Do Consumers Spend Their Money Where Their Votes Are? Evidence from Barcode-Level Sales Data[†‡]

Lei Chen[§] — *University of Melbourne*
Linxiang Ma[¶] — *University of Strathclyde*
Xiaoke Ye[*] — *University of Liverpool*

This draft: March 2025
First draft: February 2025

Abstract

We investigate the impact of ideological distance between firms and consumers on firm sales. Using granular retail scanner data, we find that, an average firm's annual sales can be up to $100,000 lower in the county where it has the largest ideological distance compared to the county with which it has the lowest ideological distance. This effect has intensified since the 2016 election, and is particularly pronounced in wealthier and more Republican-leaning counties. To understand the underlying mechanisms, we explore both demand- and supply-side factors. Leveraging the staggered entry of a conservative media and Donald Trump's 2016 campaign rallies as exogenous shocks to county-level ideology, we provide evidence that consumers prefer purchasing products from firms that align with their political beliefs. On the supply side, firms are less likely to enter politically distant markets but, once present, do not reduce product variability. Our findings highlight the cash flow consequences of political ideology and illustrate how firms may adapt their market strategies in response to ideological polarization.

JEL Classifications: D12, D22, G32, L11, P16,
Keywords: Political ideology, political polarization, consumption, cash flow, market strategy

[†]For helpful comments, we thank Muhan Hu and the seminar participants at Strathclyde. The usual disclaimer applies.

[‡]Researcher(s)' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

[§]Department of Finance, The University of Melbourne. Email: lei.chen3@student.unimelb.edu.au.
[¶]Department of Accounting & Finance, University of Strathclyde. Email: linxiang.ma@strath.ac.uk.
[*]Management School, University of Liverpool; Room 173, ULMS Grove, Chatham Street, Liverpool, L69 7ZH; +44(0)1517940748. Email: xiaoke.ye@liverpool.ac.uk.

1 Introduction

The U.S. has witnessed a deepening partisan divide over the last decades (Boxell et al., 2017; Flamino et al., 2023; Iyengar et al., 2019). Permeating various aspects of society (e.g., Huber and Malhotra, 2017; Iyengar et al., 2018), this trend also extends to the business realm (e.g, Duchin et al., 2024; Engelberg et al., 2023a; Kempf et al., 2024; Kempf and Tsoutsoura, 2024; Mccartney et al., 2024). While a growing body of research explores the impact of political polarization on firm-level outcomes (Colonnelli et al., 2023; Kempf and Tsoutsoura, 2021; Rice, 2023), its effects on corporate cash flows remain unclear. This paper examines this gap by assessing how partisan-driven shifts in retail consumer behavior influence firms' financial outcomes and shape their operating strategies.

Evidence from the 2024 YouGov US bipartisan brand survey highlights the role of political polarization in shaping consumer preferences. Over two-thirds of surveyed brands show a clear ideological skew, with some brands resonating strongly with either liberal or conservative consumers. For instance, Patagonia, known for its progressive environmental advocacy, is strongly associated with liberal-leaning consumers, with 17% of liberals considering purchasing it compared to 6.8% conservatives. Conversely, Wrangler is more popular among conservative consumers, with 12% more conservatives than liberals expressing interest in the brand.[1] While firms publicly deny tailoring their marketing strategies based on consumers' political af-

[1]For the full report, see: https://business.yougov.com/content/50424-us-bipartisan-brands-report-2024

fliction,[2] consumer purchasing patterns an implicit awareness of brands' perceived ideological positions (DellaPosta et al., 2015; Liaukonytė et al., 2023; Schoenmueller et al., 2023; Warren, 2019).

Building on this evidence of partisan-driven consumer behavior, we investigate how the ideological alignment between firms and local markets influences product sales. Specifically, we examine how the ideological distance between a firm and a county affects the sales volume of that firm's products in the county. To do so, we utilize product-level sales data from the NielsenIQ Retail Scanner Dataset, which provides detailed weekly sales information across the U.S. at a highly granular level. The extensive coverage of this dataset offers a unique opportunity to extract rich insights into the relationship between ideology and consumer behavior.

We measure the firm-county ideology distance through three steps. First, we determine a firm's ideology based on the proportion of employee donations to the Republican Party relative to total donations made to both parties in the previous presidential election cycle. Next, we assess a county's ideology as the fraction of votes cast for Republican candidates compared to the total votes for both parties' candidates in the previous election. Finally, we compute the ideology distance for each firm-county pair as the absolute difference between these two values.

Using a sample from 2007 to 2021, we find that a firm's ideology distance from a county is negatively related to the firm's sales in that county. Since the main source

[2]CEO of Kontoor Brands, the parent company of Wrangler, noted that "Our brands have not and will not employ commercial or marketing strategies based on consumers' political party affiliations, nor do we believe that those affiliations are any of our business."

of variation in our ideology measure arises from cross firm-county comparisons, we design our model to evaluate the sales of the same product produced by the same firm in the same year but sold in different counties. According to our baseline estimate, if a firm plans to sell the same product category in two counties next year, then in the county where its ideological distance is one standard deviation higher this year, sales of that product would be $\$5,700$ lower. When aggregated across all products, an average firm could expect to earn $\$42,000$ $\$64,000$ less in the county with the larger (i.e., one standard deviation higher) ideological gap. This means that, compared to counties where the firm's political belief is most aligned, its sales can be up to $\$100,000$ lower in counties with which it has the greatest ideological distance.

compared to the counties the firm has the closest political belief with, the firm's sales can be up to $\$100,000$ lower in counties it has the furthest ideological distance with.

Our results remain robust when using a range of alternative ideological distance measures and incorporating firm-level variables that may be correlated with both sales and ideology. Moreover, the granularity of our data enables us to explore different sources of variations by applying alternative fixed effects in our estimations. We find that the estimated negative relationship is not sensitive to these modifications. To account for potential correlations in sales within the same product category and within the same county, we cluster standard errors at the product and county levels in most of our analyses. We show that clustering at other levels does not influence the statistical significance of our estimates.

Next, we examine how this negative relationship varies over time. Through year-by-year regressions, we identify a growing negative relationship between firm-county ideology distance and sales, particularly since the 2016 presidential election. In terms of sales decline, the impact of a one standard deviation increase in firm-county ideology distance rose from about $\$2,000$ in 2012 to roughly $\$14,000$ in 2021. This pattern echos the increasing polarization observed across the country.

This relationship also differs across counties. First, we find that this relationship is more pronounced in counties with higher real income, higher employment rates, and higher educational attainment. We interpret these results as that more affluent consumers are better able to incorporate their political beliefs into their purchasing decisions. However, the fact that the estimates remain significantly negative even in less privileged counties highlights that ideology-driven consumption is a widespread phenomenon across the U.S. Second, we find that a county's political atmosphere also influences the relationship between ideology and consumption. Specifically, in swing counties — where the vote share for either party falls between 45% and 55% — the negative association between ideology distance and firm sales is 10% weaker. This result suggests that in less partisan counties, consumption behaviors are also less politically driven. Finally, when comparing Republican-leaning and Democratic-leaning counties, we find that the former exhibit stronger partisan consumption patterns.

We then analyze the potential mechanisms underlying the observed negative relationship between ideology distance and sales. On one hand, this relationship may stem from consumer choice — where purchasing decisions are influenced, at least in

part, by ideological alignment (demand channel). On the other hand, it may result from firm strategy, as companies may self-select into markets where their political beliefs align with local consumers (supply channel). In theory, both channels can coexist.

We leverage two exogenous shocks that arguably shift a county's political attitudes toward a more conservative stance. First, we examine the staggered entry of the Sinclair Broadcast Group into different counties, an event that has been shown to increase local residents' likelihood of supporting the Republican Party (Levendusky, 2022; Martin and Yurukoglu, 2017). Using a stacked difference-in-difference design, we show that in the years following Sinclair's entry and for Democratic-leaning counties, firms that experience an increase (decrease) in ideological distance see a decline (boost) in their sales. However, we do not observe a similar effect in Republican-leaning counties. In addition, employing a triple-difference type model, we show that in treated counties - regardless of their political stance - pro-Democratic firms experience an almost 10% drop in sales. In contrast, pro-Republican firms do not face the same negative impact. In fact, they see an increase in sales of about 6% in Democratic-leaning counties.

Although Sinclair's entry is a widely recognized ideology shock in studying ideology's impact in the US (Dasgupta et al., 2021; Pan et al., 2024; Yi, 2025), the long investigation timeline (at the annual level) may introduce confounding factors. For instance, Democratic-leaning firms may be less likely to advertise on Sinclair stations, a supply-side factor that could influence local sales. Leveraging the high granularity

of our data, we exploit a second shock, Donald Trump's 2016 political campaign, to examine the consequential short-term demand shifts. Trump rallies have been shown to produce both short- and long-term ideological effects (Enke, 2020; Grosjean et al., 2022), and we hypothesize that in counties hosting these rallies, sales of firms with different political leanings may be affected in distinct ways.

Our event study reveals that in counties hosting Trump's campaign rallies, Republican-leaning firms experience an increase in sales following the events. This effect begins to emerge within the first four weeks and peaks approximately two months after the rally. Our estimates indicate that a single Trump rally in a county can generate up to a $\$2,000$ increase in annual sales for an average Republican firm. In contrast, we find no statistically significant change in sales for Democratic-leaning firms following these rallies.

Finally, we investigate whether firms' ideology-driven strategies also contribute to the negative relationship between ideology distance and sales. We find evidence consistent with this channel through the extensive but not intensive margin. Specifically, we show that firms are less likely to sell products in counties they face a greater ideological distance. However, if they do enter, they do not reduce product variability in those counties.

This paper contributes to the growing literature on how partisan and ideological divisions shape firm outcomes. Extant work demonstrates that partisanship influences economic perceptions, which in turn can affect firms' investment decisions and cost of capital (Dagostino et al., 2023; Kempf and Tsoutsoura, 2021; Rice, 2023).

Additionally, both inter-organizational and intra-organizational political alignments play a role in shaping firm dynamics. For example, polarized media coverage can heighten investor disagreement, potentially limiting the stock market's efficiency in fairly valuing firms (Goldman et al., 2024; Luo et al., 2023). At the organizational level, ideological alignment among employees can significantly impact productivity and decision-making (Colonnelli et al., 2022; Duchin et al., 2024; Lee et al., 2014). While prior studies examine these broader implications, our paper specifically focuses on the cash flow channel of political polarization. By analyzing how ideological divisions influence firms' sales performance, this study provides novel insights into the tangible economic consequences of political polarization.

In addition, our work relates to the recent literature studying the interaction between stakeholders' political beliefs and firm strategies. Recent studies show that CEO activism can influence consumer choices (Hou and Poliquin, 2023; Jin et al., 2024), employee satisfaction (Anahit Mkrtchyan et al., 2024), and investor reactions (Brownen-Trinh and Orujov, 2023), among other outcomes. As a result, firms may adopt strategic actions to align with their stakeholders' preferences (Homroy and Gangopadhyay, 2023). We add to this literature by showing that firms strategically adjust their sales locations to enhance revenue.

Our paper also contributes to the broader literature on how political beliefs shape individual behavior. Ideology has been shown to influence the actions of various agents, including depositors (Jeung, 2021), judges (Ash et al., 2024), regulators (Engelberg et al., 2023b), and consumers (Hou and Poliquin, 2023; Qin et al., 2025). Our

study extends this literature by examining the financial consequences of ideological alignment between firms and consumers. Specifically, we quantify the impact of ideological misalignment on firms' cash flows, providing the first monetary estimate of how consumer ideology affects revenue.

This paper is particularly related to two recent endeavors on consumer political behavior. First, Hsieh et al. (2024) use a machine learning approach to document increasing partisan sorting in consumer purchases, leading to the segmentation of brands into 'red' and 'blue' products. While they focus on identifying this sorting mechanism, we quantify its financial impact by showing how ideological misalignment reduces firm sales. Second, Schoenmueller et al. (2023) examine how political polarization influences consumer brand preferences, showing that liberals exhibited greater brand-preference polarization after the 2016 election. In contrast, we analyze the broader economic consequences of ideological alignment, emphasizing its role in shaping firm revenues and strategic decisions.

The rest of the paper is organized as follows. The next section describes the data used in the analysis. section 3 shows the results, including the baseline regressions, robustness tests, and additional analyses. section 4 explores the demand and supply mechanisms. section 5 concludes.

2 Data

2.1 Retail Product Sales

We obtain product-level sale data from the NielsenIQ Retail Scanner Dataset, provided by the Kilts Center for Marketing at the University of Chicago. The retail scanner data which is collected from point-of-sale system in retail stores, is a comprehensive and granular data source. NielsenIQ data have been widely employed and accepted in research across economics, accounting, and finance, examining topics such as consumer purchasing behaviors, manufacturers' strategic decisions, product innovation, and stock market information efficiency (Argente et al., 2020, 2018; Dichev and Qian, 2022; Granja and Moreira, 2023; Meier et al., 2022).

This dataset comprises extensive retail scanner data that captures weekly sales, average prices, units sold and promotions of approximately 10.2 million products across a broad range of goods between 2006 and 2021. Each product in the dataset is identified by a unique 12-digit Universal Product Code (UPC), enabling precise tracking of sales trends at the barcode level. Additionally, the dataset supplements general sales information with numerous product-level attributes. The data encompass a vast network of retail outlets across the United States, providing a representative snapshot of the nationwide consumer market. They are collected from more than 54,000 distinct stores, including grocery, drug, mass merchandiser, and other retail establishments, spanning approximately 140 retail chains in 387 metropolitan statistical areas (MSAs) and 2,804 counties, identified by 5-digit state and county FIPS codes.

The dataset offers extensive coverage of the full range of products and the complete portfolio of companies within the consumer goods sector, enabling us to explore how market dynamics and economic trends are shaped by political ideologies and consumer behavior. NielsenIQ covers approximately $289 billion in retail sales annually, totaling around $4.91 trillion between 2006 and 2021. These sales represent a very large fraction of all retail sales in the United States (Argente et al., 2018).[3]

Nielsen categorizes each UPC within a structured hierarchy. Each UPC falls under one of 1,460 product modules, which are then sorted into 1,18 product groups. These groups are further consolidated into 10 product departments.[4] For instance, a box of Chocolate Creme Pie (UPC 032100082907) produced by Kraft Foods, Inc. (gvkey 142953) is categorized under the product module "Bakery - Pies - Frozen," within the product group "Baked Goods-Frozen," which is part of the "Frozen Foods" department.

To match each UPC to Compustat firm, we first obtain the producers' names for each UPC using the linking table provided by GS1 US, the official source of UPCs (Granja and Moreira, 2023). Each UPC is 12-digits long and includes a critical 6-9 digit company prefix issued by GS1 US. This prefix uniquely identifies the owner of a given UPC, and large companies with extensive product portfolios, such as *P&G*, often acquire more than one prefix. The *GS1 US Complete US Prefix List Active and Inactive* from GS1 US includes 691,565 UPC prefixes assigned to both US and non-US

[3]NielsenIQ claims that the database encompasses more than 50% of the total sales volume of US grocery and drug stores, and more than 30% of all US mass merchandiser sales volume.

[4]NielsenIQ completely changed the product hierarchy from 2021 onwards, but it continues to provide the product module code to enhance data comparability before and after 2021.

owners. Each record contains the owner's name, country, and address. Following Argente et al. (2020) and Granja and Moreira (2023), we match each UPC provided by NielsenIQ with prefixes obtained from GS1 US to collect the name of the producer in the US only. We successfully match 5,235,446 UPCs out of 10,264,206 total samples, 51% of the entire NielsenIQ.

Secondly, we fuzzy-match the standardized company names provided by GS1 US with the standardized historical company names reported by Compustat North America using a bigram matching method with a similarity score threshold of 0.9.[5] To maximize the pool of potential UPC owners, we additionally collect all company names from Orbis and Osiris to complement those from Compustat, as the same company may have multiple name variations.[6] We match Orbis and Osiris with Compustat using *cusip* and *cik* codes.

Thirdly, we perform two additional rounds of fuzzy matching: first, by matching the standardized company names from GS1 US with the unstandardized company names from Compustat, and second, by matching the unstandardized company names from GS1 US with the standardized company names from Compustat. After each round of fuzzy matching, we manually verify each name pair with a similarity score above 0.9 and retain only the matches we are confident are correct to minimize Type I (false positive) errors.

[5] We account for the possibility that firms may change names by collecting all historical company names from the Compustat history file *Comphist* in the fuzzy-matching process.

[6] For example, *The Procter & Gamble Company* may be listed as *PROCTER & GAMBLE CO*, *Procter & Gamble Co (The)*, or *P&G Wholesale*; all these names refer to the same company. Name standardization can eliminate some of these discrepancies, but it will not align *P&G Wholesale*, as provided by GS1 US, with its corresponding name in Compustat.

One limitation of using GS1 US data is that the UPC issuer only provides the UPC owner's name as recorded in the year the prefixes were originally purchased. As a result, we cannot track ownership changes for a given UPC if its original owner was later acquired by another company. Since our goal is to track UPC sales over time, it is essential to determine the historical ultimate ownership of each brand. We follow Arqué-Castells and Spulber (2022) and use SDC Platinum to collect M&A data, and obtain subsidiary ownership information from Orbis and Osiris to construct a dynamic corporate tree to track the change in UPC ownership. For example, *Whole Foods Market* owns UPC prefixes and was previously listed on the NASDAQ exchange. However, in 2017, it became a wholly owned subsidiary of *Amazon* and was no longer independently publicly traded. Simply relying on the name provided by GS1 US would omit all sales under the *365 by Whole Foods Market* label after 2017. To address this, we use the dynamic corporate tree to trace the ownership of *Whole Foods Market*'s prefixes, attributing these sales to *Whole Foods Market Inc.* (gvkey: 024893) before 2016 and to *Amazon.com Inc.* (gvkey: 064768) after 2017.[7]

We have successfully matched 811,181 unique UPCs to 1,202 Compustat firms. Our coverage of Compustat firms is more extensive than previous studies that used NielsenIQ, such as Argente et al. (2018) and Dichev and Qian (2022), for three reasons. First, NielsenIQ significantly expanded its store and product coverage in 2021, leading to greater data availability. Second, we incorporate additional names from supplementary data sources to improve matching accuracy. Third, we construct a dy-

[7]See the online supplemental material of Arqué-Castells and Spulber (2022) for a detailed method of constructing the corporate tree.

namic corporate tree to more comprehensively account for subsidiary relationships, ensuring continued coverage of prefixes acquired by other public companies.[8]

2.2 Ideology Distance

Our independent variable, political ideology distance, is measured at the firm-county level. Following the literature, we measure firm ideology using the donations made by its employees over the last presidential election cycle. We collect employee donation data from the Database on Ideology, Money in Politics, and Elections (DIME): Public version 4.0.[9] This dataset contains over 850 million itemized political contributions made by individuals and organizations to local, state, and federal elections from 1979 to 2024. DIME assigns unique identifiers to all individual and institutional donors, allowing for the tracking of contributions across election cycles and levels of government. Similarly, all candidates are assigned unique identifiers, enabling tracking when they run for different offices or districts. Recipient IDs can also be used to match against the database of contribution records.

Specifically, we begin by filtering out all contribution records made to presidential candidates from individuals with employer information between Cycle 2006 and Cycle 2022. To ensure consistency and improve the accuracy of our matching process,

[8]Unlike previous studies, we do not use firms' geographic information in the matching process because companies frequently change their names and headquarters locations, as recorded in the Compustat history file *Comphist*. After conducting several rounds of pilot fuzzy matching, we determined that relying solely on name-based matching, followed by manual verification of every name pair, produces the most accurate results.

[9]Bonica, Adam. 2024. Database on Ideology, Money in Politics, and Elections: Public version 4.0 [Computer file]. Stanford, CA: Stanford University Libraries. https://data.stanford.edu/dime

we first clean and standardize the format of employer names, addressing variations in spelling, abbreviations, and formatting inconsistencies. We then match these cleaned employer names with our cleaned sample firm names in a two-stage process.

In the first stage, we extract all exact matches between employer names in the contribution records and our sample firm names. This provides an initial set of matched records. In the second stage, to capture potential matches that may have slight discrepancies due to typographical errors, alternate spellings, or naming conventions, we employ the Levenshtein distance score for a fuzzy matching process. This method assigns a similarity score to each potential match, allowing us to systematically identify employer names that closely resemble those in our sample. To further ensure the accuracy of our matches, we manually review the results, verifying that contributions are correctly attributed to the intended firms.

Through this process, we successfully matched 1,476,743 employee contribution records from 1,107 firms over the sample period. Additionally, we identify the party affiliation of the contribution recipients by categorizing them into three distinct groups: the Democratic Party, the Republican Party, and others. Lastly, we aggregate the total contribution amounts from all employees within each firm to each political party at the firm-year level. This aggregation allows us to assess the overall political leanings of firms based on their employees' donation behavior year by year.

We define firm i's ideology in year t as the proportion of the total dollar amount donated to the Republican Party divided by the total amount given to both parties

over the last election cycle $-c$:

$$Ideology_{i,t} = \frac{\sum_{-c} \#Employee\ Contri.\ to\ Rep.}{\sum_{-c}(\#Employee\ Contri.\ to\ Rep. + \#Employee\ Contri.\ to\ Dem.)}$$

We measure the county-level ideology using the county-level election return of the presidential elections obtained from the MIT Election Lab. For county c in year t, we measure its ideology using the bi-party voting share to the Republican party in the previous election $-c$:

$$Ideology_{c,t} = \frac{\#Votes_{-c}^{Rep}}{\#Votes_{-c}^{Rep} + \#Votes_{-c}^{Dem}}$$

As illustrated in Figure 1, county-level ideology varies substantially both across regions and over time.

We then construct our explanatory variable, the ideology distance between firm i and county c in year t as the absolute value of the distance between the above two ideology measures:

$$Ideology\ Distance_{i,c,t} = |Ideology_{c,t} - Ideology_{i,t}|$$

This construction closely follows the literature in measuring ideology disagreement between two parties (Lee et al., 2014; Pan et al., 2024; Rice, 2023). In our sample, the average ideology distance is 0.334 with a standard deviation of 0.204. This measure also exhibits substantial time-series variation. During our sample period, the average

ideology distance was lowest in 2012, at the level of 0.301. Since then, it has gradually increased, peaking at 0.394 in 2020 (the final year of the ideology distance measure) — the highest level observed in our sample.

For robustness, we also use various alternative measures of ideology distance. Our main analysis uses the firm donation (county election) during the previous presidential election cycle to construct firm-level (county-level) ideology. While this selection helps avoid forward-looking bias, it could be less accurate than using the most recent election cycle. We then construct $Ideology\ Distance_{close}$ ($Ideology\ Distance_{countyclose}$) by using firm donation (county voting return) from the closest election cycle of any given year to mitigate this concern. Moreover, we construct two further ideology measures, $Ideology\ Distance_{roll}$ and $Ideology\ Distance_{ann}$, by using a rolling four-year window of donation preceding the focal year and annual donation of the focal year, to capture firm-level ideology more dynamically. These ideology distance measures are highly correlated with each other.

2.3 Other Data

We draw on several alternative sources for other data used in the paper. To control for county-level characteristics, we obtain population, GDP, and real income data from the U.S. Bureau of Economic Analysis, unemployment data from the USDA Economic Research Service, and house price data from the Federal Housing Finance Agency. We also obtain firm-level characteristics from WRDS Compustat database. We also get Sinclar Group's operation region data during our sample period from its

annual report, and detailed information on Donald Trump's presidential campaign rally in 2016 from the *Democracy in Action* website.[10]

Our analyses use samples aggregated at different levels. The summary statistics of these samples are presented in Table 1. Specifically, Panel A is the sample used for the main analysis. This sample includes 1,106 product categories produced by 614 firms being sold in 2,546 counties across 13 years. Panel B is a sub-sample of Panel A, focusing on the years immediately following each presidential election. Finally, Panel C aggregates data at the firm-county level, which we use for firm-level analyses and mechanism tests.

3 Political Leaning and Consumption

3.1 Baseline Analyses

We conduct our baseline analyses at the product-firm-county-year level. Specifically, for each product module produced by the same firm, we calculate the annual average dollar sales in the same calendar year from the same county. Using these data, we estimate the following regression:

$$\log Sales_{p,i,c,t} = \alpha + \beta Ideology\ Dis_{i,c,t-1} + \delta X_{c,t-1} + \gamma_{p,i,t} + \lambda_{s,t} + \varepsilon_{p,i,c,t} \quad (1)$$

[10] https://www.p2016.org/

where $\log Sales_{p,i,c,t}$ is the natural logarithm of average weekly dollar sales of product module p produced by firm i in county c and year t. Our main explanatory variable, $Ideology\ Dis_{i,c,t-1}$, is the ideology distance between firm i and county c in year $t-1$. Benefiting from the high granularity of our data, we include product $\times$ firm $\times$ year fixed effect, $\gamma_{p,i,t}$, which allows us to compare sales of the same product module by the same firm in the same year across different counties. We also include county-level population growth, GDP growth, income per capita growth, house price growth, and unemployment rate growth as control variables, and a state $\times$ year fixed effect to absorb state-specific trends.[11] We cluster the standard error at the product level to allow within-product sales correlation, and the county level to allow within-county sales correlation.

Table 2 Panel A shows the results. In column (1), we only include the product $\times$ firm $\times$ year fixed effect in the estimation. The result suggests that for the same product in the same year, a one standard deviation increase in the ideology distance (0.204) between the producing firm and the selling county is associated with a 40% decrease in this firm's sales. To alleviate the concern that there may be demographic changes in the sales location, we include a set of county-level controls and the state $\times$ year fixed effect. Column (4) produces estimates from this more comprehensive model. The association between ideology distance and sales is still statistically and economically significant. Specifically, a one standard deviation increase in the firm-

[11] We do not add county $\times$ year fixed effect as it will remove a substantial amount of the cross-county variations in our firm-county level ideology distance measure, 75% of whose variation comes from the between-county one. In the robustness check, we explore alternative fixed effect schemes, among which we also include the county $\times$ year fixed effect. The results are similar.

county ideology distance would reduce that firm's sales by 30%. As the between firm-county variation is about 75% of the overall variation in *Ideology Dis*$_{i,c,t-1}$, the majority of the estimated sales drop (22.5% out of the 30%) comes from the differences across firm-county. This estimate translates to about a $5,700 decrease in annual sales of an average product in that county, or a $7,700 decrease in annual sales of an average firm in that county.

Since our firm and county ideology measures are aggregated at the election cycle level, we expect most variation to happen in the years following each presidential election year. Hence, we repeat our estimation for those years and report the results in Panel B. The estimates are qualitatively similar and, consistent with our expectation, economically more pronounced. This pattern is further confirmed by Figure 2, which plots the estimated coefficients and their confidence intervals for every year in our sample.

Next, we decompose the estimates in Table 2 into units sold and price per unit. Using the after-election-year sample, we show that this lower level of sales associated with larger ideology distance comes from both lower number of products sold and lower price per unit. Specifically, using the most comprehensive specification, a one standard deviation increase in the firm-county ideology distance is related to a decrease in unit sales by 39% and a decrease in price per unit by 1.23%. We interpret these results as a smaller ideology distance between firm and county contributes to higher revenues through both higher consumer demand for the product at a given price and an increase in the product price. This impact through the product price

could suggest the presence of a supply-side channel, suggesting that a firm may offer different product mixes to counties based on whether it has a similar ideology with the county or not.

Our baseline results suggest that firms achieve higher sales in counties where they share similar political beliefs with consumers. This effect is driven by increases in both unit sales and price levels, indicating the presence of both demand and supply channels. These findings highlight the significance of political belief alignment between firms and consumers in influencing firms' cash flows and shaping consumers' choice sets and purchasing decisions.

3.2 Robustness Checks

We perform various robustness checks to ensure the validity of our results. We start by examining whether our results are sensitive to the construction of our ideology distance measure. Our main analysis calculates firm and county ideology based on the information over the last presidential election cycle. We prefer this method as it avoids the forward-looking bias. However, this benefit comes at the cost of not using up-to-date information. To mitigate this concern, we construct alternative measures using closer-to-focal-year information.

First, we alter the firm ideology measure by using the firm donation for the closest election cycle instead of the previous cycle[12] (while holding constant the county ide-

[12]For example, for the year 2011, now we are using the donations made for the 2012 presidential election cycle (2009-2012) instead of the 2008 election cycle (2005-2008).

ology measure) to re-construct the ideology distance measure (i.e., *Ideology Dis*$_{close}$). Second, we alter the county ideology measure by using the voting return from the closest instead of the previous election (holding constant the firm ideology measure) to construct *Ideology Dis*$_{countyclose}$. Table 4 Panel A Columns (1) and (4) show that the estimated negative associations using these alternative measures are similar to those in the main regression.

We further exploit the dynamic nature of employee donation to generate two further ideology distance measures. Specifically, we use the rolling sum of the previous four years' donations made by a firm's employees to measure this firm's ideology and construct *Ideology Dis*$_{roll}$, and employee donations made during the previous year to construct *Ideology Dis*$_{ann}$. Table 4 Panel A Columns (2) and (3) demonstrate that these alternative constructs of ideology distance do not change the estimates much.

We then examine whether model specification could influence our results. First, we control for a set of firm-level characteristics that might be correlated with its ideology and sales, including size, advertising expenditure, leverage, Tobin's Q, and investment. Table 4 Panel B column (1) shows that these additional controls further increase the magnitude of the estimated negative association.[13] Second, benefiting from the high granularity of our data, we are able to exploit various alternative variations for our estimation.

In the main analysis, we utilize the product $\times$ firm $\times$ year fixed effect to compare sales of the same product by the same firm in the same year across different counties.

[13]The number of observations drops substantially after we include these controls (about 29%). This is because advertising expenses are often missing.

An alternative channel would be to compare sales of the same product in the same county in the same year but by different firms by including the product × county × year fixed effect. However, as many counties have certain product modules only sold by one firm in a given year, using this fixed effect would unfortunately absorb too much variation in the data. To circumvent this caveat, we limit our attention to product-county-year combinations with at least two firms selling the same products and report the estimated relationship in Table 4 Panel B column (2).[14] This estimate suggests that if two firms are selling the same product in the same county at the same time, then the firm with one standard deviation higher firm-county ideology distance than the other firm last year would have a 13% lower sales volume.

In Column (3) of Table 4 Panel B, we include county × year fixed effect to control for time-varying factors that affect all firms and products in a county-year combination, such as local demographic changes and media influences, as well as product × county to control for time-invariant differences in sales patterns of a product-county pair, such as differences in tastes. Although the estimated association is substantially lower (a one standard deviation increase in ideology distance associates with a sales drop of 2%), the coefficient is still statistically significant and economically meaningful.

Finally, we revert to our baseline fixed effect scheme but now also allow sales by the same firm and sales in the same year to be correlated. Table 4 Panel B column (4) reports the estimated result of the baseline model using the four-way clustering

[14] We also use state fixed effect instead of the state × year fixed effect to prevent collinearity.

standard error at the product, firm, county, and year levels. The negative association between ideology distance and sales is still statistically significant at the 1% level.

In summary, our main finding—that a higher firm-county ideology distance is associated with lower subsequent sales of that firm in that county—is not sensitive to alternative measures of ideology or alternative modeling specifications.

3.3 Additional Analyses

In this section, we delve deeper into the ideology distance-sales relationship by exploring the time series and cross-sectional heterogeneity, as well as the aggregated effect. A growing literature has documented the increasing polarization in the US business world (for a survey, see Kempf and Tsoutsoura, 2024), a natural question to ask is whether our documented partisan consumption pattern also picks up this polarization trend. This conjecture is partly confirmed by the by-year regression illustrated in Figure 2.

Specifically, since the 2016 presidential election, the negative association between firm-county ideology distance and firm sales started to increase and peak in 2021, the final year of our sample period. In terms of economic magnitude, estimates in Table 5 show that the negative impact of a one standard deviation increase in firm-county ideology on the firm's subsequent sales jumped from just below 10% in 2012 to more than 70% in 2021. The time scale of this polarization trend roughly matches the timeline as in other recent work that studies the polarization in the US (e.g., Pan et al., 2024).

Next, we also explore whether this negative association differs across counties with different characteristics. To achieve this purpose, we re-estimate the baseline regression by interacting the ideology distance measure with several county demographic characteristics. The results are presented in Table 6. We include the same set of county-level controls, and product $\times$ firm $\times$ year and state $\times$ year fixed effects as in the baseline model. Similarly, we cluster the standard errors at the product and county levels.

We first find that the negative relationship between firm-county ideology and sales volumes is more pronounced in wealthier counties. Specifically, for counties that lie in the bottom tercile of real income in a year, a one standard deviation increase in ideology distance to a firm reduces sales of that firm in these counties by 20%. This relationship intensifies to 26% in middle tercile income counties and 29% in top tercile income counties. Additionally, this negative association is also stronger for counties with above-median employment rates and counties with higher levels of education (measured using the percentage of residents with a bachelor's and above degree).

We interpret these results as that more affluent consumers can afford to incorporate their ideological preferences into purchasing decisions, as higher income, higher employment rates, and higher education levels all suggest a county's affluence. On the other hand, it is important to note that even in less affluent counties, consumer behavior remains strongly aligned with ideological preferences, highlighting the significant role of personal beliefs in purchasing decisions.

We then proceed to investigate whether a county's overall political atmosphere

affects the ideology-consumption pattern. We first hypothesize that in counties that are not particularly partisan, the negative relationship between ideology distance and consumption would be less pronounced. Interacting the ideology distance measure with a dummy variable indicating whether the votes to any party in the previous election are in the range of 45% to 55%, we find evidence consistent with our conjecture. Specifically, in these swing counties, a one standard deviation increase in ideology-distance-induced sales drop is 10% lower than in partisan counties.

We also investigate whether ideology-driven consumption patterns differ between Republican-leaning counties (i.e., those where Republican candidates received more than 50% of the vote in the last election) and Democratic-leaning counties.[15] The last column of Table 6 suggests that while both types of counties exhibit significant partisan preferences in consumption, the effect is notably stronger in Republican counties. In fact, the estimated coefficient for Republican counties is more than twice as large as that for Democratic counties.

Finally, we aggregate sales to the firm-county level and investigate whether the documented negative relationship holds at the firm level. Table 7 presents the results with different specifications and confirms that this relationship also presents in aggregation. a one standard deviation increase in firm-county ideology distance leads to a 23% to 35% decline in the firm's subsequent year's sales in that county. This translates to an annual sales reduction of approximately $42,000 to $64,000 for an average firm in one county, again highlighting the economic significance of the effect.

[15]The results remain consistent when using alternative thresholds to define a county's political leaning.

4 Mechanism: Demand versus Supply Channels

Two potential mechanisms could explain the strong negative relationship between firm-county political distance and sales. On the one hand, we can interpret this result as consumers adjusting their purchasing decisions based on how closely a producing firm's political stance aligns with their own beliefs. On the other hand, firms might strategically target sales in regions that share their political beliefs. In this section, we examine the presence of both channels.

4.1 Demand Channel

To identify the demand channel, we use two exogenous shocks that arguably alter the political attitudes in a county, but at the same time are unlikely to affect other economic determinants of firms' sales. Our first empirical laboratory is the staggered expansion of the conservative TV network, the Sinclair Broadcast Group, into different counties during our sample period.

As of 2024, the Sinclair Broadcast Group is one of the largest television broadcasters in the US, owning or operating 185 stations across 86 Designated Market Areas (DMAs). This coverage allows them to reach nearly 40% of the US households. Sinclair's business strategy is to acquire television stations in different DMAs and replace more costly local news with national news that is shared across DMAs to achieve economies of scale. The acquired stations experience a shift in their broadcast content toward a right-leaning perspective (Martin and McCrain, 2019). Importantly,

this change in content influences the political leaning of counties where these stations enter, moving them to favor the Republican Party (Levendusky, 2022; Miho, 2025).

We collect data on the DMAs where Sinclair operates during the sample period from the company's annual report and then match them to the county level. During our sample period, Sinclair expanded from owning local TV stations in 626 counties to more than 1500 counties. One might worry that Sinclair's expansion is not random. However, Sinclair typically enters DMAs in bundles by acquiring other broadcast companies that operate in multiple DMAs (Mastrorocco and Ornaghi, 2025). This strategy makes the entries unlikely to be driven by DMA-specific characteristics. Second, the acquired stations by Sinclair are geographically and economically dispersed (Dyer et al., 2024), further mitigating the concern of a selection bias.

Nevertheless, similar to Pan et al. (2024), we match each county that experiences the Sinclair entry (treated counties) with a county that has never experienced the Sinclair entry (never-treated counties). The matching is based on pre-entry local demographics, including population growth, income growth, college degree attainment, and the Republican Party's vote share in the last election. To ensure comparability, we restrict the matching criteria so that the propensity score difference between treated and control counties is less than 0.05. We repeat this exercise for each entry cohort to take advantage of the staggered nature of Sinclair's entry. We then normalize the entry time to create a stacked balanced panel, starting two years before the entry year and extending to three years after.

Since our treatment is at the firm-county level, we aggregate the sales to the

firm-county level for analysis. How Sinclair's entry into a county affects our firm-county ideology distance measure depends on the pre-entry relationship between the firm ideology and the county ideology. Specifically, if Sinclair enters a county-firm pair with $Ideology_{c,t} > Ideology_{i,t}$ ($Ideology_{c,t} < Ideology_{i,t}$), then $Ideology\ Dis_{i,c,t}$ would increase (decrease) based on our construct. To account for this asymmetric effect, we introduce a treatment direction indicator for each firm-county pair, $Treatment\ Direction_{i,c}$, that equals $+1$ (-1) if Sinclair entered county has higher (lower) ideology score than the firm. We then run the following stacked-DID model:

$$\log Sales_{i,c,t} = \beta Treatment\ Direction_{i,c} \times Treated_{i,c} \times Post_{i,c,t} + \gamma' X + \alpha_i + \theta_{c,h} + \zeta_{c,t} + \varepsilon_{i,c,t}$$

in which we control for county- and firm-level characteristics, firm fixed effect (α_i), treatment cohort $\times$ county fixed effect ($\theta_{c,h}$), and the treatment cohort $\times$ year fixed effect ($\zeta_{c,t}$). We cluster the standard errors at the county and firm levels. The average treatment effect is captured by the coefficient β.

Table 8 Column (1) shows the estimated treatment effect. For the overall sample, although the sign of the estimated coefficient suggests that an increase in the firm-county ideology distance would reduce the firm's sales in that county, the coefficient is not statistically significant at the conventional levels. However, when we break down our sample into Republican-leaning (column (2)) and Democratic-leaning (column (3)) counties, an interesting pattern emerges. Specifically, for Democratic-leaning counties, we discover a 6.7% drop in sales after the Sinclair-induced firm-

ideology distance increase relative to never-treated Democratic counties, whereas for Republican counties, we do not observe a similar treatment effect. These results are consistent with the observation documented in Table 6 that Republican-leaning counties are *a priori* more partisan in their consumptions, and hence an exogenous shock to local ideology mostly tilts the former Democratic consumers to become more partisan.

To test the parallel trends assumption, we estimate a dynamic version of the above stacked-DID model and show the results in Figure 3. Panel (a) estimates the model using the full sample and Panel (b) compares Democratic counties against Republican counties. In both cases, we do not find significant pre-trends in sales. Moreover, Panel (b) indicates that the effect of Sinclair's entry starts to show up in the year following the entering year and peaks in the next year.

We also explore whether, in treated counties, the effect differed for Democratic versus Republican sellers. We run a triple-difference type of model, in which we indicate a county experiencing Sinclair's entry as *Treated* and the years after the entry *Post*. The last three columns of Table 8 show that Democratic-leaning companies saw substantial sales drops after Sinclair entered a county, and this drop is similar in magnitude in Republican and Democratic counties. However, in the same county, Republican-leaning companies would experience a differential impact. In the overall sample, Democratic firms would see a 9.7% sales drop after Sinclair enters a county, whereas the drop is roughly 2.1% for Republican firms. The differential impact is mostly driven by Democratic counties, in which Sinclair's entry would lead to a

10.1% sales drop for Democratic firms, but a 5.9% increase in sales for Republican firms.

Although the entry of Sinclair into a county is arguably unrelated to firms' sales in that county, one might still worry about the existence of confounding factors from outside the demand channel. For example, it might be possible that Democratic-leaning firms are less likely to advertise their products through the Sinclair network, causing residents from treated counties to be exposed less to these products. Since Sinclair stations are not the only broadcasting network in those counties, this concern is unlikely to be valid. Nevertheless, we use an alternative event to test for the demand channel.

We examine how Donald Trump's 2016 political campaign affected consumption patterns in the counties that hosted his rallies. The United States has seen a rising trend in identity politics (Bonomi et al., 2021), alongside a shift in political discourse where politicians have become more vocal on social issues (Enke, 2020; Gennaro and Ash, 2022). This transformation has deepened polarization among citizens (Burszty et al., 2020; Newman et al., 2021). Building on this trend which was arguably catalyzed by Trump's 2016 political campaign (Enke, 2020), we hypothesize that in the counties hosting the Trump rallies, identity politics influenced consumer behavior (Grosjean et al., 2022), leading to increased support for products from Republican-leaning parties, at least in the short run.

Benefiting from the high granularity of the data, we can focus on a short period (in terms of weeks) surrounding the Trump rallies. This short window allows us to

exclude as many confounding supply-side effects as possible. Specifically, we conduct an event-study type of analysis at the firm-county level to evaluate whether a Trump campaign rally leads to an increase (decrease) in the sales of Republican (Democratic) firms in the hosting county in the weeks after the campaign. Specifically, we estimate the following equation:

$$\begin{aligned}\log Sales_{i,c,w} = \alpha + \beta_1 Trump_w + \beta_2 Trump_{w-4,w-1} + \beta_3 Trump_{w-8,w-5} \\ + \beta_4 Trump_{w-12,w-9} + \delta Trump_{w+1,w+4} + FE + \varepsilon_{i,c,w}\end{aligned}$$

The dependent variable, $\log Sales_{i,c,w}$, is the natural logarithm of sales of firm i in county c in week w. The independent variables are the counts of Trump rallies in county c during weeks $w-12$ to $w-9$, $w-8$ to $w-5$, and $w-4$ to $w-1$. We also include a dummy variable of whether there is a Trump rally in week w, and counts of Trump rallies during weeks $w+1$ to $w+4$ as the placebo period. We include different fixed effects for robustness and cluster standard errors at the county and week levels for all specifications. We estimate this model separately for Republican-leaning firms and Democratic-leaning firms. Table 9 shows the results.

The results in Table 9 confirm our hypothesis that, in counties that hold Trump rallies, Republican-leaning firms experience an increase in sales in the weeks after the rally. The magnitudes of increase range from 0.7% to 0.9% over a four-week period, which roughly equals a \$1,500 to \$1,900 sales increase in a year. On the other hand, we do not find a statistically meaningful effect for Democratic-leaning

firms, although the sign of the estimated coefficients suggests a decline in their sales after Trump's rallies. We further disaggregate the estimated effects into weeks and plot the coefficients in Figure 4. The figure picks up an evident increasing trend for Republican-leaning firms and a decreasing (albeit insignificant) trend for Democratic-leaning firms.

4.2 Supply Channel

While our demand-channel analyses provide strong evidence supporting its existence, we cannot rule out the influence of supply-side factors. In fact, the negative association between firm-county ideology distance and the firm's price per unit in that county (see Table 3 Panel B) suggests that firms may adjust their product mix based on their political alignment with the region.

In this section, we investigate whether firm-county political distance influences a firm's decision to enter a county and, conditional on entering, whether the firm offers fewer products in that county. To examine the extensive margin, we construct all possible firm-county pairs for each year and create a variable to indicate whether a firm is selling products in a county in a given year. We then regress this indicator on the firm-county ideology distance and control for firm and county characteristics in the model. Moreover, we use different fixed effect schemes to control for different potential confounders. The results are presented in Table 10 Panel A.

We find robust evidence that the firm-county ideology distance is negatively associated with a firm's probability of entering a county. Specifically, a one standard

deviation increase in the firm-county ideology distance would lead to a 0.4 to 0.7 percentage points drop in the firm's entering probability.

To test the intensive margin, we count the number of products a firm sells in a county in any given year. We then regress this number on the firm-county ideology distance measure and report the results in Table 10 Panel B. In this case, we do not find evidence supporting the conjecture that firms supply fewer products in regions where they have less aligned political beliefs.

Overall, similar to that of the demand channel, we find evidence of the existence of a supply channel. This mechanism operates through the extensive margin–firms are less likely to enter counties that have larger ideology distance gaps with them. However, on the intensive margin, once firms enter, they do not appear to discriminate against these counties by offering fewer product choices.

5 Conclusion

This paper provides compelling evidence that political ideology critically shapes consumer behavior and firm strategy in the US. Using barcode-level sales data, we find that firms experience lower sales in counties where their ideological distance from the local population is greater. This effect is economically substantial, as a one standard deviation increase in firm-county ideological distance can reduce the firm's annual sales in that county by nearly $300,000.

Both demand and supply mechanisms drive this pattern. On the demand side,

consumers adjust their purchasing decisions based on ideological alignment with firms. To identify this effect, we leverage two exogenous shocks: the expansion of Sinclair Broadcast Group and Donald Trump's 2016 campaign rallies. After Sinclair enters a Democratic-leaning county, firms with greater ideological distance from that county experience a decline in sales over the following three years, an effect primarily driven by Democratic-leaning firms. Similarly, analyzing weekly sales patterns around Trump's rallies, we find that consumers increase spending on Republican-leaning firms in the short term. On the supply side, we show that firms are less likely to enter counties that do not share their ideological beliefs. However, conditional on entering, they do not reduce the variety of products offered in those counties.

Our findings have important implications for firms operating in an increasingly polarized market. As political beliefs increasingly shape consumer decisions, companies may need to incorporate ideological factors into their market entry and branding strategies. Consumers should also be mindful that making purchasing decisions based on political alignment rather than economic factors could lead to suboptimal welfare outcomes. Additionally, our results suggest that policymakers should consider the role of ideological polarization when shaping economic policy, as political alignment may influence market dynamics in ways that extend beyond traditional economic considerations.

References

Anahit Mkrtchyan, Sandvik, J., and Xu, D. (2024). Employee responses to CEO activism. *Journal of Accounting and Economics*, 78(1):101701.

Argente, D., Baslandze, S., Hanley, D., and Moreira, S. (2020). Patents to Products: Product Innovation and Firm Dynamics. *SSRN Electronic Journal*.

Argente, D., Lee, M., and Moreira, S. (2018). Innovation and product reallocation in the great recession. *Journal of Monetary Economics*, 93.

Arqué-Castells, P. and Spulber, D. F. (2022). Measuring the Private and Social Returns to R&D: Unintended Spillovers versus Technology Markets. *Journal of Political Economy*, 130(7).

Ash, E., Chen, D. L., and Naidu, S. (2024). Ideas have consequences: The impact of law and economics on American justice. *SSRN Electronic Journal*.

Bonomi, G., Gennaioli, N., and Tabellini, G. (2021). Identity, beliefs, and political conflict. *Quarterly Journal of Economics*, 136(4).

Boxell, L., Gentzkow, M., and Shapiro, J. M. (2017). Greater Internet use is not associated with faster growth in political polarization among US demographic groups. *Proceedings of the National Academy of Sciences of the United States of America*, 114(40).

Brownen-Trinh, R. and Orujov, A. (2023). Corporate socio-political activism and retail investors: Evidence from the Black Lives Matter campaign. *Journal of Corporate Finance*, 80.

Burszty, L., Egorov, G., and Fiorin, S. (2020). From extreme to mainstream: The erosion of social norms. *American Economic Review*, 110(11).

Colonnelli, E., McQuade, T., Ramos, G., Rauter, T., and Xiong, O. (2023). Polarizing corporations: Does talent flow to "good" firms? *SSRN Electronic Journal*.

Colonnelli, E., Pinho Neto, V., and Teso, E. (2022). Politics at work. *NBER Working Paper*.

Dagostino, R., Gao, J., and Ma, P. (2023). Partisanship in loan pricing. *Journal of Financial Economics*, 150(3).

Dasgupta, S., Guo, R., Ren, X., and Shu, T. (2021). Similarity breeds trust: political homophily and CEO-board communication. *SSRN Electronic Journal*.

DellaPosta, D., Shi, Y., and Macy, M. (2015). Why do liberals drink lattes? *American Journal of Sociology*, 120(5).

Dichev, I. D. and Qian, J. (2022). The benefits of transaction-level data: The case of NielsenIQ scanner data. *Journal of Accounting and Economics*, 74(1).

Duchin, R., El Karim Farroukh, A., Harford, J., Patel, T., and University, V. (2024). The economic effects of political polarization: Evidence from the real asset market.

Dyer, T., Lang, M., and Oh, J. (2024). Media conglomeration, local news, and capital market consequences. *Management Science*.

Engelberg, J., Guzman, J., Lu, R., and Mullins, W. (2023a). Partisan entrepreneurship. *SSRN Electronic Journal*.

Engelberg, J., Henriksson, M., Manela, A., and Williams, J. (2023b). The partisanship of financial regulators. *Review of Financial Studies*, 36(11).

Enke, B. (2020). Moral values and voting. *Journal of Political Economy*, 128(10):3679–3729.

Flamino, J., Galeazzi, A., Feldman, S., Macy, M. W., Cross, B., Zhou, Z., Serafino, M., Bovet, A., Makse, H. A., and Szymanski, B. K. (2023). Political polarization of news media and influencers on Twitter in the 2016 and 2020 US presidential elections. *Nature Human Behaviour*, 7(6).

Gennaro, G. and Ash, E. (2022). Emotion and reason in political language. *Economic Journal*, 132(643).

Goldman, E., Gupta, N., and Israelsen, R. (2024). Political polarization in financial news. *Journal of Financial Economics*, 155.

Granja, J. and Moreira, S. (2023). Product innovation and credit market disruptions. In *Review of Financial Studies*, volume 36.

Grosjean, P., Masera, F., and Yousaf, H. (2022). Inflammatory political campaigns and racial bias in policing. *The Quarterly Journal of Economics*, 138(1):413–463.

Homroy, S. and Gangopadhyay, S. (2023). Strategic CEO activism in polarized markets. *Journal of Financial and Quantitative Analysis*.

Hou, Y. and Poliquin, C. W. (2023). The effects of CEO activism: Partisan consumer behavior and its duration. *Strategic Management Journal*, 44(3).

Hsieh, C. H., Low, A., Sharma, V., and Zhu, Q. (2024). Partisan Consumers. *SSRN Electronic Journal*.

Huber, G. A. and Malhotra, N. (2017). Political homophily in social relationships: Evidence from online dating behavior. *Journal of Politics*, 79(1).

Iyengar, S., Konitzer, T., and Tedin, K. (2018). The home as a political fortress: Family agreement in an era of polarization. *Journal of Politics*, 80(4).

Iyengar, S., Lelkes, Y., Levendusky, M., Malhotra, N., and Westwood, S. J. (2019). The origins and consequences of affective polarization in the United States.

Jeung, J. (2021). Politically polarized depositors. *SSRN Electronic Journal*.

Jin, H., Merkley, K., Sharma, A., and Ton, K. (2024). Customers' response to firms' disclosure of social stances: evidence from voting reform laws. *Review of Accounting Studies*.

Kempf, E., Fos, V., and Tsoutsoura, M. (2024). The political polarization of corporate America. *SSRN Electronic Journal*.

Kempf, E. and Tsoutsoura, M. (2021). Partisan professionals: Evidence from credit rating analysts. *Journal of Finance*, 76(6):2805–2856.

Kempf, E. and Tsoutsoura, M. (2024). Political polarization and finance. *Annual Review of Financial Economics*, 16.

Lee, J., Lee, K. J., and Nagarajan, N. J. (2014). Birds of a feather: Value implications of political alignment between top management and directors. *Journal of Financial Economics*, 112(2).

Levendusky, M. S. (2022). How does local TV news change viewers' attitudes? The case of Sinclair Broadcasting. *Political Communication*, 39(1).

Liaukonytė, J., Tuchman, A., and Zhu, X. (2023). Spilling the beans on political consumerism: Do social media boycotts and buycotts translate to real sales impact? *Marketing Science*, 42(1).

Luo, M., Manconi, A., and Massa, M. (2023). Political affiliation and media distrust: Evidence from stock market investors. *SSRN Electronic Journal*.

Martin, G. J. and McCrain, J. (2019). Local news and national politics. *American Political Science Review*, 113(2):372–384.

Martin, G. J. and Yurukoglu, A. (2017). Bias in cable news: Persuasion and polarization. *American Economic Review*, 107(9).

Mastrorocco, N. and Ornaghi, A. (2025). Who watches the watchmen? Local news and police behavior in the United States. *American Economic Journal: Economic Policy*.

Mccartney, W. B., Orellana-Li, J., and Zhang, C. (2024). Political polarization affects households' financial decisions: Evidence from home sales. *Journal of Finance*, 79(2).

Meier, J.-M., Servaes, H., Wei, J., and Xiao, S. C. (2022). Do Consumers Care About ESG? Evidence from Barcode-Level Sales. *SSRN Electronic Journal*.

Miho, A. (2025). Small screen, Big echo? Political persuasion of local TV news: Evidence from Sinclair. *SSRN Electronic Journal*.

Newman, B., Merolla, J. L., Shah, S., Lemi, D. C., Collingwood, L., and Ramakrishnan, S. K. (2021). The Trump effect: An experimental investigation of the emboldening effect of racially inflammatory elite communication. *British Journal of Political Science*, 51(3).

Pan, Y., Pikulina, E. S., Siegel, S., and Wang, T. Y. (2024). Political Divide and Partisan Portfolio Disagreement. *SSRN Electronic Journal*.

Qin, M. S., Luo, X., Schifeling, T., and Wang, Y. (2025). When corporate silence is costly: Negative consumer responses to corporate silence on social issues. *Strategic Management Journal*, 46(4):994–1026.

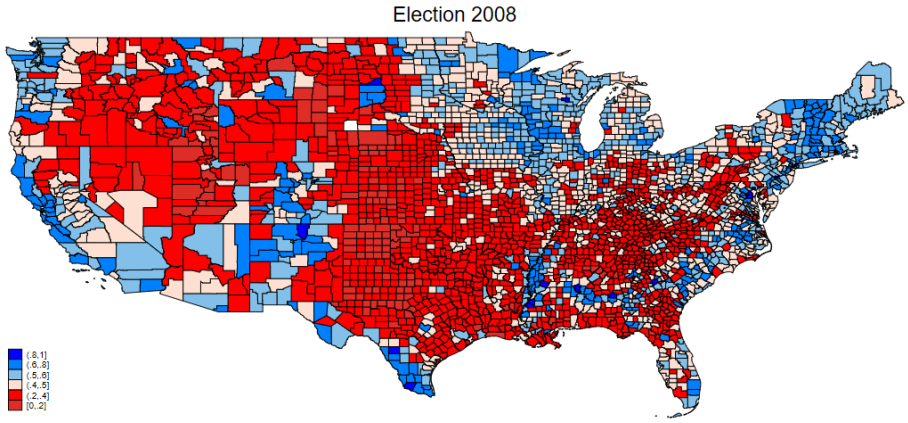
Rice, A. B. (2023). Executive partisanship and corporate investment. *Journal of Financial and Quantitative Analysis*.

Schoenmueller, V., Netzer, O., and Stahl, F. (2023). Polarized America: From political polarization to preference polarization. *Marketing Science*, 42(1).

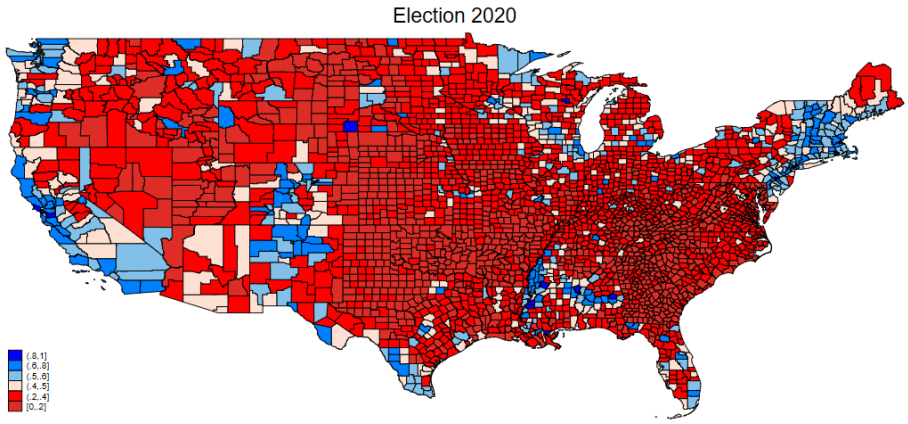
Warren, L. (2019). WSJ Report Shows Political Leanings of Levi's, Wrangler Shoppers.

Yi, Y. (2025). Does partisanship affect household consumption? *SSRN Electronic Journal*.

Figures and Tables

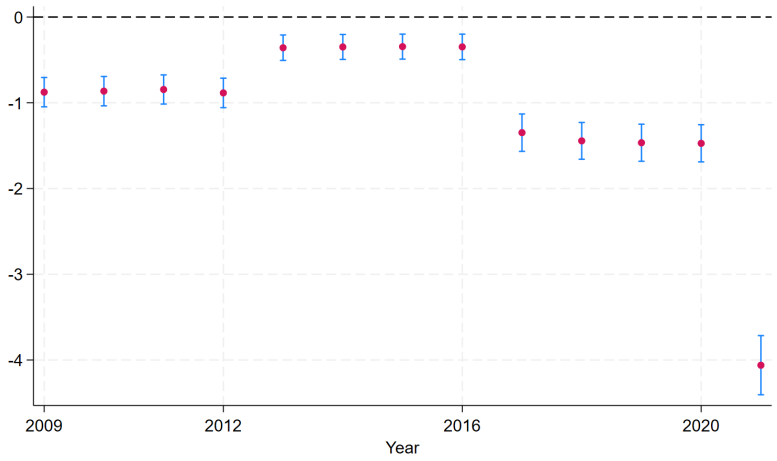


(a) County Ideology - Election 2008

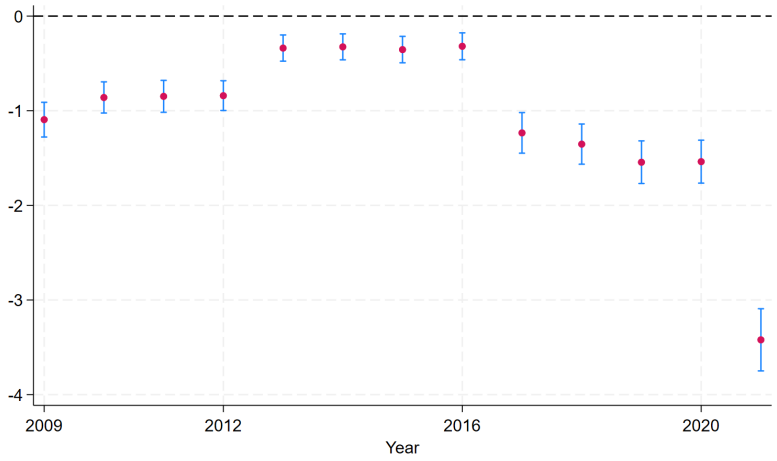


(b) County Ideology - Election 2020

Figure 1: Geographic Distribution of County Ideology. This figure plots the ideology distribution at the county level using election returns (proportion votes to Democratic party candidates) in 2008 (top panel) and 2020 (bottom panel). Red (blue) indicates more votes to the Republican (Democratic) Party Candidate.

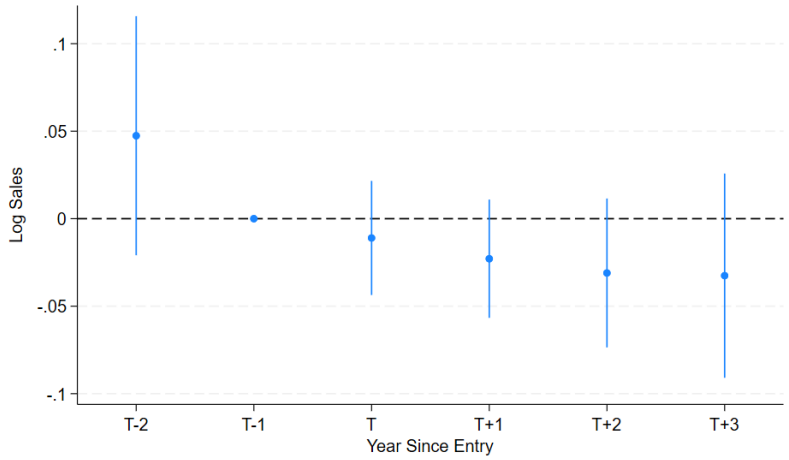


(a) Without County-level Controls

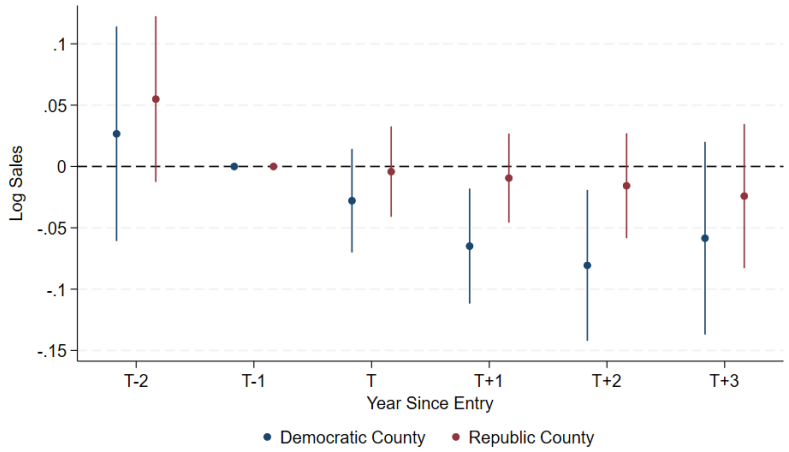


(b) With County-level Controls

Figure 2: Effect of firm-county ideology distance on product sales over time. This figure plots regression coefficients and their 95% confidence intervals for annual cross-sectional product-level regressions of *Log (Sales)* on lagged one-year firm-county *Ideology Distance*. Panel (a) does not include county-level controls and panel (b) includes county-level controls.

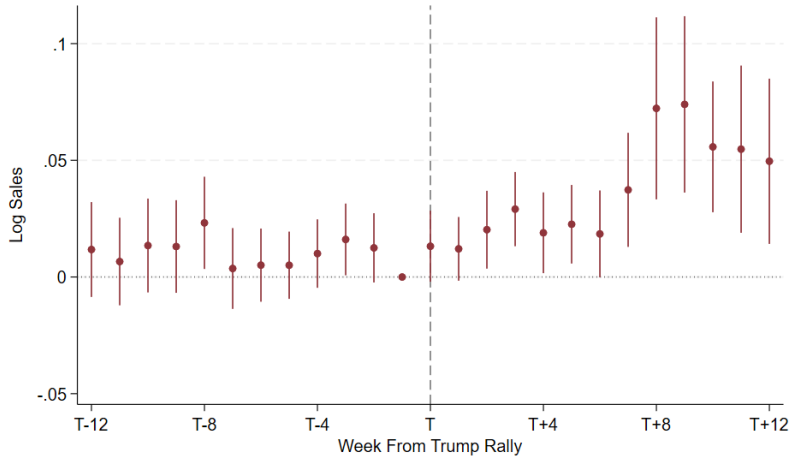


(a) Full Sample

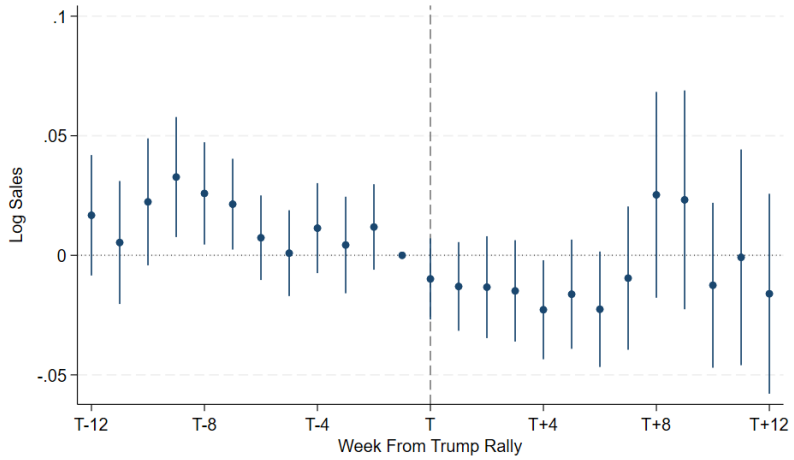


(b) Republican versus Democratic County

Figure 3: Effect of Sinclair's entry on firm sales. This figure plots the dynamic stacked DiD model using Sinclair's entry as an exogenous shock to a county's ideology. Period T is the year Sinclair enters a county. $T-1$ is the excluded baseline period. Panel (a) uses the full sample, while Panel (b) juxtaposes Republican-leaning counties (red line) and Democratic-leaning counties (blue line).



(a) Republican-Leaning Firms



(b) Democratic-leaning Firms

Figure 4: Effect of Trump Campaign Rally on Firm Sales. This figure plots the event studies of Trump's presidential campaign rally's effect on weekly sales of the Republican-leaning firms (Panel (a)) and the Democratic-leaning firms (Panel (b)) in the campaign county. Period T is the week of the campaign. T indicates the Trump Rally week.

Table 1: Summary Statistics

This table presents the summary statistics. Variable definitions can be found in Table A1.

Panel A: Sample at Product-Firm-County-Year Level

	Obs	Mean	SD	p25	p75
LogSales	37,314,818	3.692	2.154	2.136	4.859
Ideology Dis	37,314,818	0.334	0.204	0.166	0.482
Ideology Dis_{close}	40,467,198	0.338	0.203	0.170	0.487
Ideology Dis_{roll}	25,253,983	0.383	0.209	0.215	0.545
Ideology Dis_{ann}	24,215,501	0.388	0.209	0.223	0.549
Ideology $\text{Dis}_{countyclose}$	37,314,818	0.336	0.204	0.167	0.484
House Price (%Δ)	37,314,818	1.304	5.186	-1.520	4.450
Population (%Δ)	37,314,818	0.424	1.145	-0.251	0.992
Income (%Δ)	37,314,818	3.295	3.914	1.464	5.144
Unemployment (%Δ)	37,314,818	0.079	1.540	-0.700	0.400
GDP (%Δ)	37,314,818	1.022	6.174	-1.838	3.653

Panel B: Sample at Product-Firm-County-Election Year Level

	Obs	Mean	SD	p25	p75
LogSales	11,593,118	6.582	2.604	4.753	8.353
LogUnits	11,593,118	5.509	2.621	3.611	7.302
LogPrice	11,593,118	1.464	0.868	0.912	2.012
Ideology Dis	11,593,118	0.346	0.205	0.176	0.499
House Price (%Δ)	11,593,118	1.303	4.620	-1.020	3.990
Population (%Δ)	11,593,118	0.452	1.229	-0.263	1.046
Income (%Δ)	11,593,118	4.183	4.166	1.777	6.600
Unemployment (%Δ)	11,593,118	0.847	1.908	-0.600	2.100
GDP (%Δ)	11,593,118	0.038	6.266	-2.907	2.819

Panel C: Sample at Firm-County-Year Level

	Obs	Mean	SD	p25	p75
LogSales	4,031,169	6.566	2.603	3.398	6.983
Ideology Dis	4,031,169	0.378	0.214	0.206	0.539
Size	1,966,657	8.318	2.149	7.047	9.772
Advertisement	1,966,657	0.046	0.060	0.011	0.053
Leverage	1,966,657	0.526	0.484	0.372	0.621
Tobin's Q	1,966,657	2.249	1.660	1.303	2.619
Investment	1,966,657	0.036	0.033	0.016	0.044
Present	19,291,266	0.209	0.407	0	0
LogNum	4,031,447	1.356	1.273	0	2.197

Table 2: Ideology Distance and Local Product Sales

This table presents the product-level relationship between firm-county ideology distance and sales. **Panel A** uses the whole sample and **Panel B** focuses on the years following the election years. Standard errors are clustered at the product and county levels. *,**, and *** denote significance levels at 1%, 5%, and 10%.

Panel A: All Years

	logSales	logSales	logSales	logSales
Ideology Dis	-2.041***	-2.086***	-1.521***	-1.484***
	(0.111)	(0.107)	(0.103)	(0.096)
House Price		-0.017***		-0.004**
		(0.002)		(0.002)
Population		0.316***		0.347***
		(0.016)		(0.018)
Income		0.002		-0.004**
		(0.002)		(0.002)
Unemployment		0.007***		0.009***
		(0.001)		(0.001)
GDP		-0.000		0.001
		(0.001)		(0.001)
Product × Firm × Year FE	Yes	Yes	Yes	Yes
State × Year FE	No	No	Yes	Yes
Adj. R^2	0.504	0.538	0.550	0.585
Observations	37,314,818	37,314,818	37,314,818	37,314,818

Panel B: Year After Elections

	logSales	logSales	logSales	logSales
Ideology Dis	-2.679***	-2.539***	-2.111***	-1.938***
	(0.115)	(0.106)	(0.352)	(0.334)
House Price		-0.027***		-0.016***
		(0.003)		(0.003)
Population		0.292***		0.319***
		(0.018)		(0.025)
Income		-0.002		-0.015***
		(0.003)		(0.004)
Unemployment		0.245***		0.246***
		(0.013)		(0.019)
GDP		0.006***		0.009***
		(0.002)		(0.002)
Product × Firm × Year FE	Yes	Yes	Yes	Yes
State × Year FE	No	No	Yes	Yes
Adj. R^2	0.514	0.553	0.558	0.593
Observations	11,593,118	11,593,118	11,593,118	11,593,118

Table 3: Ideology Distance and Local Product Sales - Quantity and Price

This table presents the product-level relationship between: firm-county ideology distance and quantity sold (Panel A), firm-county ideology distance and price (Panel B). The sample is for years following the election years. Standard errors are clustered at the product and county levels. *,**, and *** denote significance levels at 1%, 5%, and 10%.

Panel A: Quantity				
	logUnits	logUnits	logUnits	logUnits
Ideology Dis	-2.585***	-2.452***	-2.065***	-1.897***
	(0.112)	(0.103)	(0.346)	(0.328)
County Controls	No	Yes	No	Yes
Product × Firm × Year FE	Yes	Yes	Yes	Yes
State × Year FE	No	No	Yes	Yes
Adj. R^2	0.547	0.584	0.586	0.619
Observations	11,593,118	11,593,118	11,593,118	11,593,118
Panel B: Price				
	logPrice	logPrice	logPrice	logPrice
Ideology Dis	-0.113***	-0.106***	-0.066***	-0.060***
	(0.007)	(0.006)	(0.012)	(0.011)
County Controls	No	Yes	No	Yes
Product × Firm × Year FE	Yes	Yes	Yes	Yes
State × Year FE	No	No	Yes	Yes
Adj. R^2	0.892	0.892	0.894	0.894
Observations	11,593,118	11,593,118	11,593,118	11,593,118

Table 4: Ideology Distance and Local Product Sales - Robustness

This table presents various robustness tests. **Panel A** uses alternative ideology distance measures. **Panel B** uses alternative specifications to explore different types of variations and to allow correlations across different dimensions. *,**, and *** denote significance levels at 1%, 5%, and 10%.

Panel A: Alternative Ideology Measures

	logSales	logSales	logSales	logSales
Ideology Dis_{close}	-1.654***			
	(0.099)			
Ideology Dis_{roll}		-1.454***		
		(0.080)		
Ideology Dis_{ann}			-1.690***	
			(0.084)	
Ideology $Dis_{countyclose}$				-1.504***
				(0.096)
County Controls	Yes	Yes	Yes	Yes
Product × Firm × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Cluster SE - Product	Yes	Yes	Yes	Yes
Cluster SE - County	Yes	Yes	Yes	Yes
Adj. R^2	0.585	0.594	0.594	0.586
Observations	40,467,198	25,253,983	24,215,501	37,314,818

Panel B: Alternative Specifications

	logSales	logSales	logSales	logSales
Ideology Dis	-1.852***	-0.635***	-0.105***	-1.484***
	(0.116)	(0.159)	(0.026)	(0.461)
Firm Control	Yes	No	No	No
County Control	Yes	No	No	Yes
Product × Firm × Year FE	Yes	No	Yes	Yes
Product × County × Year FE	No	Yes	No	No
State × Year FE	Yes	No	No	Yes
State FE	No	Yes	No	No
County × Year FE	No	No	Yes	No
Product × County FE	No	No	Yes	No
Cluster SE - Product	Yes	Yes	Yes	Yes
Cluster SE - County	Yes	Yes	Yes	Yes
Cluster SE - Firm	No	No	No	Yes
Cluster SE - Year	No	No	No	Yes
Adj. R^2	0.617	0.564	0.853	0.585
Observations	26,551,653	31,476,355	38,836,520	37,314,818

Table 5: Ideology Distance and Local Product Sales - Polarization

This table presents the product-level relationship between firm-county ideology distance and sales for each of the years immediately following the presidential election years. **Panel A** does not include county-level controls and **Panel B** includes the same county-level controls as the main regressions. Both panels include product × firm × year FE and state × year FE. Standard errors are clustered at the product and county levels. *,**, and *** denote significance levels at 1%, 5%, and 10%.

Panel A: Without County-level Controls

	2008	2012	2016	2020
Ideology Dis	-1.146***	-0.497***	-1.849***	-4.061***
	(0.110)	(0.093)	(0.139)	(0.176)
Product × Firm × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Adj. R^2	0.562	0.528	0.538	0.577
Observations	2,910,431	3,010,712	2,934,959	3,245,958

Panel B: With County-level Controls

	2008	2012	2016	2020
Ideology Dis	-1.075***	-0.475***	-1.708***	-3.551***
	(0.101)	(0.085)	(0.137)	(0.165)
Product × Firm × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Adj. R^2	0.614	0.598	0.583	0.627
Observations	2,808,872	2,891,825	2,794,362	3,098,059

Table 6: Ideology Distance and Local Product Sales - Heterogeneity Across Counties

This table presents the heterogeneous product-level relationship between firm-county ideology distance and sales across different county characteristics. All regressions include county-level controls, product × firm × year FE, and state × year FE. Standard errors are clustered at the product and county levels. *,**, and *** denote significance levels at 1%, 5%, and 10%.

	logSales	logSales	logSales	logSales	logSales
Ideology Dis	-0.977***	-1.433***	-0.826***	-1.413***	-0.477***
	(0.083)	(0.097)	(0.075)	(0.093)	(0.060)
Ideology Dis	-0.289***				
× Middle Income	(0.054)				
Ideology Dis	-0.445***				
× Upper Income	(0.069)				
Ideology Dis		-0.133***			
× High Employment		(0.046)			
Ideology Dis			-0.433***		
× High Education			(0.055)		
Ideology Dis				0.140***	
× Swing County				(0.037)	
Ideology Dis					-0.587***
× Republican-leaning					(0.062)
Product × Firm × Year FE	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.619	0.589	0.624	0.587	0.609
Observations	37,314,818	37,314,818	37,245,084	37,314,818	37,314,818

Table 7: Ideology Distance and Local Product Sales - Firm-Level Evidence

This table presents the firm-level estimates of firm-county ideology distance and sales. Standard errors are clustered at the firm and county levels. *,**, and *** denote significance levels at 1%, 5%, and 10%.

	logSales	logSales	logSales
Ideology Dis	-1.597***	-1.644***	-1.080***
	(0.222)	(0.233)	(0.211)
Size			0.545***
			(0.130)
Advertisement			1.229
			(1.073)
Leverage			-0.013
			(0.108)
Tobin's Q			0.055
			(0.038)
Invest			-0.213
			(1.511)
County Control	No	Yes	Yes
Firm × Year FE	Yes	Yes	No
Firm FE	No	No	Yes
State × Year FE	Yes	Yes	Yes
Adj. R^2	0.559	0.600	0.562
Observations	4,031,169	3,830,269	1,966,657

Table 8: Firm Sales After Sinclair Entry

This table presents the effects of Sinclair's entry on county-level firm sales. For all columns, the outcome variable is the logarithm of sales measured at the firm-county-year level. Columns *Rep* (*Dem*) use Republican- (Democratic-) leaning counties as the sample. Column *All* uses all counties. *Treatment Direction* takes the value of 1 (-1) if the treatment increases (decreases) the ideology distance at the county-firm level. *Treated* equals 1 if a county experiences Sinclair's entry during the sample period. *Post* equals 1 for years after Sinclair enters a county. All regressions use a balanced panel with two periods before and three periods after the entry. All models include Firm FE, cohort × county FE, and cohort *times* calendar year FE. Standard errors are two-way clustered at the firm and county levels. *,**, and *** denote significance levels at 1%, 5%, and 10%.

	Stacked DiD			**Triple Difference**		
	All	Rep	Dem	All	Rep	Dem
Treatment Direction × Treated × Post	-0.031	-0.019	-0.067**			
	(0.021)	(0.021)	(0.027)			
Treated × Post				-0.097***	-0.101**	-0.101**
				(0.034)	(0.039)	(0.044)
Republican Firm				-0.027	-0.015	-0.064
				(0.064)	(0.060)	(0.091)
Treated × Post × Republican Firm				0.076*	0.046	0.160***
				(0.043)	(0.043)	(0.056)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × County FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.829	0.816	0.852	0.829	0.816	0.852
Observations	345,556	252,797	92,759	345,556	252,797	92,759

Table 9: Trump Rally and Weekly Sales

This table presents the effect of Trump Rally on firm sales in the rally county. *Rep* (*Dem*) columns are sub-sample of Republican- (Democratic-) leaning firms. The independent variable is the dummy variable indicating whether there is a Trump rally campaign in the county during the corresponding week interval. We use different fixed effects to explore alternative variations. All standard errors are clustered at the product and county levels. *,**, and *** denote significance levels at 1%, 5%, and 10%.

	Rep	Rep	Rep	Dem	Dem	Dem
$TRUMP_0$	0.008	0.007	0.008	0.005	0.004	0.006
	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
$TRUMP_{[-4,-1]}$	0.009*	0.007*	0.008*	-0.002	0.002	-0.001
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
$TRUMP_{[-8,-5]}$	0.006	0.005	0.008*	-0.001	0.001	-0.000
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
$TRUMP_{[-12,-9]}$	0.008*	0.008*	0.007*	-0.004	-0.003	-0.004
	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
$TRUMP_{[+1,+4]}$	0.003	0.001	0.003	0.008*	0.006	0.009*
	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)	(0.004)
County FE	Yes	No	Yes	Yes	No	Yes
Firm FE	Yes	No	No	Yes	No	No
Firm × County FE	No	Yes	No	No	Yes	No
Firm × Week FE	No	No	Yes	No	No	Yes
Week FE	Yes	Yes	No	Yes	Yes	No
Adj. R^2	0.858	0.965	0.867	0.876	0.957	0.893
Observations	646,893	646,357	645,891	554,646	554,191	554,236

Table 10: Ideology Distance and Firm Sales - Supply Channel

This table presents the relationship between firm-county ideology distance and the firm's supply in that county. **Panel A** uses whether a firm sells products in a county as the outcome variable, whereas **Panel B** uses the number of products sold in a county as the outcome variable. Standard errors are two-way clustered at the firm and county levels. *,**, and *** denote significance levels at 1%, 5%, and 10%.

Panel A: Firm Presence in County

	Present	Present	Present	Present
Ideology Dis	-0.019**	-0.032**	-0.035**	-0.029*
	(0.008)	(0.013)	(0.014)	(0.015)
Firm Control	No	Yes	Yes	Yes
County Control	No	No	Yes	No
Firm FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	No
County × Year FE	No	No	No	Yes
Adj. R^2	0.602	0.629	0.640	0.648
Observations	19,291,266	7,139,592	6,545,224	6,545,224

Panel B: Number of Products Sold in County

	logNumber	logNumber	logNumber	logNumber
Ideology Dis	-0.035	-0.050	-0.050	-0.048
	(0.034)	(0.046)	(0.047)	(0.048)
Firm Control	No	Yes	Yes	Yes
County Control	No	No	Yes	No
Firm FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	No
County × Year FE	No	No	No	Yes
Adj. R^2	0.837	0.855	0.858	0.862
Observations	4,031,447	2,075,669	1,966,658	2,075,669

A Appendix

A.1 Appendix Tables

Table A1: Definition of Variables

Variable	Definition
Ideology Dis	Absolute value of the difference between county ideology and firm ideology. County ideology is measured as the proportion of votes for the Republican candidate over the votes for Republican and Democratic candidates at the last election. Firm ideology is measured as the proportion of total employee contribution to the Republican candidate over the contribution to both parties candidates during the last presidential election cycle.
Ideology Dis_{close}	Absolute value of the difference between county ideology and firm ideology. County ideology is measured as the proportion of votes for the Republican candidate over the votes for Republican and Democratic candidates at the last election. Firm ideology is measured as the proportion of total employee contribution to the Republican candidate over the contribution to both parties candidates during the closest presidential election cycle.
Ideology Dis_{roll}	Absolute value of the difference between county ideology and firm ideology. County ideology is measured as the proportion of votes for the Republican candidate over the votes for Republican and Democratic candidates at the last election. Firm ideology is measured as the proportion of total employee contribution to the Republican candidate over the contribution to both parties candidates during the past four years.
Ideology Dis_{ann}	Absolute value of the difference between county ideology and firm ideology. County ideology is measured as the proportion of votes for the Republican candidate over the votes for Republican and Democratic candidates at the last election. Firm ideology is measured as the proportion of total employee contribution to the Republican candidate over the contribution to both parties candidates during this year.

(*continued*)

Table A1 Continued

Ideology $\text{Dis}_{countyclose}$	Absolute value of the difference between county ideology and firm ideology. County ideology is measured as the proportion of votes for the Republican candidate over the votes for Republican and Democratic candidates at the closest election. Firm ideology is measured as the proportion of total employee contribution to the Republican candidate over the contribution to both parties candidates during the closest presidential election cycle.
LogSales	Natural logarithm of sales
GDP	Annual growth rate of GDP per capita
Population	Annual growth rate of population
Income	Annual growth rate of real income per capita
House Price	Annual growth rate of house price
Unemployment	Annual growth rate of unemployment rate
Size	Natural logarithm of lagged total asset
Advertisement	Advertising expenditure scaled by lagged asset
Leverage	debt to asset ratio
Tobin's Q	Total asset plus market value subtracting book value, scaled by total asset
Investment	Capital Expenditure scaled by lagged asset
Middle Income	Dummy variable equals 1 if a county lies in the middle tercile in terms of real income across all counties in that year
Upper Income	Dummy variable equals 1 if a county lies in the top tercile in terms of real income across all counties in that year
High Employment	Dummy variable equals 1 if a county has above median level of employment rate across all counties in that year
High Education	Dummy variable equals 1 if a county has above median level of percentage adults with bachelor and above degrees across all counties in that year
Swing County	Dummy variable equals 1 if a county's votes to the Republican candidate is between $45\% - 55\%$ at the last presidential election
Republican-leaning	Dummy variable equals 1 if a county's votes to the Republican candidate is higher than 50% at the last presidential election
Treated	Dummy variable equals 1 if Sinclair has a station in a county
Post	Dummy variable equals 1 for all years after Sinclair enters a county
Treatment Direction	Category variable equals 1 (-1) if Sinclair enters a county-firm pair that increases (decreases) the Ideology Dis of the county and the firm.

(*continued*)

Table A1 Continued

Rep Firm	Dummy variable equals 1 if a firm's donation to the Republican candidate is more than 50% of its total donations to the two parties.
Dem Firm	Dummy variable equals 1 if a firm's donation to the Republican candidate is less than 50% of its total donations to the two parties.
$\text{TRUMP}_{i,j}$	Dummy variable equals 1 if during the week i and j (relative from the focal week), Trump holds a campaign rally in the focal county.
Present	Dummy variable equals 1 if a firm is selling product in a county
LogNum	Natural logarithm of number of products sold