Do Consumers Care about Pay Inequality? Evidence from Household Purchase Data[†*]

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Abstract

We examine consumer reactions to the first-time disclosure of CEO-worker pay ratios by U.S. public firms in 2018, using micro-level household purchase data. We find that firms disclosing high pay ratios experience a significant decline in consumer purchases. Specifically, sales of these firms' products drop by 4.9% compared to similar products from firms without high pay ratios, purchased by the same households. This decline is demand-driven and the effect is significant only after the disclosure. Additional analyses reveal that negative consumer reactions are stronger in areas with greater inequality aversion, for high-value or hard-to-verify products, and when consumers are more likely to be exposed to pay ratio information. Overall, our results suggest that consumers are concerned about high within-firm pay dispersions, driven by their social preferences and a loss of trust, to the extent that they are aware of this information.

JEL classification: J31, M52, E20, D10

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1. Introduction

In recent years, income inequality has emerged as a prominent topic in both public discourse and academic research. A central aspect of this issue is the widening gap between CEO compensation and worker wages, which has fueled debates about fairness, equity, and corporate governance (Piwowar, 2015; Bank and Georgiev, 2019). Against this backdrop, the introduction of the CEO-worker pay ratio disclosure rule marks a pivotal moment, requiring U.S. publicly traded companies to report the ratio between CEO pay and median worker pay for the first time in 2018. This regulatory change enhances pay transparency, offering stakeholders unprecedented insight into firms' pay disparities.

Despite a growing body of research on within-firm pay dispersion and its implications for equity market reaction (Pan et al., 2022), say-on-pay voting (Crawford et al., 2021), firm performance (Faleye et al., 2013; Mueller et al., 2017; Rouen, 2020), voluntary disclosure provision (LaViers et al., 2024), executive compensation policies (Chang et al., 2023; Cheng and Zhang, 2023), and employee morale (Boone et al., 2024; Green et al., 2024), a notable gap remains: Do consumers care about pay inequality within firms? We address this question by investigating how consumers react upon learning about the CEO-worker pay ratio for the first time in 2018. Understanding consumer attitudes toward corporate pay disparity is crucial not only because consumers play a vital role in influencing corporate behavior through their purchasing decisions, but also because it provides valuable insights for corporate leaders regarding a potential feedback effect of their pay practices on firm fundamentals. Furthermore, this research is particularly timely given the ongoing debates about corporate pay transparency and the broader ethical and social responsibilities that firms have toward both their employees and consumers.

From a theoretical standpoint, the effect of corporate pay disparity on consumer behavior is ambiguous. On the one hand, consumers may be concerned about income inequality and perceive high within-firm pay dispersion negatively. Anecdotal evidence and survey research suggest that high CEO-worker pay ratios can damage consumer perceptions and reduce their intent to purchase the firm's products (Mohan et al., 2015; Mohan et al., 2018). This study goes beyond survey data and consumer perceptions to provide systematic evidence on the extent to which U.S. consumers "vote with their feet" in response to large pay inequality.[1] On the other

[1] Throughout the paper, we define consumers as retail or end-consumers in the United States during our sample period. Accordingly, our primary motivation, research design, and findings are largely relevant to U.S. consumers in recent years. Consumer preferences in other locations or time periods may differ, so our results may or may not generalize to those contexts.

hand, high pay ratios could signal that the firm employs a more talented manager, which could enhance consumer value through improved product and investment decisions. Also, it is not obvious whether consumers actually consider pay inequality when making purchasing decisions; they may find information specifically targeted at them, such as advertising campaigns, sufficient or prioritize the firm's financial performance instead. Given these contrasting possibilities, whether and how consumers respond to within-firm pay dispersion remains an empirical question.

Our research strategy comprises three key elements. First, we exploit the new disclosure mandate requiring U.S. public firms to report their CEO-worker pay ratios for the first time in 2018. This mandate provides an ideal setting for examining consumer responses to within-firm pay disparities, since these initial disclosures are highly visible and widely publicized. To conduct this analysis, we manually collect pay ratio data from the proxy statements of all firms that disclosed this information during the initial 2018 reporting period. We then identify firms with high pay ratios relative to others and examine how these disclosures affect their product sales. The revelation of a high pay ratio, which typically garners considerable public attention, serves as a salient event likely to elicit strong consumer reaction (Boone et al., 2024).

Second, we employ data from the NielsenIQ Consumer Panel, which tracks detailed shopping behaviors of approximately 40,000 to 60,000 U.S. households (the exact number varies by year). Our sample covers the period from September 2017 to April 2019 and includes a wide range of products, households, and disclosure events, resulting in millions of recorded product purchases. The granularity and scope of this dataset enable us to incorporate high-dimensional fixed effects that account for unobservable heterogeneity across households, products, and time, leading to reliable inferences from large-scale consumer data. Third, we enhance our identification of the pay disparity effect by performing a *within-household*, *within-product-group* analysis of consumer behavior. This approach isolates variations in product consumption within the same product group, by the same household, at the same time, thereby mitigating the potential impact of omitted factors.

To investigate how the disclosure of a high pay ratio influences consumer purchases, we use a stacked difference-in-differences (DiD) methodology that exploits the staggered implementation of the disclosure requirement across firms with different fiscal year-ends in 2018. The first difference captures changes in monthly spending on products after their manufacturing firms disclose a high pay ratio (referred to as treated or affected products). The control group consists of similarly priced products from firms without high pay ratios within the same product group, in the same household's shopping basket (referred to as control or unaffected products). The second difference reflects changes in monthly spending for this control group. The effect of high pay ratio disclosures on consumer purchases is then estimated as the difference between these two differences.

We begin by exploring trends in product-level sales following the disclosure of high pay ratios. Our univariate graphical analysis shows a significant decline in aggregate monthly sales for products from firms with high pay ratios, relative to those from firms without high pay ratios within the same product group. This pattern is further supported by our product-month-level DiD regressions. While aggregate sales data provide an overview of consumer responses, they reflect consumer behavior only indirectly and may obscure important details in individual purchasing decisions. In particular, aggregating household purchases at the product level results in the loss of crucial consumer-specific information, which undermines the accuracy and depth of our analysis. Products that researchers classify as similar may be perceived differently by consumers due to personal preferences and tastes, adding a layer of complexity that can lead to biases in our estimates and misclassification of control products.

To address these issues, we base our primary analyses on detailed household shopping trip-level data to assess how purchasing decisions change when a product is revealed to be from a high pay ratio firm. We compare these changes to shifts in the same household's consumption of similarly priced, unaffected products within the same product group. Our main results indicate that disclosing a high pay ratio leads to a reduction in consumer purchases of the firm's products. Specifically, monthly spending on affected products decreases by an average of 4.9% relative to similarly priced control products purchased by the same household during the same period. This finding continues to hold when we redefine a high pay ratio as one that exceeds its expected level. Further analysis shows that the post-disclosure reduction in product consumption is driven by a decrease in the number of units purchased, while prices remain relatively stable, illustrating a consumer demand channel through which corporate pay disparity affects cash flows and firm value. Additionally, our examination of the time-series dynamics shows that consumer sensitivity to high pay ratios becomes statistically significant only after the disclosure, providing evidence of parallel trends that reinforce the validity of our empirical setting.

Next, we present five sets of tests to solidify our baseline finding that high pay ratio disclosures significantly dampen consumer purchases. While it is never possible to completely rule out alternative interpretations in general, we can examine whether or not the data are consistent with particular competing explanations, thereby enhancing our understanding of the main results. First, we explore whether the negative consumer reaction is related to the pay ratio or primarily reflects changes in either of its components. Our results indicate that the adverse impact of pay disparity does not vary with CEO or worker pay levels, suggesting that the observed decline in consumer purchases is driven by pay disparity itself, rather than solely by the levels of CEO or worker pay.

Second, to validate the salience of the pay ratio, we examine whether prior disclosure of employee expenses influences consumer reaction. Specifically, we identify firms that disclose total employee wages before the implementation of the pay ratio rule. This prior wage information enables consumers to impute average worker pay that closely correlates with the later disclosed median pay. For these firms, the pay ratio could have been inferred, potentially weakening the impact of the initial disclosure. However, we find that prior wage information does not attenuate the negative consumer reaction. This finding suggests that the ex-ante costs of acquiring and processing information to infer pay ratios likely outweighed the expected benefits for consumers. The 2018 initial disclosures, therefore, represent salient events that garnered considerable consumer attention and raised awareness of within-firm pay inequality.

Third, another potential concern is that corporate pay disparities and the decline in consumer purchases may be driven by changes in underlying local economic conditions. We address this possibility by examining the effect of firm-brand name similarity. If a firm's pay ratio disclosure draws consumer attention and elicits a negative reaction, we would expect a larger decline in purchases when the brand name closely resembles the firm's name, as this similarity makes it easier for consumers to associate the disclosure with the products. In contrast, if the negative effect of pay disparity is driven by local economic conditions, firm-brand name similarity should not matter. Our results show that the negative reaction is more pronounced for products with brands that share a similar name with their manufacturing firms, suggesting that the driving factor is the firm's disclosure rather than local economic conditions.

Fourth, we examine whether the negative consumer reaction is driven by supply disruptions or reputational damage associated with product recalls. Our findings, however, show that disclosed pay ratios have no significant impact on the likelihood of subsequent product recalls, suggesting that product recalls do not explain the observed consumer responses. Fifth, we conduct falsification tests using fictitious event times or randomly assigned treated products to mitigate concerns that the observed effects of mandated disclosures are driven by omitted variables or random chance.

In the final part of the paper, we conduct a series of additional analyses to shed light on the potential mechanisms through which reported pay ratios affect consumer purchases. These analyses are motivated by the traditional model of consumer behavior, which posits that for pay ratios to influence purchasing decisions, consumers must both be concerned about pay disparities and aware of the relevant information (Kotler and Keller 2012; Kotler et al., 2017). If consumers indeed care about pay inequality, they may be reluctant to purchase from firms with high pay ratios, viewing them as socially irresponsible for contributing to broader economic inequality. We refer to this as the *social preferences channel,* where aversion to inequality leads to reduced consumption. Alternatively, a high pay ratio may lead consumers to perceive that the firm is using its resources in a self-serving manner, rather than prioritizing consumer welfare (Besharat et al., 2024). This perception erodes consumer trust and ultimately decreases product purchases—a dynamic we term the *consumer trust channel*.

We provide evidence for both channels by examining the cross-sectional heterogeneity in consumer responses to high pay ratio disclosures. First, consistent with the idea that reduced product consumption reflects consumers' prosocial preferences, we find that the negative consumer reaction is more pronounced in areas with greater aversion to inequality. Second, in line with the notion that high pay ratios undermine consumer trust, we observe a sharper decline in consumption for high-value purchases and hard-to-verify products, where trust plays a particularly important role in purchasing decisions.

Another condition for consumer reaction is their awareness of the disclosures. Consumers need to know about high pay ratios before taking action. While we cannot directly measure customer awareness, we identify three factors that typically enhance it, namely media coverage of the firm's pay ratios, the firm's visibility, and consumers' access to information. We then investigate whether increased awareness is associated with a stronger customer reaction. Our results indicate that the negative consumer reaction is more pronounced when a firm's pay ratio is covered in the media, when the firm is more prominent and visible, and when consumers have better access to information, underscoring the crucial role of awareness in shaping consumer behavior. Collectively, these cross-sectional tests illuminate the key drivers behind our main findings and raise the bar for alternative explanations. To account for our results, any omitted variable must also explain and align with these cross-sectional findings.

Our paper makes three main contributions. First, it expands the literature on within-firm pay dispersion and its broader implications. Earlier work in this area has largely focused on how pay differences within firms affect employee motivation and firm performance (Faleye et al., 2013; Mueller et al., 2017). More recently, the mandated disclosure of pay ratios has sparked growing interest in how various stakeholders react to these disclosures, particularly when large pay disparities are revealed. For instance, Pan et al. (2022) show negative market reaction to large pay ratios, especially among shareholders with prosocial preferences.

Similarly, Crawford et al. (2021) find that firms with large pay ratios experience greater opposition in say-on-pay votes, particularly when institutional ownership is high. Boone et al. (2024) document negative employee responses to high pay ratio disclosures, including reduced satisfaction with their own pay, lower productivity, and declining perceptions of the CEO's performance. Chang et al. (2023) highlight the role of media, showing that firms with high pay ratios receive increased media coverage, with a more negative tone, around the time of their proxy statement filings.

While these studies offer valuable insights, they leave a key stakeholder group unexplored: consumers. As consumers directly impact a firm's sales and profitability, understanding their responses is crucial. Our study fills this gap by investigating consumer behavior in the context of newly disclosed pay disparities. In doing so, we provide evidence for a feedback effect of pay ratio disclosures on firm fundamentals, illustrating on how these disclosures shape consumer perceptions and purchasing decisions.

Second, our findings contribute to the ongoing debate on pay transparency. While policies promoting pay transparency can help reduce wage gaps (Kim, 2015; Baker et al., 2023), they may also harm morale and productivity (Cullen and Perez-Truglia, 2022). By analyzing consumer reaction to the first-time disclosure of the pay ratio in 2018, we shed light on the consequences of transparency regarding within-firm pay dispersion. Our results indicate that such transparency can significantly alter consumer perceptions and trust, emphasizing the role of corporate information in influencing consumer behavior.

Finally, our paper also relates to the emerging literature on end-consumer responses to corporate disclosures. Asay et al. (2022) find no significant impact of corporate tax avoidance news on consumer purchasing behavior, further supporting this null result with survey evidence that consumers generally do not consider tax issues in their purchasing decisions. In contrast, there is evidence of significant consumer responses to earnings announcements (Kimbrough et al., 2024; Noh et al. 2023). Our evidence that consumers react to high pay ratios suggests that concerns over corporate pay inequality have evolved into a broader societal issue. Furthermore, by elucidating the factors that drive the negative consumer reaction, our study provides valuable insights for policymakers, business leaders, and investors interested in the implications of pay inequality on consumer behavior.

2. Background and empirical strategy

2.1. The disclosure mandate and the salience of CEO-worker pay ratio

In response to widespread criticism from the media and the public about the substantial pay gap between corporate executives and rank-and-file employees, Congress passed a pay ratio disclosure mandate in July 2010 as part of the Dodd-Frank Act. Following extensive public feedback, the SEC adopted the final version of the mandate in August 2015, requiring most public firms listed in the U.S. to disclose their CEO-worker pay ratios from the first fiscal year that began on or after January 1, 2017.[2] Thus, the first pay ratio disclosures were made in calendar year 2018, based on fiscal year 2017 data, typically reported in the proxy statement (SEC Form DEF 14A). To comply, firms must disclose the median annual total compensation of all employees (excluding the CEO),[3] the annual total compensation of the CEO, and the ratio between these two figures.

A critical aspect of the pay ratio disclosure is its deliberate and ingenious public salience. While Congress could have mandated the reporting of standardized information on employee pay, they instead chose to require the CEO-worker pay ratio—a simple yet powerful metric with the potential to influence stakeholder perceptions and actions (LaViers et al., 2022). By directly linking workers' earnings to those of executives, this ratio adds a personal dimension that resonates more deeply with the public compared to information about CEO or employee pay alone (Boone et al., 2024). This direct comparison underscores the stark pay differentials between executives and their employees, eliciting negative reactions.

Moreover, this mandate marks a significant shift in corporate compensation disclosure, representing the first rule in the SEC's history to require firms to disclose any information about their employee pay practices (Bank and Georgiev, 2019).[4] The newly disclosed pay ratios not only garner significant media attention but also alter the focus of media coverage on executive compensation. Prior to 2018, firms were only required to disclose the total compensation received by the five highest-paid executives, prompting media scrutiny to focus on the size of executive pay packages and their alignment with corporate performance.[5]

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[2] The rule does not apply to smaller reporting companies, emerging growth companies, foreign private issuers, multi-jurisdictional disclosure system filers, or registered investment companies (SEC, 2015).

[3] The SEC rule permits firms to use different methods to identify the median employee, such as payroll records or statistical sampling, provided they use a consistent compensation measure and explain their assumptions and exclusions (SEC, 2015). The rule includes worldwide, full-time, part-time, seasonal, and temporary workers of the firm and its subsidiaries as employees, but excludes contractors. Additionally, firms may exclude foreign workers who make up 5% or less of the workforce. They can also annualize pay for employees not employed for the full fiscal year, but this does not apply to seasonal or temporary workers. Cost-of-living adjustments for employees in different locations than the CEO are allowed, with the requirement to disclose a supplemental ratio without the adjustment (SEC, 2015).

[4] The only other disclosure rule concerning workers requires firms to report their total number of employees.

[5] Mullaney, T. (2015, May 18). *Why corporate CEO pay is so high, and going higher*. CNBC. https://www.cnbc.com/2015/05/18/why-corporate-ceo-pay-is-so-high-andgoing-higher.html.

However, the availability of firm-specific pay ratio data in 2018 redirected media attention towards pay disparities between CEOs and their employees, as well as the broader issue of income inequality.[6]

The enactment of the pay ratio disclosure mandate is also highly controversial, with the SEC receiving more than 287,400 comment letters, further emphasizing its impact (Piwowar, 2015). Proponents argue that the pay ratio disclosure will reduce income inequality in the U.S. and eliminate ineffective pay practices that contribute to the financial crisis. Over 70,000 individuals support the usefulness of the ratio in their comment letters to the SEC, asserting that "the public has the right to know which corporations are fuelling the yawning gap between rich and poor," and that this information would be "a useful factor for considering which businesses to support with their consumer and investment dollars." In contrast, critics question the effectiveness of pay ratio disclosure. For example, former SEC Commissioner Michael Piwowar states that the pay ratio "has nothing to do with protecting investors, ensuring fair, orderly, and efficient markets, or facilitating capital formation" (Piwowar 2015). He dissents in the same statement that the pay ratio reform is mainly the result of a populist effort to "name and shame" CEOs.

The intersection of salience and controversy amplifies the prominence of the pay ratio disclosure. Indeed, its contentious nature and personal resonance attract considerable attention and provoke strong emotional reactions, rendering pay disparity issues more memorable and impactful to individuals and media outlets (Boone et al. 2024). As public awareness of corporate pay disparities rises, so does the controversy, intensifying discussions and diverse perspectives. This cycle of attention and controversy perpetuates itself, further elevating the pay ratio's prominence. Ultimately, highly salient and controversial issues like the pay ratio disclosure can foster heightened public engagement, such as changes in consumer choices, reflecting its profound impact on public discourse and actions (Mohan et al., 2015).

2.2. Empirical motivation

[6] Svaldi, A. (2018, April 20). *Colorado CEOs earn in three days what the typical worker earns in a year, new disclosures show.* The Denver Post. https://www.denverpost.com/2018/04/20/colorado-ceo-worker-pay-gap/; Murphy, B. (2018, May 22). *CEOs paid 1,000 times more than average workers*. Urban Milwaukee. https://urbanmilwaukee.com/2018/05/22/murphys-law-ceos-paid-1000-times-more-than-average-workers/; Dornbrook, J. (2018, August 3). *Filing reveals cost of CEO change at H&R Block, extraordinarily high CEO-to-median employee pay ratio*. Kansas City Business Journal. https://www.bizjournals.com/kansascity/news/2018/08/02/hr-block-executive-compensation.html.

Our empirical framework is grounded in three key components. First, we leverage the mandated disclosure of CEO-worker pay ratios to examine consumer reaction to previously undisclosed within-firm pay dispersion. This new transparency enables consumers and other stakeholders to directly compare, for the first time, the compensation of a company's CEO with that of its median employee, thereby bringing issues of corporate pay disparities into the spotlight (Pan et al., 2022). To investigate whether consumers are concerned about these disparities, we focus on the initial release of pay ratios in 2018.

This initial reporting period is crucial for studying consumer reaction because it represents the largest influx of new information. The novelty and salience of this information are likely to attract considerable attention and elicit strong reactions (Boone et al., 2024). Emphasizing the newsworthiness of the 2018 pay ratio disclosure, Pan et al. (2022) show that the likelihood of firms receiving compensation and benefits-related news coverage surges immediately after their 2018 proxy filing dates, in contrast to a much flatter trend during the same period in 2017. Furthermore, the staggered implementation of the mandated disclosure throughout the year—across firms with different fiscal year-ends—allows us to isolate the effects of these disclosures from other economic factors, adding credence that our results truly reflect the impact of corporate pay disparities on consumer behavior.

Second, we analyze detailed consumer purchasing decisions using household shopping trip-level data from NielsenIQ. Our analysis investigates *within-household*, *within-product-group* dynamics to explore how consumers react when a manufacturer of a product they frequently purchase disclose a high pay ratio (treated product), compared to a similar product from a manufacturer without such a disclosure (control product). Specifically, to achieve a precise apples-to-apples comparison, we examine treatment and control products within the same product group, the same household’s shopping basket, and the same month, thereby reducing the potential impact of omitted variable bias.

Figure 1 illustrates our construction of treatment and control samples using three product groups: candy, nuts, and hair care. For each treated product within these groups, we track and compare its purchases with those of control products within the same product group and the same household’s shopping basket. Following Houston et al. (2024), we include only control products priced within a ± 20% range of the unit price of the treated products to reassure that both sets of products offer similar latent utility and are subject to similar budgetary constraints. Additionally, we require that treated and control products were purchased at least once in the four months prior to the disclosure event, ensuring that all products were available and known to the studied households.[7]

The granularity of the household data allows us to incorporate high-dimensional fixed effects that account for unobservable heterogeneity across event cohorts, households, and products, thereby addressing the primary sources of endogeneity in our study: A product's divergent latent utility to consumers and the endogenous demand from households. Our identification relies on the variation in product purchases by the same household around the time of the initial pay ratio disclosure. Thus, any observed changes in consumer purchases are likely due to the disclosed pay ratios rather than household or product characteristics such as consumer preferences, tastes, budgetary constraints, or local availability.

Third, we employ a stacked difference-in-differences (DiD) approach for our analysis, a method recommended for causal inference in contexts with staggered treatment events (Baker et al., 2022). Conceptually, this approach can be viewed as a stack of canonical DiD designs, with clearly defined treatment and control products across pre- and post-periods. In estimating the stacked regressions, we match each treated product with comparable, never-treated products and track both sets of products around the event. The combined set of treated and control products sharing the same event month is labeled as a cohort. We then stack the data across all such cohorts to form our testing sample and estimate the average treatment effect. Given the rich cross-sections of products, households, and disclosure events in the data, the resultant stacked sample consists of millions of product purchases by U.S. households, allowing us to draw accurate inferences from large-scale consumer data.

This stacked DiD approach is also crucial for addressing the methodological concerns raised in recent econometric literature on two-way fixed effects DiD regressions. These concerns include issues related to heterogeneous treatment timing effects and the potential negative weights on certain treatments (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Athey and Imbens, 2022; Baker et al., 2022). By using stacked regressions, we mitigate these issues, leading to more reliable estimates of consumer reaction to high pay ratios. Moreover, this method is well-suited for our setting, where we can exploit the different timing of mandated pay ratio disclosures across firms and identify a set of control products from each household's product purchases during the sample period.

[7] By design, given the annual nature of pay ratio disclosures, treated and control products are not affected by any other such disclosure events during the pre- and post-periods examined.

3. Data, summary statistics, and preliminary analysis

3.1. CEO-worker pay ratio

To identify high-pay-ratio, or treated, products, we first define firms with high pay ratios and then trace their respective products. Our analysis begins with all firms that disclosed pay ratios during the initial reporting period in 2018. We manually extract relevant information from proxy statements filed with the SEC, including the pay ratio, CEO compensation, and median worker pay. This process results in a sample of 2,373 firms. After excluding those headquartered outside the U.S., we are left with 2,285 firms. Table 1 Panel A presents the summary statistics for the pay ratio and its components within this sample, showing an average pay ratio of 124.7, an average annual total CEO compensation of $5.87 million, and average median worker pay of $80,453. Following Pan et al. (2022), we identify the earliest filing date of either the preliminary or definitive proxy statement for each firm and designate the month of this filing as the disclosure month *t*.

Reflecting the staggered nature of pay ratio disclosures, we define treatment on a monthly basis by comparing a firm's pay ratio disclosure in a given month to all reported pay ratios up to that month. A high pay ratio becomes particularly salient when juxtaposed with ratios reported by other firms, thereby increasing its potential impact on consumer behavior (Boone et al., 2024). We classify a firm as having a high pay ratio—and its products as treated—if its pay ratio falls within the highest quartile of all reported pay ratios up to month *t*.[8] For instance, if a firm files its proxy statement in February 2018, we classify it and its products as treated if its pay ratio is in the top quartile of all pay ratios reported in January and February 2018. Similarly, a firm filing in March 2018 is classified as treated if its pay ratio is in the top quartile of all pay ratios reported from January through March 2018.

We adopt this cumulative approach to identify high-pay-ratio firms because consumers are likely to form their perceptions based on all available information, rather than relying solely on data from a single month. Nonetheless, in untabulated results, we confirm that our findings remain quantitatively similar when defining a high pay ratio based on the corresponding monthly quartile.

3.2. NielsenIQ consumer panel (NCP)

[8] As a robustness check, we alternatively define a high-pay-ratio firm based on the highest tercile or quintile of all disclosed pay ratios up to month *t*. Our findings continue to hold.

Our consumer purchase data are from the NielsenIQ Homescan Consumer Panel, provided by the Kilts-NielsenIQ Data Center at the University of Chicago Booth School of Business. This dataset tracks the detailed shopping behaviors of approximately 40,000 to 60,000 U.S. households, with the specific number varying by year, continually surveyed by NielsenIQ since 2004. For our analysis, we use data from September 2017 to April 2019, which covers a period from four month before to four month after the initial reporting year of 2018.

Households participating in the survey use barcode scanners and hand-coded diary entries to record all their expenditures on household goods, along with details about the retail locations they visit during each shopping trip. To ensure high levels of continued participation and minimize sample attrition, NielsenIQ offers monetary incentives and maintains ongoing engagement with participants. For each participant, the NCP also collects a set of demographic and geographic characteristics, such as age, household income, and location details (e.g., zip code, county, and state). As shown in Figure 2, participants are geographically diverse and demographically representative, encompassing a broad range of locations, age groups, and income levels.

The NCP categorizes products into a hierarchical structure with three levels of aggregation. During our sample period, this structure includes 10 departments, 119 product groups, and 1,461 product categories (referred to as modules). Panel A of Table 2 provides an overview of these ten departments, including examples of various product groups within each department and the distribution of products purchased by surveyed households in our sample. Most of the products are frequently purchased household items commonly found in grocery stores, with the largest department being “Dry Grocery.”

Panel B provides examples of specific product modules. For example, the "Candy – Chocolate" module is part of the "Candy" product group within the "Dry Grocery" department. This type of candy is differentiated from other similar items, such as "Candy – Non-Chocolate," within the same product group and department. This example illustrates that products compared within the same product group are highly similar in nature, facilitating accurate comparisons between treated and control groups.

The NCP offers several attractions that are particularly relevant to our study. First, the high-frequency nature of the recorded shopping trips allows us to observe changes in consumer purchases shortly after the initial pay ratio disclosure, thereby increasing our assurance that these changes are attributable to the disclosures. Second, the NCP provides a representative sample of the U.S. population and covers household shopping activities across a wide range of retail outlets, such as grocery stores, pharmacies, and mass merchandise stores (Broda and

Weinstein, 2010; Baker et al., 2024). This broad coverage improves the generalizability of our findings. Third, as outlined in Section 2.2, the granularity of the NCP data supports *within-household*, *within-product-group* analyses of consumer reaction, controlling for high-dimensional fixed effects. This methodological rigor mitigates concerns about omitted factors and alternative explanations. Fourth, we exploit the richness of the data at both the household and product levels to investigate heterogeneous responses across different household demographics and product attributes. Compared to the store-level sales data from the Retail Scanner dataset, the shopping-trip level data from the NPC offers deeper and more nuanced insights into the purchasing decisions of end consumers.

To connect consumer purchases with pay ratio disclosures, we employ an algorithmic name-matching process, supplemented with manual verification. This involves using company prefix data from GS1 US to trace the producers of all products in the NCP dataset via their barcodes. GS1 US is a not-for-profit information standards organization that assigns barcodes to consumer goods, providing manufacturers with unique digital identities for their products. Through this matching process, we link pay-ratio-reporting firms to their respective products, classify treated products, and identify suitable control products that meet our specified criteria. Our primary sample for within-household analysis includes 63,619 households and 198,506 unique products, of which 35,092 are treated products manufactured by 64 high-pay-ratio firms.

Table 1 Panel B provides an overview of consumer purchases at the household-product-month level. *Total spending* represents the amount spent by household *i* on product *p* in a given month. If a household does not purchase the product in a month, we record *Total spending* as zero. The overall average *Total spending* across all observations is \$0.87, which increases to \$4.30 when considering only household-months with recorded purchases. Price data are only available for months when a household makes at least one purchase of the product. The average price per unit is \$3.09, with a median of \$2.48. These summary statistics are comparable to those reported by Houston et al. (2024).

3.3. Univariate and product-level analysis

The mandated CEO pay ratio is a salient metric that can influence the perceptions and actions of key stakeholders (Boone et al., 2024). In particular, when high pay ratios are disclosed, they draw public attention to pay disparities within firms, potentially provoking consumer reaction. To examine whether consumers are concerned about income inequality, we investigate their response to firms' initial pay ratio disclosures in 2018. We start with a product-level analysis to assess the aggregated impact of disclosing a high pay ratio, and then proceed to a more detailed household-level analysis, which is the primary focus of our study.

3.3.1. Univariate results

Figure 3 illustrates the impact on product-level consumption when its manufacturer discloses a high pay ratio. To isolate this effect from other potential confounding factors over time, we compare the consumption trend of products from manufacturers with high pay ratios to that of a control group consisting of other unaffected products within the same product group in the NCP dataset. The analysis spans from four months before (*t*-4) to four months after (*t*+4) the event month *t*, defined as the month in 2018 when a firm reports a pay ratio in the top quartile of all reported pay ratios up to that time.

Specifically, Panel A plots the percentage changes in monthly dollar consumption relative to the pre-event level in month *t*-2. We observe that the average monthly consumption of treated products declines persistently for three months following the disclosure before leveling off in the fourth month. In contrast, the consumption trend for the control group remains relatively stable before and after the disclosure event. Panel B, which tracks unit consumption, shows a similar pattern with a comparable decline in quantity purchased. This graphical analysis indicates that disclosing a high pay ratio significantly impacts consumer choices regarding the products of the disclosing firms. Given that the largest product categories in our sample are frequently purchased consumer staples (see Table 2), the observed persistent decline in consumption over several months suggests a substantial shift in consumer behavior.

3.3.2. Product-level analysis

To formalize the preceding evidence, we perform a product-level stacked DiD analysis. In this approach, each treatment observation represents the aggregate monthly sales of a product whose manufacturer reports a high pay ratio in month *t*. In line with the graphical analysis, the control group comprises other products in the same product groups that are not associated with high-pay-ratio manufacturers during the +/- 4 month estimation window. We differentiate control products by their respective product groups to align with the treated products. Each control observation represents the average monthly sales across products in the corresponding control group. We then assemble treated and control products with the same event month into cohorts and stack the data across all cohorts to estimate the impact of high pay ratio disclosures on monthly product sales. The estimated model is as follows:

$$LN\ Product\ sales_{j,p,t} = \beta_1 Treat_{j,p} \times Post_{i,t} + \alpha_{j,p,t} + \epsilon_{j,p,t} \tag{1}$$

where *j* indicates events pertaining to the disclosure of high pay ratios, *p* denotes products, and *t* stands for calendar month-years. The dependent variable is the natural logarithm of one plus the total sales of a product in a given month. *Treat* is assigned a value of one for products from manufacturers that report high pay ratios, and zero otherwise. *Post* takes a value of one for each month during the five-month period after the disclosure of a high pay ratio (including the event month), and zero otherwise. $\alpha_{j,p,t}$ captures fixed effects for *Cohort* × *Month-year* and *Cohort* × *Product* to account for unobservable differences across products and over time. We do not include *Treat* and *Post* on their own in the model because their effects are already incorporated into these fixed effects. Moreover, we cluster standard errors at the product level to address the correlation of error terms within each product.

Table 3 presents the results from estimating Equation (1). Column 1 employs a nine-month window that includes the event month, while Column 2 uses an eight-month window that excludes it. In both columns, the coefficient on the interaction term between *Treat* and *Post* is negative and statistically significant at the 1% level, indicating that disclosing high pay ratios has a detrimental effect on product sales. These results substantiate the contrasting trends illustrated in Figure 3.

4. Main results

Analyzing aggregate product-level sales offers a useful initial perspective on consumer responses to high pay ratios. However, this method only indirectly captures consumer behavior and may lead to information loss that obscures important insights into individual purchasing decisions. For instance, when household purchases are aggregated at the product level, crucial consumer-specific details are lost, which can be problematic. Products that researchers classify as similar may be perceived differently by consumers due to personal preferences and tastes. This lack of nuance can hinder our understanding of consumer behavior and lead to inaccuracies in selecting control products. Additionally, aggregate product sales represent equilibrium outcomes shaped by both consumer demand and manufacturer production decisions, making it difficult to disentangle these influences.

To address these limitations, a more rigorous and direct approach involves conducting *within-household*, *within-product-group* analyses. In our primary analyses, we utilize detailed household shopping trip-level data to examine how a household's purchasing choices change when a product in their shopping basket is revealed to be from a high-pay-ratio manufacturer.

We then compare this change to the change in the same household's consumption of a similarly priced, unaffected product within the same product group. This approach allows for a clearer understanding of consumer behavior by directly examining household-specific responses to pay ratio information.

4.1. Baseline analyses

Our baseline analyses proceed in three steps. First, we examine the relation between newly disclosed pay ratios and consumer purchases at the household-product-month level. Second, we analyze the time-series dynamics of consumer sensitivity to high pay ratio disclosures. Third, we employ an alternative measure of high pay inequality to reinforce our main findings.

We start by investigating consumer responses to high pay ratios. As detailed in Section 2.2, we use a stacked DiD model to examine consumer purchases following firms' initial disclosures in 2018. A firm is considered to have a high pay ratio, and its products are classified as treated, if the pay ratio is in the highest quartile among all reported pay ratios up to the disclosure event month. To ensure accurate comparisons, we carefully select control products that meet our specified criteria: they must never be classified as treated during the estimation window, belong to the same product group as treated products, be purchased by the same household, fall within a ±20% price range, and have been purchased at least once in the pre-disclosure period. We track both treated and control products from four months before to four months after the disclosure event. Treated and control products with the same event month are grouped into cohorts. We then stack the data across all cohorts to construct our testing sample and estimate the effect of disclosing high pay ratios based on the following model:

$$LN\ Total\ spending_{i,j,p,t} = \beta_1 Treat_{j,p} \times Post_{i,t} + \alpha_{i,j,p,t} + \epsilon_{i,j,p,t} \qquad (2)$$

where *i* refers to surveyed households, *j* indicates events pertaining to the disclosure of high pay ratios, *p* denotes products, and *t* stands for calendar month-years. The dependent variable is the natural logarithm of one plus the total amount a household spends on a specific product in a given month. *Treat* is set to one for products whose manufacturers disclose high pay ratios, and zero otherwise. *Post* is set to one for each month during the five-month period following the disclosure (including the event month), and zero otherwise. $\alpha_{i,j,p,t}$, represents a set of high-dimensional fixed effects, namely *Cohort* × *Month-year* and *Cohort* × *Household* × *Product*, that account for unobservable heterogeneity across event cohorts, households, and products. We do not include *Treat* and *Post* as separate variables in the model since the fixed effects absorb them. Moreover, we employ double clustering of standard errors by household and month-year, along with product-level clustering, to address the correlation of error terms within each product and the correlation in purchasing decisions within households in a given month.

Panel A of Table 4 reports the results from estimating Equation (2), with columns 1 and 2 using a nine-month window and an eight-month window, respectively. In both columns, the coefficient on the interaction term between *Treat* and *Post* is negative and statistically significant at the 1% level, suggesting that consumers react negatively to high pay ratios. In terms of economic significance, the coefficient in column 1 suggests that the sales of affected products decrease by 4.9% relative to similarly priced, unaffected products purchased by the same household during the same period.

Panel B of Table 4 explores the time-series dynamics of consumer reaction to pay ratio disclosures. In column 1, we modify Equation (2) by replacing *Treat* × *Post* with interactions between *Treat* and individual event time dummy variables that represent the months relative to the disclosure event. Specifically, Pre_{t-4}, Pre_{t-3}, and Pre_{t-2} are dummy variables for the three months preceding the event, while $Post_{t+1}$, $Post_{t+2}$, $Post_{t+3}$, and $Post_{t+4}$ denote the four months following the event. The month *t*-1 serves as the reference month, so the interaction between Pre_{t-1} and the treatment indicator is omitted. In addition, if a household does not make any purchases of a product in a given month, the spending for that month is recorded as zero. The results show that reductions in consumer spending become statistically significant only after the disclosure of high pay ratios. This pattern supports the parallel trend assumption for our empirical setting.

In columns 2 and 3 of Panel B, we repeat the analysis from column 1, but this time we decompose total spending into unit purchases (labeled as *Units*) and price per unit (labeled as *Price*). As described in Section 3.2, the NCP dataset provides quantity and price information for individual products sold at the household level. We calculate the total number of units purchased and the average price for a product when a household buys it multiple times within a given month. For months when a household does not purchase the product, we set *Units* to zero and *Price* to the average price paid by other households in the same county for the same product. If no purchases of the product are recorded in the county during that month, we mark *Price* as missing.

The estimates in column 2 indicate that high pay ratios have a negative and significant impact on unit consumption, suggesting that when a firm discloses a high pay ratio, consumer demand for its products declines. In contrast, column 3 shows no significant relation between price and high pay ratios. Thus, high pay ratios reduce total dollar consumption mainly by decreasing consumer demand at a given price, rather than by lowering the product price. These results support a demand-side interpretation over a supply-side one, given that supply-driven reductions in consumption typically involve an increase in the equilibrium price following a leftward shift in the supply curve, which we do not observe in the data.

In summary, our results indicate that products associated with high pay ratios experience reduced consumption, holding the product group, time, and household constant. This reduction arises from a decrease in units consumed, with price levels remaining relatively stable. These findings underscore the importance of the consumer demand channel in understanding how corporate pay disparity can affect cash flows and firm value.

For our main analyses, we define high pay ratios as those that are relatively high compared to other firms. Alternatively, a pay ratio can be considered unexpectedly high if it exceeds its expected level. To capture this concept, we isolate the unexpected component of the pay ratio using the method developed by Rouen (2020) and Boone et al. (2024). Specifically, we estimate the expected CEO and median employee pay using their models,[9] and then compute the expected pay ratio as the ratio of predicted CEO pay to predicted median employee pay. *Unexpected pay ratio* is simply the difference between the reported pay ratio and the expected pay ratio. Finally, we define $Treat_{unexpected}$ as an indicator variable that equals one for products associated with unexpectedly high pay ratios—when the computed *Unexpected pay ratio* falls within the highest quartile among all computed unexpected pay ratios up to the reporting month. Panel C of Table 4 shows that the coefficient on the interaction term between $Treat_{unexpected}$ and *Post* remains negative and statistically significant at the 1% level, indicating that unexpectedly high pay ratios negatively impact consumer purchases.

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[9] We estimate the expected CEO and median employee pay using the model developed by Rouen (2020) and Boone et al. (2024):

$$Ln(CEO\ pay_{i,t}) = \alpha + \theta_{i,t} + Industry\ FE + \epsilon_{i,t}$$

$$Ln(Median\ employee\ pay_{i,t}) = \alpha + \emptyset_{i,t} + Industry\ FE + \epsilon_{i,t}$$

In the CEO pay model, $\theta_{i,t}$ represents a vector of variables that influence CEO compensation, including firm size (measured by the natural logarithm of book assets), industry-adjusted return on assets, its five-year variance, and the natural logarithm of the CEO's tenure and age. The model also incorporates annual stock returns for fiscal years *t* and *t*–1, stock return volatility over the past five years, book leverage, the book-to-market ratio, an indicator for negative net income as a proxy for poor performance.

In the employee pay model, $\emptyset_{i,t}$, denotes a vector of variables that affect employee compensation, including annual revenue change, firm age (measured as the natural logarithm of the number of years the firm's data are available in CRSP), industry-adjusted returns on net operating assets (RNOA), and its five-year standard deviation. Additional factors include research and development (R&D) intensity and capital intensity (measured by R&D and capital expenditures scaled by total sales, respectively), the percentage of college graduates in the Core Based Statistical Area (CBSA) closest to the firm's headquarters, mean industry-level compensation within the same CBSA, and an indicator variable for states enforcing a "Right to Work" law. Employee productivity, defined as revenue per employee, is also included, along with indicators for *de minimis* exemption and foreign workers to account for the effects of non-US workers. Both the CEO and employee pay models include industry fixed effects.

4.2. Enhancing identification

In this section, we present five sets of tests to rule out alternative explanations and tighten the link between pay disparity and consumer purchases. First, we provide evidence that the reduction in consumer purchases is attributable to pay ratios, rather than the levels of CEO or worker pay alone. Second, we confirm the salience of pay ratio disclosure by demonstrating that prior wage information does not significantly affect consumer reaction. Third, we address the concern that our results may primarily reflect changes in the underlying local economic conditions by examining the effect of firm-brand name similarity. Fourth, we consider the possibility that the negative consumer reaction could be due to supply disruptions or reputational damage associated with product recalls. Fifth, we conduct falsification tests using fictitious event times or randomly assigned treated products. We describe each of these tests in more detail in the following sections.

4.2.1. Pay disparity versus pay levels

Our baseline results indicate that within-firm pay disparity negatively affects consumer product consumption. A potential concern, however, is that this effect might be driven solely by either CEO pay or median worker pay, rather than the pay ratio itself. To address this issue, we investigate whether the observed decline in consumer purchases is truly attributable to pay disparity, or if it is primarily driven by the pay levels of CEOs or workers. Given that firm-specific details are available for some, but not all, control products—many of which are not from listed firms, and our empirical approach does not rely on firm-specific controls—we are unable to directly control for CEO or worker pay in our model. Instead, we assess whether consumer responses vary based on these pay levels. If the negative consumer reaction to high pay disparity is mainly due to CEO or worker pay, we would expect this effect to be more pronounced in treated products associated with high CEO pay or low worker pay.

In Panel A of Table 5, we test this conjecture in the framework of the baseline model by splitting the *Treat* indicator into two categories: *Treat with high CEO pay,* which equals one for products from firms with high pay ratios and CEO pay above the median of treated firms in our sample, and zero otherwise; and *Treat with low CEO pay*, which equals one for products from firms with high pay ratios and CEO pay below the median of treated firms in our sample, and zero otherwise. The results show that the coefficient estimates on both *Treat with high CEO pay* and *Treat with low CEO pay* are negative and significant at the 1% level. However, there is no significant difference between the two coefficients.

In Panel B of Table 5, we perform a similar analysis for worker pay by categorizing the *Treat* indicator based on median worker pay. *Treat with high worker pay* (*Treat with low worker pay*) is set to one for products from firms with high pay ratios and median worker pay above (below) the median of treated firms in our sample. The coefficient estimates on both *Treat with high worker pay* and *Treat with low worker pay* are negative and significant at the 1% level, with no significant difference between the two coefficients. Overall, the consistent effect of within-firm pay disparity across groups with varying levels of CEO and worker pay suggests that the observed reduction in consumer purchases is attributable to pay disparity itself, rather than driven solely by the levels of CEO or worker pay.

4.2.2. Confirming the salience of the pay ratio

We consider the 2018 disclosure of median worker pay and pay ratios as a source of new information that consumers respond to. However, some firms had reported aggregate labor costs before 2018, potentially reducing the salience of the initial pay ratio disclosure. For these firms, average employee pay could be estimated by dividing non-executive aggregate wages by the number of employees using data from 10-K filings. Although this estimate differs from median worker pay, it provides a reasonable approximation (Boone et al., 2024). As a result, the initial pay ratio disclosure might have revealed less new information for these firms, leading to a less pronounced consumer reaction.

To investigate this possibility, we examine whether consumer responses to the initial pay ratio disclosure differ based on whether firms had previously reported aggregate labor costs. Specifically, we decompose the *Treat* indicator in the baseline model into two groups: *Treat with prior disclosure*, which equals one for products from firms with high pay ratios that had already reported total labor costs before 2018, and zero otherwise; and *Treat without prior disclosure*, which equals one for products from firms with high pay ratios that had not reported total labor costs before 2018, and zero otherwise. A significant difference in consumer responses between these two groups would suggest that prior disclosure influences consumer reaction to the newly reported ratios. Conversely, similar responses between the two groups would imply that, despite the availability of worker pay information before the rule, consumers still respond to the initial pay ratio disclosure due to its salience.

Table 6 shows that the coefficient estimates on both *Treat with prior disclosure* and *Treat without prior disclosure* are negative and significant at the 1% level, with no statistically significant difference between the two. Thus, prior wage information does not attenuate the negative consumer reaction to high pay ratios. Although average worker pay and the associated pay ratio could have been inferred before the disclosure, consumers still react to the reported ratios. One possible explanation is that the ex-ante costs of acquiring and processing information to infer pay ratios may have been too high for consumers relative to the expected benefits. The 2018 initial disclosures increased the salience of the reported pay ratios, drawing significant consumer attention to the widening pay dispersion within firms for the first time.

In a similar context, Pan et al. (2022) document comparable market reaction to pay ratios for firms with and without prior disclosure of total labor costs. Boone et al. (2024) also find that prior labor cost disclosures do not lessen employee responses to pay ratios. If investors and employees are not sensitive to prior disclosures of staff expenses, it is not surprising that consumers are not either. Consequently, when pay ratios were first disclosed in 2018, they represented salient information that elicited a strong consumer reaction.

4.2.3. The effect of firm-brand name similarity

To further address concerns that our results primarily reflect changes in local economic conditions during the period of pay ratio disclosures, we investigate the impact of firm-brand name similarity. If pay ratio disclosures capture consumer attention and provoke negative responses, we would expect a greater decline in consumer purchases when the brand name closely resembles the firm name. In contrast, if the observed negative effect of pay disparity is driven by local economic conditions or other unobservable macroeconomic factors, the similarity between firm and brand names should not have a significant impact.

Following the methodological approach of Noh et al. (2024), we measure the degree of similarity between the names of firms disclosing pay ratios and their associated brand names. Specifically, we use a fuzzy string-matching algorithm to calculate similarity scores between firm and brand names. Firm-brand pairs with similarity scores in the top quintile are classified as having high similarity. For example, Colgate-Palmolive has a high similarity score with its brand Colgate 360, whereas Designer Brands and its brand DSW do not. To ensure the accuracy of these classifications, we manually verify the brands with high similarity scores through company websites, brand logos, and Google searches. Finally, *High name similarity* is an indicator variable that equals one if the similarity score between the firm and brand names is in the top quintile among treated firms in our sample, and zero otherwise.

We estimate a modified version of Equation (2) that incorporates a triple interaction term between *Treat*, *Post* and *High name similarity*. The effects of other interaction terms and individual variables are subsumed by *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects. As shown in Table 7, the coefficient on *Treat* × *Post* × *High name* *similarity* is statistically significant and negative, indicating that the negative consumer reaction is more pronounced for products whose brands share a similar name with their manufacturing firms. This finding lends further credence that the results are driven by consumer responses to pay ratio disclosures rather than merely reflecting changes in the local economic environment.

4.2.4. Pay disparity and product recalls

Another possible explanation for our baseline results is that disclosing high pay ratios negatively impacts employee morale and productivity (Boone et al., 2024). Lower morale and productivity can lead to reduced product quality and a higher risk of product recalls, which in turn disrupt the supply chain and severely damage brand reputation. Consequently, these disruptions elicit negative consumer reaction, ultimately resulting in substantial declines in product consumption (De Matos and Vargas Rossi, 2007). To investigate this possibility, we examine whether high pay ratios are associated with an increased likelihood of product recalls.

The sample for this analysis includes all firms that disclosed their pay ratios during the initial reporting period of 2018. We track each firm for the quarter after the disclosure to determine whether they experienced any product recalls. Following prior studies (Kini et al., 2017; Kini et al., 2022; Chen et al., 2024), we manually collect product recall data from four U.S. regulatory agencies: the Consumer Product Safety Commission (CPSC), the Food and Drug Administration (FDA), the National Highway Traffic Safety Administration (NHTSA), and the U.S. Coast Guard (USCG). The FDA provides information on recalls of food, drugs, and medical devices, while the CPSC focuses on consumer product recalls, the NHTSA on automobile recalls, and the USCG on boat-related recalls. Each recall announcement includes details such as the recalled product, manufacturer, recall volume, and recall date.

To test whether initial disclosures of pay ratios influence product recalls, we estimate the following cross-sectional model at the firm level:

$$Product\ recall_i = \alpha + \beta\ LN\ Pay\ ratio_i + \gamma\ Control_i + Industry + \varepsilon_i \tag{3}$$

In this model, *Product recall* is an indicator variable that equals one if a product recall occurs for a firm in the quarter following its pay ratio disclosure in 2018, and zero otherwise. *LN Pay ratio* is the natural logarithm of one plus the pay ratio reported in the firm’s proxy statement. *Control* represents a set of firm-specific variables that may influence firm outcomes, including firm size, performance, growth prospects, and leverage. We also include industry fixed effects to account for unobserved heterogeneity across industries. The results in Table 8 indicate that the disclosed pay ratios do not significantly influence subsequent product recalls, suggesting that product recalls are not a factor in explaining our findings.

4.2.5. Falsification tests

The staggered implementation of mandated disclosures across firms with different fiscal year-ends allows us to isolate the effect of these disclosures from broader economic factors. To ensure our results are not driven by unobserved omitted variables coinciding with the staggered disclosures, we perform two falsification tests. These tests involve using either fictitious event times or randomly assigned treated products. If high pay ratio disclosures truly lead to reduced consumption, the effect should only appear for products from high-pay-ratio firms and only after the actual disclosure. No effect should be observed for products from firms without high pay ratios or before the disclosure.

In the first test, we use fictitious event times. Specifically, we take the actual treated products and their event months from 2018, then shift the event month one year earlier to the same month in 2017. We apply the same criteria to identify control products and repeat the baseline DiD regressions using this pseudo-event time. In the second test, we randomly assign treated products, ensuring the same number of treated products each month throughout 2018 as in our original sample. Again, we identify control products following the same procedures and repeat the baseline DiD analysis. The results, presented in Table 9, show that both falsification tests yield insignificant DiD estimators. These findings enhance our confidence that the observed effects are not attributable to omitted variables correlated with the staggered disclosures or to random chance.

5. Additional analyses

The results thus far provide robust evidence that consumers reduced their consumption of products from high pay ratio firms following the initial disclosures in 2018. In this section, we delve deeper into the potential mechanisms through which reported pay ratios affect consumer purchases to further corroborate our main inferences. According to the traditional model of consumer behavior (Kotler and Keller 2012; Kotler et al., 2017), the impact of pay ratios on purchasing decisions hinges on two key factors: (1) how pay ratio information shapes consumer perceptions, and (2) the extent of consumer exposure to this information.

We conduct a series of cross-sectional tests to investigate these factors. First, we provide evidence that pay ratio information influences consumer perceptions through channels related to social preferences and consumer trust. Second, we show that the negative consumer reaction intensifies with increased exposure to pay ratio information.

5.1. Social preferences

Pay ratio disclosures can significantly influence consumer perceptions because firms with large pay disparities are often perceived as socially irresponsible, leading to consumer disapproval. Research in psychology and economics has consistently shown that individuals have a strong psychological aversion to inequity and a preference for fairness (Akerlof, 1980; Dawes et al., 2007; Blake et al., 2015; Sheskin et al., 2016). In our context, when a firm reports a high pay ratio, it can reduce the desirability of its products by negatively affecting consumers' perceptions of fairness and equity (Trevor and Wazeter, 2006; Besharat et al., 2024). As a result, inequality-averse consumers would be reluctant to purchase products from high pay ratio firms, viewing them as emblematic of broader societal income inequality.

To substantiate these arguments, we examine whether consumer responses to high pay ratios vary according to local attitudes toward income inequality. If consumers' social preferences drive the observed relation, we would expect a stronger negative reaction in areas with greater aversion to inequality. Following previous studies (Pan et al., 2022; Hoi et al., 2019), we employ four measures to capture local residents' aversion to inequality.

The first two measures are based on the notion that inequality aversion is reflected in support for redistributive policies aimed at reducing income inequality, such as progressive taxation or minimum wages (Fehr et al., 2022; Kerschbamer and Müller, 2020). Accordingly, we use the progressivity of state income taxes—the difference between the maximum and minimum individual income tax rates—and the state's minimum wage as proxies for local inequality aversion. Specifically, *Minimum wage* refers to the minimum hourly wage (in dollars) in the state where the household is located,[10] while *Tax diff* represents the difference between the state's maximum and minimum personal income tax rates.[11]

Our third measure exploits the idea that inequality aversion is correlated with political views. Pan et al. (2022) suggest that, in the US, the Democratic Party aligns more closely with a Rawlsian view that redistribution enhances social justice, while the Republican Party leans toward a libertarian view that market outcomes are generally fair. Therefore, we use the degree of support for the Democratic Party in a county as an additional proxy for the revealed

[10] We obtain minimum wage data from the US Department of Labor, accessible at: https://www.dol.gov/agencies/whd/state/minimum-wage/history.
[11] We collect income tax data from the Tax Foundation. For states with no income tax, this variable is set to zero.

inequality aversion. *Democratic* denotes the percentage of votes the Democratic Party received in the 2016 Presidential Election in the county where the household is located.[12]

Our fourth measure pertains to local social capital. Areas with high social capital are less likely to tolerate income inequality, given their dense social networks and the prescribed values associated with cooperative norms (Hoi et al., 2019). Following Hasan et al. (2017) and Hoi et al. (2019), we construct *Social capital* using the first principal component from a factor analysis based on four factors that capture cooperative norms and social networks in U.S. counties: *Pvote*, which is voter turnouts in presidential elections; *Respn*, response rates in U.S. census surveys; *Nccs*, total numbers of nonprofit organizations; and *Assn*, total number of ten types of social organizations.[13] For all four inequality aversion measures, higher values indicate greater aversion to inequality.

Table 10 augments the baseline model by introducing triple interaction terms to assess the effect of local inequality aversion on negative consumer responses. Each specification includes one of the following interaction terms: *Treat* × *Post* × *Minimum wage*, *Treat* × *Post* × *Tax diff*, *Treat* × *Post* × *Democratic*, or *Treat* × *Post* × *Social capital*. Again, the effects of other interaction terms and individual variables are absorbed by the *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects. Across all measures of local inequality aversion, the coefficients on the triple interaction terms are negative and significant. These results suggest that the negative consumer reaction to high pay ratios is stronger in areas with greater aversion to inequality, pointing to an important role of consumers' social preferences.

5.2. Consumer trust

Alternatively, pay ratio disclosures may affect consumer trust in a firm and its products, ultimately influencing product consumption. Like investors, consumers face information asymmetry and rely on certain cues to assess a firm's trustworthiness (Boulding and Kirmani, 1993; Conyon, 2014). Krause et al. (2016) suggest that consumers increasingly use information about a firm's behavior and leadership to guide their purchasing decisions. Edelman's Trust Barometer (2021) further reveals that both firm and CEO actions significantly impact consumer trust.[14] Notably, consumers believe that a CEO's most important role is "to build trust," ranking

[12] We collect election data from the MIT Election Data and Science Lab.

[13] We construct the social capital index using data from several sources. We obtain voter turnout rate data from the MIT Election Data and Science Lab, census response rate data from the US Census Bureau, and data on the total numbers of ten types of social organizations from the County Business Patterns. Additionally, we collect data on the total numbers of non-profit organizations from the National Center for Charitable Statistics.

[14] Edelman. (2021). *Edelman trust barometer*. https://www.edelman.com/sites/g/files/aatuss191/files/2021-03/2021%20Edelman%20Trust%20Barometer.pdf.

this attribute above "producing high-quality products and services," or "increasing profits and stock price" (Edelman, 2018).[15] Moreover, a majority of consumers (63%) consider trust essential in their purchasing choices, stating, "unless I come to trust the company behind the product, I will soon stop buying it" (Edelman, 2018).

Given its high visibility and prominence, the initial pay ratio disclosure can serve as a signal that conveys information about a firm's trustworthiness. Specifically, a high pay ratio may lead consumers to perceive that the firm is using its resources in a self-serving manner, rather than prioritizing consumer welfare (Besharat et al., 2024). As a result, consumer trust erodes, leading to a decline in product consumption after the disclosure of high pay ratios. This trust-related perspective yields two predictions: (1) consumer trust is more critical for economically significant or high-value purchases, and (2) trust becomes more important when a product's attributes are not easily observable. For example, when purchasing "organic" meat products, verifying their authenticity is challenging, making the manufacturer's trustworthiness a key consideration.

Table 11 tests these predictions by investigating whether negative consumer responses to high pay ratios varies with two product characteristics. *High value products* is an indicator variable that equals one if the product price exceeds the sample median and zero otherwise. *Experience products* is an indicator variable that equals one if the product belongs to the following product departments: health and beauty aids, frozen foods, packaged meat, non-food grocery, and general merchandise; otherwise, it equals zero. These products can only be accurately evaluated after purchase. In contrast, search products, including dry grocery, dairy, deli, fresh produce, and alcoholic beverages, can be assessed before purchase through prior experience or direct inspection. The results show that the coefficients on the triple interaction terms, *Treat* × *Post* × *High value products* and *Treat* × *Post* × *Experience products*, are negative and significant, consistent with the idea that the reported pay ratio, as a signal of trustworthiness, is more relevant for high-value or hard-to-very products.

5.3. Consumer awareness

For consumers to react to pay ratio disclosures, they need to be aware of them. While we cannot directly observe customer awareness, we use various measures to capture variation in their potential exposure to these disclosures. We then conduct three sets of tests to examine whether increased exposure is associated with a more pronounced customer reaction.

[15] Edelman. (2018). *Edelman trust barometer*. https://www.edelman.com/trust/2018-trust-barometer.

The first set of tests employs two measures based on media coverage. High pay ratios that receive media coverage tend to attract more consumer attention and provoke a stronger reaction. *Newspaper* is set to one if at least one newspaper article discusses the firm's pay ratio in the month following its initial disclosure, and zero otherwise.[16] *Twitter* is set to one if at least one tweet mentions the firm's pay ratio in the month following its initial disclosure, and zero otherwise. As reported in Panel A of Table 12, the coefficients on the triple interaction terms, *Treat* × *Post* × *Newspaper* and *Treat* × *Post* × *Twitter*, are negative and significant, suggesting that media coverage amplifies negative consumer responses. These findings highlight the important role of both traditional and social media in disseminating pay ratio information.

The second set of tests exploits the fact that pay ratios reported by more prominent firms typically attract greater media and public attention, making them more salient to customers. To assess a firm's prominence or visibility, we construct four measures. *High brand value* is set to one if the product is manufactured by a firm included in the "America's Top 500 Brands" list, and zero otherwise.[17] *Admired* is set to one if the product is manufactured by a firm included in Fortune's "America's Most Admired Companies" list, and zero otherwise.[18] *High Google search index* is set to one if the manufacturing firm's average Google search volume over the three days surrounding the pay ratio disclosure date exceeds the sample median, and zero otherwise.[19] *High advertising intensity* is set to one if the manufacturing firm's advertising intensity—calculated as advertising expenses divided by sales—exceeds the sample median, and zero otherwise.[20] Panel B of Table 12 indicates that, regardless of the measure used, the coefficients on the triple interaction terms, *Treat* ×*Post* × *High brand value*, *Treat* ×*Post* × *Admired*, *Treat* × *Post* × *High Google search index*, and *Treat* × *Post*× *High advertising intensity*, are negative and significant, consistent with our conjecturer that more prominent firms are better able to attract consumer attention and elicit a stronger reaction.

The third set of tests examines how households' access to information affects consumer reaction. Households with better access to information are more likely to learn about pay ratios and respond to them. To test this conjecture, we create two measures of information access. *County with NP* is set to one if the household's county has at least one local newspaper and zero otherwise.[21] *Household with internet access* is set to one if the household has internet

[16] The data on newspaper articles are collected from Factiva and Google News.
[17] The "America's Top 500 Brands" list is sourced from Brand Finance, accessible at: https://brandfinance.com/.
[18] The "America's Most Admired Companies" list is sourced from Fortune, accessible at: https://fortune.com/.
[19] The Google search index is normalized for each firm and ranges from 0 to 100. Data on the Google search index are collected from Google Trends.
[20] Data on advertising expenses and sales are obtained from Compustat.
[21] The county-level newspaper data are collected from the Editor and Publisher Yearbook.

access at home and zero otherwise.[22] Panel C of Table 12 show that the coefficients on the triple interaction terms, *Treat* × *Post* × *County with NP* and *Treat* × *Post* × *Household with internet access*, are negative and significant, highlighting the importance of information access in fostering consumer reaction.

6. Conclusion

This paper investigates how consumers react to within-firm pay disparities, drawing on detailed household purchase data. Our analysis leverages a new disclosure rule that requires public firms in the United States to report the ratio of CEO pay to the median worker pay for the first time in 2018. We find that firms reporting high pay ratios experience a notable decline in consumer purchases. Specifically, sales of these firms' products dropped by 4.9% on average compared to similar products from firms without high pay ratios, purchased by the same households. This decline is demand-driven, with prices remain relatively stable, and the effect becomes significant only after the disclosure. Furthermore, the negative effect of pay inequality is independent of prior wage information and is not driven by factors such as CEO or worker pay levels, local economic conditions, or disruptions associated with product recalls. Our falsification tests also help mitigate concerns that the observed effects of mandated disclosures are driven by omitted variables or random chance.

We further conduct supplementary analyses to explore the potential mechanisms through which reported pay ratios influence consumer purchases. The results reveal that the negative consumer reaction is more pronounced in areas with greater inequality aversion, for high-value or less verifiable products, and when potential exposure to pay ratio information is high. Overall, our findings indicate that consumers are indeed concerned about large within-firm pay disparities, driven by social preferences and a loss of trust, to the extent that they are aware of the relevant information. These insights highlight the important intersection between a firm's pay structure and consumer behavior. As the landscape of consumer expectations continues to evolve, firms that prioritize equitable pay practices will not only contribute to societal well-being but also gain a competitive advantage in an increasingly discerning marketplace.

[22] The household internet connection data are provided by the NielsenIQ Consumer Panel.

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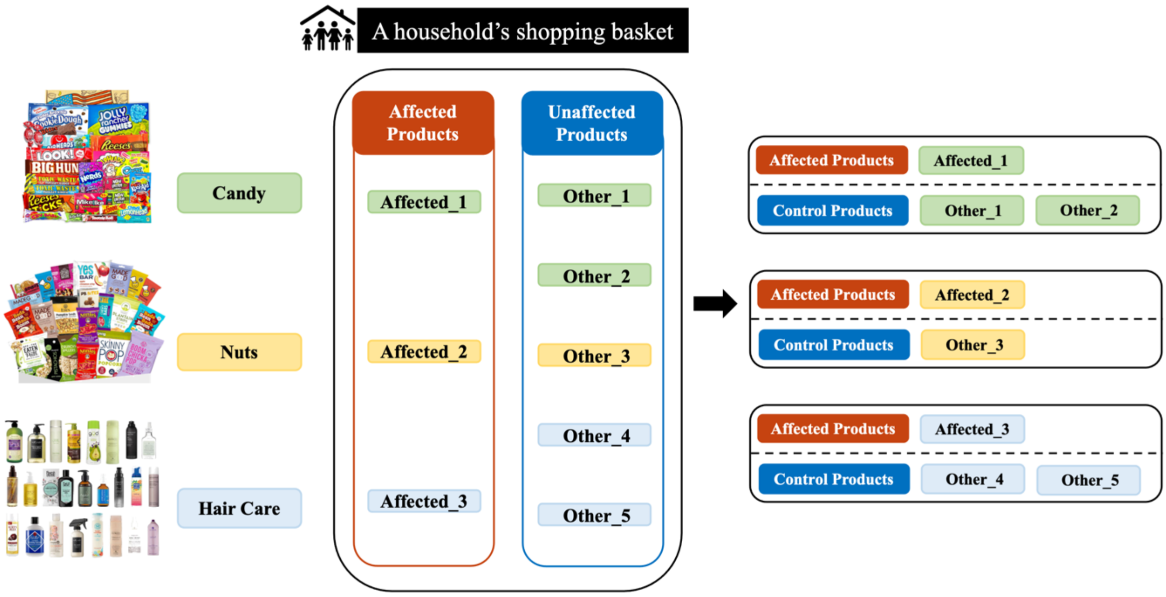
Figure 1. Conceptual framework of empirical strategy

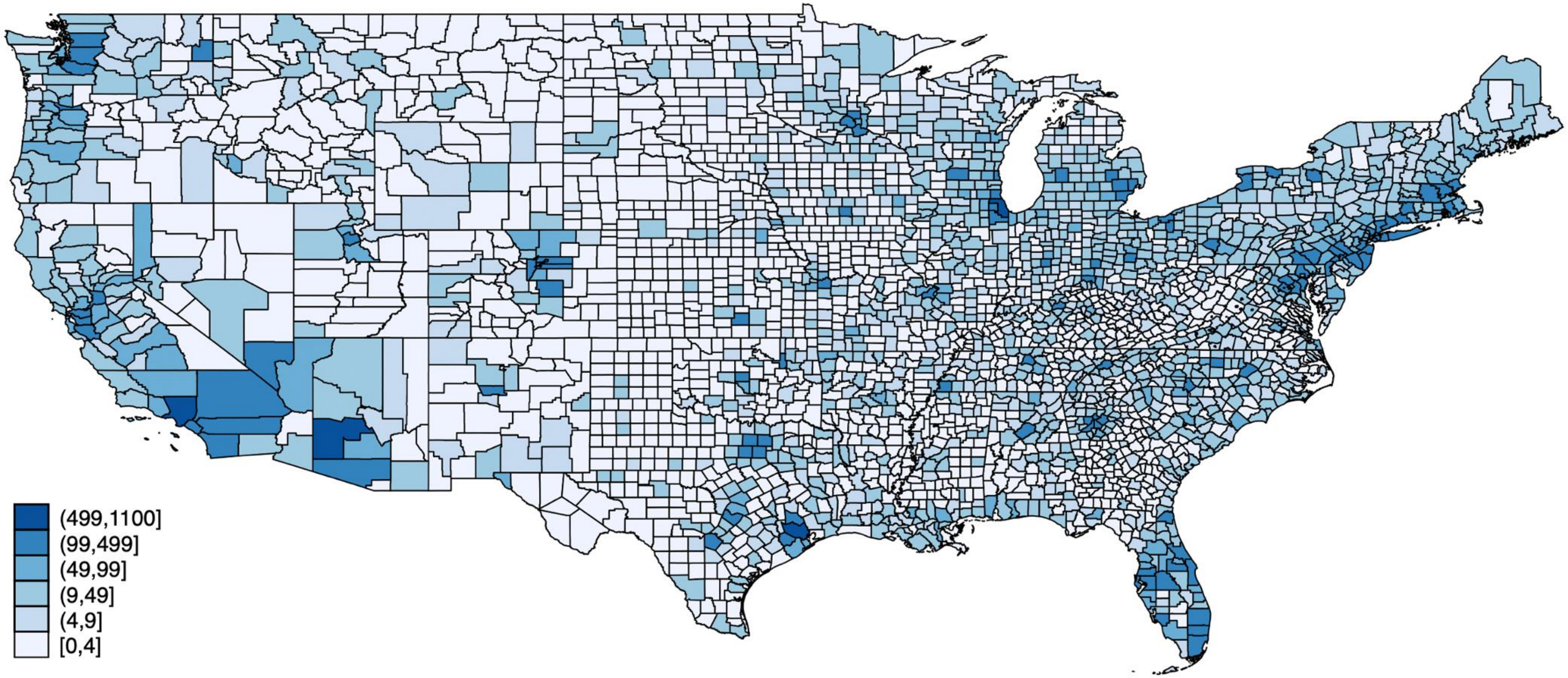


This figure visualizes the construction of treatment and control groups within the same household's shopping basket. Our empirical strategy is based on a within-household, within-product-group analysis. The control group consists of similar, never-treated products from the same product group, within the same household's shopping basket. To ensure comparability, we further restrict the control group to products with prices within ±20% of the unit price of the treated products.

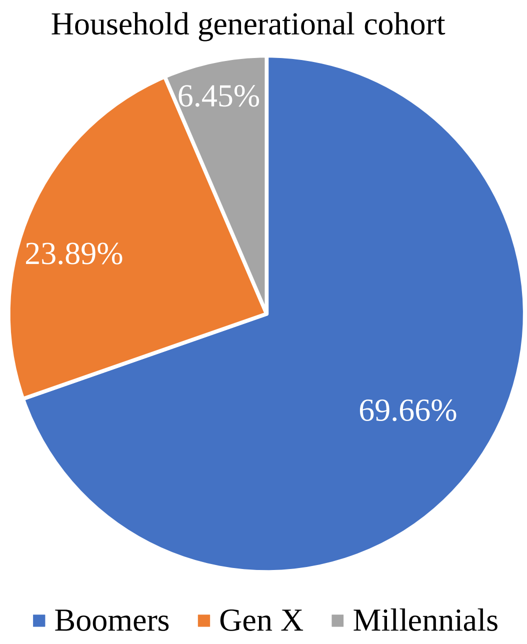
Figure 2. Distribution of households

A. By location

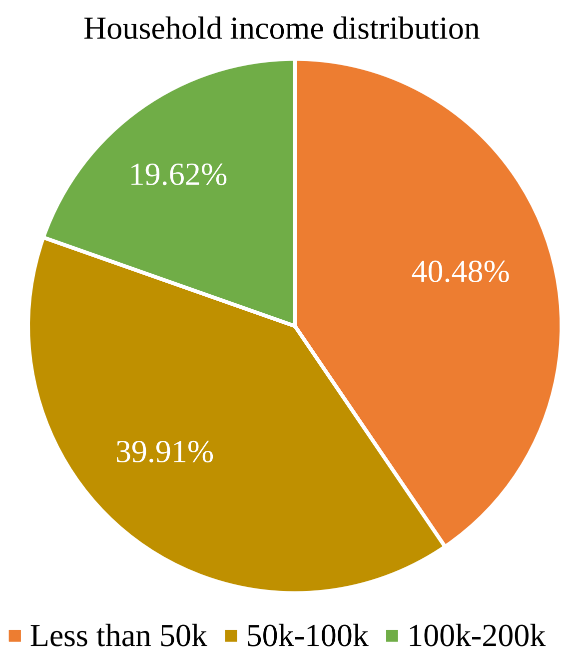
Geographic distribution of surveyed households



B. By age



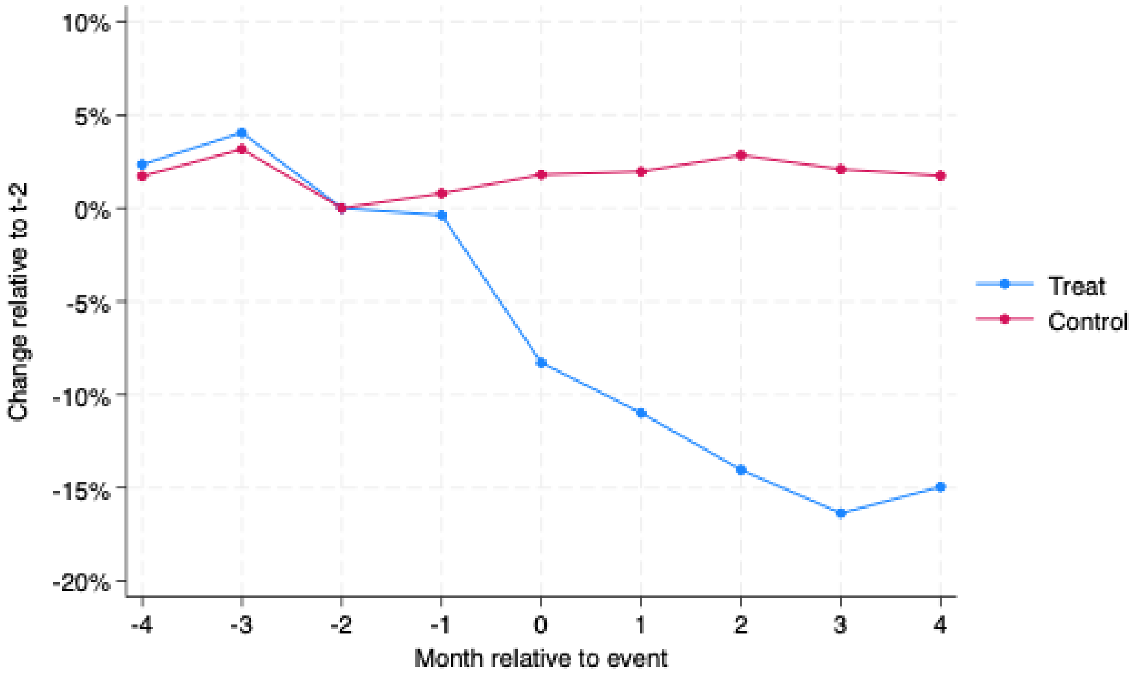
C. By income



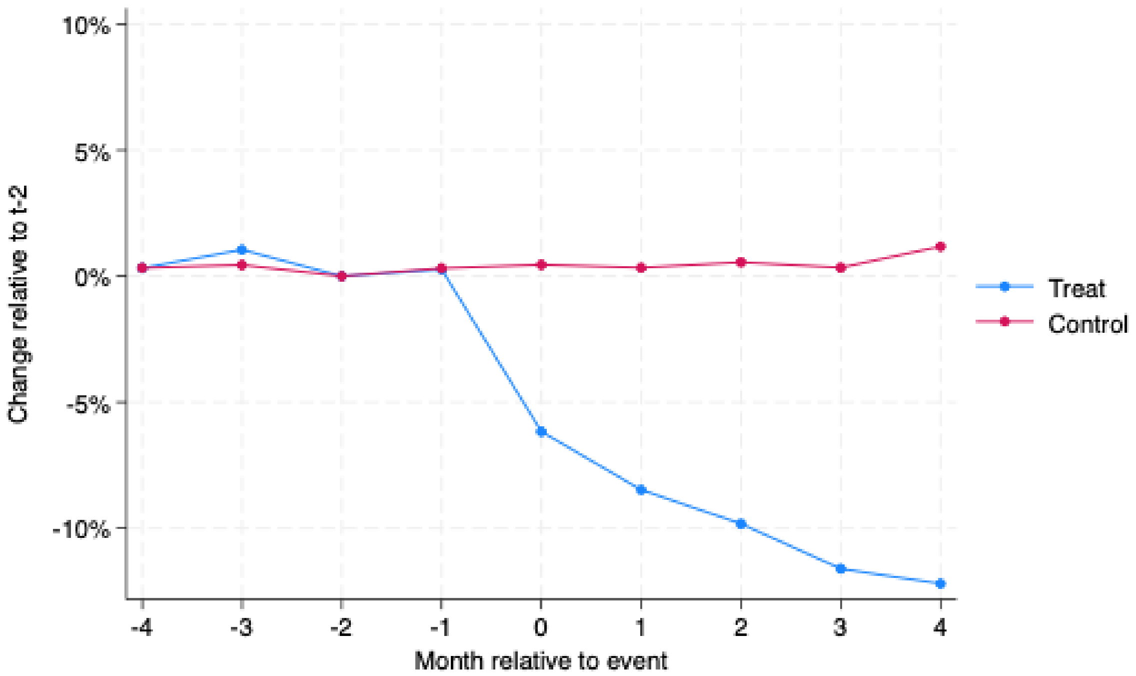
The figures illustrate the distribution of surveyed households by location, age, and income. Only households included in the treatment and control groups are considered. Figure A shows a heat map where darker blue areas indicate a larger number of surveyed households. Figure B illustrates the age distribution of household heads, with generations defined as follows: Boomers (born before 1964), Generation X (born between 1964 and 1980), and Millennials (born between 1981 and 1996). Figure C presents the income distribution of surveyed households, grouped into three categories: less than $50K, between $50K and $100K, and above $100K.

Figure 3. Trends in consumption for treated and control products

A. Consumption in terms of total spending



B. Consumption in terms of quantity purchased



The figures show the time-series variations in spending and quantity purchased for both treated and control products. Figure A depicts the percentage change in spending on these products from four months before to four months after the event, defined as the month in 2018 when a firm reports a pay ratio in the top quartile of all reported pay ratios up to that point. Figure B presents the same analysis as Figure A but focuses on the percentage change in quantity purchased instead of spending.

Table 1. Summary statistics

This table presents summary statistics for the pay ratio and its components as well as consumption measures used in our main analyses. In Panel A, *Pay ratio* is the ratio of total annual CEO compensation to total annual median worker pay, as disclosed in the firm's 2018 definitive proxy statement. *CEO pay* refers to the total annual CEO compensation, while *Worker pay* refers to the total annual median worker compensation, both reported in the same proxy statement. In Panel B, the observations are at the household-product-month level. *Total spending* represents the total amount a household spends on a specific product in a given month. *Units* indicates the quantity of a product consumed by a household within the same period. *Price* refers to the average price per unit. *LN Total spending*, *LN Units*, and *LN Price* are the logarithmic transformations, calculated as the natural logarithm of one plus the respective variable. The table reports the 10th percentile, 90th percentile, median, mean, and standard deviation for each variable.

Panel A. Summary statistics for pay ratio and its components

Variable	N	Mean	Std. dev.	25th	Median	75th
Pay ratio	2,285	124.721	309.288	27.500	60.000	130.000
CEO pay (in thousand $)	2,285	5,874.806	8,173.183	1,910.570	4,082.719	7,685.060
Worker pay (in thousand $)	2,285	80.453	64.591	43.507	62.593	97.982

Panel B. Summary statistics for consumption measures

Variables	Obs.	10th	Median	90th	Mean	Std. dev.
Total spending	19,709,658	0.000	0.000	2.980	0.868	3.574
Units	19,709,658	0.000	0.000	1.000	0.328	0.966
Price	19,662,291	0.990	2.480	5.815	3.086	2.914
LN Total spending	19,709,658	0.000	0.000	1.381	0.290	0.636
LN Units	19,709,658	0.000	0.000	0.693	0.179	0.387
LN Price	19,662,291	0.688	1.247	1.919	1.261	0.503

Table 2. Product categorization structure in NielsenIQ Homescan Consumer Panel

The tables provide an overview of the product categorization structure in the NielsenIQ Homescan Consumer Panel. Panel A lists the number of product groups in each department and the distribution of products purchased by surveyed households in our sample. Panel B details the product modules within the "Candy" group of the "Dry Grocery" department.

Panel A. List of departments

Description	Number of product groups	Distribution of products
Health and beauty care	18 (e.g., baby needs, cosmetics, cough and cold remedies, deodorant)	5.90%
Dry grocery	38 (e.g., baby food, baking mixes, candy, cereal, coffee, condiments, crackers)	56.16%
Frozen foods	10 (e.g., ice, frozen baked goods, frozen vegetables)	7.19%
Dairy	10 (e.g., cheese, eggs, yogurt)	6.40%
Deli	1	2.02%
Packaged meat	2 (fresh meat and packaged meats)	4.71%
Fresh produce	1	6.05%
Non-food grocery	12 (e.g., detergent, disposable diapers, fresheners, and household cleaners)	9.63%
Alcoholic beverages	3 (beer, wine, liquor)	0.18%
General merchandise	22 (e.g., batteries and flashlights, cookware, shoes care)	1.77%

Panel B. Example of product modules within the "Candy" group

Module name
BREATH SWEETENERS
CANDY – CHOCOLATE
CANDY – CHOCOLATE – MINIATURES
CANDY – CHOCOLATE – SPECIAL
CANDY – DIETETIC – CHOCOLATE
CANDY – DIETETIC – NON – CHOCOLATE
CANDY – HARD ROLLED
CANDY – KITS
CANDY – LOLLIPOPS
CANDY – NON – CHOCOLATE
CANDY – NON – CHOCOLATE – MINIATURES
CONFECTIONERY PASTE
GIFT PACKAGE WITH CANDY OR GUM
MARSHMALLOWS
PERISHABLE CHOCOLATE CANDY
PERISHABLE MARSHMALLOW
PERISHABLE NONCHOCOLATE CANDY

Table 3. Within-firm pay disparity and product-level sales

This table examines the impact of disclosing high pay ratios on monthly product-level sales. Column 1 presents results using an estimation window that spans from four months before to four months after the high pay ratio disclosure event. The dependent variable, *LN Product sales,* represents the natural logarithm of one plus the total sales of a product in a given month. *Treat* is assigned a value of one for products from manufacturers that disclose high pay ratios, and zero otherwise. *Post* takes a value of one for each month in the five-month period following the high pay ratio disclosure (including the event month *t*), and zero otherwise. We include *Cohort* × *Month-Year* and *Cohort* × *Product* fixed effects to account for all variation in both *Treat* and *Post* across the specifications. In column 2, we repeat the analysis from column 1, using the same specification but excluding the event month from the estimation window. The standard errors reported in parentheses are heteroscedasticity-robust and clustered at the product level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	*LN Product sales* [*t*-4, *t*+4]	*LN Product sales* [*t*-4, *t*-1] & [*t*+1, *t*+4]
	(1)	(2)
Treat × *Post*	-0.180***	-0.203***
	(0.007)	(0.008)
Cohort × Month-year FE	Yes	Yes
Cohort × Product FE	Yes	Yes
Cluster by product	Yes	Yes
Observations	816,516	725,792
Adjusted R^2	0.838	0.835

Table 4. Within-firm pay disparity and consumer purchases

This table examines consumer reaction to high pay ratio disclosures. Panel A examines the effect of disclosing high pay ratios on monthly household purchases. The dependent variable, *LN Total spending*, is the natural logarithm of one plus the total amount a household spends on a specific product in a given month. *Treat* is set to one for products whose manufacturers disclose high pay ratios, and zero otherwise. *Post* is set to one for each month during the five-month period following the disclosure (including the event month *t*), and zero otherwise. Columns 1 and 2 present results using the estimation window with and without the event month, respectively. Panel B explores the time-series dynamics of consumer responses to high pay ratio disclosures by replacing *Treat* × *Post* with interactions between *Treat* and individual event time dummy variables that represent the months relative to the disclosure event. Specifically, Pre_{t-4}, Pre_{t-3}, and Pre_{t-2} are dummy variables for the three months preceding the event, while $Post_{t+1}$, $Post_{t+2}$, $Post_{t+3}$, and $Post_{t+4}$ denote the four months following the event. The month *t*-1 serves as the reference month, so the interaction between Pre_{t-1} and the treatment indicator is omitted. We also decompose total spending into unit purchases, denoted as *Units*, and price per unit, denoted as *Price*. Panel C redefines high pay ratios as those that exceed expected levels. $Treat_{unexpected}$ as an indicator variable that equals one for products associated with unexpectedly high pay ratios—when the computed *Unexpected pay ratio* falls within the highest quartile among all computed unexpected pay ratios up to the reporting month. *Unexpected pay ratio* represents the unexpected component of the pay ratio, calculated using the method developed by Rouen (2020) and Boone et al. (2024). Across all specifications, we include *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects to account for unobservable heterogeneity across event cohorts, households, and products. The standard errors, reported in parentheses, are heteroscedasticity-robust and clustered at the product and household × month-year levels. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Baseline results

Dependent variable	*LN Total spending* [*t*-4, *t*+4]	*LN Total spending* [*t*-4, *t*-1] & [*t*+1, *t*+4]
	(1)	(2)
Treat × *Post*	-0.049***	-0.048***
	(0.003)	(0.003)
Cohort × Month-year FE	Yes	Yes
Cohort × Household × Product FE	Yes	Yes
Cluster by Product	Yes	Yes
Cluster by Household × Month-year	Yes	Yes
Observations	19,709,658	17,519,696
Adjusted R^2	0.205	0.182

Panel B. Time-series dynamics of consumer reaction

Dependent variable	*LN Total spending*	*LN Units*	*LN Price*
	(1)	(2)	(3)
$Treat \times Pre_{t-4}$	0.006	0.002	-0.002
	(0.005)	(0.004)	(0.005)
$Treat \times Pre_{t-3}$	0.007	0.003	-0.000
	(0.007)	(0.004)	(0.005)
$Treat \times Pre_{t-2}$	-0.003	-0.002	0.005
	(0.004)	(0.002)	(0.004)
$Treat \times Post_{t}$	-0.049***	-0.018***	-0.006
	(0.004)	(0.002)	(0.005)
$Treat \times Post_{t+1}$	-0.049***	-0.017***	-0.003
	(0.004)	(0.002)	(0.005)
$Treat \times Post_{t+2}$	-0.045***	-0.014***	-0.003
	(0.004)	(0.002)	(0.005)
$Treat \times Post_{t+3}$	-0.044***	-0.012***	-0.008
	(0.004)	(0.003)	(0.005)
$Treat \times Post_{t+4}$	-0.044***	-0.012***	-0.001
	(0.004)	(0.002)	(0.005)
Cohort × Month-year FE	Yes	Yes	Yes
Cohort × Household × Product FE	Yes	Yes	Yes
Cluster by Product	Yes	Yes	Yes
Cluster by Household × Month-year	Yes	Yes	Yes
Observations	19,709,658	19,709,658	19,662,202
Adjusted R^2	0.205	0.231	0.909

Panel C. Unexpectedly high pay ratios

Dependent variable	*LN Total spending* [*t*-4, *t*+4]	*LN Total spending* [*t*-4, *t*-1] & [*t*+1, *t*+4]
	(1)	(2)
$Treat_{unexpected} \times Post$	-0.047***	-0.047***
	(0.003)	(0.003)
Cohort × Month-year FE	Yes	Yes
Cohort × Household × Product FE	Yes	Yes
Cluster by Product	Yes	Yes
Cluster by Household × Month-year	Yes	Yes
Observations	16,573,455	14,731,960
Adjusted R^2	0.206	0.183

Table 5. Pay disparity versus pay levels

This table examines whether CEO or median worker pay influences consumer responses to high pay ratios. The dependent variable, *LN Total spending*, is the natural logarithm of one plus the total amount a household spends on a specific product in a given month. In panel A, we divide *Treat* two categories: *Treat with high CEO pay*, which equals one for products from firms with high pay ratios and CEO pay above the median of treated firms in our sample, and zero otherwise; and *Treat with low CEO pay*, which equals one for products from firms with high pay ratios and CEO pay below the median of treated firms in our sample, and zero otherwise. Similarly, in Panel B, we divide *Treat* into two categories based on worker pay. *Treat with high worker pay* (*Treat with low worker pay*) is set to one for products from firms with high pay ratios and median worker pay above (below) the median of treated firms in our sample. The sample spans from four months before to four months after the disclosure event. *Post* is set to one for each month during the five-month period following the disclosure (including the event month *t*), and zero otherwise. We include *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects to account for unobservable heterogeneity across event cohorts, households, and products. The standard errors reported in parentheses are heteroscedasticity-robust and clustered at the product and household × month-year levels. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Consumer reaction and CEO Pay

Dependent variable	*LN Total spending*
	(1)
Treat with high CEO pay × *Post*	-0.050***
	(0.004)
Treat with low CEO pay × *Post*	-0.048***
	(0.003)
Equal treatment effects (*p*-value)?	
(*Treat with high CEO pay* - *Treat with low CEO pay*) × *Post*	0.751
Cohort × Month-year FE	Yes
Cohort × Household × Product FE	Yes
Cluster by Product	Yes
Cluster by Household × Month-year	Yes
Observations	19,709,658
Adjusted R^2	0.205

Panel B. Consumer reaction and median worker pay

Dependent variable	*LN Total spending*
	(1)
Treat with high worker pay × *Post*	-0.045***
	(0.004)
Treat with low worker pay × *Post*	-0.052***
	(0.004)
Equal treatment effects (*p*-value)?	
(*Treat with high worker pay* - *Treat with low worker pay*) × *Post*	0.241
Cohort × Month-year FE	Yes
Cohort × Household × Product FE	Yes
Cluster by Product	Yes
Cluster by Household × Month-year	Yes
Observations	19,709,658
Adjusted R^2	0.205

Table 6. Consumer reaction and prior wage information

This table examines whether the prior disclosure of total labor costs attenuates the negative consumer reaction to high pay ratio disclosures. The sample spans from four months before to four months after the disclosure event. The dependent variable, *LN Total spending*, is the natural logarithm of one plus the total amount a household spends on a specific product in a given month. *Post* is set to one for each month during the five-month period following the disclosure (including the event month *t*), and zero otherwise. We decompose the *Treat* indicator in the baseline model into two groups: *Treat with prior disclosure*, which equals one for products from firms with high pay ratios that had already reported total labor costs before 2018, and zero otherwise; and *Treat without prior disclosure*, which equals one for products from firms with high pay ratios that had not reported total labor costs before 2018, and zero otherwise. We include *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects to account for unobservable heterogeneity across event cohorts, households, and products. The standard errors reported in parentheses are heteroscedasticity-robust and clustered at the product and household × month-year levels. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	*LN Total spending*
	(1)
Treat with prior disclosure × *Post*	-0.048***
	(0.004)
Treat without prior disclosure × *Post*	-0.050***
	(0.003)
Equal treatment effects (*p*-value)?	
(*Treat with prior disclosure – Treat without prior disclosure*) × *Post*	0.644
Cohort × Month-year FE	Yes
Cohort × Household × Product FE	Yes
Cluster by Product	Yes
Cluster by Household × Month-year	Yes
Observations	19,709,658
Adjusted R^2	0.205

Table 7. Consumer reaction and firm-brand name similarity

This table investigates whether firm-brand name similarity affects consumer reaction to high pay ratio disclosures. The sample spans from four months before to four months after the disclosure event. The dependent variable, *LN Total spending,* is the natural logarithm of one plus the total amount a household spends on a specific product in a given month. *Treat* is set to one for products whose manufacturers disclose high pay ratios, and zero otherwise. *Post* is set to one for each month during the five-month period following the disclosure (including the event month *t*), and zero otherwise. *High name similarity* is an indicator variable that equals one if the similarity score between the firm and brand names is in the top quintile among treated firms in our sample, and zero otherwise, where the similarity score is calculated using a fuzzy string-matching algorithm. We include *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects to account for unobservable heterogeneity across event cohorts, households, and products. The standard errors reported in parentheses are heteroscedasticity-robust and clustered at the product and household × month-year levels. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	*LN Total spending*
	(1)
Treat × *Post* × *High name similarity*	-0.017**
	(0.008)
Treat × *Post*	-0.047***
	(0.003)
Cohort × Month-year FE	Yes
Cohort × Household × Product FE	Yes
Cluster by Product	Yes
Cluster by Household × Month-year	Yes
Observations	19,709,658
Adjusted R^2	0.205

Table 8. Within-firm pay disparity and product recalls

This table examines the relation between reported pay ratios and subsequent product recalls. The dependent variable, *Product recall*, is an indicator variable that equals one if a product recall occurs for a firm in the quarter following its pay ratio disclosure in 2018, and zero otherwise. *LN Pay ratio* is the natural logarithm of one plus the pay ratio reported in the firm's proxy statement. We control for a set of firm-specific variables that may influence firm outcomes, including firm size, accounting and stock performance, growth prospects, and leverage. Industry fixed effects, based on two-digit Standard Industrial Classification (SIC) codes, are included. The standard errors reported in parentheses are heteroscedasticity-robust and clustered at the industry level. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	*Product recall*
	(1)
LN Pay ratio	0.003
	(0.004)
Size	0.011**
	(0.006)
Book to market	-0.012
	(0.012)
ROA	0.033***
	(0.010)
Return	-0.000
	(0.000)
Leverage	-0.014
	(0.012)
Industry FE	Yes
Observations	2,005
Adjusted R^2	0.138

Table 9. Falsification tests

This table presents the results of falsification tests conducted using either fictitious event times or randomly assigned treated products. Panel A reports the results of tests using pseudo-event month-years. $Post_{pseudo}$ is set to one for each month in the five-month period following the pseudo-event month in 2017 (one year before the true event month in 2018), and zero otherwise. Panel B reports the results of tests based on randomly assigned treated products, where $Treat_{pseudo}$ is an indicator variable for these randomly assigned treated products.

Panel A. Falsification tests using pseudo-event month-years

Dependent variable	*LN Total spending* [*t*-4, *t*+4]	*LN Total spending* [*t*-4, *t*-1] & [*t*+1, *t*+4]
	(1)	(2)
$Treat \times Post_{pseudo}$	0.004	0.006
	(0.004)	(0.004)
Cohort × Month-year FE	Yes	Yes
Cohort × Household × Product FE	Yes	Yes
Cluster by Product	Yes	Yes
Cluster by Household × Month-year	Yes	Yes
Observations	15,046,712	13,363,276
Adjusted R^2	0.234	0.215

Panel B. Falsification tests based on randomly assigned treated products

Dependent variable	*LN Total spending* [*t*-4, *t*+4]	*LN Total spending* [*t*-4, *t*-1] & [*t*+1, *t*+4]
	(1)	(2)
$Treat_{pseudo} \times Post$	-0.001	0.000
	(0.001)	(0.001)
Cohort × Month-year FE	Yes	Yes
Cohort × Household × Product FE	Yes	Yes
Cluster by Product	Yes	Yes
Cluster by Household × Month-year	Yes	Yes
Observations	19,709,658	17,519,696
Adjusted R^2	0.205	0.182

Table 10. The role of inequality aversion

The table investigates how consumers' aversion to inequality affects their responses to high pay ratio disclosures. The sample spans from four months before to four months after the disclosure event. The dependent variable, *LN Total spending*, is the natural logarithm of one plus the total amount a household spends on a specific product in a given month. *Treat* is set to one for products whose manufacturers disclose high pay ratios, and zero otherwise. *Post* is set to one for each month during the five-month period following the disclosure (including the event month *t*), and zero otherwise. We use four measures to capture local inequality aversion. *Minimum wage* refers to the minimum hourly wage (in dollars) in the state where the household is located. *Tax diff* represents the difference between the state's maximum and minimum personal income tax rates. *Democratic* denotes the percentage of votes the Democratic Party received in the 2016 Presidential Election in the county where the household is located. *Social capital* is the social capital index constructed following Hasan et al. (2017) and Hoi et al. (2019). We include *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects to account for unobservable heterogeneity across event cohorts, households, and products. The standard errors reported in parentheses are heteroscedasticity-robust and clustered at the product and household × month-year levels. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	*LN Total spending*			
	(1)	(2)	(3)	(4)
Treat × *Post* × *Minimum wage*	-0.003***			
	(0.000)			
Treat × *Post* × *Tax diff*		-0.092***		
		(0.018)		
Treat × *Post* × *Democratic*			-0.021***	
			(0.004)	
Treat × *Post* × *Social capital*				-0.004***
				(0.001)
Treat × *Post*	-0.023***	-0.047***	-0.041***	-0.050***
	(0.005)	(0.003)	(0.003)	(0.003)
Cohort × Month-year FE	Yes	Yes	Yes	Yes
Cohort × Household × Product FE	Yes	Yes	Yes	Yes
Cluster by Product	Yes	Yes	Yes	Yes
Cluster by Household × Month-year	Yes	Yes	Yes	Yes
Observations	19,255,833	19,255,833	19,255,833	19,241,869
Adjusted R^2	0.211	0.211	0.211	0.211

Table 11. The role of consumer trust

This table explores the role of consumer trust by examining whether consumer reaction is stronger for high-value purchases and products with unobservable attributes. The sample spans from four months before to four months after the disclosure event. The dependent variable, *LN Total spending*, is the natural logarithm of one plus the total amount a household spends on a specific product in a given month. *Treat* is set to one for products whose manufacturers disclose high pay ratios, and zero otherwise. *Post* is set to one for each month during the five-month period following the disclosure (including the event month *t*), and zero otherwise. *High value products* is an indicator variable that equals one if the product price exceeds the sample median and zero otherwise. *Experience products* is an indicator variable that equals one if the product belongs to the following product departments: health and beauty aids, frozen foods, packaged meat, non-food grocery, and general merchandise; otherwise, it equals zero. We include *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects to account for unobservable heterogeneity across event cohorts, households, and products. The standard errors reported in parentheses are heteroscedasticity-robust and clustered at the product and household × month-year levels. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	*LN Total spending*	
	(1)	(2)
Treat × *Post*× *High value products*	-0.165***	
	(0.004)	
Treat × *Post* × *Experience products*		-0.077***
		(0.003)
Treat × *Post*	-0.013***	-0.025***
	(0.003)	(0.003)
Cohort × Month-year FE	Yes	Yes
Cohort × Household × Product FE	Yes	Yes
Cluster by Product	Yes	Yes
Cluster by Household × Month-year	Yes	Yes
Observations	19,709,658	19,709,658
Adjusted R^2	0.206	0.205

Table 12. The role of consumer awareness

This table investigates whether increased consumer awareness is associated with more negative consumer reaction. The sample spans from four months before to four months after the disclosure event. The dependent variable, *LN Total spending*, is the natural logarithm of one plus the total amount a household spends on a specific product in a given month. *Treat* is set to one for products whose manufacturers disclose high pay ratios, and zero otherwise. *Post* is set to one for each month during the five-month period following the disclosure (including the event month *t*), and zero otherwise. We creases three sets of measures to capture the extent of customer awareness. Panel A exploits variation in media coverage. *Newspaper* is set to one if at least one newspaper article discusses the firm's pay ratio in the month following its initial disclosure, and zero otherwise. *Twitter* is set to one if at least one tweet mentions the firm's pay ratio in the month following its initial disclosure, and zero otherwise. Panel A unitizes variation in firm visibility. *High brand value* is set to one if the product is manufactured by a firm included in the "America's Top 500 Brands" list, and zero otherwise. *Admired* is set to one if the product is manufactured by a firm included in Fortune's "America's Most Admired Companies" list, and zero otherwise. *High Google search index* is set to one if the manufacturing firm's average Google search volume over the three days surrounding the pay ratio disclosure date exceeds the sample median, and zero otherwise. *High advertising intensity* is set to one if the manufacturing firm's advertising intensity—calculated as advertising expenses divided by sales—exceeds the sample median, and zero otherwise. Panel C focuses on consumers' information access. *County with NP* is set to one if the household's county has at least one local newspaper and zero otherwise. *Household with internet access* is set to one if the household has internet access at home and zero otherwise. We include *Cohort* × *Month-year* and *Cohort* × *Household* × *Product* fixed effects to account for unobservable heterogeneity across event cohorts, households, and products. The standard errors reported in parentheses are heteroscedasticity-robust and clustered at the product and household × month-year levels. ∗∗∗, ∗∗, and ∗ indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Consumer reaction and media coverage

Dependent variable	*LN Total spending*	
	(1)	(2)
Treat × *Post* × *Newspaper*	-0.034***	
	(0.005)	
Treat × *Post* × *Twitter*		-0.042**
		(0.017)
Treat × *Post*	-0.021***	-0.049***
	(0.004)	(0.003)
Cohort × Month-year FE	Yes	Yes
Cohort × Household × Product FE	Yes	Yes
Cluster by Product	Yes	Yes
Cluster by Household × Month-year	Yes	Yes
Observations	19,709,658	19,709,658
Adjusted R^2	0.205	0.205

Panel B. Consumer reaction and firm visibility

Dependent variable	*LN Total spending*			
	(1)	(2)	(3)	(4)
Treat ×Post × High brand value	-0.033***			
	(0.005)			
Treat × Post × Admired		-0.031***		
		(0.005)		
Treat × Post × High Google search index			-0.018***	
			(0.005)	
Treat × Post× High advertising intensity				-0.009*
				(0.005)
Treat × Post	-0.021***	-0.028***	-0.041***	-0.044***
	(0.005)	(0.004)	(0.003)	(0.004)
Cohort × Month-year FE	Yes	Yes	Yes	Yes
Cohort × Household × Product FE	Yes	Yes	Yes	Yes
Cluster by Product	Yes	Yes	Yes	Yes
Cluster by Household × Month-year	Yes	Yes	Yes	Yes
Observations	19,709,658	19,709,658	19,709,658	19,709,658
Adjusted R^2	0.205	0.205	0.205	0.205

Panel C. Consumer reaction and information access

Dependent variable	*LN Total spending*	
	(1)	(2)
Treat × Post × County with NP	-0.002*	
	(0.001)	
Treat × Post × Household with internet access		-0.011***
		(0.002)
Treat × Post	-0.049***	-0.040***
	(0.003)	(0.003)
Cohort × Month-year FE	Yes	Yes
Cohort × Household × Product FE	Yes	Yes
Cluster by Product	Yes	Yes
Cluster by Household × Month-year	Yes	Yes
Observations	19,255,833	19,255,833
Adjusted R^2	0.211	0.211