Consumer Responses to the Revelation of Corporate Social Irresponsibility

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Abstract: Using micro-level data of US weekly brand-level sales, we examine end-consumer responses to public revelations of corporate social irresponsibility (CSI). Despite survey evidence that suggests end consumers care about CSI, we find that the vast majority of CSI revelations are not followed by changes in sales. It is only when we narrow our focus to a small number of highly visible CSI events that we find a 5.8% reduction in weekly brand-level sales over the four-week period following the event. This suggests that visibility plays a critical role in reducing end consumers' awareness and integration costs with respect to CSI. While the direct consumer response is limited, it is likely that CSI events carry broader economic consequences beyond direct consumer responses. Consistent with this notion, we find that analysts reduce their long-term forecasts following the revelation of visible CSI events and discuss these issues in earnings conference calls. Overall, our findings highlight the importance of visibility in shaping consumer behavior towards CSI and suggest that the costs of highly-visible CSI events extend beyond immediate changes in end-consumer purchasing behavior.

Keywords: Corporate Social Responsibility (CSR), Environmental, Social, and Governance (ESG); Consumer behavior; Analyst forecasts

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1. Introduction

There is a growing focus among activists and the general public on corporations that profit from social injustices. Although governments and international agencies have introduced regulations targeting specific social issues like forced labor and exploitation, the success of such regulation in reducing corporate human rights abuses has been limited.[1] Recognizing the lack of progress through traditional regulation, advocates for change have suggested that stakeholders could play a significant role in reversing this trend (Hart and Zingales, 2017; De Bettignies and Robinson, 2018). Among the broad array of stakeholders, end consumers could have a substantial impact since their actions (i.e., purchase decisions) directly influence a company's profits. Furthermore, survey evidence indicates that consumers have a preference for socially responsible corporations and are willing to pay more for their products.[2] Yet, to date, there is limited large-sample empirical evidence on the role of end consumers as a stakeholder-disciplining mechanism. In this paper, we examine end-consumers' pro-social preferences by assessing the extent to which they modify their purchasing behavior in response to revelations of corporate social irresponsibility (CSI).

Conceptually, it is unclear what to expect from end consumers. First, they need to become aware of a corporation's social responsibility activities and incidences of misconduct before these can be a factor in their purchasing decisions (e.g., Mohr et al. 2001; Servaes and Tamayo 2013). To this end, media coverage and information dissemination are key elements of visibility and consumer awareness of firms' corporate social activities. Second, end consumers assert in surveys that they value and will pay more for products produced by socially responsible corporations. Yet

[1] For instance, an estimated 50 million people are living in modern slavery in 2021 (28 million of which are in forced labor) representing an increase of 10 million since 2016 (ILO 2022).
[2] See, e.g., Smith and Alcorn (1991), Mohr, Webb, and Harris (2001) Ramesh, Saha, Goswami, and Dahiya (2019), Kronthal-Sacco, Van Holt, Atz, and Whelan (2020), Koh, Burnasheva, and Suh (2022).

it is unclear whether end consumers will react and actually alter their purchasing behavior when doing so is costly to them.[3] Third, even if consumers are aware and willing to pay for corporate social responsibility (CSR), they need to be able to make purchasing decisions in accordance with their beliefs. Each year, an average of 6,000 CSI events are revealed (in addition to the thousands of incidents of environmental and governance misconduct) and it is questionable whether consumers are able to process and integrate this information into the many purchasing decisions they make.[4] Therefore, it remains unclear to what extent individual preferences of end consumers will aggregate into a significant change in purchasing behavior.

To provide large scale empirical evidence of aggregate end-consumer buying decisions following the revelation of CSI, we require data on instances of CSI and on end-consumer purchasing behavior. We obtain data on CSI events from RepRisk and create three different samples that reflect various degrees of visibility (estimated based on media coverage). First, we use a broad sample of 2,439 CSI events that RepRisk classifies as "severe" or "highly severe" (*CSI All RepRisk*). Second, to focus on events with greater potential to change consumer purchasing decisions, we restrict our first sample to incidents that also exhibit negative social sentiment in the news media around their public revelation, computed using RavenPack. This procedure yields our second sample of 402 CSI events (*CSI Neg Sent*). Third, we use RavenPack to further refine our second sample and select only incidents with abnormal media coverage around their public revelation. This procedure yields a third sample of 76 highly visible CSI events (*CSI Neg Sent High Vis*).

[3] There is also a free-rider problem where the consumption choice of an individual has a limited impact on large corporations. The free-rider problem is stronger for end consumers (with little individual purchasing power) than for corporate consumers (Dai et al. 2021; Darendeli et al. 2022).

[4] According to RepRisk, there are over 60,000 severe CSI revelations for firms during our sample period, 2012 to 2019. Also, see Blankespoor et al. (2020) for a review of the disclosure processing cost literature, which highlights the importance of "awareness" and "integration" costs of incorporating information in decision making.

To measure end-consumer purchases, we obtain micro-level data on weekly brand-level sales from Nielsen Retail Scanner Data (RMS). Nielsen collects scanner data provided by approximately 50,000 participating retail stores (e.g., grocery, drug, mass merchandisers) across the US. The high frequency nature and granularity of the data allows us to observe changes in consumers' purchasing decisions in tight windows around the revelation of CSI events and compare the sales of two brands within the same product category and week—ensuring high precision estimates. The intersection of the Nielsen RMS data and RepRisk yields a large panel of 7,961,882 company-brand-week sales observations spanning from 2012 to 2019.

In both the broadest sample of 2,439 severe social issues (*CSI All RepRisk*) and the second sample of 402 severe social issues with negative media sentiment (*CSI Neg Sent*), we fail to find consistent evidence of a significant change in consumer behavior. It is only when we focus on our most narrow sample of 76 CSI incidences with both negative media sentiment and high visibility (*CSI Neg Sent High Vis*) that we detect a 5.8% reduction in weekly brand sales for a four-week period following the event. This is consistent with the extent of visibility of CSI incidences playing a key role in end-consumers' reaction to CSI. In further analyses, we observe that the reduction in sales is driven by a decrease in consumer demand rather than a reduction in supply (e.g., from product recalls). We also find that US consumers place similar weights on domestic and foreign incidents but react more strongly to certain CSI events, such as human rights abuses.

Since the revelation of CSI does not occur randomly, we also employ a matched brand-cohort difference-in-differences research design to better isolate the effect of highly visible CSI events on brand sales. Our objective is to focus on a more comparable sample of treated and control brands and isolate variation in brand sales that is due only to the revelation of CSI and unrelated to factors that affect media coverage, such as past sales and growth (Stabler and Fischer, 2020).

Using a more stringent research design that relies on treatment and control samples that are similar on critical characteristics, including their product category, lagged sales, and sales growth, we find that treatment and control brands have similar trends in sales prior to a CSI event, while the weekly sales of treated brands decrease by 1.6% in the five weeks after the revelation of a CSI event. Although directionally consistent with our main analysis, the magnitude of the estimated effect is lower, consistent with our overall conclusion that changes in end-consumer purchasing decisions are limited for most CSI events.

While the end-consumer reactions to the revelation of CSI we document are relatively modest, we posit that CSI events can carry economic consequences that extend beyond the direct changes in end-consumers' purchasing behavior. For instance, corporations that experience allegations of irresponsible behavior may respond with costly actions to alleviate (i) the immediate or anticipated reduction in sales (e.g., social responsibility commitments) and (ii) possible regulatory action. That is, firms may anticipate the reduction in consumer demand around the revelation of CSI events and take costly actions to mitigate the reputation damage. Moreover, other stakeholders (such as potential employees or supply-chain partners) may also influence managerial actions in response to negative consumer reactions or impose longer-term costs on corporations subject to alleged social misbehavior (e.g., Choi et al. 2023; Dai et al. 2021; Darendeli et al 2022). Such actions by firms and other stakeholders could reduce long-term earnings growth but do not necessarily manifest through large and prolonged reductions in sales. Under this scenario, the modest consumer reactions we document may not represent the full economic impact of CSI on firms.

To shed light on this issue and provide a more holistic set of findings, we assess the long-term consequences of CSI by examining financial analysts' forecast revisions and the extent to which CSI topics are discussed in earnings conference calls following CSI events. We focus on analyst

revisions rather than stock market reactions to CSI events because the discussion of CSI in earnings conference calls allows us to better attribute analysts' reactions to the CSI events, which is particularly important if the revelation of CSI events is timed with non-CSI corporate events.

Consistent with our earlier results, we fail to document any significant change in the frequency with which analysts reduce their forecasts for the broadest sample of severe social issues (*CSI All RepRisk*). However, we find that long-term forecasts of earnings growth are downgraded at a higher frequency in both the sample of severe social issues with negative media sentiment (*CSI Neg Sent*) and the smaller sample of highly visible CSI events with negative media sentiment (*CSI Neg Sent High Vis*). Similarly, we find that the frequency of price target downgrades is higher following *CSI Neg Sent High Vis*. Consistent with the downward revisions being attributable to CSI events, we find that—subsequent to the revelation of CSI events—CSI issues are more frequently discussed in earnings conference calls. Taken together, these findings are consistent with the notion that the revelation of visible CSI events carries broader economic consequences that extend beyond the direct changes in end-consumers' purchasing behavior. The reductions in long-term earnings growth and price targets suggest that analysts anticipate costly actions by firms in response to negative consumer responses or pressure from other stakeholders. Relative to prior studies that tend to link ESG activities to firm value via a higher risk premium (e.g., Hong and Kacperczyk 2009, Bolton and Kacperczyk 2021), we provide some of the first empirical evidence showing ESG activities can impact firm value via a cash flow channel.

Our study contributes to three related streams of literature. First, it contributes to the literature on consumer preferences for CSR. Prior studies document that end consumers assert that they care about corporate social behavior and that they are willing to pay for better social performance (e.g., Smith and Alcorn, 1991; Mohr, et al. 2011; Kronthal-Sacco, et al. 2020; Koh, et al. 2022; Ramesh

et al. 2019). However, much of this evidence is drawn from surveys or experiments where CSR is operationalized by the level of charitable donations towards community projects or pro-social issues. Hence, survey responses may not necessarily equate to changing purchasing decisions when doing so is costly to consumers. Our paper contributes to this literature by quantifying end-consumers' response to CSI using large scale archival data. In particular, our evidence suggests that the actual purchasing decisions of end consumers are largely unaffected by corporate social irresponsibility events, with only a limited number of highly visible incidents having any discernible impact on sales. Moreover, the importance of visibility of CSI incidences in shaping consumer behavior, our analyses incorporating both public and private firms, and our focus on social issues—as opposed to broader spectrum of ESG news (i.e., including environmental and governance issues)—differentiates our study from two concurrent working papers that examine consumers' responses to E&S ratings (Meier et al. 2023) and ESG news (Houston et al. 2023).[5] Our focus on social issues avoids pooling events that are of a different nature, and allows us to quantify consumers' attitudes to social issues, which is timely and relevant to current discourse, where social issues are at the forefront of the public debate. In contrast to these studies, in the context of social issues, consumer responses are limited to a small number of highly visible negative CSI events.

Second, our paper contributes to the broader literature on stakeholder preferences for CSR. Prior work documents responses to increased dissemination of CSR information by institutional

[5] Houston et al. (2023) utilize a similar dataset that tracks consumption of a selected sample of households over time, and examine the effect of all ESG (not just S) events in RepRisk. They find a reduction in sales of affected individual products, relative to unaffected products. Notably they find that salience about climate issues differentially affects consumer responses. While Meier et al. (2023), focus on consumer purchase responses to changes in firms' E&S ratings and find that a one-standard-deviation increase in firms' E&S rating is associated with an increase in annual sales of 9.2% in the following year. In an additional test that is more closely related to our study, they also examine E&S events and find that monthly sales are reduced by approximately 1% for each negative E&S event revealed over the previous six-month period.

investors (e.g., Christensen et al. 2017, Flammer 2013, Heath et al. 2023), retail investors (Hartzmark and Sussman 2019, Li et al. 2023), analysts (Derrien et al. 2022), lenders (e.g., Chava 2014, Barigozzi and Tedeschi 2015, Wang 2022), employees (Choi et al. 2023), and corporate consumers (e.g., She 2021; Dai et al. 2021; Darendeli et al. 2022). Conceptually, some of these documented effects could be caused by changes in consumer purchases and costly firm actions in anticipation of consumer and other stakeholder responses. The increased reduction in the frequency of long-term earnings growth forecasts and price targets that we document is consistent with this notion.

Third, our paper also relates to the emerging literature that examines end-consumer responses to corporate information and disclosures. For example, Asay et al. (2022) fail to document a meaningful consumer purchase response to news about corporate tax avoidance, arguably a form of CSI. Moreover, they validate their null result by providing confirmatory survey evidence that consumers tend not to factor tax issues into their purchasing decisions. While corporate tax avoidance differs from the type of corporate social misconduct we examine (e.g., forced labor), the consistent findings suggest our inference of a limited consumer response is generalizable across many dimensions of CSI. In contrast, there is evidence of significant consumer responses to the average earnings announcement (Noh et al. 2023), indicating end consumers may care more about financial performance than CSR performance.

2. Data and Sample Selection

We obtain consumer purchasing data from Nielsen RMS, CSI events from RepRisk Incidents database, news sentiment and media coverage from RavenPack, equity analyst forecasts and

following from I/B/E/S, earnings conference call transcripts from Capital IQ, and firm characteristics from Compustat.

2.1 Consumer purchasing data

A key aspect of our study is the use of micro-level retail sales data from Nielsen RMS, which collects weekly point-of-sale data from approximately 50,000 participating retail store locations across all US markets.[6] Sales are recorded at the Universal Product Code level (UPC code or barcode) and reported on a weekly basis for each retail location. The first 6-9 digits of the UPC code represent a unique identifier (GCP code) of the company that manufactures the product, as assigned by GS1 (the organization that administers the UPC barcodes in the US). We use GS1 data to map UPC codes to company information. Each unique UPC code is associated with a brand and a product group (e.g., Lay's Original Potato Chips belong to the Frito-Lay brand and are in the "snack" product category).

To construct our main dataset, we begin by collecting all weekly retail point-of-sale data at the UPC level from Nielsen RMS from 2012 to 2019, and aggregate UPC codes at the brand level.[7] Because a brand may have multiple products that fall under more than one category (e.g., Frito-Lay sells both "snacks" and "cookies"), we assign each brand to the product category that accounts for the highest proportion of its annual sales, as reported by Nielsen. Using the unique UPC code brand prefixes, we link the brands to the entity that registered the UPC code prefix with GS1.[8] This process leads to a sample of 38,715,545 brand-week sales observations from 156,020 brands

[6] In terms of coverage, Nielsen RMS covers approximately 52% of the total US drug store sales, 26% of the US grocery store sales, and 21% of the US mass merchandiser sales as of 2017.

[7] A brand (e.g., Frito-Lay) with multiple products (e.g., Lay's Potato Chips, Doritos, Sun Chips) of different quantities (e.g., a single 1 oz. bag of Doritos, an eighteen pack of 1 oz. bags of Doritos, a 13 oz. "Party Size" bag of Doritos) has a unique UPC code for each product and quantity. In our main analysis, we aggregate weekly sales numbers from all these UPC codes under the brand name to generate weekly brand-sales.

[8] For example, all Frito-Lay products would be linked to PepsiCo. However, in a subsequent analysis, for a subset of the sample period (2018-2019) we do not aggregate the data and keep the weekly sales at the more granular level of brand-product-store. Results are discussed in Section 3.4 below and reported in Table 6.

belonging to 26,141 unique companies. From this initial sample, we require at least 40 weeks of non-missing sales data for each brand-year and require brands to have a minimum of $100,000 in annual (calendar year) sales. Finally, we require firms to have at least $10 million in annual sales to avoid issues with small firms dominating the sample. This yields a sample of 9,068,003 brand-week observations, spanning 4,553 unique companies. We detail our sample selection and data screening procedures in Table 1. We use this data to construct our main dependent variable *log_sales,* which we measure as the logarithm of a brand's weekly sales, and a control variable *Size,* which is the logarithm of total annual firm sales over the past year.

2.2. Incidences of corporate social irresponsibility

We obtain CSI incidences from the RepRisk database. RepRisk covers over 200,000 public and private companies from around the world and collects firm-specific news of negative ESG incidents from over 100,000 public sources—e.g., print media, online media, social media, and regulatory filings. RepRisk classifies each news incident as pertaining to environmental (E), social (S), or governance (G). However, incidences can span across multiple classifications. We focus on social (S) incidences pertaining to retail companies—both public and private—reported in English, over our sample period from 2012 to 2019. RepRisk also provides a severity score that captures the harshness of the incident. The severity is judged along three dimensions: (1) what are the consequences of the incident with respect to health and safety, (2) what is the extent of the impact, i.e., individual versus a large group of people or population, and (3) was the incident caused by an accident, by negligence, or intent. We focus on incidents that RepRisk classifies as "severe" or "highly severe" and drop those classified as "low severity". We also require the CSI incident to be classified as "sharp" by RepRisk, which indicates that a clear criticism of the company associated with the CSI incident exists in the underlying incident reported. This

screening procedure provides comfort that a specific firm and/or brand is explicitly mentioned and related to the alleged corporate social misconduct. This yields a broad sample of 2,439 social incidents.

We then refine our initial sample further to isolate more visible and significant CSI events that are more likely to attract consumer attention. We apply two more layers of filtering using data from Ravenpack. First, we retain only the RepRisk social incidents that are accompanied by a negative social sentiment score in the three days surrounding the incident. This screening validates that the event was accompanied by press coverage and that the event generated negative sentiment. This procedure yields a second sample of 402 events. Next, we incorporate measures of consumer attention to further refine this second sample. Specifically, we remove events without abnormally high media coverage (known as *"Buzz"* in Ravenpack)—defined as the number of articles on a given day relative to the average daily number of articles in the preceding 30 days—in the 3-day window surrounding the revelation of the negative social news. This procedure yields a reduced (third) sample of 76 highly visible incidences of CSI.

We merge our incidences of CSI to the Nielsen RMS brand-week data using a fuzzy name matching algorithm and, for a subsample, manually verify that our matches are accurate. The intersection between Nielsen RMS and RepRisk corresponds to a sample of 7,961,882 brand-weeks, spanning 34,483 brands and 4,546 unique companies (private and public). The sample selection procedure is summarized in Table 1.

For all three samples of CSI events discussed above, we aggregate our RepRisk incident data into firm-week intervals and create an indicator equal to one for the week of an incident and the following three weeks (i.e., the indicator is switched on for a four-week period). Specifically, the variable *CSI All RepRisk* is created using the broad sample of 2,439 incidents, the variable *CSI*

Neg Sent is created using our sample of 402 incidents, and the variable *CSI Neg Sent High Vis* is created using our refined sample of 76 incidents. We focus on the four-week period consistent with the time interval used by RepRisk to identify new ESG risk events.[9]

Given that the revelation of social misconduct is sometimes bundled with environment and/or governance events, we also create three indicators (*EG All RepRisk*, *EG Neg Sent*, and *EG Neg Sent High Vis*) using the same procedure we employ to identify CSI events. Those indicators are equal to one if an E or a G incident occurs in the same week as the CSI event, and zero otherwise. We include the EG indicators as control variables in our regression models.

2.3. Descriptive statistics

Table 2 presents descriptive statistics for our variables of interest across the entire sample. In Panel A, we provide summary statistics such as mean, median, and standard deviation. The average weekly brand sales amount to $215,516, and 3.8% of all brand-week observations involve a CSI incident. In Panel B we examine time trends for our three samples of CSI events: *CSI All RepRisk* (2,439 events), *CSI Neg Sent* (402 events), and *CSI Neg Sent High Vis* (76 events). Notably, the social events in our sample do not exhibit a consistent temporal pattern. For instance, in 2014, there were the most CSI revelations—across all three samples—followed by a decrease in 2016, and an increase in 2018 and 2019.

[9] In order to avoid the reporting of duplicate CSI events, RepRisk includes the first instance of a given CSI event/issue over a rolling four-week window. We perform sensitivity tests using differing windows, e.g., two-week or six-week periods, to ensure our inferences are not sensitive to this design choice.

3. Consumers' Response to the Revelation of Corporate Social Irresponsibility

3.1 Average consumer response

Our main empirical analysis focuses on consumers' responses to the revelation of CSI events. We examine weekly sales measured at the brand level and estimate the following ordinary least squares (OLS) regression with fixed effects:

$$log\_sales_{b,t} = \beta_0 + \beta_1 Treatment_{i,t} + \beta_2 X_{b,t} + \delta_b + \theta_{p,t} + \varepsilon \qquad \text{(Eq. 1)}$$

The dependent variable (*log_sales*) measures the logarithm of the dollar amount of sales of brand *b* in week *t* (see Section 2.1). Our main independent variable of interest (*Treatment*) is an indicator set to one for each week during the four-week period after the revelation of a CSI event (including the week of the event). As we discuss in Section 2.2, we construct three versions of *Treatment* that capture different degrees of CSI severity and visibility. Our first version (*CSI All RepRisk*) captures our initial sample of 2,439 unique events of CSI that RepRisk classifies as "severe" or "highly severe." We then refine this sample by retaining only the RepRisk social incidents that are accompanied by a negative social sentiment score from Ravenpack in the three days surrounding the incident. This yields a sample of 402 events and forms the basis for our second treatment (*CSI Neg Sent*). Our third version of treatment further screens out events with limited media attention (*buzz*). This yields a sample of 76 events and forms the basis for our third treatment (*CSI Neg Sent High Vis*). $X_{b,t}$ represents a vector of controls. We control for size (*Size*), measured as the logarithm of a firm's annual sales in the prior year, and indicators for contemporaneous revelation of negative environmental and/or governance misconduct (*EG All Rep Risk*, *EG Neg Sent*, and *EG Neg Sent High Vis*). We include fixed effects for individual brands

(δ_b) and product category by year-week ($\theta_{p,t}$). This fixed-effect structure removes non-time-varying differences across brands and time-varying product trends. Hence, this research design allows us to compare the sales of two brands within the same product category and week (e.g., the sales of Lay's potato chips with sales of Pringles potato chips during the same four-week period), while controlling for unobservable time-invariant differences across brands. The β_1 coefficient captures changes in consumers' preferences related to the revelation of CSI. We cluster standard errors by brand.[10]

We report the results in Table 3. In Column (1), the independent variable of interest is *CSI All RepRisk*, based on the broader sample of severe CSI events. We observe an economically and statistically insignificant association between brand sales and *CSI All RepRisk*. Importantly, this finding is not due to low statistical power. Consistent with Bloom (1995), we compute the minimum detectible effect size (MDES), which measures the magnitude of effect that a given estimator could reliably detect. Our research design can reliably detect a treatment effect on the order of 0.015 or larger, which corresponds to less than 1/100 of one sample standard deviation in sales. Hence, our preliminary evidence indicates that most CSI events are not followed by meaningful changes in brand sales, which suggests that consumers may not be able to process and integrate a large amount of information into the many purchasing decisions they make.

We then examine whether CSI events with more negative sentiment and higher visibility are followed by a meaningful reduction in sales. Specifically, in Column (2), we examine *CSI Neg Sent*, in Column (3) we examine *CSI Neg Sent High Vis*, and in Column (4) we examine all three classifications of events together, enabling us to isolate the incremental effect of high visibility.[11]

[10] Results are robust to clustering standard errors by product category or by firm (see Internet Appendix Section A2).
[11] When we include all three indicators for CSI in the same specification, we modify the variables such that the estimated coefficients capture their incremental effect. Specifically, *CSI All RepRisk* in Column (4) only captures CSI

We find consistent results across these specifications; a statistically significant reduction in sales only following the revelation of CSI events with high visibility. This suggests that end consumers tend to have limited awareness of CSI incidences, and only change their purchasing behavior in response to a small number of highly visible CSI events.

However, while the results become statistically stronger for *CSI Neg Sent High Vis* compared to *CSI Neg Sent* and *CSI All RepRisk*—consistent with *CSI Neg Sent High Vis* capturing events with greater media attention that are likely to be factored in the consumers' purchasing decisions––the magnitude of the effect remains small (5.8% reduction in weekly brand sales) even for those highly visible events.[12] Hence, while survey evidence indicates that end consumers care about CSI issues, the majority of CSI revelations are not followed by a meaningful change in consumer behavior. We also observe a small but statistically insignificant reduction in sales following negative E or G events, which indicates that consumers react more to social issues than issues related to the environment and governance.[13]

3.2 Consumer responses to different types of CSI events

Social responsibility encompasses a broad set of issues and end consumers might react more to issues about certain social incidents than others. For instance, consumers may care more about forced labor issues than issues relating to local community participation. Grouping all social issues together might then obfuscate nuances in what matters to end-consumers' purchasing decisions.

events that are not part of *CSI Neg Sent* or *CSI Neg Sent High Vis*, while *CSI Neg Sent* captures those events that are not captured by *CSI Neg Sent High Vis*.

[12] For robustness, in Section A3 of the Internet Appendix, we also examine a third version of our treatment (*CSI Neg Sent High Vis V2*), which filters on *buzz* that is measured over a longer time window (i.e., 90 days rather than 30 days prior to the revelation of a CSI event). Results are consistent with those reported in Table 3 in that we observe a reduction in sales of 6.1%.

[13] In light of the findings in Noh et al. (2023), which show that consumer foot traffic changes following a firm earnings announcement, we verify that our results are not confounded by the release of firms' earnings around the revelation of a CSI incident. In Internet Appendix Section A4, we replicate our main analysis reported in Table 3 for the sample of private firms only, which do not have public earnings releases and thus are not subject to this concern. We find that our results hold in this sample and our inferences are unchanged.

To gauge which type of corporate misconduct consumers react more strongly to, we examine two categories of CSI. The first category is employee relations, which includes CSI issues related to (1) forced labor, (2) child labor, (3) freedom of association, (4) discrimination in employment, (5) occupational health, and (6) poor employment conditions, as classified by RepRisk. The second category is community relations, which includes CSI issues related to (1) human rights abuses, (2) impact on communities, (3) local participation, and (4) social discrimination (see Section A5 of the Internet Appendix for a detailed breakdown of the RepRisk issue classifications).

We then reproduce the analysis reported in Column (3) of Table 3 for each category. Note that we conduct this analysis on *CSI Neg Sent High Vis* (our most refined sample) given that end consumers, on average, react only to this small subset of CSI events. Similar to Table 3 Column (3), we include both the *EG Neg Sent High Vis* and *Size* controls, as well as fixed effects for brand and product-category by year-week in all regressions. We report the results in Table 4. In column (1), we do not find a statistically significant association between brand sales and CSI events related to employee relations. In contrast, in Column (2) we find a significant negative association between brand sales and CSI events related to community relations, such as human rights abuses: brand sales decrease by 6.8% in the four weeks following the revelation of a CSI event related to community relations. However, this result should be viewed with caution given the high correlation between several issues within RepRisk (see Section A5 in the Internet Appendix). For example, several CSI events are related to both human rights abuses and forced labor.

We also decompose our CSI events into those that took place in the US versus overseas. It is plausible that US end consumers react differently to CSI events that are geographically more proximate and directly impact the US population, to which they might be more sympathetic. Results are reported in Table 4 Columns (3) and (4). The evidence suggests that US consumers

react similarly to CSI events that take place in the US and overseas as the coefficients in the two columns are similar in magnitude and not statistically different from each other.

3.3 Matched sample

The analysis reported thus far includes a large number of brands that do not have a CSI event over the sample period and does not account for the likelihood that the revelation of CSI events is not random. For instance, the media likely targets firms with stronger brands (Stabler and Fischer, 2020). To address this issue, we employ a matched brand-cohort difference-in-differences research design.

We exploit the richness of our data to construct a matched sample of brands that are statistically indistinguishable on important observable characteristics—including their product category, sales in the week prior to the event, and sales growth. Specifically, we select cohorts of treatment and control brands for each week where there is a *CSI Neg Sent High Vis* event, so that we are focusing only on the most visible CSI incidents which are followed by a statistically significant reduction in sales. For each cohort we retain only 5 weeks of pre- and post-period data relative to the incident of CSI. Our matched sets of treated and control brands satisfy the following requirements: (1) The treated brand has experienced a *CSI Neg Sent High Vis* event in a given week and does not have other *CSI Neg Sent High Vis* events in the pre and post period examined; (2) the treated brand is matched with a control brand that (a) is in the same product category as the treated brand; (b) has sales in the week prior to the event within half a standard deviation of treated brands; (c) has lagged sales growth that is within one standard deviation of treated brands; (d) has standard deviation of lagged sales growth that is within one standard deviation of treated brands and, most crucially, (e) is a brand with no *CSI Neg Sent High Vis* events in the pre and post periods examined.[14] Our

[14] When a treated brand has multiple candidate control brands that satisfy the requirements above, we use a maximum of three control brands based on the closest matches in terms of lagged sales.

objective is to isolate variation in brand sales of the same product-category that is solely due to the revealed CSI events with high visibility, and unrelated to firms' past sales or growth.

The two distributions resulting from this procedure are precisely matched in terms of sales in the week prior to the event and on preceding sales growth (i.e., sales trend)—the differences in means between treatment and control brands are statistically insignificant and only -0.005 and -0.003, respectively (untabulated). This matching process results in a sample of 129,063 weekly observations of stacked cohorts of treated and control brands.

Using this matched sample, we estimate a stacked cohort difference-in-differences regression that compares brand sales for treated and control brands, five weeks pre-treatment to five weeks post-treatment using the following OLS regression:

$$log\_sales_{b,t} = \beta_0 + \beta_1 Treat + \beta_2 Post + \beta_3 Treat * Post_{b,t} + \delta_b + \theta_t + \varepsilon \qquad \text{(Eq. 2)}$$

We include both brand-by-cohort fixed effects that controls for any non-time-varying differences across brands in each cohort, and year-week-by-cohort fixed effects to control for common trends in brand sales.[15]

We report the results in Table 5. The estimated average treatment effect is -1.6% (*t*=-3.57), which suggests that consumers reduce their purchases of brands implicated by the revelation of social irresponsibility. However, the effect is lower than the effects we estimate in the unmatched sample, reinforcing the conclusion that the overall effect of CSI on consumer purchases is modest.

In Figure 1, we present dynamic effects, replacing *Treat * Post* with weekly indicators. We find that treatment and control brands have similar pre-treatment trends in their sales. In the aftermath of the revelation of CSI, we observe a steady decline in sales of treated brands during

[15] The brand-cohort fixed effects structure controls for firm size, trend, and product category.

the three weeks following the CSI event. Notably, the negative trend in sales reverses around the fourth and fifth week following the revelation of CSI. This finding suggests that the negative effect of CSI on consumer purchases is relatively short lived.

Overall, the evidence in Tables 3, 4, and 5 suggest that end consumers have social preferences that influence their spending behavior. However, the response of end consumers to the revelation of CSI events is only detectable for the most visible CSI events and, even for those events, the average effect is quite modest.

3.4 Store-level data and product recalls

In the following sub-sections, we use more disaggregated data to examine whether the reduction in sales we document is consistent with a demand contraction (i.e., end consumers change their purchasing behavior following the revelation of CSI) or with a reduction in product supply due to product recalls (i.e., firms recall their products in response to the revelation of a CSI event if those products contain dangerous or unethically-sourced inputs).

3.4.1 Separating quantity and price responses

Our results, thus far, suggest a reduction in sales to end consumers following the revelation of highly visible CSI events. However, it is unclear whether this is driven by a reduction in quantity sold (demand effect). In order to shed light on this question, we use a sub-sample of our data with higher granularity. Specifically, we use two years of data (from 2018-2019) that is disaggregated at the brand-product-store-week level (e.g., quantity of Lay's potato chips sold at a given store *s*, in week *t*), which provides both quantity and price information. We estimate the following OLS regression model with fixed effects:[16]

[16] For those two years our sample comprises 75,426,657 observations.

$$Y_{b,p,s,t} = \beta_0 + \beta_1 Treatment_{i,t} + \beta_2 X_{b,t} + \delta_{b,p} + \theta_{p,s,t} + \varepsilon \qquad \text{(Eq. 3)}$$

We examine two dependent variables ($Y_{b,p,s,t}$). First, we examine *log_quantity*, defined as the logarithm of the product *p* quantity sold by brand *b*, in the store *s*, and in week *t*. Second, similar to our main analysis (in section 3.1) we examine *log_sales*, defined as the logarithm of sales (in USD) by brand *b*, of product *p*, in store *s*, for week *t*. Our main independent variable of interest (*Treatment*) is an indicator set to one for each week during the four-week period after the revelation of a CSI event (including the week of the event). Consistent with our main analysis, we construct three versions of *Treatment* in order to capture different degrees of CSI severity and visibility (*CSI All RepRisk*, *CSI Neg Sent*, and *CSI Neg Sent High Vis*). We use the same vector of controls used in Table 3 ($X_{b,t}$). We include fixed effects for individual brands-products ($\delta_{b,p}$) and for product category by store by year-week ($\theta_{p,s,t}$). This high dimensional fixed-effect structure removes non-time-varying differences across brand-products (like product quality) and time-varying product trends at the store level. Hence, this research design allows us to compare the sales of two brands within the same product category, store, and week (e.g., the sales of Lay's potato chips with sales of Pringles potato chips at the same store during the same week). The β_1 coefficient captures changes in consumer purchases around the revelation of CSI.

We report the results in Table 6. In Panel A we find no reduction in the quantity sold following the revelation of CSI events for our broad sample (*CSI All RepRisk*). In contrast, we find a small reduction in quantity following CSI events with more negative tone (*CSI NEG Sent*) and an 8.4% reduction in product quantities sold following the revelation of high visible CSI events (*CSI Neg*

Sent High Vis). This finding suggests that our main results are driven by a reduction in the quantity sold, consistent with a reduction in consumer demand.[17]

In Panel B, we examine product sales (*log_sales*)—consistent with our main variable of interest but now measured at the store level—to verify the robustness of our main analysis for this disaggregated sample data. We find that results are qualitatively similar to those reported in Table 3. Specifically, in Column (1), we observe an economically and statistically insignificant association between store-level brand-product sales and *CSI All RepRisk*. In Columns (2) through (4), where we examine whether CSI events with higher visibility are followed by a meaningful reduction in sales, we continue to find consistent results across all specifications. Notably, the magnitude of the observed reduction in sales following our highly visible events (*CSI Neg Sent High Vis*) is 6%, almost identical to the 5.8% that we observe in Table 3 when we examine the entire time-series of sales at a more aggregated level. This suggests that our data aggregation process is unlikely to affect our inferences and corroborates the magnitude of our findings.

3.4.2. Product recalls

A plausible threat to our inferences is that the observed reduction in sales could be driven by a reduction in supply rather than consumers reducing their demand. Specifically, firms may recall their products in response to the revelation of a CSI event (e.g., if products contain dangerous or unethically sourced inputs). Under this scenario, our documented reduction in dollar sales (and quantity) are not necessarily attributable to consumers' pro-social preferences but an artifact of constrained product supply.

To assess the extent of this concern, we conduct an extensive online search for any product recalls that occur around the time when the 76 highly visible CSI events are revealed. We find

[17] In untabulated results, we find no evidence of a reduction in price.

seven instances of recalls. We then conduct two tests to mitigate the threat to our inferences and report these results in the Internet Appendix Section A6. First, in Table IA 5 Panel A, we replicate our main analysis in Table 3 but exclude all of the brand-week observations that fall in the calendar year in which a company recalls its products (reducing our sample by 26,888 brand-week observations). We find our results are qualitatively unchanged when we omit product recalls. Second, in Table IA 5 Panel B, we replicate the analysis reported in Table 6 Panel A—using the disaggregated store-level data—and exclude observations in the four-week period following the revelation of a CSI event for which a company also recalls its products. Notably, we observe only two recalls and our sample is reduced by only 2,662 observations (relative to Table 6 Panel A). We find results are unchanged. Overall, these findings alleviate the concern that our results are driven by reductions in supply and support our inference that end-consumers pro-social preferences drive the observed reduction in sales following the revelation of CSI events.

4. Assessing Long-term Consequences Using Analyst Reactions

The modest responses by end consumers to the revelation of CSI events cast doubt on whether end-consumers' behavior represents a viable mechanism for creating broad social change. However, CSI events may carry economic consequences that extend beyond the direct changes in end-consumers' purchasing behavior. For example, firms might take costly actions to alleviate (i) the immediate or anticipated reduction in sales (e.g., increased investment in CSR activities, marketing campaigns, and community and employee outreach) and (ii) possible regulatory action. Such actions could reduce long-term earnings growth but do not necessarily manifest through large and prolonged reductions in sales. Under this scenario, the modest consumer reactions we document may not represent the full economic impact of CSI on firms. Assuming financial analysts

understand the full implications of the revelation of CSI events, we can use their responses to gauge whether there are broader consequences that extend beyond end-consumers' purchasing behavior.[18]

Specifically, we assess long-term firm-level implications of CSI events by examining (i) reductions in analysts' forecasts of long-term earnings and price targets, and (ii) discussions of CSI events in earnings conference calls for the sample of publicly listed firms within our dataset.[19] We focus on analyst outcomes rather than market returns because we are better able to attribute changes in analyst outcomes to the revelation of CSI events through the examination of earnings conference call transcripts. This is especially important if the revelation of CSI events is timed with non-CSI corporate events (e.g., earnings announcements).

4.1 Determinants of revealed corporate social irresponsibility

As a first step, we examine the characteristics of publicly listed firms with revealed CSI events relative to those without, and differences in characteristics of CSI firms across our different levels of treatment (i.e., *CSI All RepRisk*, *CSI Neg Sent*, and *CSI Neg Sent High Vis*). The goal of this analysis is to gain a better understanding of the types of public firms for which CSI is revealed. This descriptive evidence will also form a basis for the control variables in the subsequent empirical analyses in sections 4.2 and 4.3.

We obtain analyst forecasts data from I/B/E/S and construct a panel of firm-year-quarter observations. We combine this dataset with our sample of public companies that experienced at least one CSI event, and then restrict this sample to firm-year-quarters with active analysts—i.e.,

[18] We note a concurrent working paper, Derrien et al. (2022) also examines changes in analyst forecasts to RepRisk E, S and G events (most firms have several ESG events each year). Our evidence differs from Derrien et al. in terms of our sole focus on social (S) events and our finding that highlights the importance of visibility, such that only a limited number of visible social events are associated with significant long-term analyst revisions .

[19] We limit these analyses to publicly listed firms because equity analysts do not cover private firms.

firm-year-quarters with at least one forecast revision in the calendar quarter when the CSI event is revealed. This yields a final sample of 913 firm-year-quarter observations. Across our different levels of treatment, we note that 325 firm-year-quarters have at least one revelation of a *CSI All RepRisk* event, while 83 firm-year-quarters contain at least one *CSI Neg Sent* event, and 23 firm-year-quarters contain at least one *CSI Neg Sent High Vis* event.

In terms of potential determinants of CSI revelation, we examine the following firm characteristics: a firm's return on assets (*ROA*) measured as quarterly sales scaled by quarterly total assets; *Sales Growth*, measured as the average quarterly sales growth over the last year; *Market Cap*, measured as the logarithm of price multiplied by the number of shares outstanding at the beginning of the quarter; stock price momentum (*Momentum*), measured over the past three months; the Herfindahl–Hirschman index of quarterly sales (*HHI*), measured using 3-digit SIC code industry classification; *Negative Earnings Surprise*, an indicator equal to one if there is a negative earnings surprise in a quarter; *Earnings Surprise*, measured as the difference between the actual EPS and the mean estimate EPS from the I/B/E/S summary file; consistent with Bochkay et al. (2023) we construct *Material Events*, which measures the issuance of 8-K filings for material firm events; and *Analyst Following*, measured as the number of analysts who issue at least one annual EPS forecast in a fiscal year.

To gauge the relative importance of these firm characteristics, in Table 7 Panel A we report the difference in means between treated and untreated quarters. CSI events are more likely revealed in quarters where the firm has lower performance (*ROA*), higher sales growth (*Sales Growth*), and higher market capitalization (*Market Cap*), which is consistent with Stabler and Fischer (2020) who find that media tend to target larger and high growth firms. CSI events are also more likely to be revealed in more competitive markets, when there are fewer material events regarding a firm,

and when more analysts follow a firm. The mean stock price momentum and earnings surprise do not significantly differ across treated and untreated quarters.

In Table 7 Panel B, we report firm characteristics across the three different levels of treatment. CSI events with higher visibility are more likely revealed in quarters with lower performance (*ROA*), higher market capitalization, and when more analysts follow a firm. Notably, this analysis also reveals that the mean sales growth and industry competition are not significantly different across the different levels of treatment, i.e., these factors do not appear to explain which CSI events are more visible to consumers.

4.2 Earnings growth forecasts and price targets

Next, we examine whether the revelation of CSI has long-term economic consequences for firms. We construct two outcome variables for this analysis: *EPS Downgrades*, which measures the number of times analysts reduce their long-term growth (LTG) forecasts in a calendar quarter, and *Price Target Downgrades*, which captures the number of times analysts reduce their price target in a calendar quarter. In Table 7 Panel C, we report descriptive statistics. *EPS Downgrades* has a mean of 2.19 and a standard deviation of 2.13, whereas *Price Target Downgrades* has a mean of 3.76 and a standard deviation of 4.90. The independent variables of interest are our indicators *CSI All RepRisk*, *CSI Neg Sent*, *CSI Neg Sent High Vis*, which identify the quarters where CSI events are revealed. We observe that 36% of our firm-year-quarter observations have *CSI All RepRisk* events, while 9% have *CSI Neg Sent*, and 2% have *CSI Neg Sent High Vis* events.

In Table 8, we report the results from OLS analyses on the relation between analysts forecast revisions and CSI. Similar to our main analysis, we include each indicator separately (Columns (1) to (3)) and then all together (in Column (4)). In each regression model, we include the determinants of the revelation of CSI events discussed in section 4.1, as well as, firm and year-

quarter fixed effects to control for non-time-varying firm characteristics and common trends in forecasts revisions.

In Panel A of Table 8 we examine downgrades in long-term EPS growth, while in Panel B we examine price target revisions. In Panel A, consistent with our prior analyses, we find a positive association between the frequency with which analysts downgrade their long-term EPS growth forecasts and CSI that is only statistically and economically significant for CSI events with negative tone and high media attention (*CSI Neg Sent* and *CSI Neg Sent High Vis*). For these events, the frequency with which analysts reduce their forecasts increases by 32% and 62%, respectively (relative to the mean number of downgrades).[20] In Panel B, we find a similar pattern for price target downgrades. Specifically, there is a statistically and economically significant positive association between reductions in analysts' price targets only for *CSI Neg Sent High Vis*. The coefficient of 2.426 translates into a 65% increase in the frequency of analyst price target downgrades after CSI events (relative to the mean number of downgrades).[21]

4.3 Discussion of CSI events during earnings conference calls

One concern with our analyses of analyst outcomes is that the revelation of CSI events may be timed with other (unrelated but value relevant) negative news. Hence, to address the concern that financial analysts might be revising their forecasts for reasons other than the observed revelations

[20] To confirm that CSI events also have short-term market implications, we examine short-term EPS revisions (i.e., next quarter) and find the frequency of analysts' downgrades increases following the revelation of a CSI event. This also provides assurance that the highly visible CSI events we examine are economically meaningful. See Internet Appendix Section A7.

[21] Given our focus on downgrades, a potential concern is that the revelation of CSI events could be associated with periods of higher uncertainty, and thus an increase in overall analyst activity (i.e., we observe both more decreases and increases in forecasts). To mitigate this concern, in untabulated analysis, we find no association between the frequency of *upgrades* in long-term EPS growth forecasts, or price target *increases*, following the revelation of CSI.

of CSI, we examine whether incidents of social misconduct are discussed in the company's quarterly conference calls immediately following the revelation of a CSI event.

We obtain conference calls transcript from Capital IQ and construct a sample of firm year-quarter observations. Since firms that have never committed social irresponsibility during our sample period might have different narratives in their conference calls, we focus on a cleaner sample of firms that have at least one CSI event during our sample period. To measure the discussion of CSI events during a conference call, we use a bag of words and bigrams used by RepRisk to identify the ten subcategories of CSI discussed in Section 3.2. We then use ChatGPT to expand this social-words list in order to reduce instances of false negatives and manually verify the full list of words, removing terms that have alternative meanings in financial contexts. The full list of words used in this analysis is reported in the Internet Appendix Section A8. Following this procedure, we create the *Social Discussion Indicator*, which is equal to one if during the quarterly conference call after the revelation of a CSI event at least one of the social words is used, and zero otherwise.

In Table 9, we report results for OLS regressions where we regress the *Social Discussion Indicator* on our indicators for CSI (*CSI All RepRisk*, *CSI Neg Sent*, and *CSI Neg Sent High Vis*), which identify the quarters in which firms' CSI events are revealed. Similar to our main analysis, we include each indicator separately (from Columns (1) to (3)) and then all together (in Column (4)). Our regressions include firm fixed effects to control for time-invariant firm characteristics, and year-quarter fixed effects to control for common trends in conference call discussions.

We find that the estimated association between the occurrence of CSI events and the conference-call discussions of CSI is increasing in magnitude as we go from our larger sample of CSI events to our smallest sample of highly visible CSI events, consistent with the importance of

media attention in this setting. In our intermediate sample of CSI events with negative media sentiment (*CSI Neg Sent*), we find that analysts and firm management are 10 percentage points more likely to discuss CSI events in the conference call after the event is revealed. In the more refined sample (*CSI Neg Sent High Vis*), we find that analysts and firm management are 18 percentage points more likely to discuss CSI events during conference calls.

Importantly, in a falsification test presented in the Internet Appendix Section A9, we find no significant results when analyzing conference calls just *prior* to the revelation of CSI events. This null result offers additional assurance that analysts are indeed reacting to the revelation of CSI.

Taken together, results reported in Tables 8 and 9 provide evidence that analysts reduce their forecasts of long-term EPS growth after the revelation of CSI events. This finding is consistent with the notion that the revelation of visible CSI events carries broader economic consequences beyond the direct changes in end-consumers' purchasing behavior. The reductions in long-term EPS growth and price target downgrades suggest that analysts anticipate costly actions by firms in response to negative consumer responses or pressure from other stakeholders. Relative to prior studies that tend to link ESG activities to firm value via a higher risk premium (Bolton and Kacperczyk 2021), we provide empirical evidence showing ESG activities can impact firm value via a cash flow effect.

5. Conclusions

Recognizing the lack of other solutions, advocates for social change have increasingly turned to stakeholders as a disciplining mechanism that could incentivize corporations to act in a socially responsible manner (e.g., Hart and Zingales 2017). One stakeholder group that could function as a disciplining mechanism for corporations is end consumers because their behavior has a direct

impact on a company's profits. In surveys, a large proportion of end consumers state that they change their purchasing behavior based on corporate social conduct. Our evidence suggests that the actual response of end consumers to the revelation of CSI events is, on average, economically insignificant, questioning whether end consumers are a sufficient disciplining mechanism that can lead to a significant social change.

We base this conclusion on the findings that only a limited number of highly visible CSI events are followed by detectable reductions in brand-level sales and, even for those events, the estimated effect is modest. However, we do find that analyst responses to visible CSI events are stronger than justified by the immediate end-consumer responses, which suggest that firms take costly actions to alleviate negative consumer responses subsequent to visible CSI events or that other stakeholders may also impose costs on socially irresponsible firms. These stakeholders may include socially responsible investors, employees, corporate consumers, politicians, or regulatory agencies.

Our finding that the actual changes in end-consumer purchasing decisions after the revelation of CSI events are modest appears contradictory to the well-documented survey and experimental evidence that consumers state that they care about CSI and are willing to alter purchasing decisions accordingly. We suggest that the apparent inconsistency could be due to the limited ability of end consumers to make purchasing choices that incorporate the many CSI events that are revealed for extended periods of time; a conjecture that is only testable in a controlled experiment.

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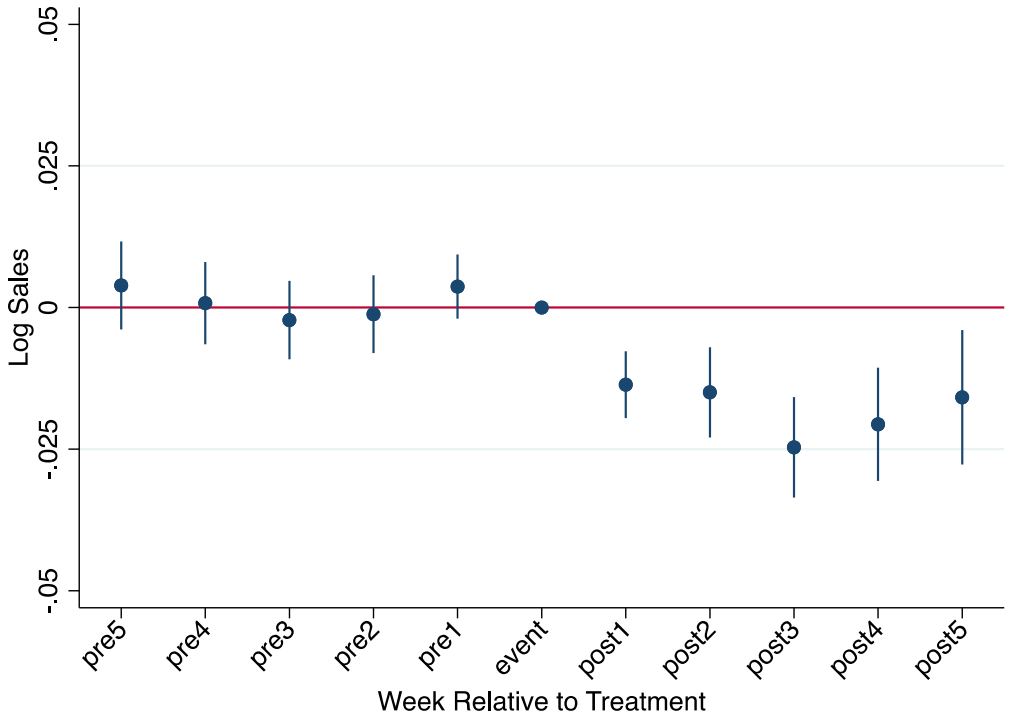
Figure 1: Brand sales and CSI events within Brand-cohorts



This figure plots the coefficients and 95% confidence intervals on *log_sales* resulting from Equation (2). The X-axis represents the week relative to the week in which the *CSI Neg Sent High Vis* event occurs (t=0).

Table 1: Sample selection

Panel A: Sample selection

	No. of brand-week	No. of brands	No. of firms
Nielsen RMS initial sample of brand-week sales from 2012 to 2019	38,715,545	156,020	26,141
Less: brand-year with less than 40 weeks or $100,000 of sales	(27,101,605)	(105,810)	(12,748)
Less: firm-years with less than $10 million of sales	(2,545,937)	(14,270)	(8,840)
Equals: total Nielsen RMS brand-week sales	9,068,003	35,940	4,553
Less: firms not in RepRisk and Ravenpack	(1,106,121)	(1,457)	(7)
Equals: final brand-week sales sample	**7,961,882**	**34,483**	**4,546**

Panel B: RepRisk events selection

	No. of events	No. of brands	No. of firms
Total RepRisk events from 2012 to 2019	167,134	-	54,537
Less: events not "severe" or "highly severe"	(103,108)	-	(30,377)
Less: events/firms unable to be matched to final brand-week sales sample	(61,587)	-	(23,969)
Equals: *CSI All RepRisk* sample	**2,439**	**6,490**	**191**
Less: events without negative social sentiment score in Ravenpack	(2,037)	(3,883)	(151)
Equals: *CSI Neg Sent* sample	**402**	**2,607**	**40**
Less: events without abnormally high media coverage (“Buzz”) in Ravenpack	(326)	(1,046)	(12)
Equals: *CSI Neg Sent High Vis* sample	**76**	**1,561**	**28**

This table reports our sample selection procedures. Panel A details our data screens and sample attrition for our final brand-week sales sample from Nielsen RMS. Panel B details our three samples of CSI events from RepRisk.

Table 2: Sample characteristics

Panel A: Descriptive Statistics

Obs. 7,961,882	Mean (1)	Std Dev (2)	p10 (3)	p50 (4)	p90 (5)
Sales (in $)	215,516	984,185	2,690	25,475	398,349
log_sales	10.228	2.034	7.898	10.145	12.895
CSI All RepRisk	0.038	0.190	0.000	0.000	0.000
CSI Neg Sent	0.006	0.079	0.000	0.000	0.000
CSI Neg Sent High Vis	0.002	0.041	0.000	0.000	0.000
EG All RepRisk	0.048	0.214	0.000	0.000	0.000
EG Neg Sent	0.004	0.065	0.000	0.000	0.000
EG Neg Sent High Vis	0.001	0.036	0.000	0.000	0.000
Size	17.813	2.590	14.584	17.547	21.470

Panel B: Events by Year

	CSI All RepRisk (1)	*CSI Neg Sent* (2)	*CSI Neg Sent High Vis* (3)
2012	269	26	5
2013	321	46	10
2014	451	76	16
2015	314	57	13
2016	219	46	11
2017	235	32	2
2018	347	57	7
2019	283	62	12
Total	2,439	402	76

This table reports summary statistics for our data. In Panel A, we report the mean, the standard deviation, the 10th, 50th, and 90th deciles of the distribution our main variables of interest. In Panel B, we report the annual distribution of the social events we examine. *Sales* is the dollar amount of a brand's weekly sales. *Log_sales*, is the logarithm of a brand's weekly sales. *CSI All RepRisk* is an indicator equal to one during the four-week period following a CSI event from RepRisk (including the week of the event). We retain only events that RepRisk classifies as "severe" or "highly severe." *CSI Neg Sent* is an indicator equal to one that further refines *CSI All RepRisk* by retaining only events that also have a negative social sentiment from RavenPack. *CSI Neg Sent High Vis* is an indicator equal to one that further refines *CSI Neg Sent* by retaining only events that have abnormal media coverage ("*buzz*") centered around the 3 days of the revelation date. Sentiment and "*buzz*" are computed from RavenPack (where abnormal media coverage is the average daily number of articles compared to the average daily number of articles in the preceding 30-day window). *EG All RepRisk* is an indicator equal to one during the four-week period following a negative environmental or governance event from RepRisk (including the week of the event). *EG Neg Sent* is an indicator equal to one that further refines *EG All RepRisk* by retaining only events that also have a negative sentiment from RavenPack. *EG Neg Sent High Vis* is an indicator equal to one that further refines *EG Neg Sent* by retaining only events that have abnormal media coverage ("*buzz*") centered around the 3 days of the revelation date. *Size* is the logarithm of a firm's past year sales.

Table 3: Brand sales and CSI events

Dependent Variable: *log_sales*	(1)	(2)	(3)	(4)
CSI All RepRisk	0.006			0.003
	(0.005)			(0.005)
CSI Neg Sent		-0.027**		-0.014
		(0.011)		(0.011)
CSI Neg Sent High Vis			-0.061***	-0.058***
			(0.019)	(0.019)
Controls:				
EG All RepRisk	-0.007			
	(0.005)			
EG Neg Sent		-0.004		-0.003
		(0.012)		(0.012)
EG Neg Sent High Vis			0.031	
			(0.021)	
Size	0.458***	0.458***	0.458***	0.458***
	(0.009)	(0.009)	(0.009)	(0.009)
Observations	7,961,882	7,961,882	7,961,882	7,961,882
Unique events	2,439	402	76	2,439
Adjusted R-squared	0.833	0.833	0.833	0.833
Brand FE	Yes	Yes	Yes	Yes
Product-by-YearWeek FE	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between brand weekly-sales and CSI events. The dependent variable (*log_sales*) is the logarithm of a brand's weekly sales. In each regression model, we include controls for contemporaneous EG events, *Size*, brand fixed effects, and product category by year-week fixed effects. Variables are described in Table 2. The t-statistics are based on standard errors clustered by brand. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Table 4: Brand sales and CSI events by topic and geographic location

Dependent Variable: *log_sales*				
Topic:	*Employee*	*Community*	*US*	*Non-US*
	(1)	(2)	(3)	(4)
CSI Neg Sent High Vis	-0.026	-0.068***	-0.079*	-0.061***
	(0.021)	(0.020)	(0.041)	(0.020)
F Statistic of Difference in Coefficients	7.24***		0.20	
Observations	7,961,882	7,961,882	7,961,882	7,961,882
Unique events	53	49	29	47
Adjusted R-squared	0.833	0.833	0.833	0.833
Controls	Yes	Yes	Yes	Yes
Brand FE	Yes	Yes	Yes	Yes
Product-by-YearWeek FE	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between brand weekly-sales and *CSI Neg Sent High Vis* events dividend into topics (Employee and Community) and geographic location (US and Non-US). The dependent variable (*log_sales*) is the logarithm of a brand's weekly sales. The independent variables of interest are showed in the column's header. In each regression model, we include controls for *EG Neg Sent High Vis*, *Size*, brand fixed effects, and product category by year-week fixed effects. Variables are described in Table 2. The t-statistics are based on standard errors clustered by brand. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Table 5: Brand sales and CSI events within Brand-cohorts

Dependent Variable: *log_sales*	(1)
*Treatment * Post*	-0.016***
	(0.004)
Observations	129,063
Adjusted R-squared	0.994
Brand-by-Cohort FE	Yes
YearWeek-by-Cohort FE	Yes

This table reports coefficient estimates from stacked cohort difference-in-differences regressions. The dependent variable (*log_sales*) is the logarithm of a brand's weekly sales. *Treatment* is an indicator equal to one for firms that experience a *CSI Neg Sent High Vis* event, and zero otherwise. *Post* is an indicator equal to one in the week of the event and the 5 weeks after the event, and zero in the five weeks prior to the event. Treated brands are matched with up to three control brands within the same product-category, past sales, sales trend, and standard deviation of sales trend. We include brand-by-cohort fixed effects and year-week-by-cohort fixed effects. Statistical significance is based on standard errors clustered by brand. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Table 6: Brand-product-store sales and CSI events

Panel A: Product Quantity

Dependent Variable: *log_quantity*	(1)	(2)	(3)	(4)
CSI All RepRisk	-0.002			-0.003
	(0.005)			(0.007)
CSI Neg Sent		-0.030**		-0.016*
		(0.013)		(0.009)
CSI Neg Sent High Vis			-0.082**	-0.084**
			(0.042)	(0.041)
Observations	75,426,657	75,426,657	75,426,657	75,426,657
Unique events	630	119	19	630
Adjusted R-squared	0.737	0.737	0.737	0.737
Controls	Yes	Yes	Yes	Yes
Brand-by-Product FE	Yes	Yes	Yes	Yes
Product-by-Store-by-YearWeek FE	Yes	Yes	Yes	Yes

Panel B: Sales

Dependent Variable: *log_sales*	(1)	(2)	(3)	(4)
CSI All RepRisk	-0.001			-0.004
	(0.005)			(0.006)
CSI Neg Sent		-0.020*		-0.010
		(0.011)		(0.007)
CSI Neg Sent High Vis			-0.058*	-0.060*
			(0.033)	(0.032)
Observations	75,426,657	75,426,657	75,426,657	75,426,657
Unique events	630	119	19	630
Adjusted R-squared	0.675	0.675	0.675	0.675
Controls	Yes	Yes	Yes	Yes
Brand-by-Product FE	Yes	Yes	Yes	Yes
Product-by-Store-by-YearWeek FE	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between weekly sales at the brand-product-store level and CSI events. In Panel A, the dependent variable is *log_quantity*, defined as the logarithm of the product p quantity sold by brand b, in the store s, and in week t. In Panel B, the dependent variable is *log_sales*, defined as the logarithm of sales (in USD) by brand b, of product p, in store s, for week t. Controls and fixed effects are consistent with Table 3, and the t-statistics are based on standard errors clustered at the brand-product level. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Table 7: Descriptive statistics

Panel A: Covariate difference in means (Treated vs. Untreated Firm-Quarters)

	Untreated	Treated	Difference in Means
Number of Firm-Quarters:	588	325	
	(1)	(2)	(2) - (1)
ROA	0.29	0.24	-0.05***
Sales Growth	0.02	0.03	0.01**
Market Cap	23.4	24.9	1.50***
Momentum	1.03	1.02	-0.01
HHI	3117	2688	-429***
Earnings Surprise	0.09	0.09	0.00
Negative Earnings Surprise	0.25	0.25	-0.03
Material Events	1.86	1.66	-0.20*
Analyst Following	18.1	24.0	5.90***
Average Weekly Sales	28.55	51.57	23.02***
Brand Count	60.7	85.4	24.70***

Panel B: Covariates difference in means by levels of treatment

	CSI All RepRisk	*CSI Neg Sent*	*CSI Neg Sent High Vis*	Differences in Means		
Quarterly Observations:	242	60	23			
	(1)	(2)	(3)	(2) - (1)	(3) - (2)	(3) - (1)
ROA	0.25	0.19	0.21	-0.06***	0.02	-0.04*
Sales Growth	0.03	0.03	0.04	0.00	0.01	0.01
Market Cap	24.6	25.9	25.6	1.30***	-0.30	1.00***
Momentum	1.02	1.02	0.98	0.00	-0.04	-0.04
HHI	2591	2989	2920	398	-69	329
Earnings Surprise	0.11	0.02	0.07	-0.09	0.05*	-0.04
Negative Earnings Surprise	0.23	0.18	0.22	-0.05	0.04	-0.01
Material Events	1.77	1.15	1.78	-0.65***	0.55	-0.10
Analyst Following	20.9	32.8	34.0	11.9***	1.20	13.1***
Average Weekly Sales	53.95	49.32	32.37	-4.63	-16.95	-21.58**
Brand Count	95.9	55.7	52.3	-40.20***	-3.40	-43.60**

Panel C: Summary statistics - main variables of interest

Quarterly Observations: 893	Mean	Std Dev	p10	p50	p90
	(1)	(2)	(3)	(4)	(5)
EPS Downgrades	2.190	2.130	0.000	2.000	5.000
Price Targets Downgrades	3.760	4.900	0.000	2.000	11.000
CSI All RepRisk	0.360	0.480	0.000	0.000	1.000
CSI Neg Sent	0.090	0.290	0.000	0.000	0.000
CSI Neg Sent High Vis	0.020	0.150	0.000	0.000	0.000

This table reports summary statistics for the data used in the analysis reported in Table 8. In Panel A, we report the difference in means between treated and untreated quarters, whereas in Panel B we report the difference in means across different level of treatment. Finally, in Panel C, we report the mean, the standard deviation, the 10^{th}, 50^{th}, and 90^{th} deciles of the distribution our main variables of interest. *ROA* is measured as quarterly sales scaled by quarterly total assets. *Sales Growth* is measured as the average quarterly sales growth over the last year. *Market Cap* is measured as the logarithm of price multiplied by the number of shares outstanding at the beginning of the quarter. *Momentum* is stock price momentum measured over the past three months. *HHI* is the Herfindahl–Hirschman index of quarterly sales, measured using 3-digit SIC code industry classification. *Earnings Surprise* is measured as the difference between the actual EPS and the mean estimate EPS from the I/B/E/S summary file. *Negative Earnings Surprise* is an indicator equal

to one if there is a negative earnings supply in a quarter, and zero otherwise. *Material Events* measures the issuance of 8-K filings for material firm events. *Analyst Following* is the number of analysts who issue at least one annual EPS forecast in a quarter. *Average Weekly Sales* is the average of a firm's weekly sales across all brands (in millions), calculated for the full calendar year. *Brand Count* is the number of unique brands with weekly sales data in our sample for each firm in the current calendar year.

Table 8: Analysts forecasts downgrades and CSI events

Panel A: Long-Term Growth

Dependent Variable:	EPS Downgrades			
	(1)	(2)	(3)	(4)
CSI All RepRisk	0.261			0.191
	(0.172)			(0.177)
CSI Neg Sent		0.732*		0.698*
		(0.369)		(0.345)
CSI Neg Sent High Vis			0.981**	1.357**
			(0.470)	(0.556)
Controls:				
ROA	1.310	1.419	1.298	1.351
	(0.988)	(1.018)	(1.009)	(0.983)
Sales Growth	−4.562***	−4.357**	−4.499**	−4.375**
	(1.609)	(1.646)	(1.642)	(1.615)
Market Cap	0.532	0.537	0.542	0.505
	(0.352)	(0.351)	(0.353)	(0.353)
Momentum	−1.584**	−1.516**	−1.479**	−1.453**
	(0.624)	(0.637)	(0.607)	(0.616)
HHI	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Negative Earnings Surprise	0.320**	0.336**	0.318**	0.318**
	(0.157)	(0.156)	(0.151)	(0.154)
Earnings Surprise	−0.022	−0.018	−0.017	−0.015
	(0.019)	(0.015)	(0.015)	(0.015)
Material Events	−0.002	−0.002	−0.006	−0.002
	(0.029)	(0.027)	(0.028)	(0.028)
Analyst Following	0.082**	0.081**	0.077**	0.078**
	(0.037)	(0.037)	(0.036)	(0.037)
Observations	893	893	893	893
Adjusted R-squared	0.369	0.372	0.371	0.373
Firm FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes

Table 8: Analysts forecasts downgrades and CSI events (continued)

Panel B: Price Targets

Dependent Variable:	Price Target Downgrades			
	(1)	(2)	(3)	(4)
CSI All RepRisk	−0.065			−0.143
	(0.438)			(0.409)
CSI Neg Sent		0.650		−0.079
		(0.999)		(1.035)
CSI Neg Sent High Vis			2.512**	2.426*
			(0.951)	(1.358)
Controls:				
ROA	−0.788	−0.719	−0.887	−0.866
	(2.756)	(2.713)	(2.689)	(2.675)
Sales Growth	−8.261	−8.076	−8.094	−8.078
	(5.003)	(5.008)	(4.979)	(4.985)
Market Cap	0.109	0.075	0.038	0.055
	(0.562)	(0.573)	(0.545)	(0.532)
Momentum	−4.284**	−4.209**	−3.979**	−3.980**
	(1.842)	(1.822)	(1.763)	(1.756)
HHI	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Negative Earnings Surprise	2.026***	2.029***	1.991***	1.998***
	(0.491)	(0.492)	(0.475)	(0.471)
Earnings Surprise	−0.078*	−0.074**	−0.064**	−0.063**
	(0.039)	(0.035)	(0.031)	(0.030)
Material Events	0.285**	0.288**	0.282**	0.281**
	(0.108)	(0.109)	(0.107)	(0.107)
Analyst Following	0.282***	0.282***	0.271***	0.271***
	(0.082)	(0.082)	(0.078)	(0.078)
Observations	893	893	893	893
Adjusted R-squared	0.345	0.345	0.350	0.348
Firm FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between analysts' forecasts downgrades and CSI events. In Panel A, we examine long-term growth forecasts. The dependent variable (*EPS Downgrades*) measures the quarterly number of EPS forecasts downgrades. In Panel B, we examine price target downgrades. The dependent variable (*Price Target Downgrades*) measures the number of price target downgrades in a quarter. In both panels in Column (1), the independent variable is *CSI All RepRisk,* which is an indicator equal to one during the quarter of a CSI event from RepRisk. We retain only events that RepRisk classifies as "severe" or "highly severe." In Column (2), the independent variable is *CSI Neg Sent,* which is an indicator equal to one during the quarter of *CSI All RepRisk* events that have a negative social sentiment from RavenPack. In Column (3), the independent variable is *CSI Neg Sent High Vis,* which is an indicator equal to one during the quarter of *CSI Neg Sent* events that have high media coverage (*buzz*) from RavenPack (calculated over a 30-day window). Control variables are described in Table 7. All specifications include firm fixed effects and year-quarter fixed effects. The t-statistics are based on standard errors clustered at the firm level. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Table 9: Conference calls discussion of CSI events

Dependent Variable:	Social Discussion Indicator			
	(1)	(2)	(3)	(4)
CSI All RepRisk	0.013			0.000
	(0.024)			(0.023)
CSI Neg Sent		0.104*		0.070
		(0.052)		(0.051)
CSI Neg Sent High Vis			0.158*	0.184**
			(0.090)	(0.090)
Controls:				
ROA	−0.529***	−0.516**	−0.528***	−0.520***
	(0.186)	(0.191)	(0.187)	(0.189)
Sales Growth	1.060	1.089	1.067	1.083
	(0.698)	(0.689)	(0.692)	(0.694)
Market Cap	−0.038	−0.037	−0.035	−0.037
	(0.035)	(0.035)	(0.035)	(0.033)
Momentum	0.034	0.035	0.043	0.042
	(0.052)	(0.053)	(0.053)	(0.052)
HHI	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Negative Earnings Surprise	0.003	0.001	0.003	0.001
	(0.023)	(0.023)	(0.022)	(0.022)
Earnings Surprise	−0.004*	−0.004	−0.004	−0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
Material Events	0.004	0.004	0.003	0.003
	(0.006)	(0.006)	(0.006)	(0.006)
Analyst Following	0.003	0.002	0.002	0.002
	(0.006)	(0.006)	(0.006)	(0.006)
Observations	913	913	913	913
Adjusted R-squared	0.102	0.108	0.108	0.108
Firm FE	Yes	Yes	Yes	Yes
Year*Quarter FE	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between conference calls discussion of CSI events and the occurrence of CSI events in a quarter. The dependent variable (*Social Discussion Indicator*) is an indicator equal to one if social topics identified by RepRisk are discussed during the conference call, and zero otherwise. In column (1), the independent variable is *CSI All RepRisk*, which is an indicator equal to one during the quarter of a CSI event from RepRisk. We retain only events that RepRisk classifies as "severe" or "highly severe." In column (2) the independent variable is *CSI Neg Sent*, which is an indicator equal to one during the quarter of *CSI All RepRisk* events that have a negative social sentiment from RavenPack. In column (3), the independent variable is *CSI Neg Sent High Vis*, which is an indicator equal to one during the quarter of *CSI All RepRisk* events that have a negative social sentiment and high media coverage (*buzz*) from RavenPack (calculated over a 30-day window). All specifications include firm fixed effects and year-quarter fixed effects. The t-statistics are based on standard errors clustered at the firm level. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Internet Appendix

"Consumer Responses to the Revelation of Corporate Social Irresponsibility"

By HANS B. CHRISTENSEN, EMMANUEL T. DE GEORGE, ANTHONY R. JOFFRE, AND DANIELE MACCIOCCHI

Table of Contents:

This appendix provides additional information to supplement the analyses presented in the manuscript:

- **Section A1** provides examples of the news media articles about the corporate social irresponsibility events examined in our analyses.
- **Section A2** presents the results reported in Table 3 of the manuscript with standard errors computed using alternative clustering. See Table IA 1.
- **Section A3** replicates the analysis reported in Table 3 Column 4 of the manuscript using an alternative treatment (*CSI Neg Sent High Vis_v2*), which is based on a longer time-window when computing abnormal news coverage (*buzz*). See Table IA 2.
- **Section A4** replicates the analysis reported in Table 3 of the manuscript using only the sample of private firms. See Table IA 3.
- **Section A5** examines the underlying social topics (as classified by RepRisk) of the CSI events in our sample. See Table IA 4.
- **Section A6** replicates the analysis reported in Tables 3 and 6 of the manuscript excluding observations plausibly related to product recalls. See Table IA 5.

- **Section A7** replicates the analysis reported in Table 8 of the manuscript examining downgrades in short-term analysts' EPS forecasts. See Table IA 6.
- **Section A8** presents the bag of words and bigrams used in the textual analysis of conference calls (Table 9 of the manuscript).
- **Section A9** presents a falsification test of our conference call discussion analysis (reported in Table 9 of the manuscript) using the conference call immediately preceding a CSI event. See Table IA 7.

Section A1. Examples of news media articles

1) Article published on October 24, 2012, about Apple, Inc. being accused of human rights abuses and forced labor. [Source: Macworld]

Foxconn builds products for many vendors, but its mud sticks to Apple

By Michael Kan
Macworld | OCT 24, 2012 4:45 AM PDT

The name Foxconn has become shorthand for the human costs of building the iPhone in China, linking Apple to bad publicity about worker suicides, deaths from a plant explosion and rioting factory workers.

Still, as allegations of abusive labor practices continue to be levelled at Foxconn, Apple has been the main target of complaints about the manufacturer's working conditions.

"It was the main reason for us to target Apple," Chan said, referring to Jobs's comment defending Foxconn. While media and labor groups were looking into the root cause behind the problem, Apple ignored inquiries into the matter, she said.

2) Article published on December 12, 2019, about Starbucks and Nespresso accused of forced labor and human rights abuses. [Source: Reuters]

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Picked by slaves: coffee crisis brews in Brazil

By Fabio Teixeira 16 MIN READ

SLAVE-BLIGHTED

A Thomson Reuters Foundation investigation over six months uncovered extensive slave labor running largely unchecked in Brazil's billion-dollar coffee industry despite years of efforts to clean up the sector - which could now put sales at risk.

Exclusively obtained data, analysis of public records, and dozens of interviews revealed coffee produced by forced labor was stamped slavery-free by top certification schemes and sold at a premium to major brands such as Starbucks and Nespresso.

Labor inspectors said they were hampered by a shortage of staff, money and political will - and fear abuse is rising even though consumer demand for slave-free products is increasing.

3) Article published on November 30, 2016, about Unilever, Neslté, and Procter and Gamble accused of forced and child labor. [Source: Amnesty International]

4) Article published on March 12, 2018, about Microsoft accused of gender discrimination. [Source: Reuters]

Section A2. Different clustering for Table 3

Table IA 1: Brand sales and CSI events, different clustering

Dependent Variable: *log_sales*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CSI All RepRisk	0.006			0.003	0.006			0.003
	(0.011)			(0.010)	(0.011)			(0.009)
CSI Neg Sent		-0.027		-0.014		-0.027**		-0.014
		(0.017)		(0.019)		(0.012)		(0.013)
CSI Neg Sent High Vis			-0.061**	-0.058**			-0.061***	-0.058***
			(0.028)	(0.026)			(0.022)	(0.021)
Controls:								
EG All RepRisk	-0.007				-0.007			
	(0.007)				(0.008)			
EG Neg Sent		-0.004		-0.003		-0.004		-0.003
		(0.013)		(0.013)		(0.017)		(0.016)
EG Neg Sent High Vis			0.031				0.031	
			(0.021)				(0.028)	
Size	0.458***	0.458***	0.458***	0.458***	0.458***	0.458***	0.458***	0.458***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.014)	(0.014)	(0.014)	(0.014)
SE clustered by:	Firm	Firm	Firm	Firm	Product category	Product category	Product category	Product category
Observations	7,961,882	7,961,882	7,961,882	7,961,882	7,961,882	7,961,882	7,961,882	7,961,882
Unique events	2,439	402	76	2,439	2,439	402	76	2,439
Adjusted R-squared	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833
Brand FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product-by-YearWeek FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between weekly brand sales and CSI events. Variables are described in Table 2 in the manuscript. In Columns (1)-(4), the t-statistics are based on standard errors clustered by firm. In Columns (5)-(8), the t-statistics are based on standard errors clustered by product category. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Section A3. Different news media buzz

Table IA 2: Brand sales and high visibility CSI events

Dependent Variable: *log_sales*	
	(1)
CSI All RepRisk	0.003
	(0.005)
CSI Neg Sent	-0.018
	(0.011)
CSI Neg Sent High Vis_v2	-0.061***
	(0.022)
Controls:	
EG Neg Sent	-0.002
	(0.012)
Size	0.458***
	(0.009)
Observations	7,961,882
Unique events	2,439
Adjusted R-squared	0.833
Brand FE	Yes
Product-by-YearWeek FE	Yes

This table reports coefficient estimates from OLS regressions examining the relation between weekly brand sales and CSI events with high visibility. The dependent variable (*log_sales*) is the logarithm of a brand's weekly sales. The independent variable of interest is *CSI Neg Sent High Vis_v2*, which is an indicator equal to one during the week of a CSI event from RepRisk and the next three weeks. We retain only events that RepRisk classifies as "severe" or "highly severe," and events that have a negative social sentiment and high media coverage (buzz) from RavenPack, calculated over a 90-day window (rather than using a 30-day window as do in the rest of the analysis). All variables are described in Table 2 in the manuscript. The t-statistics are based on standard errors clustered by brand. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Section A4. CSI Events in Private Firms

Table IA 3: Brand sales and CSI events in private firms

Dependent Variable: *log_sales*				
	(1)	(2)	(3)	(4)
CSI All RepRisk	0.007			0.004
	(0.006)			(0.006)
CSI Neg Sent		-0.033**		-0.028**
		(0.013)		(0.013)
CSI Neg Sent High Vis			-0.036*	-0.034*
			(0.020)	(0.020)
Observations	6,661,653	6,661,653	6,661,653	6,661,653
Adjusted R-squared	0.830	0.830	0.830	0.830
Controls	Yes	Yes	Yes	Yes
Brand FE	Yes	Yes	Yes	Yes
Product-by-YearWeek FE	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between brand weekly-sales and CSI events in the sample of private firms. The dependent variable (*log_sales*) is the logarithm of a brand's weekly sales. In each regression model, we include controls for contemporaneous EG events, *Size*, brand fixed effects, and product category by year-week fixed effects. Variables are described in Table 2 of the manuscript. The t-statistics are based on standard errors clustered by brand. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Section A5. Underlying Social Topics of CSI Events

Table IA 4: CSI events by topic

Panel A: Social Issue Count

	Child labor	*Employee Discrimination*	*Forced Labor*	*Human Rights Abuses*	*Impacts on Communities*	*Local Participation Issues*	*Occupational Health*	*Poor Employment Conditions*	*Social Discrimination*
CSI All RepRisk 2,439 Unique Events	549	183	498	1,119	1,302	221	645	817	20
CSI Neg Sent 402 Unique Events	109	36	98	223	152	34	141	165	4
CSI Neg Sent High Vis 76 Unique Events	20	7	12	31	23	1	28	38	1

Panel B: Correlation Matrices

Treatment: *CSI All RepRisk*

Observations: 2,439	*Child labor*	*Employee Discrimination*	*Forced Labor*	*Human Rights Abuses*	*Impacts on Communities*	*Local Participation Issues*	*Occupational Health*	*Poor Employment Conditions*	*Social Discrimination*
Child labor	1.00								
Employee Discrimination	0.01	1.00							
Forced Labor	**0.54**	**0.07**	1.00						
Human Rights Abuses	**0.48**	0.03	**0.48**	1.00					
Impacts on Communities	**-0.31**	**-0.13**	**-0.30**	**-0.31**	1.00				
Local Participation Issues	**-0.15**	0.01	**-0.09**	**0.05**	**0.23**	1.00			
Occupational Health	**0.23**	**0.20**	**0.17**	**0.08**	**-0.42**	**-0.14**	1.00		
Poor Employment Conditions	**0.20**	**0.31**	**0.24**	**0.20**	**-0.52**	**-0.14**	**0.39**	1.00	
Social Discrimination	**-0.04**	0.03	**-0.03**	**0.03**	-0.01	0.02	**-0.03**	-0.02	1.00

Treatment: *CSI Neg Sent*

Observations: 402	*Child labor*	*Employee Discrimination*	*Forced Labor*	*Human Rights Abuses*	*Impacts on Communities*	*Local Participation Issues*	*Occupational Health*	*Poor Employment Conditions*	*Social Discrimination*
Child labor	1								
Employee Discrimination	-0.03	1							
Forced Labor	**0.51**	0.05	1						
Human Rights Abuses	**0.46**	**-0.12**	**0.47**	1					
Impacts on Communities	**-0.38**	**-0.12**	**-0.34**	**-0.27**	1				
Local Participation Issues	**-0.19**	0.00	**-0.11**	0.07	**0.35**	1			
Occupational Health	**0.30**	0.03	**0.26**	**0.14**	**-0.37**	**-0.13**	1		
Poor Employment Conditions	0.05	**0.22**	**0.19**	0.05	**-0.48**	**-0.20**	**0.31**	1	
Social Discrimination	-0.06	-0.03	-0.06	0.04	0.08	**0.24**	-0.07	**-0.08**	1

Treatment: *CSI Neg Sent High Vis*

Observations: 76	*Child labor*	*Employee Discrimination*	*Forced Labor*	*Human Rights Abuses*	*Impacts on Communities*	*Local Participation Issues*	*Occupational Health*	*Poor Employment Conditions*	*Social Discrimination*
Child labor	1								
Employee Discrimination	-0.09	1							
Forced Labor	**0.56**	-0.01	1						
Human Rights Abuses	**0.54**	-0.17	**0.45**	1					
Impacts on Communities	**-0.33**	**-0.21**	**-0.29**	**-0.20**	1				
Local Participation Issues	-0.07	-0.04	-0.05	-0.10	0.18	1			
Occupational Health	0.35	-0.05	0.12	**0.31**	**-0.38**	-0.09	1		
Poor Employment Conditions	0.12	0.05	**0.29**	0.19	**-0.49**	-0.12	**0.33**	1	
Social Discrimination	-0.07	-0.04	-0.05	-0.10	-0.08	-0.01	-0.09	-0.12	1

This table reports the count of the individual social topics underlying the CSI events in our sample, as well as the correlations between those topics. Panel A reports the count of CSI events by the individual social topics underlying the CSI events in our sample across our three levels of treatment (*CSI All RepRisk*, *CSI Neg Sent*, and *CSI Neg Sent High Vis*). Panel B reports the Pearson correlation coefficients between the individual social topics. Variables are described in Table 2 of the manuscript. Bolded numbers indicate correlations that are statistically significant at the 10% level.

Section A6. Product Recalls

Table IA 5: Exclusion of product recalls

Panel A: Brand sales and negative social events – no recalls

Dependent Variable: *log_sales*	(1)	(2)	(3)	(4)
CSI All RepRisk	0.006			0.003
	(0.005)			(0.005)
CSI Neg Sent		-0.029**		-0.016
		(0.012)		(0.012)
CSI Neg Sent High Vis			-0.076***	-0.071***
			(0.021)	(0.021)
Observations	7,934,994	7,934,994	7,934,994	7,934,994
Adjusted R-squared	0.833	0.833	0.833	0.833
Controls	Yes	Yes	Yes	Yes
Brand-by-Product FE	Yes	Yes	Yes	Yes
Product-by-Store-by-YearWeek FE	Yes	Yes	Yes	Yes

Panel B: Brand-Product-Store quantity sold and negative social events – no recalls

Dependent Variable: *log_quantity*	(1)	(2)	(3)	(4)
CSI All RepRisk	-0.002			-0.003
	(0.005)			(0.007)
CSI Neg Sent		-0.030**		-0.017*
		(0.013)		(0.009)
CSI Neg Sent High Vis			-0.082*	-0.084**
			(0.042)	(0.041)
Observations	75,423,995	75,423,995	75,423,995	75,423,995
Adjusted R-squared	0.737	0.737	0.737	0.737
Controls	Yes	Yes	Yes	Yes
Brand-by-Product FE	Yes	Yes	Yes	Yes
Product-by-Store-by-YearWeek FE	Yes	Yes	Yes	Yes

Panel A reports coefficient estimates from OLS regressions examining the relation between weekly sales at the brand level and CSI events. We exclude the observations in the calendar year in which a company recalls its products. Controls and fixed effects are consistent with Table 3 in the manuscript, and the t-statistics are based on standard errors clustered at the brand level. Panel B reports coefficient estimates from OLS regressions examining the relation between weekly quantity sold at the brand-product-store level and CSI events. We exclude the observations for the four-week period after a CSI event occurring around the time when a company recall its products. Controls and fixed effects are consistent with Table 6 in the manuscript, and the t-statistics are based on standard errors clustered at the brand-product level. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Section A7. Downgrades in Analysts Short-Term Forecasts

Table IA 6: Analysts Short-Term Forecast Downgrades and negative social events

Dependent Variable:	EPS Downgrades			
	(1)	(2)	(3)	(4)
CSI All RepRisk	−0.011			−0.121
	(0.623)			(0.633)
CSI Neg Sent		0.991		0.376
		(0.846)		(0.910)
CSI Neg Sent High Vis			2.368**	2.489*
			(0.912)	(1.293)
Controls:				
ROA	−4.765	−4.652	−4.854	−4.776
	(3.267)	(3.249)	(3.259)	(3.224)
Sales Growth	−4.897	−4.617	−4.740	−4.618
	(4.949)	(4.975)	(4.934)	(4.983)
Market Cap	−0.326	−0.368	−0.386	−0.384
	(0.808)	(0.815)	(0.839)	(0.835)
Momentum	−3.834	−3.725	−3.550	−3.532
	(2.512)	(2.541)	(2.437)	(2.447)
HHI	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Negative Earnings Surprise	1.484***	1.491***	1.453***	1.465***
	(0.513)	(0.520)	(0.503)	(0.508)
Earnings Surprise	−0.075	−0.069	−0.062	−0.060
	(0.104)	(0.100)	(0.098)	(0.097)
Material Events	0.124	0.127	0.120	0.121
	(0.101)	(0.098)	(0.099)	(0.099)
Analyst Following	0.430***	0.429***	0.420***	0.420***
	(0.105)	(0.106)	(0.103)	(0.103)
Observations	893	893	893	893
Adjusted R-squared	0.426	0.427	0.429	0.428
Firm FE	Yes	Yes	Yes	Yes
Year*Quarter FE	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between analysts' short-term EPS forecasts downgrades and CSI events. The dependent variable (*EPS Downgrades*) measures the quarterly number of short-term (next quarter) EPS forecasts downgrades. Control variables are described in Table 7 of the manuscript. All specifications include firm fixed effects and year-quarter fixed effects. The t-statistics are based on standard errors clustered by firm. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.

Section A8. Bag of words used in textual analysis of conference calls

- **Child labor:** "child labor", "child work", "child employment", "youth labor", "youth work", "youth employment", "underage labor", "underage work", "underage employment", "juvenile labor", "juvenile work", "juvenile employment", "child worker", "youth worker", "underage worker", "juvenile worker", "child prostitution", "child trafficking", "child labour", "child exploitation", "hazardous work", "child slavery", "minimum age", "child soldier", "international labour organization", "unicef", "fair labor standards act")
- **Forced labor:** "forced labor", "forced work", "forced employment", "compulsory labor", "compulsory work", "compulsory employment", "bonded labor", "bonded work", "bonded employment", "debt bondage", "modern slavery", "involuntary labor", "involuntary work", "involuntary employment", "exploitative practices", "forced labour", "forced overtime", "human trafficking", "slave labor", "slavery", "slave trade", "forced prostitution", "forced marriage", "indentured servitude", "peonage", "convict labor", "penal labor", "serfdom", "corvee", "impressment", "chattel slavery", "international labour organization", "international labor rights fund", "fair labor association"
- **Employee discrimination:** "discrimination", "ageism", "racism", "sexism", "pay gap", "ableism", "equal pay", "harassment", "gender bias", "gender inequality", "gender equality", "lgbt", "equal employment", "eeoc", "civil rights act", "gender equality", "gender equity", "glass ceiling", "affirmative action", "equal opportunit", "workplace diversity"
- **Human rights abuses:** "human right", "worker exploitation", "employee exploitation", "staff exploitation", "blood diamond", "conflict mineral", "labor practice", "employment practice", "work practice", "cyberattack", "cybercrime", "labor abuse", "employment abuse", "work abuse", "violence", "privacy violation", "torture", "forced disappearance", "genocide", "crime(s)? against humanity", "conflict diamond", "human trafficking", "police brutality", "prisoner abuse", "refugee abuse", "genocide", "ethnic cleansing", "amnesty international"
- **Impacts on communities:** "community impact", "community effect", "community consequence", "community engagement", "community involvement", "community relation", "community support", "community outreach", "community development", "food speculation", "health impact", "community program", "community project", "community initiative", "community partnership", "community well-being", "community health", "community safety", "community investment", "community improvement", "community revitalization", "local communit", "indigenous communit", "vulnerable communit", "disadvantaged communit", "affected communit", "community consultation", "community collaboration", "community dialogue", "community feedback", "community input", "land grab", "community-based organizations", "ngo partnerships", "non-governmental organizations", "civil society organization", "opioids", "water scarcity"
- **Local participation issues:** "local participation", "community involvement", "community engagement", "local input", "public participation", "stakeholder engagement", "stakeholder involvement", "community consultation", "community relations", "local decision-making", "public input", "local voice", "public consultation", "local empowerment", "community empowerment", "participatory decision-making",

"community participation", "local development", "community development", "local collaboration"

- **Occupational health:** "occupational health", "occupational safety", "workplace health", "workplace safety", "worker health", "worker safety", "employee health", "employee safety", "staff health", "staff safety", "work environment", "hazardous conditions", "health risk", "safety risk", "occupational hazard", "work-related injur", "work-related illness", "asbestos", "occupational diseases", "workplace accidents", "workplace incidents", "workplace injuries", "safety regulations", "safety standard", "health and safety", "osha "
- **Social discrimination:** "social discrimination", "caste discrimination", "ethnic discrimination", "linguistic discrimination", "minority discrimination", "social exclusion", "social marginalization", "marginalized groups", "vulnerable groups", "disadvantaged groups", "social equity", "social equality", "social justice", "social inequality", "equal treatment", "equal right", "social integration", "social cohesion", "racism", "racial inequality", "racial equality"
- **Poor employment conditions:** "poor employment condition", "poor work condition", "unfair labor", "unfair work", "unfair employment", "low wage", "long hours", "precarious work", "secure employment", "temporary work", "temp work", "contract work", "sweatshop", "labor right", "worker right", "employee right", "staff right", "employment right", "labor standard", "work standard", "employment standard", "labor protection", "work protection", "employment protection", "migrant worker", "migrant labor"

Section A9. Falsification Test: Conference Call Discussion of CSI

Table IA 7: Conference calls discussion of negative social events, falsification test

Dependent Variable:	Social Discussion Indicator			
	(1)	(2)	(3)	(4)
CSI All RepRisk $_{t+1}$	−0.006			0.008
	(0.030)			(0.033)
CSI Neg Sent $_{t+1}$		−0.014		−0.029
		(0.044)		(0.036)
CSI Neg Sent High Vis $_{t+1}$			−0.020	−0.022
			(0.077)	(0.078)
Observations	908	908	908	908
Adjusted R-squared	0.102	0.102	0.102	0.100
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year*Quarter FE	Yes	Yes	Yes	Yes

This table reports coefficient estimates from OLS regressions examining the relation between conference calls discussion of CSI events and the occurrence of CSI events. The dependent variable (*Social Discussion Indicator*) is an indicator equal to one if social topics identified by RepRisk are discussed during the conference call, and zero otherwise. The independent variables (CSI events) are measured in the quarter subsequent to the conference call. Specifically, they are indicators equal to one if there is a CSI event from RepRisk in the next quarter, and no such event in the current quarter. The t-statistics are based on standard errors clustered by firm. The asterisks (*, **, and ***) indicate statistical significance at the 10%, 5%, and 1% respectively.