Retail Consumer Responses to Corporate Negative Tax News: Evidence from Impacted Firms and their Competitors

Preetika Joshi
McGill University
preetika.joshi@mcgill.ca

Allison Koester
Georgetown University
allison.koester@georgetown.edu

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Abstract: We provide new evidence on whether firms incur tax-related reputation costs in the form of retail consumer backlash and whether competitor firms benefit from this backlash. Leveraging AI-identified negative corporate tax-related news stories related to 50 consumer products firms and granular brand-week sales data, we find a 5.0% decrease in sales of brands produced by firms subject to a negative tax news story. A stacked cohort difference-in-difference research design that compares affected brands to unaffected competitor brands yields similar inferences. Cross-sectional analyses indicate the sales decline is driven by severe negative tax news stories. We also find a 7.7% increase in consumers' purchases of competitor brands following an affected firm's severe tax news story, indicating a firm's negative tax news benefits its competitors. Our study provides new evidence on both the direct and indirect effects of corporate tax-related reputation costs.

"The single biggest danger here is reputation. We're living in a word of 24-hour news, a world where information is amplified and distributed in seconds and, ***most critically in the case of complicated tax arrangements****, where complex issues are brutally summarized… Public opinion…is powerful."*

– PwC's 16th Annual Global CEO Survey (2013. p.8)

I. INTRODUCTION

This paper investigates the existence and consequences of corporate tax-related reputational costs by examining whether retail consumers adjust their purchasing behavior in response to a firm's negative tax-related news coverage. Legitimacy theory states that organizations continuously try to ensure that they carry out activities in accordance with societal boundaries and norms (Deegan et al. 2002). Firms that violate these norms can experience a loss of stakeholder trust (Grappi et al. 2013). When global CEOs were asked which stakeholder group has a significant influence on their firms' business strategy, 80% selected the "customers and clients" stakeholder group (the most frequently selected group) (PwC 2013). Consumer dissatisfaction can encourage brand switching (Oliver 1980), and 61% of American consumers say that firms not paying their fair share of taxes bothers them "a lot" (Pew 2023). Thus, we investigate whether and the extent to which retail consumers shift their product consumption away from affected firms after a negative tax news story and towards products manufactured by competitors.

There is a large body of survey, experimental, and anecdotal evidence suggesting that corporate executives are concerned about consumers' negative perceptions of corporate strategies. Public company executives rank "potential harm to [their] company reputation" as the second most important disincentive to engage in tax planning (Graham et al. 2014 Figure 1). Experimental evidence reveals that participants have a lower willingness to pay for products sold by aggressive tax planning firms (Hardeck and Hertl 2014; Hardeck et al. 2021). Finally, a large body of anecdotal evidence points to instances where negative tax-related news coverage of major

multinational corporations such as Starbucks and Amazon triggers negative consumer responses (Bergin 2014). Big 4 accounting firm partners note that they are "seeing a huge shift in perception around issues of reputational risk and that's really starting to affect how companies approach tax planning" (Harpaz 2013).

In spite of survey, experimental, and anecdotal evidence that corporate executives are concerned with the reputation-damaging effects of their firms' tax planning activities, large-sample archival research generally fails to find that firms face backlash from various stakeholder groups following negative tax-related news stories (Gallemore et al. 2014; Hanlon and Slemrod 2009; Austin and Wilson 2017; Asay et al. 2024). We re-visit the discrepancy between executives' perceptions of significant consumer backlash and the *lack* of archival evidence that consumer backlash occurs by exploiting a novel dataset of AI-identified negative tax news stories. We join a recent line of research that utilizes newly available technology, datasets, and/or settings to shed new light on previously explored research questions (e.g., Nesbitt et al. 2023 and Asay et al. 2024 in the tax literature; Kim et al. 2024 in the auditing literature).

Prior archival research on corporate tax reputation costs in accounting has been limited to researchers' hand-collected articles using researcher-identified keywords and news sources (e.g., Chen et al. 2019; Lee et al. 2021; Asay et al. 2024). In contrast to this manual approach, the RepRisk dataset harnesses the power of artificial intelligence (AI) and machine learning (ML) to screen more than 150,000 public sources of information to identify and categorize "risk incident" news articles for more than 200,000 public and private firms around the globe (RepRisk 2022). RepRisk has been used in both published and concurrent work in the accounting, finance, and

economics literatures to examine corporate reputation risk determinants and outcomes.[1] Our sample selection criteria identify 50 publicly traded consumer products firms with at least one usable negative tax news story and with weekly retail store product-level sales information in the Neilson Retail Scanner (NRS) dataset. The NRS dataset, which also has been used in both published and concurrent work in the accounting, finance, and economics literatures, contains consumer purchases data (units sold and price per unit sold) at the universal product code (UPC) level. These data are collected at retailers' point-of-sale systems and span the universe of U.S. retail consumer markets.[2] Combining the RepRisk and NRS datasets allows us to examine *weekly* changes in consumers' in-store purchases of specific products in the weeks surrounding a producer firm's negative tax-related news story.

We aggregate product-week sales data to the brand-week level to accommodate consumers' real-world purchasing behavior, as retail consumers are generally loyal to products within a particular brand (rather than a particular UPC) (KPMG 2019). We illustrate our data aggregation using three popular cereal brands – Special K, Frosted Flakes, and Reese's Puffs. The firm Kellogg's manufactures Special K and Frosted Flakes while the firm General Mills manufactures Reese's Puffs. All three cereals are sold in dozens of package sizes and flavors, and each size-flavor combination has a unique UPC. There are 134 unique UPCs associated with the Special K brand, 82 UPCs associated with the Frosted Flakes brand, and 38 UPCs associated with the Reese's Puffs brand in the NRS dataset. We expect consumers who prefer Frosted Flakes cereal to be more loyal to the Frosted Flakes brand irrespective of the UPC associated with individual

[1] Examples of published work include Dai et al. (2021), Raghunandan and Rajgopal (2022), Gantchev et al. (2022), Li and Cuervo-Cazurra (2023), and Duchin et al. (2024). Examples of concurrent work include Glossner (2021), Christensen et al. (2023), Akey et al. (2024), Dupont et al. (2024), and Lin et al. (2024).

[2] Examples of published work include Dichev and Quan (2022) and Asay et al. (2024). Examples of concurrent work include Baker et al. (2023), Christensen et al. (2023), Meier et al. (2023), and Burd and Furgeson (2024).

products under that brand. Thus, we aggregate UPC-week sales information to the brand-week unit of analysis. We require non-missing brand sales information for seven consecutive weeks centered on the negative tax news story week and that a firm have zero negative tax news stories in the three weeks prior to the central week (to ensure a "clean" pre-event period).[3] Our final sample consists of 50 publicly traded consumer products firms and 387 firm-weeks with a tax news story. These 387 firm-weeks contain sales information for 27,801 unique brand-weeks.

Our first hypothesis examines the existence of corporate tax-related reputational cost by considering a change in retail consumers' purchasing behavior of brands owned by firms with negative tax news stories (affected brands). We compare brand sales in the week of and three weeks following a firm's negative tax news story, relative to the three weeks prior to the negative tax news story. Brand fixed effects control for time-invariant consumer demand for and producer/retailer supply of products within a brand, allowing us to use each brand as its own control. Week-year fixed effects control for variation in consumer purchasing behavior at retail stores during a year (e.g., within-year seasonality) and for variation across years (e.g., a decline in consumers' in-person purchasing behavior over time). We find a 5.0% on-average decline in a brand's retail sales following a negative tax news story, relative to a brand's retail sales in the three weeks prior. This result is consistent with consumers decreasing their within-brand purchases in response to a firm's negative tax news coverage. This decline in sales is due to a decrease in the number of units sold, not a decrease in sales price per unit sold. Thus, while consumers decrease their purchases of affected brands in response to a firm's negative tax news stories, the affected brands' price per unit do not change. Our findings are robust to controlling for the presence of a non-tax-related negative news story during the seven-week window, increasing confidence that we

[3] Analyses conducted at the UPC-week level would unnecessarily eliminate observations where a particular product size is not purchased in each of the seven consecutive weeks considered in our tests.

are capturing a *tax*-news specific consumer response. Weekly analyses reveal that the decreases in both sales dollars and units sold occur in the first and second weeks following the firm's negative tax news story. By the third week, sales dollars and units sold are statistically *indistinguishable* from the third week prior to the negative tax news story. Thus, the decline in consumer purchases of brands produced by a firm with a negative tax news is short-lived. The short-lived nature of the consumer response is consistent with recent work on the short-lived customer response to firms' data breaches that reveal customers' personal information (Agarwal et al. 2024) and material climate change events (Lim et al. 2024).

Cross-sectional analyses reveal that a brand's sales dollars and units sold decrease following a negative tax news story *only* when the story is classified as severe by RepRisk. This finding highlights that studies based on news stories that happen to capture less severe corporate tax news will appropriately conclude that consumers do not respond to corporate negative tax news coverage. We also find that the negative response is present for brands with and without names that are similar to the name of the firm featured in the tax news story.

We also test our first hypothesis using unaffected brands produced by firms without a negative tax news article in the seven-week period studied as the control group (a stacked cohort difference-in-difference (DiD) research design). We refer to these brands as competitor brands.[4] We find that affected brands experience a decline in sales (driven by a decline in units purchased) after the negative tax news story, relative to their own sales in the pre-period and relative to competitor brands' sales in both the pre- and post-periods. While these findings provide the same inferences as our primary specification that does not include competitor brands, these results do

[4] Competitor brands (discussed in detail in Section 3) refer to brands within the same consumer goods category (what NRS refers to as a "module) as affected brands and the firm that owns the competitor brand cannot have a negative tax news event in the seven-week window centered on the affected brand's week *t* tax news story.

not shed light on whether consumers simultaneously *increase* their purchase of unaffected brands to satisfy their demand for a specific type of product or simply go without. Our second hypothesis addresses this question.

Specifically, our second hypothesis considers that retail consumers may substitute away from affected brands and towards competitor brands. The psychology literature defines substitutes as multiple means that can achieve the same underlying goal (Kruglanski et al. 2002), with means referring to goods, services, or actions. Given our focus on consumer goods that meet consumers' daily needs (e.g., cereal, shampoo, etc.), there are many brands within each consumer goods category with the potential to meet consumers' basic needs. We expect the consumers who *decrease* their purchases of affected brands to *increase* their purchases of competitor brands as opposed to completely forgoing the purchase of consumer goods necessities. When an affected brand's tax news story is severe, we find an *increase* in consumer purchases of competitor brands. In terms of economic magnitude, our results suggest a 7.7% increase in competitor brand sales dollars after an affected firm experiences a severe negative tax news story, relative to competitor brand sales in the weeks prior. This finding is consistent with a firm's negative tax news stories altering a firm's competitive landscape by benefiting its competitors through brand substitution.

Our findings contribute to the corporate tax reputation literature by providing some of the first evidence that firms indeed face consumer backlash, albeit short-lived, following severe negative corporate tax news stories. Our findings provide insights to firms interested in aligning their tax planning practices with consumer expectations. We also contribute to an emerging literature that examines consumer responses to corporate ESG-related information. Concurrent studies document that subsets of retail consumers care about highly visible corporate socially irresponsible events (Christensen et al. 2023), positive corporate social responsibility ratings

(Meier et al. 2023), and negative ESG-related news (Dube et al. 2023). We extend this literature by examining consumer responses to a specific type of corporate news story that tax accounting researchers have focused their attention on for several decades. Finally, our brand substitution analyses reveal that consumers substitute *towards* brands produced by competitor firms as they move *away from* brands produced by a firm subject to a severe negative tax news story. These findings shed light on the competitive effects of a firm's negative tax news coverage – a previously unexplored aspect of corporate tax reputation costs in the accounting literature.

The paper most similar to ours is Asay et al. (2024), which examines our first hypothesis using a mixed methods approach. Asay et al. (2024) also use the NRS database to examine a change in retail sales units for UPCs produced by six firms subject to 29 negative tax news articles. The study fails to find that retail consumers change their purchasing behavior of products produced by these six firms as a function of negative corporate tax news. We build upon this important study by leveraging data from technological advancements in AI and ML, which generate a dataset with an 8-fold increase in the number of firms (from six to 50) and nearly 13-fold increase in the number of firm-weeks with a negative tax news article (from 29 to 387). This more comprehensive dataset provides researchers with a new opportunity to examine the relation between negative corporate tax news and consumer purchasing behavior for both impacted firms and their competitors. When we limit our analyses to the six firms in the Asay et al. (2024) sample, we also fail to detect a change in consumer purchasing behavior following negative tax news stories. This suggests that our primary finding that firms face tax-related reputational costs from consumers is attributable to our leveraging a dataset with more extensive news coverage than is possible through manual hand-collection efforts. We further distinguish our study from Asay et al. (2024) by our examination of retail consumers' substitution towards brands produced by affected firms' competitors – a research

question that Asay et al. (2024) do not consider. To our knowledge, we are the first to examine the spillover effects of tax-related reputation costs on competitor firms.

Our study is subject to several caveats. First, while the NRS data span the universe of U.S. consumer markets and include weekly sales information for each product sold at 30,000 to 50,000 unique grocery, drug, mass merchandiser, and other store types each year, these sales are in person purchases and generally considered "business-to-consumer." Thus, our findings may not generalize to online sales or to business-to-business sales. Second, it is possible that our findings are affected by a correlated omitted variable. While we ensure that our findings are not driven by non-tax negative news stories that coincide with negative tax news stories and can think of no other corporate news information that would *systematically* coincide with our events of interest, our study (like all archival studies without an exogenous shock) is vulnerable to this possibility.

II. RELATED LITERATURE & HYPOTHESES DEVELOPMENT

2.1 Literature on tax-related reputation costs

An established stream of literature considers how the potential for public perception of corporate tax activities affects corporate reputation, with many papers suggesting that reputation costs are why certain firms do not engage in as much tax avoidance as legally possible (Hanlon and Slemrod 2009; Graham et al. 2014; Gallemore at al. 2014; Asay et al. 2024). This phenomenon is often referred to in literature as the "under sheltering puzzle" (Gallemore at al. 2014). We provide a high-level summary of the key arguments and findings in this literature. Per the Scholes and Wolfson framework, effective tax planning considers all parties, all taxes, and all costs (Scholes and Wolfson 1992). Costs associated with tax planning include both direct costs (e.g., fees paid to internal and external tax planners, legal fees, and penalties/interest) and indirect costs (e.g., managerial time and reputation concerns). The term "tax-related reputation costs" refers to

the negative impact on a company's public image, credibility, and standing in the eyes of various stakeholders (e.g., customers, investors, regulators, and the general public) due to its engagement in tax avoidance practices.

Both survey and anecdotal data suggest that *perceived* tax-related reputational cost is a significant factor that managers consider when making tax planning decisions. Graham et al. (2014) analyze survey responses from 600 corporate tax executives and document that reputational concerns is the *second* most important reason for why firms choose to not adopt a particular tax strategy. Anecdotal evidence also substantiates the presence of tax-related reputational costs. In 2012, consumers in the U.K. boycotted Starbucks stores following revelations that Starbucks had been using various strategies to minimize its corporate tax liabilities in the U.K. Protesters and various stakeholders accused Starbucks of not paying its fair share of taxes at a time when the U.K. was facing significant economic challenges. Starbucks suffered a drop in U.K. sales following these boycotts. The boycott and negative publicity prompted Starbucks to make a voluntary commitment to pay more taxes in the U.K., even if the company was not legally obligated to do so (Bergin 2014). While the Starbucks U.K. boycott is a real-world example of how a company's engagement in tax avoidance practices can trigger a negative response from diverse stakeholders, it is not clear that this single example of one firm in one country at one point in time is generalizable.[5] Survey evidence of 531 U.S. consumers fails to indicate that these consumers are willing to boycott a firm due to negative corporate tax-related news information (Asay et al. 2024).

Prior archival research that examines public firms' tax-related reputation costs can be divided into two streams: a larger stream that seeks to capture how *stakeholders* respond to the revelation of a firm's tax activities, and a smaller stream that considers how *firms* response to the

[5] In fact, archival research using Starbucks' data from annual and quarterly SEC filings fails to document a decline in sales or decrease in net store openings in the UK surrounding this 2012 event (Asay et al. 2024 Online Appendix).

revelation of their tax activities to thwart a negative response by stakeholders. Our paper contributes to the first stream, which examines the existence of tax-related reputation costs by evaluating how different stakeholders (e.g., investors, employees, and consumers) respond to news about a firm's tax practises. On the investor side, Hanlon and Slemrod (2009) find that investors exhibit a small negative response (-0.53 percent of stock price) to the public revelation of the firm's involvement in tax shelter activities. The authors interpret this finding as consistent with tax-related reputation damage outweighing the shareholder wealth benefits of cash tax savings. Gallemore et al. (2014) extend these findings by documenting that this negative effect reverses in 30 days, indicating that investors' negative response to tax sheltering activities is short lived. Dhaliwal et al. (2022) consider investor responses to corporate tax avoidance and find that shareholder perspectives on corporate tax avoidance differ over time as a function of societal norms. More recent work finds that U.S. firms that benefited from preferential advance tax rulings issued by the Luxembourg government exhibit a small *positive* stock price increase (0.24 percent) to the unexpected revelation of these favorable tax rulings (Nesbitt et al. 2023). This finding is interpreted as investors responding favorably to a reduction in these firms' tax uncertainty.

On the employee side, Lee et al. (2019) use Glassdoor ratings data to show that employees' perceptions of their managers and employers are less favorable following news about their employers' tax avoidance activities. On the consumer side, Gallemore et al. (2014) fail to find that a firm's annual revenues decline when the firm is revealed to have engaged in tax shelter activities, and Hoopes et al. (2018) fail to find any impact of the revelation that some Australian firms paid no income tax on consumer perceptions of these firms. Asay et al. (2024) use a mixed methods approach and finds that U.S. consumers do *not* change their purchasing behavior as a function of negative corporate tax information. (We discuss this paper in greater detail in Section 4.3.) In sum,

a discrepancy exists between executive perceptions of tax-related reputation costs and archival evidence in the current academic literature regarding corporate tax-related reputation costs.

2.2 Hypotheses development

Our first research question regarding the existence of corporate tax-related reputational costs is grounded in the legitimacy theory literature, which states that organizations continuously try to ensure that they carry out activities in accordance with societal boundaries and norms (Deegan et al. 2002). In the context of corporate taxation, this theory suggests that corporate executives are striving to understand and adhere to societal views about both the spirit and the letter of the web of international and domestic tax laws their firm may be subject to through its operating, investing, and financing activities. When a company engages in practices that give rise to a negative tax news story, customers may perceive this as a violation of societal norms. Such a perception can result in a loss of trust, harm the company's reputation, and trigger a range of reactions from customers including negative word-of-mouth behavior, complaints, product boycotts, and participation in various forms of protests (Grappi et al. 2013).

Consumer responses to corporate wrongdoings have been extensively studied in the marketing literature, with one stream arguing that punitive actions against companies engaged in wrongdoing are driven by assessments of immorality and negative emotions like anger, contempt, and disgust resulting from these moral judgments (Antonetti and Maklan 2016a; 2016b). At the heart of this literature is the concept of "appraisal of immorality" (Valor et al. 2022). If a company is viewed to be involved in an action that is unequivocally labeled as "immoral" by society, it is easy to connect the appraisal of immorality to the ensuing negative reactions from stakeholders. Based on the above, we state our first hypothesis as follows:

Hypothesis 1: There is a negative relation between a firm's negative tax news stories and retail consumer purchases of the firm's brands.

We note, however, that we may fail to find evidence of a negative relation. There is ambiguity in several aspects of tax law, and societal perspectives on where corporate tax planning stands on the spectrum of legitimacy can change over time (Dhaliwal et al. 2022). Consequently, consumers may not perceive a company's tax-related behavior as "immoral." In addition, the second model in the marketing literature argues that consumers are willing to forgive companies for their unethical behavior and may not engage in punitive behavior against the company (Aktar 2013). Finally, recent research finds that firms pass a portion of the taxes they pay through to customers in the form of higher prices (Jacob et al. 2023; Baker et al. 2023). In other words, consumers bear a large portion of the corporate tax incidence. This line of research suggests that consumers could respond favorably to news articles that discuss firms' tax avoidance and tax planning outcomes, due to consumers' expectation that these activities allow firms to lower their product prices (i.e., there is less tax burden to pass through to customers).

Our second research question considers the competitive environment in which firms operate. If consumers indeed decrease their purchases of brands manufactured by firms with negative tax news coverage as our first hypothesis predicts, then consumers may meet their needs by substituting towards competitor firms' brands. The marketing literature provides insight into understanding retail consumers' preferences for specific brands and these brands' substitutability with other brands (e.g., Aaker 1996; Keller 1993; Erdem and Swait 1998). Hamilton et al. (2014) provides an integrative framework identifying three drivers of consumers' substitution decisions: (1) opportunities (e.g., default purchasing or brand loyalty), (2) externally imposed restrictions (e.g., product discontinuation), and (3) internally imposed restrictions (e.g., consumers' preferences). We exploit the third driver and test whether consumers who place self-imposed restrictions on purchasing affected brands substitute towards competitor brands. This literature

offers the following to distinguish between *substitutes* and *substitution*: "…a consumer might deliberate between two substitutes without being committed to either of them; we describe this process as choice rather than as substitution. When the consumer becomes committed to an alternative, however— whether this commitment is emotional or behavioral—items become imperfect substitutes, and the consumer may engage in substitution" (Hamilton et al. 2014, p.307). Thus, the distinct definitions allow consumers to engage in *substitution* even when there are small differences between brands.

Customer dissatisfaction can encourage brand switching (Oliver 1980), and our first hypothesis predicts that a firm's negative tax news stories will induce customer dissatisfaction with the products and brands produced by the firm subject to the negative tax news story. While switching costs (such as consumer energy required to identify alternative brands) can be an impediment to brand substitution (Bolton et al. 2004), we do not expect switching costs to be large in our retail store setting due to similar "type" brands physically grouped together (e.g., cereal produced by many different firms is physically located in a single aisle within a store). Thus, we expect that as retail consumers substitute away from brands produced by firms with a negative tax news article, these consumers are more likely to substitute with competitor brands rather than completely forgo purchasing a particular type of product (e.g., cereal). This expectation leads to our second and final hypothesis:

Hypothesis 2: There is a positive relation between a firm's negative tax news stories and retail consumer purchases of competitor firms' brands.

The following illustrates our two predictions. Assume that Kellogg has a negative tax news article in a given week. Our first hypothesis predicts that brands produced by Kellogg (e.g., Frosted Flakes) will experience a decline in sales following the negative tax news article, and our second

hypothesis predicts that brands produced by Kellogg's competitors (e.g., Kraft Heinz's Frosted Shredded Wheat) experience an increase in sales following Kellogg's negative tax news article.

III. DATA, SAMPLE SELECTION, AND RESEARCH DESIGN

3.1 Retail consumer purchases data

Retail consumer purchases data are obtained from the Neilson Retail Scanner Dataset (often referred to as the Retail Measurement Services (NRS) dataset) maintained by the James M. Kilts Center for Marketing at the Chicago Booth School of Business.[6] The NRS data are derived from retail store point-of-sale systems spanning the universe of U.S. consumer markets and include weekly sales information for each product sold at 30,000 to 50,000 unique grocery, drug, mass merchandiser, and other store types each year. In terms of retail coverage, NRS provides an average of more than 12 billion retail transactions annually (approximately $220 billion of annual retail sales) from 371 Metropolitan Statistical Areas and 2,500 counties. The transactions represent 53% of all sales in grocery stores, 55% in drug stores, and 32% in mass merchandisers in the U.S. market (Argente et al., 2018). Each product is identified by a globally recognized universal product code (UPC) identifier that tracks a product as it passes through a supply chain and a distribution chain on its way to its ultimate consumer. Products in NRS include food and non-food grocery items, health, and beauty aids, and select general merchandise.

NRS data are first available in 2006 and we end our analyses in December 2019, as to avoid the global disruption to U.S. in-store purchases due to the COVID-19 global pandemic in 2020 and the resulting acceleration of U.S. consumers shifting their purchases towards online vendors

[6] Neilson also collects consumer information through the Nielsen Consumer Panel (NCP) dataset. The NRS data are collected at retailer locations via point-of-sale systems that scan each purchased product's barcode, while the NCP data are collected at the household level via Nielsen-provided scanners consumers use in their homes to record their purchased products' barcode. An advantage of NRS relative to NCP is that NRS captures all sales for a given product-retailer-week irrespective of whether a consumer has self-selected into NCP participation, ensuring that the universe of product sales price and volume at a retailer location and time period are included in the dataset.

(Charm et al. 2020; Gundes et al. 2023). UPCs are classified into 11 mutually exclusive departments, which can be segmented into 118 mutually exclusive groups and further segmented into 1,309 mutually exclusive modules.[7] The data are organized at the week-store-UPC unit of observation, with retailer store names masked to anonymize the data.[8]

3.2 Corporate tax news data

We identify tax news stories using the RepRisk ESG Risk Incidents dataset. RepRisk identifies material ESG risks and violations of international standards with the potential for reputational, compliance, and financial impacts on a company through an "outside-in approach to ESG risks by analyzing information from public source and stakeholders and intentionally excluding company self-disclosures" (RepRisk 2022, p.2). Data are collected through daily AI- and machine-learning-based screenings of more than 150,000 public sources of information.[9] Sources include "print media, online media, social media including Twitter and blogs, government bodies, regulators, think tanks, newsletters, and other online sources. These sources range from the international to the regional, national, and local level" (RepRisk 2022, p.1). These collection efforts have identified more than 200,000 public and private firms around the globe (7% of firms are publicly traded and 24% of firms are headquartered in North America) with at least one risk incident news article beginning in January 2007 (RepRisk 2022). A risk incident appears only once in the dataset unless the risk profile of the incident changes (e.g., there is a development in the issue, the issue appears again in a more influential source, or the issue appears again in public

[7] The 11 departments (in alphabetical order) are (1) Alcoholic Beverages, (2) Dairy, (3) Deli, (4) Dry Grocery, (5) Fresh Produce, (6) Frozen Foods, (7) General Merchandise, (8) Health & Beauty Care, (9) Magnet Data, (10) Non-Food Grocery, and (11) Packaged Meat. The 9th department contains less than 0.001% percent of UPCs.
[8] Retail store names being masked precludes our ability to empirically examine a retailer's negative news incidents.
[9] https://www.reprisk.com/research-insights/resources/methodology. The RepRisk dataset does not include the name of the information source or the geographic location of the news source provider. Note that generative AI, which creates new (and sometimes false) content, was not widely available until November 2022 meaning our study is not impacted by AI-created content (McKinsey 2024).

source information for the same firm in the same country after a six-week period). RepRisk incidents are associated with negative short-window abnormal stock returns, consistent with the dataset capturing material ESG incident information (Gantchev et al. 2022; Akey et al. 2024). RepRisk sorts news articles into 28 mutually exclusive ESG issues: six are environmental ("E"), ten are social ("S"), seven are governance ("G"), and the remaining five are "cross-cutting" issues that span beyond only the E, S, or G classification).[10] Similar to other ESG rating agencies such as MSCI and FTSE Russell,[11] RepRisk classifies taxation as a governance issue. See Appendix A for examples of tax-related articles for a firm in our final sample.

3.3. Sample selection

To identify the firm name associated with each UPC, we first match the NRS data with the GS1 U.S. Data Hub (GS1) data by UPC prefix. GS1 is the organization that issues UPC prefixes to firms that produce products, and the GS1 dataset contains each firm's name, address, and UPC prefix.[12] Appendix B provides the details of this matching process. We identify 45,567 unique firm names associated with 2,464,862 UPCs through this matching process. To examine consumer responses to negative corporate tax-related news stories, we focus on firms that are more likely to be scrutinized by consumers for their tax practices. We therefore limit our sample to U.S. publicly

[10] The 28 issues were selected and defined by RepRisk in accordance with key international standards related to ESG issues and business conduct per the World Bank Group Environmental, Health, and Safety Guidelines, the IFC Performance Standards, the Equator Principles, the OECD Guidelines for Multinational Enterprises, and the ILO Conventions (RepRisk 2022). RepRisk's classification scheme distinguishes between "tax evasion" (defined as "general efforts to not pay taxes by illegal means…this includes, for example, tax fraud, use of tax havens, etc.") and "tax optimization" (defined as "the practice of minimizing tax liability through tax planning, While not illegal, it may be associated with abuse of the law") (RepRisk 2022, p.4). These distinctions are not clear (e.g., operating in a tax haven, which includes countries such as Switzerland and Singapore, is not tax evasion, and abuse of tax law seems more akin to tax evasion than tax optimization. Our review of a sample of incidents in each classification suggests the distinction is unlikely to be discernable to the on-average individual retail consumer. Thus, we do not distinguish between these two classifications in our analyses.

[11] See MSCI's *ESG Ratings Methodology* (June 2023) at https://perma.cc/8MP4-MUHQ and FTSE Russell's *ESG Scores* (Mar 2024) available at https://perma.cc/8B57-9SLS.

[12] Private-label goods are excluded from our analyses because Neilsen masks the UPCs of private-label goods to ensure retailers are not identifiable (https://www.chicagobooth.edu/research/kilts/research-data/nielseniq).

traded firms, as the majority of business journalism is devoted to large public firms (Roush 2011) and research that examines the effects of media scrutiny on firm outcomes focus on large public firms (e.g., Chen et al. 2019; Samuels et al. 2021; Holzman et al. 2023). We match these firms' names with U.S. publicly traded firm names in Compustat using a fuzzy matching algorithm and subsequent manual verification, yielding an NRS-GS1-Compustat matched sample of 316 firms. This sample size is comparable to other studies using an NRS-GS1-Compustat merged sample (e.g., 311 firms in Dichev and Qian (2022), 372 firms in Argente et al. (2018), and 309 firms in Guo (2018). Table 1 Panel A outlines our sample selection process beginning with these 316 firms.

Our next data step matches the 316 firms with the RepRisk data by firm name and address using a fuzzy matching algorithm. We lose 251 firms due to RepRisk not identifying any tax-related news articles that reference these firms, yielding 65 firms at the NRS-GS1-Computat-RepRisk intersection with at least one tax-related news story. We collapse multiple tax stories about a firm within a week into a single firm-week, as to align the RepRisk data with the weekly nature of the consumer purchases data, yielding 725 firm-weeks with at least one corporate tax-related news story.

We next use information from the NRS Product file to aggregate UPC-week sales information to the brand-week level.[13] Our research design utilizes each brand's sales as its own control by focusing on the seven-week window centered on the week of a firm's negative tax news story. This approach requires that a firm not have any negative tax news story in the three weeks prior to the story week (i.e., a "clean" pre-period), which eliminates 253 of the 725 firm-weeks with a tax news story. Within the remaining 472 firm-weeks with a tax news story, the 65 firms have sales information for 31,032 brand-weeks. Requiring brand sales information for seven

[13] For example, the Kellogg's manufactures 5,325 UPCs, of which 5,080 have non-missing brand codes associated with 368 unique brands.

consecutive weeks centered on the negative tax news story in week *t* results in a loss of 15 firms and 85 firm-weeks, yielding a final sample of 50 firms with 387 firm-weeks with a tax news story. See Appendix C for a list of these 50 firms' names. Within this final sample we have retail consumer purchases information for 27,801 brand-weeks. As each tax news story week requires three weeks of pre-news story and three weeks of post-news story data, we expand the brand-weeks by a factor of seven to yield 194,607 brand-weeks included in our first regression. Panel A of Table 1 details this sample selection process, and also shows that we lose 16 observations when singletons are dropped due to the inclusion of brand fixed effects, yielding a final sample of 194,591 brand-weeks used to test our first hypothesis regarding a change in consumer purchasing behavior following a firm's negative tax news story.

Panel B of Table 1 outlines the sample selection process for the "competitor brands" used to test our second hypothesis. We begin with the 27,801 brand-weeks in our H1 sample, which correspond to 4,266 brands within 600 NRS-defined modules. We identify competitor brands as brands belonging to firms with sales information within these 600 modules that do not experience a negative tax news event in the seven-week window centered on a treated brand's tax news story in week *t*. The process of identifying competitor brands is discussed in detail in Section 3.4. Our final sample of competitor brands comprises 223 firms with sales information within 565 modules relating to 43,132 brand-weeks. As we require non-missing sales data in weeks *t*-3 through *t*+3, we expand the brand-weeks by a factor of seven. After losing 3,122 singleton observations due to the inclusion of brand fixed effects, we have a final sample of 298,802 brand-weeks to test our second hypothesis regarding substitution.

3.4 Research design

We test our first hypothesis, which predicts a negative relation between a firm's negative tax news and consumer purchases of the firm's products, using the following equation:

$$\mathbf{Y_{bt} = \alpha + \beta_1 * Post_{jt} + \gamma_b + \lambda_t + \varepsilon_{bt}} \quad \textbf{[1]}$$

Our outcome of interest (Y) is one of three variables that measure different aspects of retail consumer purchases at the brand (subscript *b*) and week (subscript *t*) unit of analysis. The variable $Sales_{bt}$ is the natural log of the sales dollars of brand *b* products sold in week *t*, $Units_{bt}$ is the natural log of the total number of units of brand *b* products sold in week *t*, and $Price_{bt}$ is the natural log of the weighted-average price per unit of brand *b* products sold in week *t*.[14]

We consider a seven-week period centered on a firm's negative tax news story (i.e., the negative tax news story occurs in week *t* and we focus on brand sales in weeks *t*-3 through *t*+3). Our independent variable of interest, $Post_{jt}$, is an indicator set equal to one in weeks *t* through *t*+3 when a negative tax news story about the firm that produces brand *b* (firm *j*) occurs in week *t*, and zero in weeks *t*-3 through *t*-1. Brand fixed effects (γ_b) control for time-invariant consumer demand for, and firm supply of, brand *b* products, as some brands are more popular than others (Christensen et al. 2023). The brand fixed effects allow us to exploit *within-brand* variation in sales, units, and price between the *Post* = 0 and *Post* = 1 time periods, meaning each brand serves as its own control. Week-year fixed effects (λ_t) control for time-series variation in consumer purchasing behavior at retail stores, as consumer purchases vary throughout a year due to seasonality and consumers' in-store purchases have decreased over time during our sample period due to the emergence of online purchasing opportunities (Charm et al. 2020; Gundes et al. 2023). We estimate an OLS regression with standard errors clustered by brand to account for serial correlation of product purchases within a brand. Appendix D provides detailed variable definitions.

[14] While the raw NRS data are at the retailer-week-UPC specific (which provides purchase location), we are unable to exploit location in our study because RepRisk does not include the geographic location of the news source.

Our first hypothesis regarding the relation between a firm's negative tax news coverage and its brand's sales is tested by the *Post* coefficient (β_1). This hypothesis predicts a negative coefficient on *Post*, consistent with consumers decreasing their consumption of brands owned by a firm with a negative tax news story in the week of and weeks following the story, relative to the weeks prior to the story. While Equation [1] uses each brand produced by a firm that has a usable negative tax news article as its own control (a simple pre/post research design), we also test our first hypothesis using competitor brands produced by firms without a negative tax news article in the seven-week period studied as the control group (a difference-in-difference (DiD) research design). As negative tax news stories occur in various weeks during our 10-year sample period, different observations are impacted by an event at different points in times (i.e., a staggered DiD). Recent literature suggests that staggered DiDs can produce biased estimates of the treatment effect (Baker et al. 2022). These papers provide several recommendations to mitigate econometric concerns arising from the use of a staggered DiD, and we follow the stacked cohort DiD recommendation.

To identify the competitor brands of a treated brand, we return to our sample of 316 U.S. publicly traded firms in the NRS-GS1-Compustat matched sample. The NRS Products file reveals that these 316 firms sell products associated with 18,211 unique brands (untabulated). We use the NRS Products file to determine each brand's *primary* module based on sales data at the brand level. Recall that UPCs are classified into 11 mutually exclusive departments, which can be segmented into 118 mutually exclusive groups and further segmented into 1,309 mutually exclusive modules. While there is a one-to-one relation between a UPC and a module, there can be a one-to-many relation between a brand and a module. To illustrate, Kellogg's brand 579916 (description = Kel Frosted Flakes) is only within module 1344 (description = Cereal Ready to Eat),

while Kellogg's brand 579592 (description = Kellogg Keebler Cracker) spans 15 modules (module 1330 (Snacks-Pretzels), 1341 (Crackers-Sandwich), 1354 (Crackers-Graham), 1408 (Graham Crackers & Desser Crum), etc.). We determine a brand's *primary* module by identifying the module that comprises most of the brand's sales dollars (e.g., for Kellogg's brand 579592 (description = Kellogg Keebler Cracker), module 1408 (Graham Crackers & Desser Crum) has the highest sales percentage (48.89%). Thus, we assign all sales information for brand 579592 *only* to module 1408. We lose two of the 316 firms due to the firm's brand(s) not being assigned to a module by NRS or because no module comprises most of a brand's sales (e.g., a brand's sales is split exactly 50/50 between two modules), yielding 314 U.S. publicly traded firms with 18,211 brands assigned to 1,075 modules (untabulated).

Next, we identify competitor brands for the 4,266 brands owned by the 50 firms in our main regression sample ("treated brands"). To be considered a competitor brand for a treated brand, a brand must be within the same module as the treated brand and the firm that owns the competitor brand cannot experience a negative tax news event in the seven-week window centered on a treated brand's tax news story in week *t*.

We illustrate this identification process using Kellogg's and its negative tax news story during the week ending Saturday, March 11, 2017. As previously mentioned, Kellogg's brand 579916 (description = Kel Frosted Flakes) is within only module 1344 (description = Cereal Ready to Eat). In addition to brand 579916, Kellogg's produces products associated with 87 other brands within module 1344 with sales information during the week ending March 11, 2017.[15] Moreover, five other publicly traded firms – General Mills, Kraft Heinz Foods, United Natural Foods, Nestle, and BG Foods – produce products associated with 89 brands within module 1344 with sales

[15] Across the NRS coverage period (2006-2019), Kellogg's has a total of 285 brands within module 1344 and 10 other firms have a total of 340 brands within module 1344, but not all firm-brand pairs have sales information each week.

information this same week. Examples of popular brands within module 1344 by some of these other firms include General Mills brands Cheerios, Lucky Charms, and Cinnamon Crunch, as well as Kraft Heinz Foods brands Cocoa Puffs and Frosted Shredded Wheat. The RepRisk data reveal that both General Mills and United Natural Foods had one or more negative tax news stories during the 7-week event window centered around March 11, 2017. As such, the brands owned by General Mills and United Natural Foods are *not* classified as competitor brands for Kellogg's brands in module 1344 in this seven-week window, while brands owned by the remaining three firms (Kraft Heinz Foods, Nestle, and BG Foods) *are* classified as competitor brands for Kellogg's brands in module 1344 in this seven-week window. The classification of competitor brands for each treated brand therefore varies across brand weeks.

After identifying competitor brands by brand-week, we next create module-week cohorts for each tax news article date. Within each module-week cohort, we include brands produced by firms with a negative tax news story in that week ("treated" observations) and event-specific competitor brands ("control" observations). The event-specific competitor brands are produced by firms that do not have a negative tax news story during the seven-week sample period (i.e., firms that never had a negative tax news story during our entire sample period and firms with a tax news story in another seven-week period). We stack the module-week cohorts into one dataset and estimate the following regression using a stacked cohort DiD:

$$\mathbf{Y_{bt} = \alpha + \beta_1 * Treat_{jt} + \beta_2 * Post_{mt} + \beta_3 * Treat_{jt} * Post_{mt} + \gamma_b + \lambda_{mt} + \varepsilon_{bt}} \qquad \mathbf{[2]}$$

The dependent variables are $Sales_{bt}$, $Units_{bt}$, and $Price_{bt}$. As we again consider a seven-week period, the independent variable $Treat_{jt}$ is an indicator set equal to one if a firm that produces brand b (firm j) has a negative tax news story in weeks t-3 through t+3 (and zero otherwise). As we estimate Equation [2] using both treated brands and competitor brands, the $Post_{mt}$ indicator is

set equal to one in weeks *t* through *t*+3 when a negative tax news story about a firm that produces brand *b* within module *m* occurs in week *t*, and zero in weeks *t*-3 through *t*-1. We include brand fixed effects (γ_b) to control for consumer demand for a particular brand. Because we are comparing treated and competitor brands within a module, we include module-week-year fixed effects (λ_{mt}) to control for time-series variation in consumer purchasing behavior within a module. Our first hypothesis is tested by the interaction term $Treat_{jt}*Post_{mt}$. We predict $\beta_3 < 0$, consistent with consumers decreasing their consumption of brands produced by a firm with a negative tax news story subsequent to the story, relative to brands in the same module produced by firms *without* a negative tax news story.

We test our second hypothesis, which predicts a positive relation between a firm's negative tax news and consumer purchases of competitor brands, using the following equation:

$$\mathbf{Y_{bt} = \alpha + \beta_1 * Post_{mt} + \gamma_b + \lambda_t + \varepsilon_{bt}} \quad \textbf{[3]}$$

Similar to Equation [1], we include brand fixed effects (γ_b) and week-year (λ_t) fixed effects. Distinct from Equations [1] and [2], Equation [3] is estimated using only sales information for firm *j*'s competitor brands centered on firm *j*'s negative tax news story. Thus, our second hypothesis is tested using *only competitor firms'* sales information and $Post_{mt}$ is our variable of interest. We predict a positive coefficient on $Post_{mt}$, consistent with consumers *increasing* their consumption of competitor brands owned by firms *without* a negative tax news story in the week of and weeks following a treatment firm's negative tax news story, relative to the weeks prior to the story.

IV. RESULTS

4.1 Descriptive statistics

Table 2 Panel A presents information on how the 194,591 brand-weeks included in our sample are distributed across time. The earliest years in our sample (2010 through 2012) have the

fewest observations, with a large increase in observations in 2013 and a peak number in 2016. Descriptive statistics for the Equation [1] variables are reported in Table 2 Panel B. The mean sales dollars by brand-week (the unlogged value of *Sales*) is $396,829, with a median of $49,271. The mean (median) number of units sold within each brand-week is 109,413 (9,224), and the mean (median) weighted average price per unit within each brand-week is $7.62 ($4.46). These descriptive statistics are consistent with retails selling a high volume of low-priced goods. We use logged values of the three sales-related variables ($Sales_{bt}$, $Units_{bt}$, and $Price_{bt}$) to reduce the influence of outlier observations on our regression results.

Pearson correlations for the Equation [1] variables are reported in Table 2 Panel C. *Sales* is positively correlated with both *Units* and *Price* ($\rho = 0.96$ and $\rho = 0.17$, respectively). *Units* and *Price* are negatively correlated ($\rho = -0.10$), and the *Post* indicator is slightly negatively correlated with both *Sales* and *Units* ($\rho = -0.01$) and is uncorrelated with *Price*.

4.2 Regression results testing consumer response to negative corporate tax news (H1)

Table 3 present the results from estimating Equation [1] using *Sales*, *Units*, and *Price* as dependent variables. Panel A reveals a negative and highly statistically significant relation between *Post* and *Sales* (column 1). In terms of economic significance, the *Post* coefficient indicates a 5.0% decline in sales dollars (=exp(-0.051)-1) generated by a brand in the weeks following its producer firm being subject to a negative tax-related news story, relative to in the weeks prior to the negative tax news story. We offer a back-of-the-envelope calculation to quantify a 5.0% decline in terms of dollars. Affected brands generate weekly sales of $395,736 in the *Post* = 0 period, suggesting that a 5.0% decline in sales in the *Post* = 1 period equates to $19,787 of foregone weekly sales per brand (= 5% * $395,736). A firm in our sample has on average 27.42 brands (21 brands at the median), suggesting an on-average $542,554 decline in *weekly* sales for affected firms ($415,523

at the median). In annual terms, this weekly number translates into $28.2 million in foregone *annual* sales for a sample firm on average ($21.6 million at the median). The negative relation is driven by a decrease in the total number of units sold, not a decrease in the weighted average price per unit sold (columns 2 and 3, respectively). These findings suggest that while consumers decrease their purchases in response to negative tax news stories regarding a firm producing a particular brand, the affected brands' price per unit do not change. Results are robust to winsorizing the dependent variables (untabulated).

We conduct additional tests to ensure our findings are not attributable to alternative explanations. First, to ensure our results are specific to negative *tax*-related news stories, as opposed to *any* type of negative news story, we re-estimate Equation [1] after controlling for negative non-tax news stories during weeks *t* through *t*+3. The variable *Post Non-Tax*$_{jt}$ is an indicator set equal to one when a negative *non-tax* news story about the firm that produces brand *b* (firm *j*) occurs in any week in weeks *t* through *t*+3, and zero otherwise. The *Post Non-Tax* mean is 0.29 (Table 2 Panel B), and the regression results presented in Table 3 Panel B show that the *Post Non-Tax* coefficient is insignificant in all three columns. Most importantly, the *Post* coefficient in columns 1 and 2 continues to be negative and highly significant and of a similar magnitude as the *Post* coefficients in Table 3 Panel A columns 1 and 2. These findings alleviate the potential concern that our results are attributable to other types of negative news stories in the same period, and confirm that our findings are tax news-specific.

Note that we are not considering the universe of non-tax news stories; we are only capturing (a lack of) consumer response to non-tax news stories within the 7-week period centered on a negative tax news story. Having said that, concurrent work by Christensen et al. (2023) that focuses on the "social" incidents in the RepRisk dataset (a category that is a component of our non-tax

news stories) fails to find evidence of a consumer response to both a broad sample of 2,439 severe social-related incidents and a smaller sample of 402 severe social issues with negative media sentiment. It is only when they focus on the 76 social incidents with both negative media sentiment and high visibility that they detect any decline in consumer purchases. They document a 5.8% reduction in weekly brand sales in the four weeks following these 76 social incidents – a magnitude that is similar to our finding of a 5.0 percent on-average decline in weekly brand sales following negative tax news stories.

Second, it is possible that a negative *non*-tax news story in the pre-period drives brand sales downward in that period, such that our *Post* coefficient is simply capturing a brand's sales returning to their baseline level in the post period. Re-estimating Equation [1] after controlling for the presence of a negative non-tax news story during weeks *t*-3 through *t*-1 yields no change in inferences (untabulated). This alternative explanation cannot be made for a negative *tax* news story in the pre-period, as Table 1 Panel A shows that our sample selection requires a "clean" pre-period (i.e., no negative tax news story in the three weeks prior to week *t*) for our Equation [1] analyses. Finally, Lin et al. (2024) note that RepRisk introduced developments related to machine learning and expanded the scope of ESG issues covered in 2011. Our findings are insensitive eliminating observations prior to 2011 (untabulated).

4.3 Comparing our findings to Asay et al. (2024)

In contrast to our findings, Asay et al. (2024) fail to find any change in sales of products produced by firms that experience a negative tax-related news story. We consider whether the difference in findings is due to differences in the firms or time periods in our respective samples or due to differences in research design choices. Asay et al. (2024) examine changes in weekly sales data after 29 negative tax-related news articles for six firms (Abbott Laboratories, Bayer,

General Mills, Microsoft, PepsiCo, and Starbucks). When we limit our sample to these six firms, we identify 190 weeks with a negative tax news story (more than a 6-fold increase in stories relative to Asay et al. 2024). Requiring seven weeks of data centered on a week in which one of these six firms experienced a negative tax news story and requiring three weeks of "clean" pre-period data yields a regression sample of 33,880 brand-weeks.

Table 4 shows the results from estimating Equation [1] using *Sales* as the dependent variable and data for only the six firms in Asay et al. (2024). Using all available data for these six firms within our sample period (2010 through 2019), we find an insignificant relation between *Sales* and *Post* (Column 1). This insignificant coefficient is consistent with results reported in Asay et. al (2024). We next consider if a time period difference could be influencing our findings, as Asay et al. (2024) utilize data spanning 2011 through 2017 while we utilize data spanning 2010 through 2019. Using all available data for these six firms within the Asay et al. (2024) sample period (2011 through 2017), we again document an insignificant relation between *Sales* and *Post* (Column 2).[16] In contrast, when we estimate Equation [1] using data for all 50 of *our* sample firms and limit our time period to the years in Asay et al. (2024) (2011 through 2017, which yields a sample of 146,429 brand-weeks), we again observe a negative and highly statistically significant *Post* coefficient (β_1 = -0.044 and standard error = 0.013; untabulated).

Finally, we also consider whether the difference in findings is due to research design choices. First, we re-estimate Equation [1] after including a time trend control variable and replacing week-year fixed effects with week fixed effects following Asay et al. (2024) with no impact our inferences from Table 3 (untabulated). Second, we note that Asay et al. (2024) use UPC-week as the unit of analyses, whereas we use brand-week as the unit of analysis. We re-

[16] Including a time trend control variable and replacing the week-year fixed effects with week fixed effects following Asay et al. (2024) yields no change in inferences (untabulated).

estimate Equation (1) using *Sales* as the dependent variable at the UPC-week level and follow the Asay et al. (2024) research design presented in their Table 3 Column 3 (i.e., the inclusion of a time trend control variable, the use of UPC fixed effects and week fixed effects, and standard errors clustered by firm x date) using their six firms and time period (2011 through 2017). This regression yields an insignificant *Post* coefficient (untabulated). In contrast, re-estimating Equation (1) using *Sales* as the dependent variable and these research design choices for *our* sample of 50 firms during *our* time period (2010 through 2019) yields a negative and highly statistically significant *Post* coefficient (β_1 = -0.031 and standard error = 0.008; untabulated). These results indicate that research design choices are not driving the difference in findings between the two studies. Collectively, these findings highlight the importance of utilizing technological advancements such as AI- and ML-based screenings which, relative to a manual approach, identify a materially larger sample of firms subject to negative tax news stories.

4.4 Dynamic effects (H1)

We next consider the dynamic effects of our primary inference by decomposing the *Post* indicator into individual weeks and re-estimate Equation [1], with $Week_{jt-3}$ serving as the base that all other weeks are relative to. The results of this estimation are reported in Table 5, with coefficient magnitudes and 90% confidence intervals for each *Week* coefficient presented in Figure 1. In Table 5 all three columns show a positive and highly significant coefficient on $Week_{jt-2}$ and $Week_{jt-1}$ and an insignificant coefficient during the week of the negative tax news story ($Week_{jt}$). In contrast, the coefficients on $Week_{jt+1}$ and $Week_{jt+2}$ are negative and highly significant when both *Sales* and *Units* are the dependent variables (and insignificant when *Price* is the dependent variable). Turning to economic magnitudes, when *Sales* is the dependent variable the $Week_{jt+1}$ coefficient magnitude of -0.034 represents a 3.3% decline in the week following a negative tax

news story (=exp(-0.034)-1), relative to the benchmark week of $Week_{jt-3}$. Sales are even lower (by 5.7%) in the second week, and then return to a level that is statistically indistinguishable from the benchmark week by the third week. The short-lived nature of this consumer response is consistent with recent work on the short-lived nature of customers' negative response to firms' data breaches that reveal customers' personal information (Agarwal et al. 2024) and customers' only-temporary shift toward sustainable products after being impacted by material climate change events (Lim et al. 2024).

4.5 Cross-sectional tests (H1)

We next explore two sources of cross-sectional variation in our sample. The first source relates to story severity. The RepRisk database classifies each news story according to the perceived "severity of the risk incident, i.e., a function of its consequences with respect to ESG issues, the extent of its impact, and its type" (RepRisk 2022, p.7). We use this classification to create $Severe\ Tax\ Story_{jt}$, a binary variable set equal to one if any of the negative tax news stories about the firm that produces brand *b* (firm *j*) in week *t* are classified by RepRisk as *severity* > 1, and zero otherwise following Lin et al. (2024). Seventy-one percent of brand-weeks include at least one story classified as $Severe\ Tax\ Story_{jt} = 1$ (Table 2 Panel B). We posit that severe news stories will garner a larger negative consumer response. Table 6 Panel A shows the results from estimating Equation [1] after bifurcating the sample by *Severe Tax Story*. We find that the negative relations between *Post* and both *Sales* and *Units* documented in Table 3 are driven by severe tax news stories (columns 1 and 2), with no evidence of a change in consumer behavior for non-severe tax news stories (columns 4 and 5). This cross-sectional result reveals the importance of considering tax news story severity, and highlights that studies based on articles that happen to

capture less severe corporate tax news will appropriately conclude that consumers do not respond to corporate negative tax news coverage.[17]

The second source of cross-sectional variation relates to similarity between a firm's name and a brand's name. We manually compare the brand names to firm names using the brand description variable (*brand_descr*) available in NRS. We create *Name Similarity*$_b$, a binary variable set equal to one if brand *b*'s description contains either the full name or first three letters of firm's *j* name, and zero otherwise. For example, *Name Similarity* = 1 for Kellogg's brand 579806 (KEL ALL-BRAN) and *Name Similarity* = 0 for Kellogg's brand 579701 (KEEBLER TOAST).[18] Sixteen percent of brand-weeks are classified as *Name Similarity* = 1 (Table 2 Panel B). We posit that brands with a name more similar to the firm name in the negative tax news article will garner a larger negative consumer response. Table 6 Panel B shows the results from estimating Equation [1] after bifurcating the sample by *Name Similarity*. However, we find that the relations between *Post* and both *Sales* and *Units* are negative and statistically significant in both subgroups, and an F-test reveals that the coefficient in column 1 (2) when *Name Similarity* = 1 is statistically indistinguishable from the coefficient in column 4 (5) when *Name Similarity* = 0 (F-statistic of 0.27 and 0.11, respectively; untabulated).[19]

4.6 Stacked cohort difference-in-difference regression model (H1)

Table 7 presents the results from estimating Equation [2] (a stacked cohort DiD model) which uses both treated brands and competitor brands (i.e., brands without a negative tax news story in a seven-week window). When the dependent variable is *Sales*, the *Treat*$_{jt}$**Post*$_{mt}$

[17] Only 33 percent of the stories that contribute to the *Post Non-Tax* = 1 variable are classified as *severity* > 1 by RepRisk (untabulated), which could explain the insignificant *Post Non-Tax* coefficients in our analyses.

[18] We acknowledge that this may be a noisy variable, as some brands will be classified as *Name Similarity* = 0 yet have their parent company name listed on their packaging (e.g., a box of Frosted Flakes cereal (the brand) displays the Kellogg's (the parent firm) logo in the upper portion of the packaging).

[19] The sample sizes in Table 6 are slightly lower than in Tables 3 and 4 due to singleton observations in each subsample being dropped from the regression.

coefficient is negative and highly statistically significant. Interestingly, the -0.016 coefficient is identical to the value reported in Table 5 of Christensen et al. (2023) when examining 76 social incidents with both negative media sentiment and high visibility.

We obtain a similar inference when *Units* is the dependent variable. We fail to find a statistically significant coefficient when *Price* is the dependent variable. These results indicate that relative to competitor brand sales, treated brand sales *decrease* when a firm producing the treated brand experiences a negative tax news article.[20] While these findings provide the same inferences as those presented in Table 3, the Table 7 results do not shed light on whether consumers simultaneously *increase* their purchase of unaffected brands to satisfy their demand for a specific type of product or simply go without. Our second hypothesis addresses this question.

4.7 Equation [3] regression results testing H2 (substitution analyses)

Our second hypothesis considers whether consumers, who appear to shift their preferences *away* from a firm's brands following the firm's negative tax news, turn *towards* competitor firm brands to meet their needs. We test this hypothesis by estimating Equation [3], with competitor brands' sales information as the dependent variable. Table 8 presents descriptive statistics for our Equation [3] variables which are at the competitor brand level. Panel A illustrates how the 298,802competitor brand-weeks are distributed across time. Similar to our Equation [1] sample, the earliest years (2010 through 2012) have the fewest observations, with a large increase in 2013 and a peak number in 2016. The unlogged value in Panel B show that the mean sales dollars for a competitor brand-week is $178,238, the mean number of units sold is 53,860, and the mean

[20] Note that the coefficients in Tables 3 and 7 should not be juxtaposed because the comparison groups in each table are different. Table 3 considers a change in within-brand (and therefore within-firm) sales as a function of a firm's negative tax news incidents, while Table 7 considers a change in within-brand sales of affected firms as a function of a firm's negative tax news incidents relative to the change in within-brand sales for brands owned by unaffected firms that compete with affected firms.

weighted average price per unit is $7.90. When comparing these statistics to those for treated firm brand-weeks (Table 2 Panel B), competitor brand-weeks have a lower unit volume but sell at a similar price per unit. Pearson correlations in Panel C fail to reveal a statistically significant relation between *Post* and our three dependent variables, suggesting that consumers do not substitute towards competitor brands after a treatment firm experiences a negative tax news story.

Table 9 Panel A presents the Equation [3] baseline regression results. Column 1 (2) shows an insignificant relation between $Post_{mt}$ and $Sales_{bt}$ ($Units_{bt}$), indicating that there is *no change* in the sales dollars or units sold by competitor brands surrounding a treatment firm's negative tax news story. This finding is consistent with the correlations in Table 8 Panel C. Column 3 reveals a small negative and marginally significant relation between $Post_{mt}$ and $Price_{bt}$, which can be interpreted as competitor brands lowering their product price to lure consumers away from treated brands. However, we caution against drawing strong inferences from the Column 3 results given the marginal nature of the statistical significance. The results in Panel B show that including *Post Non-Tax*$_{jt}$ as a control variable yields similar inferences as those presented in Panel A.[21]

Given our findings that consumers reduce purchases of a treated brand *only* when a firm's negative tax news story is considered severe (Table 6 Panel A), we next consider the severity of a treatment firm's negative tax news story on brand substitution. We split the competitor brand-week sample based on *Severe Tax Story*$_{mt}$ (an indicator set to one if a treated firm with brand sales in module *m* has a tax news story classified as *Severe Tax Story*$_{jt}$ = 1, and zero otherwise). Table 10 Panel A shows that when *Severe Tax Story*$_{mt}$ = 1, there is a positive and statistically significant relation between $Post_{mt}$ and both $Sales_{bt}$ and $Units_{bt}$ (columns 1 and 2). These findings indicate

[21] For consistency with our robustness tests for our first hypothesis, we also re-estimate Equation [3] after controlling for the presence of a negative non-tax news story for competitor firms during weeks *t*-3 through *t*-1 and observe no change in inferences relative to the results presented in Table 9 (untabulated).

that consumers increase their consumption of *competitor* brands owned by firms *without* a negative tax news story in the weeks following a treated firm's severe negative tax news story, relative to the weeks prior. These findings are consistent with our second hypothesis. In terms of economic magnitude, the $Post_{mt}$ coefficient magnitude in column 1 indicates a 7.7% increase in *competitor brand* sales dollars (=exp(0.074)-1) after a firm experiences a severe negative tax news story, relative to competitor brand sales in the weeks prior to the firm's negative tax news story.

When a treatment firm's tax news story is not severe, the $Post_{mt}$ coefficient is insignificant (columns 4 through 6). This result is intuitive given our previously discussed finding that consumers do not decrease their purchases of treated firm brands when a negative tax news story is not severe (i.e., brand substitution is not necessary in this situation). We again consider the alternative explanation that a negative *non*-tax news story for competitor brands in the pre-period drives their brand sales downward in the pre-period, such that the $Post_{mt}$ coefficient in columns 1 and 2 of Table 9 Panel A is simply capturing competitor brands' sales returning to their baseline level in the post period. Re-estimating Equation [3] after controlling for the presence of a negative non-tax news story during weeks *t*-3 through *t*-1 for competitor firms yields no change in inferences relative to Table 10 Panel A (untabulated).

Our second cross-sectional test of H2 considers substitutable product availability. We expect that consumers are able to more easily substitute towards an unaffected competitor brand when the number of competitor brands available within a module is higher. To test for this possibility, we create the variable *High Number of Competitor Brands*$_{mt}$, an indicator set equal to one if the percentage of competitor brands (i.e., the number of competitor brands divided by the total number of brands) in module *m* and week *t* is greater than the sample median, and zero otherwise. Given the Table 10 Panel A finding that substitution occurs *only* for severe stories, we

estimate Equation [3] within the *Severe Tax Story*$_{mt}$ = 1 subsample after bifurcating on *High Number of Competitor Brands*$_{mt}$. Table 10 Panel B shows that substitution towards competitor brands occurs *only* in the severe tax story subsample when there is a high number of competitor brands (columns 1 and 2), consistent with consumers being able to more easily substitute towards an unaffected competitor brand when the number of competitor brands available within a module is higher.[22] These findings examine the competitive effects of a firm's negative tax news coverage – a previously unexplored aspect of corporate tax reputation costs in the accounting literature.

V. CONCLUSION

This study provides new evidence that firms face tax-related reputation costs from consumer backlash to negative tax news stories and explores the competitive effects of tax-related reputation costs. We are able to shed light on these research questions using data at the intersection of a granular dataset of 387 firm-weeks of AI-identified negative corporate tax news stories and brand-week sales data for 50 consumer products firms. Our primary research design uses each brand's weekly sales information as its own control, and we find a 5.0% decline in sales dollars generated by a brand in the weeks following its producer being subject to a negative tax-related news article, relative to in the weeks prior to the negative tax news article. The negative relation begins in the week following the negative tax news article, increases in magnitude in the following week, and then by the third week returns to a level that is statistically indistinguishable from the benchmark week. The negative relation is driven by a decrease in units sold, not a decline in sales price per unit. These findings are found only in the subsample of negative tax news stories classified as severe, highlighting the importance of article content in studies that consider consumer responses to negative corporate tax news.

[22] Inferences are robust to defining the *High Number of Competitor Brands* = 1 variable within the sample of 172,456 brand-weeks with *Severe Tax Story* = 1 (the sample that is bifurcated in the regression results in Panel B) (untabulated).

We consider the competitive effects of this short-term consumer backlash to negative tax news stories by considering product sales of competitor firm brands. We find that consumer purchases of *competitor* brands owned by firms without a negative tax news story *increase* after a firm's severe negative tax news story, consistent with a firm's negative tax news stories benefiting its competitors. We document that competitor brand sales increase by 7.7% following a treatment firm's severe negative tax news story, relative to competitor brand sales prior to the negative tax news article. Collectively, our findings support the notion that consumers substitute *towards* brands produced by competitor firms as they move *away from* brands produced by a firm subject to a severe negative tax news story. To our knowledge, we are the first study to examine the spillover effects of tax-related reputation costs to competitor firms.

Our findings offer some of the first large-sample evidence that firms do experience a short-term negative consequence from retail consumers in response to negative tax news articles. We expect our findings to be of interest corporate executives concerned with aligning their firms' tax planning practices with consumer expectations as well as firms seeking to benefit from their competitors' negative tax news stories. We acknowledge that our research questions consider only potential costs of firms' negative tax news stories, and that the dollar amount of tax savings from firms employing the tax strategies discussed in the negative tax news articles may outweigh these short-term negative consequences. We view documenting this cost as a necessary first step, and look forward to future research that considers the cost-benefit trade-off.

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APPENDIX A
Examples of Non-Severe and Severe Tax Stories for One Firm in our Sample (Apple)

Example One (*Severe Tax Story*$_{jt}$ = 0)
The Wall Street Journal Asia
Corporate News: U.S. Firms Keep $1.2 Trillion Overseas (link)
By Jessica Holzer
August 1, 2013

WASHINGTON – The biggest U.S. companies, including General Electric Co. and **Apple Inc.**, are parking nearly $1.2 trillion in profits offshore, according to a new report from a liberal advocacy group. The report by U.S. PIRG, which analyzed the public filings of the top 100 U.S. publicly traded companies, also found that 82 companies maintain subsidiaries in low-tax jurisdictions located abroad. The report comes as President Barack Obama calls for a corporate tax overhaul that could remove incentives for companies to park profits overseas. Governments across the globe are mobilizing to close loopholes in international tax rules that allow multinational companies to legally shift profits to places that impose little or no tax.

Companies such as **Apple**, Google Inc. and Starbucks Corp. have come under fire from politicians in the U.S. and U.K. who say they use such strategies to avoid billions of dollars of taxes. Earlier this month, the Group of 20 industrial and developing nations backed a plan developed by the Organization for Economic Cooperation and Development to overhaul international tax rules. The plan aims to plug holes in the web of bilateral tax treaties that allow companies to shift their profits to the lowest-tax jurisdictions, regardless of where those profits were earned. U.S. law generally allows companies to not record or pay taxes on profits earned by overseas subsidiaries if the money isn't brought back to the U.S. When U.S. companies repatriate profits earned abroad, they are taxed at the U.S.'s 35% rate minus amounts they paid to foreign jurisdictions. Just 21 of the 100 companies disclosed what they would pay in U.S.

Example Two (*Severe Tax Story*$_{jt}$ = 1)
New York Times
Apple's Web of Tax Shelters Saved It Billions, Panel Finds (link)
By Nelson D. Schwartz and Charles Duhigg
May 20, 2013

WASHINGTON — Even as **Apple** became the nation's most profitable technology company, it avoided billions in taxes in the United States and around the world through a web of subsidiaries so complex it spanned continents and went beyond anything most experts had ever seen, Congressional investigators disclosed on Monday.

The investigation is expected to set up a potentially explosive confrontation between a bipartisan group of lawmakers and Timothy D. Cook, **Apple**'s chief executive, at a public hearing on Tuesday. Congressional investigators found that some of **Apple**'s subsidiaries had no employees and were largely run by top officials from the company's headquarters in Cupertino, Calif. But by officially locating them in places like Ireland, **Apple** was able to, in effect, make them stateless —

exempt from taxes, record-keeping laws and the need for the subsidiaries to even file tax returns anywhere in the world. "**Apple** wasn't satisfied with shifting its profits to a low-tax offshore tax haven," said Senator Carl Levin, a Michigan Democrat who is chairman of the Senate Permanent Subcommittee on Investigations that is holding the public hearing Tuesday into **Apple**'s use of tax havens. "**Apple** successfully sought the holy grail of tax avoidance. It has created offshore entities holding tens of billions of dollars while claiming to be tax resident nowhere." Thanks to what lawmakers called "gimmicks" and "schemes," **Apple** was able to largely sidestep taxes on tens of billions of dollars it earned outside the United States in recent years. Last year, international operations accounted for 61 percent of **Apple**'s total revenue. Investigators have not accused **Apple** of breaking any laws and the company is hardly the only American multinational to face scrutiny for using complex corporate structures and tax havens to sidestep taxes. In recent months, revelations from European authorities about the tax avoidance strategies used by Google, Starbucks and Amazon have all stirred public anger and spurred several European governments, as well as the Organization for Economic Cooperation and Development, a Paris-based research organization for the world's richest countries, to discuss measures to close the loopholes. Still, the findings about **Apple** were remarkable both for the enormous amount of money involved and the audaciousness of the company's assertion that its subsidiaries are beyond the reach of any taxing authority.

"There is a technical term economists like to use for behavior like this," said Edward Kleinbard, a law professor at the University of Southern California in Los Angeles and a former staff director at the Congressional Joint Committee on Taxation. "Unbelievable chutzpah." While **Apple**'s strategy is unusual in its scope and effectiveness, it underscores how riddled with loopholes the American corporate tax code has become, critics say. At the same time, it shows how difficult it will be for Washington to overhaul the tax system. Over all, **Apple**'s tax avoidance efforts shifted at least $74 billion from the reach of the Internal Revenue Service between 2009 and 2012, the investigators said. That cash remains offshore, but **Apple**, which paid more than $6 billion in taxes in the United States last year on its American operations, could still have to pay federal taxes on it if the company were to return the money to its coffers in the United States. John McCain of Arizona, who is the panel's senior Republican, said: "**Apple** claims to be the largest U.S. corporate taxpayer, but by sheer size and scale, it is also among America's largest tax avoiders." In prepared testimony expected to be delivered to the Senate committee by Mr. Cook and other **Apple** executives on Tuesday, the company said it "welcomes an objective examination of the U.S. corporate tax system, which has not kept pace with the advent of the digital age and the rapidly changing global economy." The executives plan to tell the lawmakers that **Apple** does not use tax gimmicks, according to the prepared testimony. Mr. Cook is also expected to argue that some of **Apple**'s largest subsidiaries do not reduce **Apple**'s tax liability, and to press for a sweeping overhaul of the United States corporate tax code — in particular, by lowering rates on companies moving foreign overseas earnings back to the United States. **Apple** currently assigns more than $100 billion to offshore subsidiaries. Atop **Apple**'s offshore network is a subsidiary named **Apple** Operations International, which is incorporated in Ireland — where **Apple** had negotiated a special corporate tax rate of 2 percent or less in recent years — but keeps its bank accounts and records in the United States and holds board meetings in California….

Notes: This appendix provides two examples of tax stories (one classified as *Severe Tax Story*$_{jt}$ = 0 and the other classified as *Severe Tax Story*$_{jt}$ = 1) for one sample firm (**Apple**).

APPENDIX B
NRS Details regarding UPC Prefixes

GS1-issued UPC prefixes range in length from five to ten digits and a firm uses its GS1-issued UPC prefix to generate a unique 12-digit UPC for each individual product the firm produces. As all UPCs are 12 digits, the shorter the UPC prefix the more unique UPCs a firm can create (and the more a firm has to pay GS1 for its UPC prefix). For example, a 6-digit UPC prefix (e.g., *123456*) can be used to generate up to one million unique UPCs (UPC *123456*000000 through UPC *123456*999999) while a 7-digit UPC prefix (e.g., *1234567*) can be used to generate only one hundred thousand unique UPCs (UPC *1234567*00000 through UPC *1234567*99999). If a firm plans to produce less than one hundred thousand unique products, then a 7-digit UPC prefix is sufficient. However, if over time a firm that initially purchased a 7-digit UPC prefix expands its product offerings to more than one hundred thousand unique products, there is no way to "extend" its 7-digit UPC prefix. Rather, the firm must obtain a new UPC prefix for its 100,001st product.

The following illustrates the prefix-UPC matching with the sample firm Kellogg's Company. Kellogg's has 21 unique UPC prefixes in the GS1 database, one of which is the 6-digit prefix *038000*. Matching the UPC prefix *038000* in the GS1 database to all 12-digit UPCs that begin with these six digits in the NRS database identifies 3,033 unique UPCs. For example, UPC *038000*196874 identifies Kellogg's Pop-Tarts Bites Tasty Filled Pastry Bites 28.2 oz, UPC *038000*673009 identifies Kellogg's Ego Frozen Buttermilk Pancakes Easy Breakfast 12 count, and UPC *038000*78665 identifies Kellogg's Special K cereal 16.5-ounce box. We match each of the 21 unique UPC prefixes associated with Kellogg's in the GS1 database with 5,325 unique UPCs in the NRS database.

Next, we aggregate all UPCs within a brand using the NRS Product file. The NRS Product file contains additional attributes of each UPC, including brand code and brand description. We use each UPC's brand code to aggregate UPCs at the brand level. Out of Kellogg's 5,325 UPCs, 5,080 have non-missing brand codes associated with 368 unique brands.

Notes: This appendix provides a detailed discussion of how universal product code (UPC) prefixes (issued by GS1) are matched to UPCs (the data in the NRS dataset). This matching is a necessary step to identify the firm that produces each UPC.

APPENDIX C
Firms in our Sample

	Firm Name		Firm Name
1	3M Company	26	Johnson & Johnson Consumer Inc.
2	Abbott Laboratories Inc.	27	J.M. Smucker Company
3	Abbvie Inc.	28	Kellogg Company
4	Apple Inc.	29	Kimberly-Clark Corporation
5	Avon Products Inc.	30	Kraft Heinz Foods Company
6	Bayer AG	31	Mattel Inc.
7	Bristol-Myers Squibb Company	32	McKesson Corporation
8	Bunge Edible Oil Corporation	33	Microsoft Corporation
9	Campbell Soup Company	34	Mohawk Industries Inc.
10	Cardinal Health Inc.	35	Mondelez International, Inc.
11	Clorox Company	36	Newell Brands Inc.
12	Coca-Cola Company	37	PepsiCo Inc.
13	Colgate-Palmolive Company	38	Procter & Gamble Company
14	Conagra Brands Inc.	39	PVH Corporation
15	ConocoPhillips Company	40	Rite Aid Corporation
16	Constellation Brands Inc.	41	Sherwin-Williams Company
17	Eastman Kodak Company	42	Spectrum Brands Inc.
18	Electronic Arts Inc.	43	Starbucks Coffee Company
19	General Mills Inc.	44	Stryker Corporation
20	Green Mountain Coffee Roasters Inc.	45	Sysco Corporation
21	Hanesbrands Inc.	46	Treehouse Foods Inc.
22	Hasbro Inc.	47	Tyson Foods Inc.
23	Honeywell International Inc.	48	United Natural Foods Inc.
24	Hormel Foods Corporation	49	Weyerhaeuser Company
25	HP Inc.	50	Whirlpool Corporation

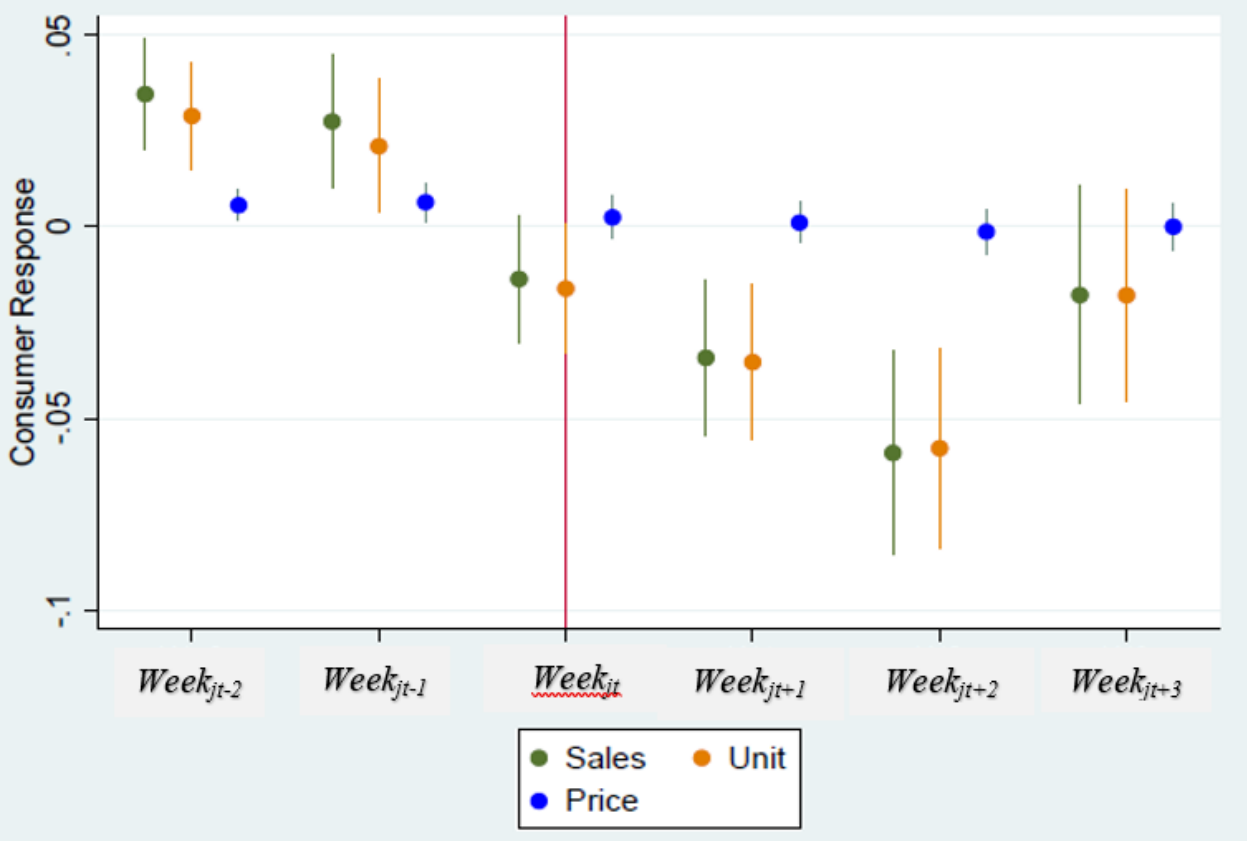
Notes: This appendix lists the 50 firms in our final sample in alphabetical order.

APPENDIX D
Variable Definitions

Variable	Definition
High Number of Competitor Brands$_{mt}$	Indicator set equal to one if the total percentage of competitor brands (calculated as the number of competitor brands divided by the total number of brands) in module *m* during week *t* is higher than the median percentage of competitor brands across all module-weeks, and zero otherwise
Name Similarity$_{b}$	Indicator set equal to one if brand *b*'s description contains either the full name or first three letters of firm's *j* name (where firm *j* is the firm that produces brand *b*)), and zero otherwise
Post$_{jt}$	Indicator set equal to one in weeks *t* through *t*+3 when a negative tax news story about the firm that produces brand *b* (firm *j*) occurs in week *t*, and zero in weeks *t*-3 through *t*-1.
Post$_{mt}$	Indicator set equal to one in weeks *t* through *t*+3 when a negative tax news story about a firm that produces brand *b* within module *m* occurs in week *t*, and zero in weeks *t*-3 through *t*-1.
Post Non-Tax$_{jt}$	Indicator set equal to one when a negative *non*-tax news story about the firm that produces brand *b* (firm *j*) occurs in any week in weeks *t* through *t*+3, and zero otherwise.
Price$_{bt}$	Natural log of the weighted average price per unit of brand *b* products sold at retailers in week *t*.
Sales$_{bt}$	Natural log of the weighted average sales (weighted average price per unit x total number of units) of brand *b* products sold at retailers in week *t*.
Severe Tax Story$_{jt}$	Indicator set equal to one if any of the negative tax news stories about the firm (firm *j*) that produces brand *b* products in a given week is classified by RepRisk as *severity* > 1, and zero if all negative tax news stories about firm *j* in a given week are classified by RepRisk as *severity* = 1 (less severe).
Severe Tax Story$_{mt}$	Indicator set equal to one if any of the negative tax news stories about the firm (firm *j*) that produces brand *b* products within module *m* in a given week are classified by RepRisk as *severity* > 1, and zero if all negative tax news stories about firm *j* in a given week are classified by RepRisk as *severity* = 1 (less severe).
Treat$_{jt}$	Indicator set equal to one if the firm that produces brand *b* (firm *j*) has a negative tax news story in weeks *t*-3 through *t+3,* and zero otherwise.
Units$_{bt}$	Natural log of the weighted average number of units of brand *b* products sold at retailers in week *t*.
Week$_{jt+/-i}$	Indicator set equal to one for *i* = [-3,+3] in week *t*+/-*i* if a negative tax news story about the firm that produces brand *b* (firm *j*) occurs in week *t* = 0, and zero otherwise.

Notes: This appendix provides variable definitions. Variables are listed in alphabetical order.

FIGURE 1
Consumer Response to a Negative Tax-Related News Story, by Week



Notes: This figure plots the coefficients (dots) presented in Table 5, which estimates Equation [1] after replacing the $Post_{jt}$ variable with individual weeks (with $Week_{jt-3}$ as the base that all other weeks are relative to). Each point estimate (circle) is accompanied by a 90% confidence interval (vertical line) calculated using standard errors clustered by brand. The x-axis (y-axis) displays individual weeks (coefficient estimate magnitudes). Appendix D provides variable definitions.

TABLE 1
Sample Selection

Panel A: Sample for testing Hypothesis 1 (Equation [1])

	Firms	Firm-Weeks	Brand-Weeks
Intersection of NRS, GS1, and Compustat	316		
Less: no tax-related RepRisk news story	(251)		
Sample with a negative tax news story in week *t*	65	725	
Less: observations without a "clean" pre-period (i.e., no negative tax news story in the three weeks prior to week *t*)	(0)	(253)	
Sample with a negative tax news story in week *t* and a clean pre-period	65	472	31,032
Less: observations without brand sales in the seven consecutive weeks centered on the negative tax news story in week *t*	(15)	(85)	(3,231)
Sample with a negative tax news story in week *t* and a clean pre-period and brand sales in the seven consecutive weeks centered on the negative tax news story in week *t*	50	387	27,801
x 7 weeks required for our sample window			x 7
			194,607
Less: singletons dropped due to the inclusion of fixed effects			(16)
Equation [1] regression sample			**194,591**

TABLE 1 (cont.)
Sample Selection

Panel B: Sample for testing Hypothesis 2 (Equation [3])

	Firms	Modules	Brand-Weeks
Observations without a negative tax news story during the entire 2010-2019 sample period with brand sales in the 6,173 module weeks [a]	186	575	59,940
Plus: Observations without a negative tax news story in the seven consecutive weeks centered on another firm's negative tax news story in week *t* with brand sales in the 6,173 module weeks	+ 50	+ 0	+ 14,622
Initial sample of competitor brands	236	575	74,562
Less: observations without seven consecutive weeks of brand sales centered on the negative tax news story in week *t*	(13)	(10)	(31,430)
Sample of competitor brands	223	565	43,132
x 7 weeks required for our sample window			x 7
			301,924
Less: singletons dropped due to the inclusion of fixed effects			(3,122)
Equation [3] regression sample			**298,802**

Notes: This table details our sample selection for the brand-weeks used to test our first and second hypotheses (Panels A and B, respectively).

[a] We begin the construction of the H2 sample with the 27,801 brand-weeks in the H1 sample, which correspond to 4,266 brands within 600 NRS-defined modules and 6,173 module-weeks.

TABLE 2
Descriptive Statistics for the Sample Used to Test H1

Panel A: Brand-week frequencies by year

Year	N	Percent
2010	3,412	1.75%
2011	1,229	0.63%
2012	3,116	1.60%
2013	16,976	8.72%
2014	14,339	7.37%
2015	25,028	12.86%
2016	45,223	23.24%
2017	40,518	20.82%
2018	25,621	13.17%
2019	19,129	9.83%
Total	194,591	100.00%

Panel B: Summary statistics

Variable	N	Mean	SD	P25	P50	P75
$Sales_{bt}$ (unlogged)	194,591	396,829	1,364,465	3,716	49,271	225,375
$Units_{bt}$ (unlogged)	194,591	109,413	476,002	730	9,224	45,389
$Price_{bt}$ (unlogged)	194,591	7.62	16.00	2.72	4.46	8.02
$Sales_{bt}$	194,591	10.05	3.27	8.22	10.81	12.33
$Units_{bt}$	194,591	8.49	3.24	6.59	9.13	10.72
$Price_{bt}$	194,591	1.56	0.87	1.00	1.49	2.08
$Post_{jt}$	194,591	0.57	0.49	0.00	1.00	1.00
Post Non-Tax$_{jt}$	194,591	0.29	0.45	0.00	0.00	1.00
Severe Tax Story$_{jt}$	194,591	0.71	0.45	0.00	1.00	1.00
Name Similarity$_{b}$	194,591	0.16	0.36	0.00	0.00	0.00

Panel C: Correlations

Variables	[1]	[2]	[3]	[4]	[5]	[6]
[1] $Sales_{bt}$	1.00					
[2] $Units_{bt}$	0.96***	1.00				
[3] $Price_{bt}$	0.17***	-0.10***	1.00			
[4] $Post_{jt}$	-0.01***	-0.01***	0.00	1.00		
[5] *Post Non-Tax*$_{jt}$	0.02***	0.01***	0.02***	0.55***	1.00	
[6] *Severe Tax Story*$_{jt}$	0.00	-0.01***	0.02***	-0.01***	-0.01***	1.00
[7] *Name Similarity*$_{b}$	0.02***	0.04***	-0.06***	0.00	-0.03***	-0.08***

Notes: This table provides summary statistics for the Equation [1] variables, additional control variables, and cross-sectional partitioning variables. Panel A reports the distribution of brand-week observations by year. Panel B reports descriptive statistics. Panel C presents Pearson correlations, with ***, **, and * denoting significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.

TABLE 3
Testing H1: Consumer Response to a Firm's Negative Tax-Related News Stories

Panel A: Primary results

Variables	DV = $Sales_{bt}$ [1]	DV = $Units_{bt}$ [2]	DV = $Price_{bt}$ [3]
$Post_{jt}$	-0.051***	-0.048***	-0.003
	(0.012)	(0.012)	(0.002)
N	194,591	194,591	194,591
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.853	0.858	0.940

Panel B: Controlling for non-tax negative news stories

Variables	DV = $Sales_{bt}$ [1]	DV = $Units_{bt}$ [2]	DV = $Price_{bt}$ [3]
$Post_{jt}$	-0.060***	-0.057***	-0.003
	(0.013)	(0.013)	(0.002)
Post Non-Tax$_{jt}$	-0.015	-0.015	0.000
	(0.012)	(0.011)	(0.002)
N	194,591	194,591	194,591
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.853	0.858	0.940

Notes: This table presents results from estimating Equation [1]. Panel A presents our primary results, and Panel B includes a variable that controls for the presence of a negative non-tax news story (*Post Non-Tax*$_{jt}$) in weeks *t* through *t*+3. Standard errors (clustered by brand) are presented in parentheses below the regression coefficients. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.

TABLE 4
Testing H1: Using the Six Firms in Asay et al. (2024)

	DV = $Sales_{bt}$	DV = $Sales_{bt}$
Variables	[1]	[2]
$Post_{jt}$	-0.021	0.006
	(0.027)	(0.024)
N	33,880	25,617
Time Period	Our sample period (2010-2019)	Asay et al. (2024) period (2011-2017)
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr
R2	0.836	0.856

Notes: This table presents results from estimating Equation [1] using the sample of six firms in Asay et al. (2024). Standard errors (clustered by brand) are presented in parentheses below the regression coefficients. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.

TABLE 5
Testing H1: Consumer Response to Negative Tax-Related News Stories, by Week

Variables	DV = $Sales_{bt}$ [1]	DV = $Units_{bt}$ [2]	DV = $Price_{bt}$ [3]
$Week_{jt-2}$	0.035***	0.029***	0.005**
	(0.009)	(0.008)	(0.002)
$Week_{jt-1}$	0.028***	0.021**	0.006**
	(0.011)	(0.010)	(0.003)
$Week_{jt}$	-0.014	-0.016	0.0025
	(0.010)	(0.010)	(0.003)
$Week_{jt+1}$	-0.034***	-0.035***	0.0011
	(0.012)	(0.012)	(0.003)
$Week_{jt+2}$	-0.059***	-0.058***	-0.001
	(0.016)	(0.015)	(0.003)
$Week_{jt+3}$	-0.018	-0.017	-0.000
	(0.017)	(0.016)	(0.004)
N	194,591	194,591	194,591
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.853	0.858	0.940

Notes: This table presents results from estimating Equation [1] after splitting the *Post* variable into individual weeks (with $Week_{jt-3}$ as the base that all other week indicators are relative to) using the seven-week window. Standard errors (clustered by brand) are presented in parentheses below the regression coefficients. These coefficients and their 90% confidence intervals are plotted in Figure 1. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.

TABLE 6
Testing H1: Cross-Sectional Analyses

Panel A: Bifurcating on negative tax news story severity

	Severe Tax Story$_{jt}$ = 1			*Severe Tax Story*$_{jt}$ = 0		
	DV=*Sales*$_{bt}$	DV=*Units*$_{bt}$	DV=*Price*$_{bt}$	DV=*Sales*$_{bt}$	DV=*Units*$_{bt}$	DV=*Price*$_{bt}$
Variables	[1]	[2]	[3]	[4]	[5]	[6]
Post$_{jt}$	-0.042**	-0.046**	0.003	0.036	0.037	-0.001
	(0.020)	(0.019)	(0.003)	(0.024)	(0.023)	(0.004)
N	138,619	138,619	138,619	55,932	55,932	55,932
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.871	0.875	0.938	0.876	0.878	0.961

Panel B: Bifurcating on firm name and brand name similarity

	Name Similarity$_{b}$ = 1			*Name Similarity*$_{b}$ = 0		
	DV=*Sales*$_{bt}$	DV=*Units*$_{bt}$	DV=*Price*$_{bt}$	DV=*Sales*$_{bt}$	DV=*Units*$_{bt}$	DV=*Price*$_{bt}$
Variables	[1]	[2]	[3]	[4]	[5]	[6]
Post$_{jt}$	-0.053**	-0.046*	-0.007	-0.038***	-0.036**	-0.001
	(0.025)	(0.025)	(0.005)	(0.014)	(0.014)	(0.002)
N	30,206	30,206	30,206	164,372	164,372	164,372
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.858	0.866	0.953	0.853	0.856	0.937

Notes: This table presents results from estimating Equation [1] after bifurcating the sample as a function of whether the negative tax news story is considered to be severe or not (*Severe Tax Story*$_{jt}$) in Panel A and whether the brand name and firm name are similar or not (*Name Similarity*$_{b}$) in Panel B. Standard errors (clustered by brand) are presented in parentheses below the regression coefficients. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.

TABLE 7
Testing H1: Stacked Cohort Analyses

Variables	DV = $Sales_{bt}$ [1]	DV = $Units_{bt}$ [2]	DV = $Price_{bt}$ [3]
$Treat_{jt}$	0.556***	0.533***	0.0235
	(0.104)	(0.101)	(0.019)
$Post_{mt}$	0.0041	0.005	-0.001
	(0.036)	(0.038)	(0.006)
$Treat_{jt} * Post_{mt}$	-0.016**	-0.017***	0.001
	(0.006)	(0.006)	(0.001)
N	475,513	475,513	475,513
Fixed Effects	Brand, Module-Week-Yr	Brand, Module-Week-Yr	Brand, Module-Week-Yr
R2	0.745	0.765	0.885

Notes: This table presents results from estimating Equation [2]. Standard errors (clustered by brand) are presented in parentheses below the regression coefficients. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.

TABLE 8
Descriptive Statistics for the Competitor Brands Sample Used to Test H2

Panel A: Brand-week frequencies by year

Year	N	Percent
2010	11,646	3.90
2011	5,412	1.81
2012	9,267	3.10
2013	36,700	12.28
2014	21,975	7.35
2015	30,750	10.29
2016	59,029	19.76
2017	41,830	14.00
2018	46,593	15.59
2019	35,600	11.91
Total	298,802	100.00%

Panel B: Summary statistics

Variable	N	Mean	SD	P25	P50	P75
$Sales_{bt}$ (unlogged)	298,802	178,238	679,592	776	12,001	91,927
$Units_{bt}$ (unlogged)	298,802	53,860	255,158	130	2,175	19,427
$Price_{bt}$ (unlogged)	298,802	7.90	13.00	2.75	4.94	9.08
$Sales_{bt}$	298,802	8.95	3.22	6.66	9.39	11.43
$Units_{bt}$	298,802	7.34	3.31	4.87	7.68	9.87
$Price_{bt}$	298,802	1.61	0.91	1.01	1.60	2.21
$Post_{mt}$	298,802	0.57	0.49	0.00	1.00	1.00
Post Non-Tax$_{jt}$	298,802	0.11	0.31	0.00	0.00	0.00
Severe Tax Story$_{mt}$	298,802	1.62	0.55	1.00	2.00	2.00
High Number of Competitor Brands$_{mt}$	298,802	28.83	31.55	8.00	17.00	35.00

Panel C: Correlations

Variables	[1]	[2]	[3]	[4]	[5]	[6]
[1] $Sales_{bt}$	1.00					
[2] $Units_{bt}$	0.96***	1.00				
[3] $Price_{bt}$	0.05***	-0.23***	1.00			
[4] $Post_{nt}$	0.00	0.00	0.00	1.00		
[5] *Post Non-Tax*$_{jt}$	0.08***	0.10***	-0.09***	0.30***	1.00	
[6] *Severe Tax Story*$_{mt}$	-0.02***	-0.04***	0.06***	-0.00*	-0.08***	1.00
[7] *High Number of Competitor Brands*$_{mt}$	-0.06***	-0.11***	0.18***	0.00	-0.00*	-0.02***

Notes: This table provides summary statistics for the competitor firm brand variables used to estimate Equation [3], as well as additional control variables and cross-sectional partitioning variables. Panel A reports the distribution of competitor firm brand-week observations by year. Panel B reports descriptive statistics. Panel C presents Pearson correlations, with ***, **, and * denoting significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.

TABLE 9
Testing H2: Brand Substitution

Panel A: Primary results

Variables	DV = $Sales_{bt}$ [1]	DV = $Units_{bt}$ [2]	DV = $Price_{bt}$ [3]
$Post_{mt}$	-0.017	-0.013	-0.003*
	(0.010)	(0.010)	(0.002)
N	298,802	298,802	298,802
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.849	0.866	0.931

Panel B: Controlling for non-tax negative news stories

Variables	DV = $Sales_{bt}$ [1]	DV = $Units_{bt}$ [2]	DV = $Price_{bt}$ [3]
$Post_{mt}$	-0.014	-0.010	-0.004**
	(0.011)	(0.011)	(0.002)
Post Non-Tax$_{jt}$	-0.011	-0.014	0.0027
	(0.019)	(0.018)	(0.003)
N	298,802	298,802	298,802
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.849	0.866	0.931

Notes: This table presents results from estimating Equation [3], which uses competitor firm brands sales information. Panel A presents our primary results, and Panel B includes a variable that controls for the presence of a negative non-tax news story related to a competitor firm (*Post Non-Tax*$_{mt}$). Standard errors (clustered by competitor brand) are presented in parentheses below the regression coefficients. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.

TABLE 10
Testing H2: Cross-Sectional Analyses

Panel A: Bifurcating on tax news story severity

	Severe Tax Story$_{mt}$ = 1			*Severe Tax Story*$_{mt}$ = 0		
	DV=*Sales*$_{bt}$	DV=*Units*$_{bt}$	DV=*Price*$_{bt}$	DV=*Sales*$_{bt}$	DV=*Units*$_{bt}$	DV=*Price*$_{bt}$
Variables	[1]	[2]	[3]	[4]	[5]	[6]
Post$_{mt}$	0.074***	0.078***	-0.005	-0.019	-0.014	-0.005
	(0.027)	(0.026)	(0.005)	(0.0162)	(0.016)	(0.003)
N	172,456	172,456	172,456	126,289	126,289	126,289
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.870	0.886	0.937	0.881	0.892	0.943

Panel B: Bifurcating on the number of competitor brands within the module-week (within the *Severe Tax Story*$_{mt}$ = 1 subsample)

	High Number of Competitor Brands$_{mt}$ =1			*High Number of Competitor Brands*$_{mt}$ = 0		
	DV=*Sales*$_{bt}$	DV=*Units*$_{bt}$	DV=*Price*$_{bt}$	DV=*Sales*$_{bt}$	DV=*Units*$_{bt}$	DV=*Price*$_{bt}$
Variables	[1]	[2]	[3]	[4]	[5]	[6]
Post$_{mt}$	0.133***	0.134***	-0.001	0.027	0.029	-0.001
	(0.032)	(0.031)	(0.005)	(0.048)	(0.047)	(0.008)
N	118,937	118,937	118,937	53,454	53,454	53,454
Fixed Effects	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr	Brand, Week-Yr
R2	0.872	0.889	0.938	0.899	0.906	0.940

Notes: This table presents results from estimating Equation [3], which uses competitor firm brands sales information, after bifurcating the sample. Panel A bifurcates the sample as a function of whether a firm with a tax new story and brand sales in the same module as competitor firm brands has at least one story classified as severe or not (*Severe Tax Story*$_{mt}$). Within the *Severe Tax Story*$_{mt}$ = 1 subsample, Panel B bifurcates the sample as a function of whether there is a high (or low) number of competitor brands within a module-week (*High Number of Competitor Brands*). Standard errors (clustered by competitor brand) are presented in parentheses below the regression coefficients. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 level, respectively (two-tailed p-values). Appendix D provides variable definitions.