2 Supplier Reallocation and the Costs of Cutting Trade

Next, we 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 supplier involved in the E&S incident. We also ask whether supplier cuts and switches are associated with changes in U.S. customers' cost structure.

6.2.1 Reallocation and New Suppliers

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

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

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

The coefficient of interest in specification (2) is β_3. This coefficient identifies reallocation to control suppliers that share a customer link with at least one treated supplier,

[30] If a supplier sells more than one product, we require all products to be homogeneous for indicator assignment. Our findings are robust if we instead require at least one of the products sold by the supplier to be categorized as homogeneous according to Rauch (1999).

while also controlling for potential spillover effects on other treated suppliers (pinned down by the coefficient β_2). As in Berg et al. (2021), β_3 identifies marginal post-treatment changes in trade between control suppliers and customers linked to treated suppliers for a marginal increase in the fraction of treated suppliers in the cohort.[31] We expect the sign of this coefficient to be positive if customers switch from suppliers with E&S incidents to other international suppliers.

The findings in Table 8 support our predictions. Column (1) confirms a negative and statistically significant 31.1% drop in trade between treated suppliers and their customers after the treatment. 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 1.40% after the treatment—U.S. customers partially replace their incident-affected suppliers with other international suppliers. Finally, column (1) shows no spillover effects on the treated group, suggesting that the extent of trade cuts with treated suppliers is independent of other treated suppliers' incidents.

Next, we ask whether U.S. customers switch to suppliers from the same country as the treated suppliers, or to suppliers from different countries. On the one hand, switching to suppliers from the same country may be less costly. On the other hand, the supplier's E&S incident might hurt the reputation of all suppliers in the country and motivate customers to find new partners in other countries to diversify their supply chain risks. We thus split the indicator $Treat\ Cust,\ Control\ Supp_{j,c}$ into two indicator variables: $Treat\ Cust,\ Control\ Supp,\ Same\ Country_{j,c}$, indicating control suppliers (of customers linked to treated suppliers) from the same country as the treated supplier, and $Treat\ Cust,\ Control\ Supp,\ Diff\ Country_{j,c}$, indicating control suppliers located in

[31] Unlike specification (1), specification (2) includes less-restrictive sets of fixed effects, which allow us to estimate β_2 and β_3 separately (see Berg et al., 2021). Berg et al. (2021) focus on direct treatment spillovers to control and treated groups rather than indirect spillovers through the network, as we do in this section. In this sense, our estimation strategy also bears a resemblance to the reallocation specifications of Giroud and Mueller (2019).

other countries. Column (2) of Table 8 shows that the reallocation effects appear only in the sample of suppliers from other countries, suggesting that E&S incidents can have negative reputational spillovers within a country.

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

Table 8 focuses on existing supplier-customer relationships. Appendix Table A8 shows that, relative to the control group, U.S. customers with incident-exposed suppliers also increase the number of *new suppliers* by around 20.6% and the number of *new sourcing countries* by around 16.2% after the incident. Collectively, our findings suggest that customers readjust supply chains on both the intensive and the extensive margins.

6.2.2 The Costs of Cutting Trade

Appendix Table A9 shows that re-optimizing supply chains is costly for U.S. importers. Customers that cut trade with suppliers affected by an E&S incident show 0.9% lower profitability (as measured by their gross profit margins) after the incident. On the other hand, customers that do not implement trade cuts do not experience statistically significant changes in their gross profit margins after the incident. These findings suggests increased costs of goods sold (arising, e.g., from second-best supplier sourcing) or constraints in selling products (arising, e.g., from lack of alternative inputs), and confirm the economic relevance of the trade cuts in our sample. By showing that ESG-driven trade cuts are costly, these findings contribute to recent academic work (e.g., Schiller, 2018; Koenig and Poncet, 2022; and Dai et al., 2021a,b) and policy debates on the costs and benefits of sustainable supply chains.

6.3 Supplier E&S Improvements and Trade Reversals

What happens to international suppliers when trade with their U.S. customers decreases? In Table 9, Panel A, we start by reporting the dynamics of treated and control suppliers' average RepRisk E&S ratings around E&S incidents. The RepRisk ratings of suppliers affected by E&S incidents experience large and statistically significant decreases after the incident relative to the control group, and these rating differences last around three years. Since the E&S ratings of treated and control suppliers are economically and statistically similar before the incident, Panel A alleviates concerns that supplier E&S characteristics were already deteriorating before the incident, thus supporting our identifying assumptions.

Next, we study whether import cuts by U.S. customers trigger adjustments in suppliers' 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.[32] Second, we ask whether U.S. customers' trade cuts and international suppliers' ESG rating improvements are jointly associated with future trade reversals.

Table 9, Panel B, reports the dynamic response of suppliers' RepRisk ESG ratings following trade cuts by U.S. customers. In this panel, we test whether a supplier's post-

[32]Similar to RepRisk ESG incidents, RepRisk ESG ratings are updated daily based on negative news in the media. The ratings range from AAA (best) to D (worst) scale, and are widely used by asset managers to monitor the ESG performance of their portfolio (see, e.g., `corpgov.law.harvard.edu`). We limit the sample to suppliers that have RepRisk ESG ratings around the initial incident.

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

Consistent with Panel A, Panel B shows that on average, RepRisk ESG risk ratings decrease after the E&S incident, and this pattern persists over time. However, column (1) shows that the negative effect of the incident on ESG 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)).[33]

Next, we ask whether improved ESG ratings can be associated with trade reversals. We group treated and control relationships into cohorts of $[t+3, t+6]$ years from the supplier's initial E&S incident, classifying observations in year $t+3$ relative to the incident as "post-incident" observations in which suppliers may adjust their E&S policies, and observations in years $[t+4, t+6]$ from the incident as "post-adjustment" observations. Next, we split treated relationship cohorts into subsamples based on i) different distributional cuts of total trade changes ($\Delta Trade$) between the "pre-incident"

[33] 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 find no evidence of such spillovers, which suggests 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.

($[t-3, t-1]$) and post-incident ($[t+1, t+3]$) periods, and ii) changes of affected suppliers' ESG ratings during the post-incident period.[34]

The independent variables thus include four mutually exclusive interaction terms between indicator variables for customer trade cuts between the pre- and post-incident periods ($Cut\ Trade = 1$), and supplier rating improvements in the post-incident period ($Inc\ Rating = 1$). We set the indicator variable *Cut Trade* equal to one if Δ*Trade* is negative (column (1)), if Δ*Trade* is less than -25% (column (2)), and if Δ*Trade* is less than 67% (column (3)).[35]

We report the findings in Panel C of Table 9. Two sets of findings emerge. First, the joint presence of customer trade cuts and supplier ESG rating improvements is associated with subsequent trade reversals, and these trade reversals are increasing in the original trade cut. Relative to the control group, trade cuts, cuts below the 25th percentile, and cuts lower than 50% are associated with relative increases between the *post-incident* and the *post-adjustment* period of 49.9%, 83.4%, and 115.3%, respectively.[36]

Second, *only* the joint presence of trade cuts and ESG rating improvements is associated with 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 findings in Table 9 support the interpretation that U.S. customers may use real trade activity as an effective mechanism to discipline their suppliers' E&S performance.[37]

[34]To simplify the analysis, we focus on absolute trade cuts within the same relationship relative to the pre-incident period, as opposed to trade cuts relative to relationships in the control group. On average, trade with control group suppliers increases after the incident (as documented in Section 6.1), so absolute trade cuts are smaller than relative trade cuts.

[35]This test is similar 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, we cannot perform such test, and a causal interpretation of the findings of Panel C is therefore limited.

[36]These estimates come from different subsamples of treated firms and thus are not quantitatively comparable to our baseline estimates from Table 3.

[37]One potential concern is that RepRisk's ESG ratings are based on RepRisk's incidents (`www.reprisk.com`), such that their reversal may be mechanically driven by the exclusion of repeated incidents from our sample. However, only a subset of all the incident-exposed suppliers in our sample faces rating reversals, which makes this explanation unlikely. Additionally, Appendix Table A10 shows similar trade reversals for a subset of suppliers based on their Sustainalytics ESG ratings, further mitigating this concern.

7 Robustness Tests

Table 10 reports robustness tests for our baseline specifications from Table 3. Panel A shows that our findings are robust to alternative measures of trade intensity: the number of individual shipments (column (1)), the total shipment weight (in tonnes, column (2)), and the total shipment quantity (the individual units in a shipment, column (3)). The estimates are consistent across different measurement choices, and column (3) shows even larger effects when we measure trade using shipment quantities.

In Panel B, we report the findings based on alternative matching samples. In column (1), we match treated and control samples based on customer firms' four-digit SIC industries. That is, for each cohort, we include only control customers operating in the same industry as treated customers. In column (2), we match on customer firms' four-digit SIC industry and size deciles. In column (3), we match on customer firms' four-digit SIC industry and size deciles, as well as on supplier country: we include only control suppliers from the same country as treated suppliers. The findings are economically and statistically robust to these alternative choices, confirming that the control group choice does not systematically affect our main estimates. 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 the interpretation of international reallocation. Finally, column (4) shows that the findings are robust to restricting the sample to customer-country pairs with at least one treated and one control supplier in the same country.

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 incident. In column (1), we include only suppliers that do not face such confounding incidents in a narrower $[t-2, t+2]$ year window. In column (2), we include only suppliers that do not face such incidents 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. The estimates are consistent with the previous ones.

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

Panel E shows that our findings are robust to alternative control group choices. In column (1), we only keep the control group of *never treated* suppliers and customers, further mitigating concerns of repeated treatment over time outside the incident window (Baker et al., 2022). Since our findings are robust to removing control suppliers of the same customer, column (1) also mitigates bias concerns arising from possible stable unit treatment value assumption (SUTVA) violations arising from spillovers. In columns (2) and (3) we keep only control suppliers operating in *different industries* than the treated suppliers, further mitigating within-industry spillover concerns. Specifically, in column (2) we remove from the control group suppliers operating in the same industry of treated suppliers in the same event cohort. In column (3), we also remove from the control group suppliers that share the same customers with suppliers affected by E&S incidents.

Panel F shows that the findings are robust to alternative sample selection choices and empirical specifications. Column (1) shows economically larger trade cuts when we weigh each observation by pre-incident trade volume between the same customer and supplier, reducing potential concerns that our estimates may be driven by small and economically negligible suppliers. Column (2) shows that the findings are also robust to removing cohort-year $t+1$ from the sample, which mitigates potential concerns that the partial trade cuts may reflect pre-existing contractual agreements between customers and suppliers. In column (3), we expand the sample beyond E&S incidents to include (G)overnance-related incidents that may have downstream reputational externalities—bribery and fraud incidents. Since cargo shipments are measured at a high frequency, in column (4) we also confirm that our findings are consistent even when we use quarterly instead of annual data. Column (5) shows that the findings are consistent if we use containers scaled by the total size of the customer's annual imports as the outcome variable. Indeed, *relative* container imports decrease by 0.006 for treated suppliers after the treatment, a 21.27% drop relative to the sample mean.

8 Discussion and Conclusions

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

We document partial trade adjustments. Shipments from affected suppliers decrease by 29.9% compared to those from unaffected suppliers. Customers switch to other suppliers, especially to those in other countries, but do not always terminate their relationships. Trade reverses over the long run if the supplier's E&S performance improves after trade is cut, suggesting that partial trade adjustments could be an effective mechanism to discipline suppliers with exit.

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

The option to cut (rather than engage with) the supplier also suggests previously unstudied benefits of having suppliers outside the firm's boundaries. First, customers can pick 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. Finally, another aspect of the theory of the firm suggested by this paper is that a publicly listed U.S. firms might be attractive for impact investors interested in affecting the E&S performance of foreign suppliers possibly outside of their investment universe (Landier and Lovo, 2020).

Our findings also speak to the policy debate on regulatory outsourcing of global supply chain monitoring activities. International suppliers' E&S activities are beyond the reach of domestic governments. However, governments can impose domestic supply chain regulations to gain extraterritorial reach. One recent example of "regulatory outsourcing" is the Dodd-Frank Wall Street Reform and Consumer Protection Act's section 1502 on conflict minerals, with which the U.S. government forces multinationals to indirectly regulate firms along their supply chains (Sarfaty, 2015).[38] 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.

We show that U.S. firms' supplier E&S management (by exit) is effective beyond the case of conflict minerals, especially when firms face stronger investor pressure. In this respect, the currently discussed Scope 3 emissions reporting requirements could help investors gather more knowledge on firms' supply chain environmental performance, put necessary pressure on suppliers when needed, and thus help governments that adopt Scope 3 regulations to achieve extraterritorial reach.

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

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

This figure displays the dynamic effects of supplier E&S incidents on international trade. To estimate the dynamic effects of E&S incident exposure, we replace the *Treat Supp*×*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.

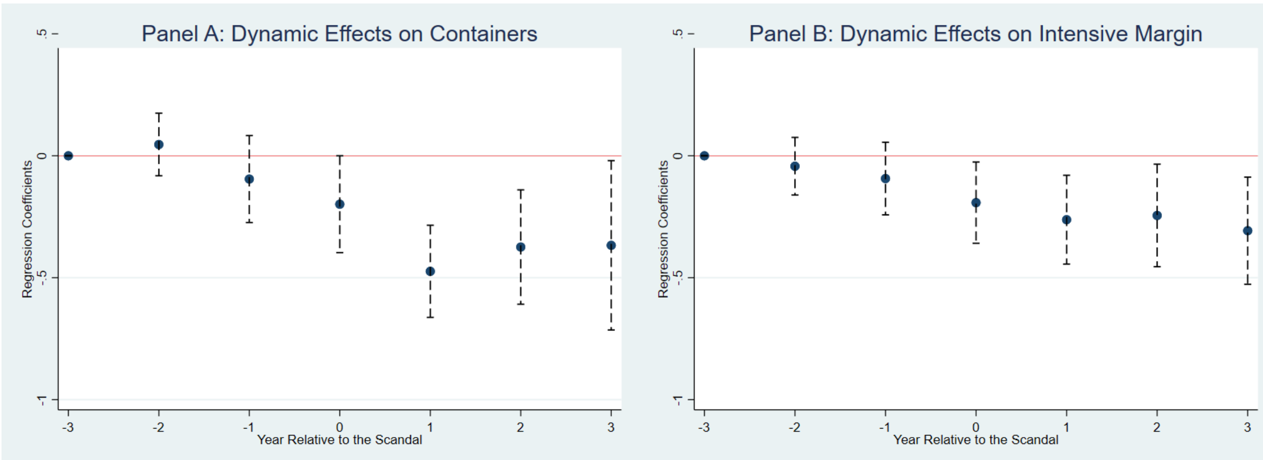

Figure 2: Dynamic Effects Conditional on Investor E&S Preferences

This figure displays the dynamic effects of supplier E&S incidents on international trade. To estimate the dynamic effects of E&S incident exposure, we replace the *Treat Supp*×*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. Panels A and B report the effects for the sub-sample of firms with high and low investor E&S preferences, respectively. Specifically, Panel A reports dynamic effects in the sub-sample of U.S. customers that either received an ESG proposal or have above-median ESG-investor holdings in the pre-incident period. Panel B reports dynamic effects in the remaining sub-set of U.S. customers with incident-affected suppliers. In this figure, we plot the estimated interaction coefficients and their associated 90% confidence intervals.

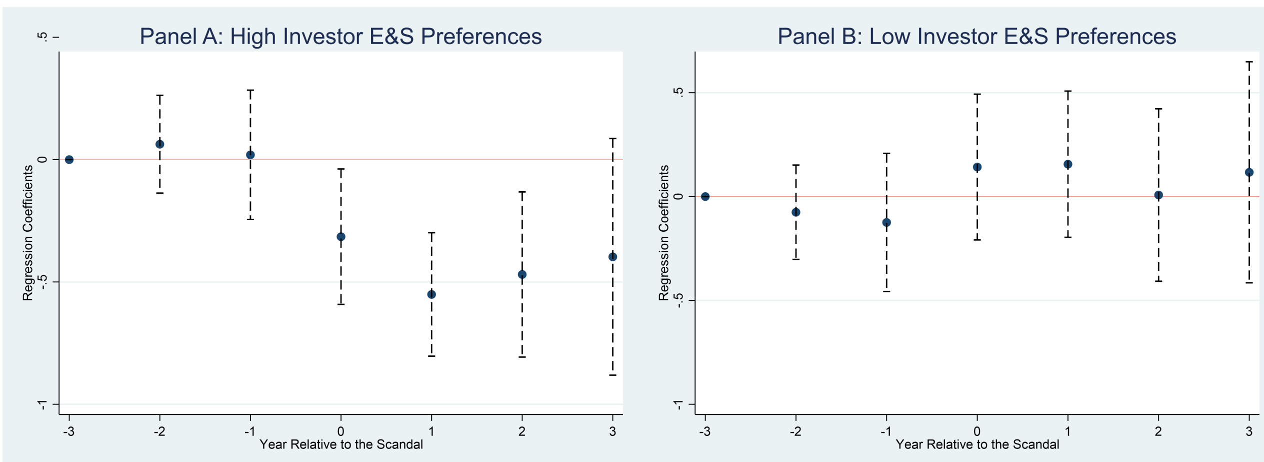

Table 1: Summary Statistics

Panel A reports the sample distribution across cohorts (i.e., event years of supplier incidents). Panel B reports the distribution of treated relationships across the Fama-French 48 industries of customers. Panel C reports descriptive statistics for the variables used in our main analyses. The definitions of the variables are provided in Appendix Table A1.

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 Mining	1
Recreation	25	Petroleum and Natural Gas	47
Printing and Publishing	13	Personal Services	2
Consumer Goods	55	Business Services	26
Apparel	100	Computers	56
Healthcare	1	Electronic Equipment	75
Medical Equipment	8	Measuring and Control Equipment	22
Pharmaceutical Products	37	Business Supplies	31
Chemicals	78	Shipping Containers	3
Rubber and Plastic Products	5	Transportation	35
Textiles	16	Wholesale	65
Construction Materials	13	Retail	231
Construction	3	Restaraunts, Hotels, Motels	9
Steel Works Etc	33	Banking	15
Fabricated Products	2	Insurance	1
Machinery	96	Trading	1
Electrical Equipment	23	Other	18
Automobiles and Trucks	79		

Table 1: Summary Statistics (Continued)

Panel C: Summary Statistics of Variables

Variable	Obs.	Mean	Std. Dev.	P25	P50	P75
Treat Supp	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 Incidents

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

Panel A: Entire RepRisk Sample

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

Panel B: In-sample Incidents

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

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

This table shows the effect of supplier E&S incidents on trade relationships. The dependent variable in column (1) is *Containers*, defined as the number of containers received by a U.S. customer from a given supplier over the year. The dependent variables in columns (2) and (3) are *1(Trade>0)* and *Containers*, respectively. Column (3) requires a relationship-cohort-year to have a positive amount of trade to be included in the regression sample. Columns (1) and (3) are estimated using Poisson regressions. Column (2) is estimated using OLS regressions. All columns control for relationship×cohort and customer firm×year×cohort fixed effects. All 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. =	Containers	1(Trade>0)	Containers
		Extensive Margin	Intensive Margin
	(1)	(2)	(3)
Treat Supp×Post	-0.299***	-0.043***	-0.183**
	(0.083)	(0.014)	(0.079)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	939,578	939,578	412,184
Pseudo. R^2	0.719		0.787
Adjusted R^2		0.134	

Table 4: Cross-sectional Tests: Incident Characteristics

This table shows cross-sectional estimates based on incident characteristics. The dependent variable is *Containers*. Column (1) partitions incidents into incidents that are primarily related to environmental issues (*Treat Supp, E*) and social issues (*Treat Supp, 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 supplier incidents (*High Reaction*) and a group with low negative market reaction to supplier incidents (*Low Reaction*). Column (4) partitions incidents into incidents that occur in periods when the Media Climate Change Concerns Index (Ardia et al., 2022) is above the sample median (*High Attention*) and below the sample median (*Low Attention*). All columns control for relationship×cohort and customer firm×year×cohort fixed effects. All columns are estimated using Poisson regressions. 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. =	Containers			
	(1)	(2)	(3)	(4)
Treat Supp, E×Post	-0.319***			
	(0.105)			
Treat Supp, S×Post	-0.262*			
	(0.136)			
Treat Supp, High Severity×Post		-0.435***		
		(0.120)		
Treat Supp, Low Severity×Post		-0.168		
		(0.109)		
Treat Supp, High Reaction×Post			-0.413***	
			(0.135)	
Treat Supp, Low Reaction×Post			-0.181*	
			(0.095)	
Treat Supp, High Attention×Post				-0.345***
				(0.088)
Treat Supp, Low Attention×Post				-0.216
				(0.168)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	939,578	939,578	939,578	939,578
Pseudo R^2	0.719	0.719	0.719	0.719

Table 5: Customer Characteristics and Investor E&S Preferences

This table shows the differential effects of the same supplier incident on trade with customers with different financial and investor characteristics. The dependent variable is *Containers*. In Panel A, *High MTB* is a binary variable indicating customers with above-median market-to-book ratios at the beginning of the event year. *High ROA* is a binary variable indicating customers with above-median returns on assets at the beginning of the event year. *High GrossMargin* is a binary variable indicating customers with above-median gross margins at the beginning of the event year. *High CustESG* is a binary variable indicating customers with above-median Refinitiv ESG ratings in the event year. In Panel B, columns (1) and (2) use the same sample as in Table 3. *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, and equal to zero otherwise. All columns include supplier×year×cohort FE and customer firm×year×cohort fixed effects. All columns are estimated using Poisson regressions. 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.

Panel A: Firm Characteristics

Dep. Var. =	Containers			
	(1)	(2)	(3)	(4)
Treat×Post×High MTB	-0.287			
	(0.429)			
Treat×Post×High ROA		-0.011		
		(0.355)		
Treat×Post×High GrossMargin			-0.466	
			(0.541)	
Treat×Post×High CustESG				-0.724*
				(0.400)
Partition Var.×Treat	Yes	Yes	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	119,880	119,880	119,880	119,880
Pseudo R^2	0.715	0.715	0.715	0.715

Table 5: Customer Characteristics and Investor E&S Preferences (continued)

Panel B: Investor E&S Preferences

Dep. Var. =	Containers		
	(1)	(2)	(3)
Treat×Post×High IO_ESG	-0.602*		
	(0.331)		
Treat×Post×ESG Proposal		-1.009**	
		(0.225)	
Treat×Post×Public Cust			-0.191**
			(0.081)
Partition Var.×Treat	Yes	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	119,880	49,775	14,319,501
Pseudo R^2	0.715	0.714	0.623

Table 6: Retail Sales Volume and Prices

This table shows the effects of supplier E&S incidents on retail sales using transaction-level data from Nielsen scanner data (henceforth Nielsen). We first match Nielsen manufacturers to their Compustat ultimate holder using a combination of fuzzy and manual name-matching procedures, and we only keep Compustat firms that appear as a match. Second, we expand the resulting panel to include information on average prices and quantities of retail products sold by U.S. importers in each Zip-3 code, product module, and quarter. Third, we collapse our main stacked panel into a customer firm-year-cohort sample, and merge it with the Compustat-Nielsen matched data. In this collapsed panel, *customer events* are years in which at least one of the U.S. importer's suppliers is affected by an E&S incident, and the control group consists of U.S. customers with no suppliers affected by E&S incidents. Fourth, we perform stacked difference-in-differences tests around customer events to study the effect of supplier incidents on average prices and quantities sold by U.S. importers. The dependent variables are *Log(Quantity)* in column (1) and *Log(Price)* in column (2). *Treat Cust* is a binary variable indicating customers with at least one supplier affected by E&S incidents in a cohort. *Post* is a binary variable indicating observations after the incident. *Post (Year τ)* is a binary variable indicating year τ relative to the incident. All columns control for *Size*, *Lev*, *R&D*, *Cpax*, and *Cash*. All columns control for firm×3-digit zip code×module×cohort and 3-digit zip code×module×quarter×cohort fixed effects. Variable definitions are in Table A1. All columns are estimated using OLS regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

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

Table 7: Supplier Characteristics and Switching Costs

This table shows cross-sectional estimates based on supplier characteristics and switching costs. The dependent variable is *Containers*. Column (1) 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 columns are estimated using Poisson regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Containers			
	(1)	(2)	(3)	(4)
Treat Supp, Public×Post	-0.352***			
	(0.126)			
Treat Supp, Private×Post	-0.279***			
	(0.103)			
Treat Supp, Large×Post		-0.218**		
		(0.090)		
Treat Supp, Small×Post		-0.593***		
		(0.168)		
Treat Supp, High HHI×Post			-0.174	
			(0.119)	
Treat Supp, Low HHI×Post			-0.409***	
			(0.115)	
Treat Supp, High Differentiation×Post				-0.281***
				(0.088)
Treat Supp, Low Differentiation×Post				-0.530**
				(0.208)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	939,578	939,578	939,578	939,578
Pseudo R^2	0.719	0.719	0.719	0.719

Table 8: International Supply Chain Reallocation

This table documents trade reallocation along the supply chain network. The dependent variable is *Containers*. *%Treat Supp* is the fraction of suppliers affected by an E&S incident in any given cohort. *Treat Cust, Control Supp* is a binary variable indicating control suppliers of "treated" customers (i.e., customers with at least one supplier affected by an E&S incident). *Treat Cust, Control Supp, Same Country* is a binary variable indicating control suppliers of "treated" customers located in the same country of the treated supplier. *Treat Cust, Control Supp, Diff Country* indicates control suppliers in other countries. *Treat Cust, Control Supp, High SuppE&S* is a binary variable indicating control suppliers of "treated" customers with average pre-incident RepRisk ESG rating above the top quintile of the sample distribution. *Treat Cust, Control Supp, Low SuppE&S* indicates control suppliers of "treated" customers with average pre-incident RepRisk ESG rating below the top quintile of the sample distribution. All columns control for relationship×cohort and customer firm×cohort fixed effects. Variable definitions are in Table A1. All columns are estimated using Poisson regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	Containers		
	(1)	(2)	(3)
Treat Supp×Post	-0.311***	-0.311***	-0.361***
	(0.095)	(0.095)	(0.106)
%Treat×Treat Supp×Post	1.142	1.142	1.166
	(0.868)	(0.868)	(0.836)
%Treat×Treat Cust, Control Supp×Post	1.402***		
	(0.506)		
%Treat×Treat Cust, Control Supp, Same Country×Post		1.018	
		(0.940)	
%Treat×Treat Cust, Control Supp, Diff Country×Post		1.493***	
		(0.566)	
%Treat×Treat Cust, Control Supp, High SuppE&S×Post			23.410***
			(7.695)
%Treat×Treat Cust, Control Supp, Low SuppE&S×Post			-4.710
			(3.226)
Size	0.230***	0.230***	0.146*
	(0.016)	(0.016)	(0.079)
Leverage	-0.626***	-0.626***	-0.306
	(0.053)	(0.053)	(0.286)
R&D	4.206***	4.209***	1.789
	(0.499)	(0.499)	(1.880)
Capx	-1.287***	-1.287***	-4.461***
	(0.215)	(0.215)	(0.958)
Cash	0.236***	0.236***	0.237
	(0.071)	(0.071)	(0.355)
Pair×Cohort FE	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes
Obs.	936,183	936,183	37,126
Pseudo R^2	0.626	0.626	0.621

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

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

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

Panel B: Future Supplier Risk

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

Panel C: Trade Reversal

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

Table 10: Additional Robustness

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

Panel A: Alternative Trade Measures

Dep. Var. =	Shipment	Weight	Item
	(1)	(2)	(3)
Treat Supp×Post	-0.248***	-0.267***	-0.345***
	(0.081)	(0.081)	(0.117)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Cohort×Year FE	Yes	Yes	Yes
Obs.	936,179	936,179	936,179
Pseudo R^2	0.678	0.801	0.814

Table 10: Additional Robustness (Continued)

Panel B: Matching Sample

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

Panel C: Alternative Restrictions on Confounding Incidents

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

Table 10: Additional Robustness (Continued)

Panel D: Alternative Fixed Effects

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

Table 10: Additional Robustness (Continued)

Panel E: Alternative Control Groups

Dep. Var. =	Containers		
Control Group =	Control suppliers of never treated customers	(i) Treated customers' unaffected suppliers selling products different from treated suppliers of the same cohort; (ii) Control customers' suppliers selling products different from treated suppliers of the same cohort	Control customers' suppliers selling products different from treated suppliers of the same cohort
	(1)	(2)	(3)
Treat Supp×Post	-0.157*	-0.463***	-0.262***
	(0.082)	(0.106)	(0.091)
Pair×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	No	Yes	No
Year×Cohort FE	Yes	No	Yes
Obs.	264,019	191,774	69,482
Pseudo R^2	0.652	0.726	0.637

Panel F: Alternative Samples and Specifications

Dep. Var. =	Containers				
	Weighted Regression	Remove year $t+1$	Including corruption, bribery, fraud	Quarterly data	Scaled by size
	(1)	(2)	(3)	(4)	(5)
Treat Supp×Post	-0.345***	-0.272***	-0.215***	-0.286***	-0.006***
	(0.129)	(0.082)	(0.077)	(0.080)	(0.002)
Pair×Cohort FE	Yes	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes	Yes
Obs.	607,646	779,080	975,553	3,581,751	939,578
Pseudo R^2	0.709	0.727	0.716		0.598
Adj. R^2				0.476	

Supplemental Material to "ESG Shocks in Global Supply Chains"

Intended for Online Publication

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
Shipment	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
Item	The natural logarithm of the number of individual items shipped from the supplier to the customer in the year.	Panjiva
Quantity	The total quantity sold by the firm in a given product module and Zip-3 area in a quarter.	Nielsen
Price	The value-weighted price of all products sold by the firm in a given product module and Zip-3 area in a quarter.	Nielsen
Treat Supp	A binary variable that equals one if the supplier is subject to an E&S incident.	RepRisk
Treat Cust	A binary variable that equals one if any of the firm's suppliers is subject to an E&S incident.	RepRisk
Post	A binary variable that equals one for the periods following the supplier's E&S incident.	RepRisk
Size	The natural logarithm of the asset size of the customer firm.	Compustat
Leverage	The sum of short-term and long-term debt scaled by total assets.	Compustat
R&D	The ratio of R&D expenditure to total assets. Missing values are replaced with zero.	Compustat
CAPX	The ratio of capital expenditure to total assets.	Compustat
Cash	The ratio of cash and cash equivalents to total assets.	Compustat
Treat Supp, E	The product of *Treat Supp* and a binary variable that equals one if the incident is primarily related to environmental issues.	RepRisk
Treat Supp, S	The product of *Treat Supp* and a binary variable that equals one if the incident is primarily related to social issues.	RepRisk
Treat Supp, High Severity	The product of *Treat Supp* and a binary variable that equals one if the supplier incident is coded as a high- or medium-severity incident.	RepRisk
Treat Supp, Low Severity	The product of *Treat Supp* and a binary variable that equals one if the supplier incident is not coded as *High Severity*.	RepRisk
Treat Supp, High Reaction	The product of *Treat Supp* and a binary variable that equals one if the customer's market reaction over a [-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

Table A1: Variable Definitions (Continued)

Variable	Definitions	Data Source
Treat Supp, High Attention	The product of *Treat Supp* and a binary variable that equals one if the average daily Media Climate Change Concerns index in the year is above the sample median.	RepRisk, Ardia et al. (2022)
Treat Supp, Low Attention	The product of *Treat Supp* and a binary variable that equals one if the average daily Media Climate Change Concerns index in the year is below the sample median.	RepRisk, Ardia et al. (2022)
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 relative to the aggregate container shipments to the SIC industry 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 relative to the aggregate container shipments to the SIC industry 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 MTB	A binary variable that equals one if the customer firm's ratio of market value of equity to book value of equity at the beginning of the event year is above the sample median.	Compustat
High ROA	A binary variable that equals one if the customer firm's ratio of operating income before depreciation and amortization to total assets at the beginning of the event year is above the sample median.	Compustat

Table A1: Variable Definitions (Continued)

Variable	Definitions	Data Source
High GrossMargin	A binary variable that equals one if the customer firm's ratio of gross margins at the beginning of the event year is above the sample median.	Compustat
High 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

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: Log(1+Containers) as the Dependent Variable

This table shows the effect of supplier E&S incidents on trade relationships. The dependent variable is *Log(1+Containers)*, defined as the natural logarithm of one plus the number of containers received by a U.S. customer from a given supplier over the year. Column (2) requires a relationship-cohort-year to have a positive amount of trade to be included in the regression sample. Both columns are estimated using OLS regressions. All 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)	Log(1+Containers)
		Intensive Margin
	(1)	(2)
Treat Supp×Post	-0.112***	-0.091*
	(0.039)	(0.054)
Pair×Cohort FE	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes
Obs.	939,578	412,184
Adjusted R^2	0.387	0.641

Table A4: Incident Characteristics: Robustness Tests on Extensive Margin

This table shows cross-sectional estimates based on incident characteristics. The dependent variable is *1(Trade>0)*. Column (1) partitions incidents into incidents that are primarily related to environmental issues (*Treat Supp, E*) and social issues (*Treat Supp, 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 supplier incidents (*High Reaction*) and a group with low negative market reaction to supplier incidents (*Low Reaction*). Column (4) partitions incidents into incidents occurred during periods with Media Climate Change Concerns Index (Ardia et al., 2022) above the sample median (*High Attention*) and below the sample median (*Low Attention*). All columns control for relationship×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. All columns are estimated using OLS regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Dep. Var. =	1(Trade>0)			
	(1)	(2)	(3)	4
Treat Supp, E×Post	-0.043**			
	(0.018)			
Treat Supp, S×Post	-0.044*			
	(0.023)			
Treat Supp, High Severity×Post		-0.042**		
		(0.021)		
Treat Supp, Low Severity×Post		-0.044**		
		(0.019)		
Treat Supp, High Reaction×Post			-0.052**	
			(0.022)	
Treat Supp, Low Reaction×Post			-0.033	
			(0.022)	
Treat Supp, High Attention×Post				-0.055***
				(0.018)
Treat Supp, Low Attention×Post				-0.021
				(0.024)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	939,578	939,578	939,578	939,578
Adj. R^2	0.134	0.134	0.134	0.134

Table A5: Customer Characteristics and 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)*. In Panel A, *High MTB* is a binary variable indicating customers with above-median market-to-book ratio at the beginning of the event year. *High ROA* is a binary variable indicating customers with above-median returns on assets at the beginning of the event year. *High GrossMargin* is a binary variable indicating customers with above-median gross margins at the beginning of the event year. *High CustESG* is a binary variable indicating customers with above-median Refinitiv ESG ratings in the event year. In Panel B, columns (1) and (2) of the table use the same sample as in Table 3. *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 columns are estimated using OLS regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

Panel A: Firm Characteristics

Dep. Var. =	1(Trade>0)			
	(1)	(2)	(3)	(4)
Treat×Post×High MTB	0.003			
	(0.060)			
Treat×Post×High ROA		0.038		
		(0.069)		
Treat×Post×High GrossMargin			-0.061	
			(0.062)	
Treat×Post×High CustESG				-0.179**
				(0.077)
Partition Var.×Treat	Yes	Yes	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	156,296	156,296	156,296	104,340
Adj. R^2	0.263	0.263	0.263	0.273

Table A5: Customer Characteristics and Investor E&S Preferences: Robustness Tests on Extensive Margin (continued)

Panel B: Investor E&S Preferences

Dep. Var. =	1(Trade>0)		
	(1)	(2)	(3)
Treat×Post×High IO_ESG	-0.140**		
	(0.061)		
Treat×Post×ESG Proposal		-0.173**	
		(0.079)	
Treat×Post×Public Cust			-0.006
			(0.018)
Partition Var.×Treat	Yes	Yes	Yes
Supplier×Year×Cohort FE	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes
Obs.	156,296	67,206	16,823,743
Adj. R^2	0.263	0.295	0.199

Table A6: Cross-sectional Tests: Financial Constraints and Hedging

This table shows the differential effects of the same supplier incident on trade with customers with different degrees of financial constraints and supply chain risk hedging. As in our main tests, these tests are performed using Poisson regressions, and the dependent variable is *Containers*. *High KZindex* is a binary variable that equals one if the customer firm's KZ Index is above the sample median. *High WWindex* is a binary variable that equals one if the customer firm's WW Index is above the sample median. *High Purchase Commitment* is a binary variable that equals one if the customer firm's ratio of purchase obligations to cost of goods sold is greater than the sample median. All columns control for supplier×cohort and customer firm×year×cohort fixed effects. Variable definitions are in Table A1. All columns are estimated using Poisson regressions. All continuous variables are winsorized at the 1% and 99% levels. Standard errors are clustered at the supplier-cohort level and displayed in parentheses. *, **, and *** indicate significance levels of 10%, 5%, and 1%, respectively.

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

Table A7: 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. All columns are estimated using OLS regressions. 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	-0.034			
	(0.024)			
Treat Supp, Private×Post	-0.049***			
	(0.018)			
Treat Supp, Large×Post		0.004		
		(0.018)		
Treat Supp, Small×Post		-0.122***		
		(0.023)		
Treat Supp, High HHI×Post			0.014	
			(0.020)	
Treat Supp, Low HHI×Post			-0.092***	
			(0.019)	
Treat Supp, High Differentiation×Post				-0.038**
				(0.015)
Treat Supp, Low Differentiation×Post				-0.088**
				(0.044)
Pair×Cohort FE	Yes	Yes	Yes	Yes
Firm×Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	939,578	939,578	939,578	939,578
Adj. R^2	0.134	0.134	0.134	0.134

Table A8: Switching to New Suppliers

This table shows the effect of supplier E&S incidents on the number of new suppliers established by treated U.S. importers. We collapse our main sample into a cohort-customer firm-year sample over a $[t-3, t+3]$ years window around the incident year t. As in our main tests, we estimate Poisson regression models where the dependent variable is the number of new suppliers established in a year (columns (1) and (2)), and the number of new countries the U.S. customer sources from in a year (columns (3) and (4)). *Treat Cust* is a binary variable indicating customers with at least one supplier affected by E&S incidents in a cohort. *Post* is a binary variable indicating observations after the incident. All columns control for year-cohort and firm-cohort fixed effects. All columns are estimated using Poisson regressions. 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. =	New suppliers		New supplier Countries	
	(1)	(2)	(3)	(4)
Treat×Post	0.205***	0.206***	0.164***	0.162***
	(0.036)	(0.035)	(0.038)	(0.038)
Size		0.058		0.094
		(0.073)		(0.063)
Leverage		-0.325		-0.018
		(0.256)		(0.199)
R&D		2.682		0.537
		(1.635)		(1.149)
Capx		0.308		0.410
		(0.752)		(0.711)
Cash		-0.079		-0.099
		(0.257)		(0.232)
Firm×Cohort FE	Yes	Yes	Yes	Yes
Year×Cohort FE	Yes	Yes	Yes	Yes
Obs.	30,912	30,886	28,865	28,839
Pseudo R^2	0.780	0.779	0.245	0.245

Table A9: Trade Cuts and Gross Profit Margins

This table shows the effect of trade cuts following supplier incidents on future gross margins. We collapse our main sample into a cohort-customer firm-year sample over a $[t-3, t+3]$ years window around the incident year t. The dependent 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 affected by E&S incidents in a cohort. *Post* is a binary variable indicating observations after the incident. *CutTrade* is a customer-specific indicator that equals one if trade growth between the pre-incident and the post-incident period is below the sample median, and zero otherwise. Trade growth for each customer is computed as the weighted average trade growth across all its suppliers, weighted by the pre-incident trade level. All columns controls for year-cohort and firm-cohort fixed effects. All columns are estimated using OLS regressions. 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	-0.002	-0.006*	-0.008**	-0.009**
	(0.003)	(0.004)	(0.004)	(0.004)
Treat Cust (CutTrade=0)×Post	0.005	0.003	0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Size	0.006**	-0.003	-0.015***	-0.015***
	(0.003)	(0.003)	(0.003)	(0.003)
Leverage	0.009	0.025***	0.037***	0.046***
	(0.008)	(0.009)	(0.014)	(0.013)
R&D	-0.009**	-0.007***	-0.000	0.003
	(0.004)	(0.003)	(0.003)	(0.004)
Capx	0.239***	0.158***	0.136***	0.014
	(0.028)	(0.031)	(0.037)	(0.042)
Cash	0.034***	0.029***	0.018*	0.028**
	(0.010)	(0.008)	(0.011)	(0.012)
Year×Cohort FE	Yes	Yes	Yes	Yes
Firm×Cohort FE	Yes	Yes	Yes	Yes
Obs.	29,187	27,432	24,698	21,830
Adj. R^2	0.922	0.925	0.923	0.923

Table A10: Robustness: 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 window of $[t-3, t+6]$ years around the incident year t. The dependent variable is the supplier's Sustainalytics ES score, defined as the average of the firm's environmental and social scores from Sustainalytics. *Treat* is a binary variable indicating whether the supplier is affected by a incident in year t, and *Post (n)* is a binary variable indicating the n-th year after the incident. For each supplier, we aggregate trade changes between years $t-1$ and $t+1$ across all U.S. customers, and we partition the sample based on distributional cuts of these trade changes. Columns (1) and (2) correspond to firms with and without trade cut, respectively. All columns control for supplier-cohort and year-cohort fixed effects. All columns are estimated using Poisson regressions. In Panel B, we construct a cohort-relationship-year sample over a $[t-3, t+6]$ years window around the incident year t. The dependent variable is *Containers*. *Treat* is a binary variable indicating suppliers affected by incidents. *Post4* is a binary variable indicating observations in the interval $[t+4, t+6]$ after the incident. *CutTrade* is a relationship-specific indicator equal to one if average trade growth from the $[t-3, t-1]$ period to the $[t+1, t+3]$ period falls below 0, and zero otherwise. *Inc_Rating* is a supplier-specific indicator equal to one if the supplier improved its Sustainalytics ES score between year $t-1$ and year $t+3$, and zero otherwise. All columns controls for relationship-cohort and firm-year-cohort fixed effects. All columns are estimated using Poisson regressions. 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 Sustainalytics ES Score	
	ΔTrade < 0	ΔTrade > 0
	(1)	(2)
Treat×Post(0)	-0.023**	-0.054***
	(0.010)	(0.010)
Treat×Post(+1)	-0.028**	-0.053***
	(0.012)	(0.013)
Treat×Post(+2)	-0.029*	-0.055***
	(0.015)	(0.015)
Treat×Post(+3)	-0.026	-0.082***
	(0.020)	(0.018)
Treat×Post(+4)	-0.042	-0.103***
	(0.027)	(0.024)
Treat×Post(+5)	0.015	-0.099***
	(0.036)	(0.030)
Treat×Post(+6)	0.056	-0.069**
	(0.043)	(0.033)
Supplier×Cohort FE	Yes	Yes
Year×Cohort FE	Yes	Yes
Obs.	2,109	2,121
Adj. R^2	0.252	0.226

Table A10: Robustness: Trade Cuts, E&S Improvements, and Trade Reversal (Continued)

Panel B: Trade Reversal

Dep. Var. =	Containers
	where CutTrade=1 is defined if ΔTrade < 0
	(1)
Treat×Post (CutTrade=1,Inc_Rating=1)	1.278***
	(0.384)
Treat×Post (CutTrade=1,Inc_Rating=0)	0.122
	(0.893)
Treat×Post (CutTrade=0,Inc_Rating=1)	0.261
	(0.210)
Treat×Post (CutTrade=0,Inc_Rating=0)	0.463
	(0.365)
Relationship×Cohort FE	Yes
Firm×Cohort×Year FE	Yes
Obs.	62893
Pseudo R^2	0.822