Dissecting the Product Market Consequences of Bankruptcy: Evidence from 300 Million Retail Transactions*

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Abstract

Leveraging data on millions of retail transactions, this paper traces the multifaceted product market consequences of bankruptcy, disentangling the role of prices, quantities, and product offerings in the performance of bankrupt firms and their competitors, assessing implications for consumer welfare. Our firm-, product-, time-, point of sale-level matching test strategy shows that the continuing products of bankrupt firms generate less revenue due to lower quantities sold — not prices — relative to similar products of solvent competitors. While in bankruptcy, firms alter their portfolio of products, retaining those with higher "product-beta" — our novel measure of a product's risk based on the sensitivity of sales to consumption — suggesting an option-like strategy that bets on state-contingent upside payoffs. Those firms discontinue the supply of products to entire geographies, withdrawing from economically disadvantaged and underserved locations, creating "product deserts." Our granular matching further shows that rivals engage in price wars only *after* a firm files for bankruptcy and *where* (store shelves) they compete with similar products. Rival price-predatory behavior appears to be a consequence — not a cause — of bankruptcy. Only firms undergoing liquidation engage in product fire sales, while firms that eventually emerge from bankruptcy maintain their prices on par with those of local competitors.

KEYWORDS: Corporate bankruptcy, product market competition, consumer prices
JEL CLASSIFICATION: G32, G33, L11, M21.

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1 Introduction

Prolonged poor product market performance often precedes distress, which can lead firms to seek court protection from creditors. The capital market's reaction to bankruptcy filings is both immediate and severe. Take the example of Eastman Kodak, which was underperforming for several years before filing for bankruptcy in January 2012. Upon filing, Moody's immediately downgraded the firm's bonds to "junk," and its stock price fell 55% (see also Shumway and Warther (1999)). This paper shows that the collapse of a firm's financial contracts and trade agreements leads to multifaceted product market consequences, distinctively affecting its operating decisions, rivals' strategies, and consumer welfare. It does so focusing on the consumer goods industry, a large, integrated segment of the U.S. economy employing directly (indirectly) 2.7 (22.3) million workers, contributing $390 billion ($2.5 trillion) to the 2022 GDP. This industry serves as a laboratory in which we follow manufactured products until they reach end-users with significant switching costs. Leveraging *big data* from NielsenIQ's Retail Scanner, we go beyond conventional product market performance metrics by examining firm operational choices at the product and location levels while decomposing revenues into prices and quantities for bankrupt firms and their competitors. We complement our retailer-level data with NielsenIQ's Homescan panel, which tracks the purchases of households across the U.S. The combination of retailer-level and household-level data provides a new, comprehensive view of both sides of consumer product markets, allowing us to map firms' decisions to local socioeconomic characteristics.

We show that firms report sharp, significant declines in sales and product availability right after they enter bankruptcy relative to non-bankrupt peers of similar prior performance. While intuitive, these results serve as a benchmark for a deeper investigation of underlying drivers. We find that firms in bankruptcy are more likely to discontinue less important, lower quality, and less systematically risky ("lower beta") products, and withdraw from economically disadvantaged locations already underserved by retailers ("product deserts"). For continuing products, bankrupt firms generate less revenue due solely to lower quantities sold — not prices — relative to similar products sold by solvent competitors at the very same stores. Solvent rival firms cut the prices of their products only *when* and *where* they compete with products of bankrupt firms.

We note that the product market outcomes of firms in bankruptcy may not be interpreted as causal. For example, deteriorating market conditions *per se* may push firms into bankruptcy. To characterize the nature of bankruptcy cases in our sample of consumer good producers, we leverage large language models (LLMs) to systematically analyze the narratives surrounding each filing. This analysis reveals that bankruptcies are not primarily driven by slow-moving firm and industry revenue declines and changes in consumer preferences. Instead, they occur most frequently in fast-growing segments and are often triggered by disruptive shocks stemming from recessions, technological innovation, and large-scale crises (e.g., COVID-19), alongside firm-idiosyncratic factors that strongly predict bankruptcy timing. While our overarching goal is to dissect economic relations between bankruptcy and outcomes across firms, product markets, and consumers, we employ econometric techniques meant to reduce estimation biases.

As a start, we follow the approach of Fracassi et al. (2022), who study the consequences of another salient, selected corporate event using the same data: the acquisition of consumer goods manufacturers by private equity firms. Specifically, we leverage the granularity of the data to implement a combination of firm-, store-, time-, and product-level matching procedures. At the firm level, we match bankrupt firms to non-bankrupt peers that exhibit similar performance in various measures of sales and product offerings before filing. Non-bankrupt matches are drawn from markets in which bankrupt firms themselves do not operate, so as to avoid peer spillover effects affecting our comparisons. At the product-store level, we match each product from a bankrupt firm to similar products of competing firms sold at the same store at the same time ("shelf neighbors"). We then contrast changes in performance of bankrupt firms and their products relative to product–location–time-matched controls using a stacked-cohort differences-in-differences (DID) estimation.[1] As we discuss below, this strategy captures the competitive dynamics we are interested in and rules out confounders along multiple dimensions, including pre-bankruptcy performance trends and shocks to a local product market as granular as a store aisle.

Our baseline firm-level estimates show that companies that enter bankruptcy experience lower growth in gross sales, number of unique products sold, stores ("points of sale"), and locations (counties) of products sold. The economic magnitudes of the coefficients reported are significant. For example, firms experience 36.8 percentage points (p.p.) lower sales growth rates relative to matched counterfactuals immediately after bankruptcy filings. As bankrupt firms discontinue products and reduce their availability at stores and entire geographic areas, we characterize these adjustments through two complementary survival analyses. First, we examine the characteristics of products most likely to be withdrawn from all shelves across the country. Second, we investigate local factors that predict the likelihood of bankrupt firms withdrawing their products from specific counties.

Our tests also show that firms in bankruptcy retain products and local operations that represent larger shares of their revenues as well as products with high market shares. This indicates a strategic focus on maintaining flagship products and presence in key markets. These companies are also more likely to keep higher-quality products (cf. Argente et al. (2017)) as well as riskier products. To measure a product's riskiness, we develop a novel measure, *Product Beta*. It captures the sensitivity of a product's sales to consumption fluctuations within its broader product category. The preference for products with higher betas suggests bankrupt firms pursue a growth-option strategy. Simply put, by retaining only products with higher upside potential, these firms better position themselves for recovery in a market upturn.[2] Our county-level survival results show how socioeconomic factors shape the exit decisions of bankrupt firms: they are more likely to abandon counties with higher shares of low-income, unemployed, and less-educated residents. Notably, bankrupt firms are substantially more likely to exit "product deserts" — areas already underserved by

[1]The approach addresses estimation concerns associated with staggered DID tests (Callaway and Sant'Anna (2021); Goodman-Bacon (2021)). Simply put, our estimates rely entirely on comparing "treated" units with their "never-treated" matched controls.

[2]Campello and Kankanhalli (2024) provide a theoretical characterization of this notion of "betting for resurrection" strategy.

retailers and characterized by lower product prices. In all, those firms' strategies seem to disproportionately affect geographic areas with challenging socioeconomic conditions, hurting consumer welfare.

We also uncover significant effects of bankruptcy on the performance of *continuing products*. Our estimates point to a joint, proportional decline in sales and quantities — but not prices — of products manufactured by firms in bankruptcy. To wit, a bankrupt firm's product sales growth drops by 5 p.p. immediately following its court filing, a figure that is entirely explained by a 5 p.p. decline in quantities sold — prices remain constant.[3] New to the literature, the evidence we bring to bear dissects the impact of bankruptcy on firm product sales along *both* the extensive margin (fewer product offerings at points of sale) and the intensive margin (lower quantities sold of continuing products).

Exploiting case-level information on bankruptcy filings, we further characterize how product market outcomes respond to various legal aspects of court proceedings. At the firm level, we show that even companies that eventually emerge from bankruptcy experience significant and persistent declines in sales and product offerings, alleviating concerns that our results are driven by liquidating firms. At the product level, we show that firms undergoing *de facto* liquidation cut the prices of their products relative to bankrupt firms that re-emerge. Firms undergoing restructuring maintain longer-term pricing policies despite poor sales, while those permanently discontinuing operations liquidate their inventory to generate short-term cash flows. We additionally show that involuntary filings initiated by creditors — which provide quasi-exogenous variation in bankruptcy timing — yield marginally more severe disruptions than voluntary filings.

Our product-level results rely on comparisons between products from bankrupt firms and their non-bankrupt shelf neighbors. Notably, these estimates may also capture competitive local spillover effects.[4] To address this concern and more precisely measure competitors' reactions, we implement two complementary tests that compare products directly competing with bankrupt firms' products ("close competitors") against otherwise similar products that do not face direct competition ("far competitors"). The first approach compares prices within products and across stores, while the second compares prices within stores and across product categories. For concreteness, consider a gallon of milk from a bankrupt dairy producer sitting beside a non-bankrupt competitor's milk in a grocery store's refrigerated section in city A. Our first test compares the non-bankrupt competitor's milk price changes at this store with the identical milk carton it sells in other same-chain store in city B, where the bankrupt brand is absent. Our second test compares the non-bankrupt competitor's milk carton price changes with the pricing of its same-store offering of heavy cream in city A. These counterfactuals isolate local competitive responses by exploiting the intuition that non-bankrupt firms would primarily adjust pricing strategies for products directly competing against bankrupt firms' offerings rather than across their entire product portfolio or across all of their points of sale.

[3] We note that observed pricing may reflect retailers' — rather than manufacturers' — sales strategies. To address this, we leverage information on retailer-initiated sales efforts such as coupons, deals, and product featuring from both the Retail Scanner and Homescan datasets. We find no evidence that retailers adjust pricing-related strategies of products manufactured by bankrupt firms.

[4] These effects emerge when treated and controls are both affected by the treatment. They occur, for example, if a store customer chooses not to buy the product of a bankrupt firm and instead buys a similar product from its non-bankrupt rival (Berg et al., 2021).

In both exercises, we find differential price cuts for "close" competitor products relative to "far" competitors following the bankruptcy filing, revealing that strategic price adjustments occur precisely where direct rivalry exists. These results show that product market rivals engage in price wars to drive the distressed firm out of the markets (stores) where they *both contemporaneously sell competing products, after bankruptcy*. Indeed, we find no evidence of rival predatory pre-emptive behaviors (no pricing pre-trends). Our novel findings reveal remarkably localized evidence of price wars while controlling for shocks to particular products or locations.

We go further in characterizing the dynamics of price wars along multiple dimensions. We show, for example, that non-bankrupt competitors' price responses are stronger against high-revenue and higher-beta products, suggesting that rivals intensify pricing pressure to undermine the bankrupt firm's strategic positioning for recovery. We also show how retail chain size modulates competition following bankruptcy filings. Notably, aggressive price competition occurs primarily on the shelves of smaller retailers, while no price-cutting is observed at large retail chains. Major retailers' bargaining power with suppliers constrains manufacturers' ability to engage in aggressive pricing strategies.[5] Simply put, large retailers act as a buffer against extreme price competition.

Our work provides several new insights to the existing literature on firm financing and product markets. Early empirical studies largely supported the so-called "deep pocket" hypothesis, whereby financially healthy firms use war chests to finance predatory strategies against distressed rivals.[6] These studies, however, were constrained by data limitations that led to oversimplified characterizations of the strategic responses employed by both financially distressed and non-distressed competitors.[7] Access to big data has recently allowed researchers to examine product markets more granularly (see Dichev and Qian (2022) for a discussion). For example, Kim (2020) and Granja and Moreira (2022) report that credit-constrained firms reduce prices to liquidate inventories and innovate less. Critically, however, firm-level sales data *cannot differentiate* between cuts in a firm's product portfolio (or availability at points of sale) and lower revenues of continuing products — extensive and intensive margins of product sales are conflated. Moreover, it *cannot identify* revenue changes stemming from changes in product prices or quantities sold. By departing from commonplace measures of performance, we show that the consequences of financial contracting extend beyond firm-level sales or profits. We contribute on a number of new dimensions by, among other things, decomposing revenues into prices and quantities, assessing each margin's role in product performance, and developing a product-level risk measure to gauge risk-taking in product markets. In addition, the product-store-time dimension we exploit enables us to characterize firms' operational decisions as a function of local

[5] We confirm that consumer goods prices are generally lower in large retail chains.

[6] Examples include Chevalier (1995) and Phillips (1995). Campello (2006) shows that the effect of debt financing can boost competitive performance to some extent, after which further leverage and investment become value-destroying.

[7] More recent empirical work investigating interactions between firm financing and product markets includes, among others, Frésard (2010), Cookson (2017), and Mendes (2024). See Frésard and Phillips (2022) for a comprehensive review.

economic and demographic conditions, product attributes, and competitive landscape, overcoming prior data limitations to provide more refined mapping of firm strategic responses into product market outcomes.

Our paper is also related to the literature on corporate bankruptcy. Benmelech et al. (2019) and Bernstein et al. (2019) show that establishment liquidations impose negative spatial spillovers, depressing local employment and triggering further closures. Using an online experimental approach, Antill and Hunter (2025) find that consumers are less willing to pay for products of firms that file for bankruptcy. He et al. (2024) show that firms divert resources away from customer relationships during restructuring, sacrificing investment in intangible customer capital such as marketing, R&D, worker compensation, and sales expenses. Our paper extends this literature in several dimensions. While Benmelech et al. (2019) and Bernstein et al. (2019) analyze bankruptcy at the establishment level, our transaction-level data enables us to identify previously undocumented dimensions: bankrupt firms' market exit and product discontinuation strategies. Our geographic analysis uniquely reveals that disinvestment in local presence during bankruptcy (He et al., 2024) concentrates in economically disadvantaged and underserved communities ("product deserts"). Our product-level analysis further reveals product retention patterns consistent with active portfolio management to preserve flagship offerings and upside potential. Our identification strategy isolates competitive price adjustments that occur exclusively where direct product rivalry exists, revealing localized dynamics not visible in conventional industry-level analyses. Complementing Antill and Hunter's (2025) experiment measuring consumer attitudes, we demonstrate that retail sales effects manifest primarily through quantities, not prices. Finally, we qualify prior findings (Kim, 2020) by showing that aggressive price-cutting strategies are only used by liquidating firms, while restructuring firms maintain pricing parity with competitors.

2 Corporate Bankruptcy and Market Dynamics

It is important that we provide context to our analysis by discussing bankruptcy filings and their disruptive market consequences. For concreteness, we largely focus on consumer goods manufacturers.

2.1 Capital Market Reactions

Corporate bankruptcy in the U.S. is governed primarily by two chapters of the Bankruptcy Code: Chapter 11 (reorganization) and Chapter 7 (liquidation). While investors may anticipate a likely filing, well-established evidence shows a pronounced "announcement effect" in the days surrounding filings, indicating that these events convey unanticipated information to the market (Datta and Iskandar-Datta (1995)). The literature further documents a strong link between bankruptcy and delisting, with most firms facing a transition to over-the-counter (OTC) trading or outright cancellation of equity (Macey et al. (2008)). The delisting process triggers sharp reductions in stock liquidity and valuation, as institutional investors are often restricted from holding OTC securities (see, e.g., Shumway (1997) and Shumway and Warther (1999)).

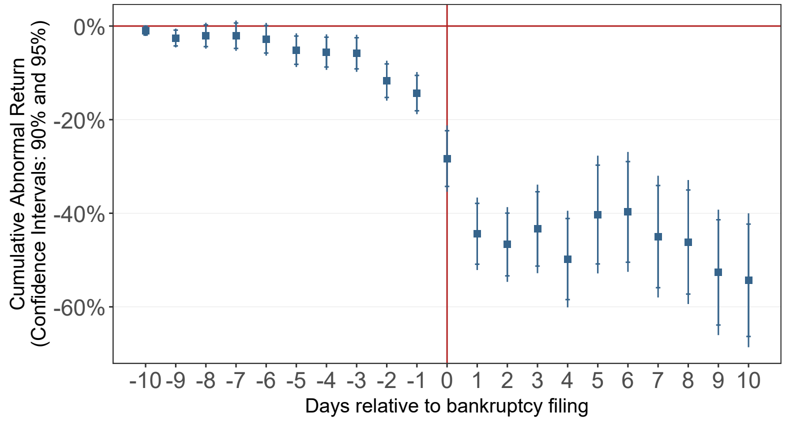

Figure 1. BRD Sample: CARs around Filings. This figure presents cumulative abrnormal returns (CARs) within a $[-10, +10]$ trading days window around the bankruptcy filings of 160 firms from the BRD sample that filed between 2007 and 2020. CARs are based on the Fama-French five-factor model over an estimation window of $[-252, -30]$ trading days before the filing. The bars indicate 90% and 95% confidence intervals based on Kolari and Pynnönen's (2010) standard errors to correct for cross-sectional correlation.

We illustrate the notion that filing for bankruptcy marks a structural break in a firm's trajectory by analyzing data from the Florida–UCLA–LoPucki Bankruptcy Research Database (BRD) over our sample period (2007–2020). These data comprise 464 bankruptcy filings by publicly traded companies, 24 of which are consumer goods manufacturers.[8] Figure 1 depicts an event study of CARs within the $[-10, 10]$ trading day window around the overall BRD sample filings. In short, it points to sharp, persistent devaluations around bankruptcy filings, reaching an average CAR of –54.3% 10 trading days after the filings. Although Figure 1 also shows evidence of anticipation by the markets, with an average CAR of –8.3% within the $[-10, -1]$ trading day window before the filing, most of the price effect is concentrated immediately after the filings. In particular, the day of the filing and the next trading day show average ARs of –14.3% and –16.1%, respectively. For consumer goods producers, these immediate reactions are even more dramatic, with average ARs in these trading days of –22.1% and –30.0%, respectively, and an average CAR of –64.4% 10 trading days after the filings.

These stylized facts reinforce the notion that bankruptcy filings represent structural breaks in corporate market trajectories, even when anticipated. Rather than being merely procedural conclusions to prolonged distress, formal bankruptcy filings represent pivotal moments that alter investment outlooks and strategic possibilities — they create distinct "before" and "after" regimes in the firm's market evolution despite their partial predictability.

2.2 Corporate Bankruptcy in the Consumer Goods Product Market

Our paper focuses on consumer goods manufacturing because of the high number of large historical bankruptcies. These industries serve as a laboratory in which we follow manufactured products until they reach end-users. As we detail below, bankruptcy cases in our final sample come from multiple

[8]Not surprisingly, filings cluster during the Great Financial Crisis and at the onset of the COVID-19 pandemic. See Figure A.1 for a breakdown of cases by year and chapter in the BRD sample.

data sources, covering 227 filings by both public and private firms between 2007 and 2020.[9] We present descriptive statistics and visualizations of firm characteristics, exploring various dimensions that shape corporate bankruptcies and their market outcomes.[10]

Received knowledge suggests that industry conditions dictate bankruptcy filings. As such, if most of our sample cases were driven by declining sectors such as records, tapes, and photographic supplies, our empirical results would largely capture evolving factors such as industry excess capacity and consumer tastes instead of firm-specific factors.[11] To investigate this possibility, we classify industries according to their growth trajectories following Maksimovic and Phillips (1998). For each Nielsen product group,[12] we calculate the log difference in total sales between our sample's final years (2018–2020) and early years (2006–2008), then organize these groups into growth quartiles ranging from declining (first quartile) to high-growth (fourth quartile). Finally, we assign each firm to the product group representing its largest sales share and examine the distribution of bankruptcy cases across group quartiles. Of our 227 cases, we find that 42, 50, 52, and 83 are assigned to quartiles one (declining) through four (high-growth), respectively. That is, high-growth industries account for almost twice as many bankruptcy cases as declining industries, suggesting that most cases in our sample cannot be attributed to industry-wide time variation associated with business decline.

To gain deeper insights into the narratives surrounding corporate bankruptcies, we leverage the advanced web search and information synthesis capabilities of large language models (LLMs). Specifically, the AI assistant (Google Gemini 2.5 Pro's "Deep Research" capability) was tasked with systematically investigating U.S. companies with bankruptcy information. For each company, the AI was instructed to perform simulated live web searches to identify the primary reasons underpinning its bankruptcy filing, targeting the specific filing date provided in our dataset. This process emphasized broad consultation of sources, prioritizing official documents such as SEC filings and court records, followed by reputable financial news outlets.

Following the information retrieval phase, the AI assistant was responsible for two key outputs. First, it generated a concise, factual summary of 300 characters encapsulating the main reasons for the firm's bankruptcy. Alongside the summary, the AI provided the full URL(s) for all facts included in the summary, ensuring verifiability. To confirm the quality of information and mitigate the typical hallucination problems from LLMs, we also performed a human analysis of the sources gathered by the AI assistant. Second, the AI assigned each bankruptcy event to a category describing the main reason behind the filing. The classification utilized a predefined list of five broad categories: (*i*) Idiosyncratic Firm-Level, (*ii*) Industry-Specific Shock, (*iii*) Macroeconomic Shock, (*iv*) Technological Change, and (*v*) Other.[13] This filtering process yielded a final sample of 157 distinct bankruptcy events with complete information.

[9] Our sample includes prominent cases such as Eastman Kodak, Hostess Brands, Spectrum Brands, Polaroid, and Lenox Corp.

[10] Figure A.2 presents several dimensions of these bankruptcies across listing status.

[11] Notably, our empirical strategy controls for such conditions by leveraging comparisons within granular product categories and close product market competitors. See Section 4.2 and Section 8.2.

[12] "Group" is Nielsen's second largest product classification, encompassing 125 distinct categories (see details in Section 3.1).

[13] To avoid hallucination, we instructed the model to assign missing values in cases where, despite comprehensive searches, no reliable information could be identified to determine the cause of bankruptcy.

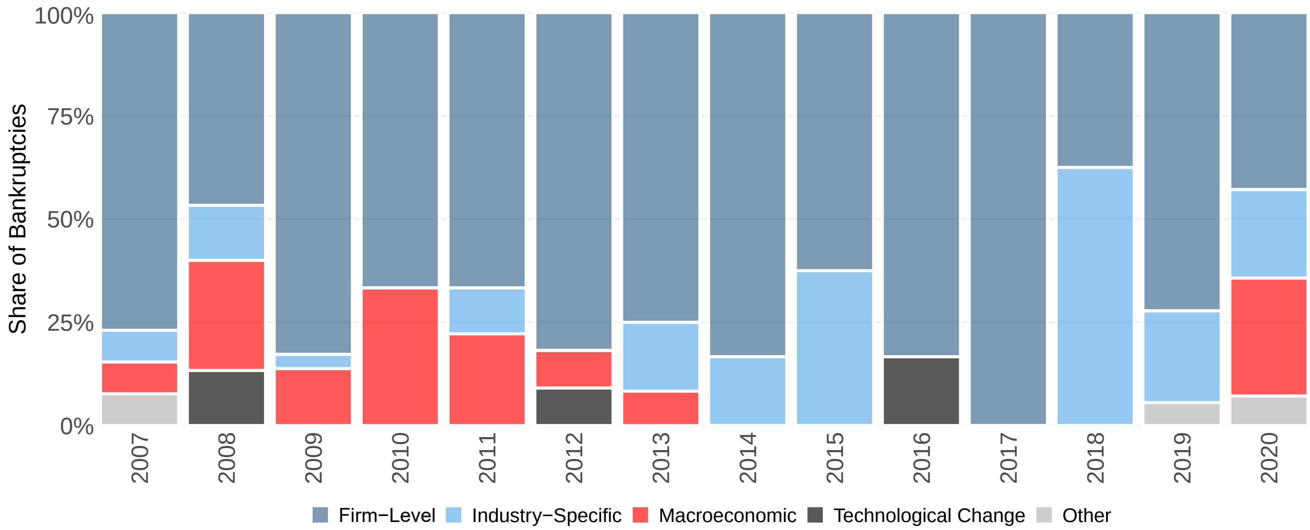

Figure 2. LLM-Classified Reasons for Bankruptcy (2007–2020). This figure presents the annual percentage distribution of 157 U.S. company bankruptcy events across five broad underlying cause categories. The classification into broad categories was performed by the LLM (Google Gemini) based on its analysis of bankruptcy summaries.

Figure 2 shows the percentage of bankruptcies per category in each year of the sample. One can see that crisis years — particularly the Great Recession (2008–2012), as well as the COVID-19 recession in 2020 — are associated with a predictable increase in the number of bankruptcies caused by macro shocks. Industry-specific shocks are also a substantial factor behind firms' financial distress. Most notably, however, the majority of bankruptcies in our sample period are classified by the LLM as the result of idiosyncratic, firm-level factors.

To further explore the specific qualitative factors driving bankruptcies within the five categories, we construct word clouds with the most distinctive terms from the AI-generated bankruptcy summaries.[14] We calculate the Term Frequency-Inverse Document Frequency (TF-IDF) scores for each word, measuring how important a word is to a document in a collection or corpus. It assigns higher weight to words that are frequent within the summaries of a particular bankruptcy category but relatively uncommon across other categories, thereby helping to identify distinctive terms for each specific narrative. The result is shown in Figure 3. Each panel corresponds to one of the bankruptcy categories, visually revealing the salient vocabulary associated with that category. The font size of each word directly reflects its TF-IDF score, offering an intuitive representation of its distinctiveness and importance within that narrative.

The word clouds in Figure 3 illustrate the distinct vocabularies associated with each bankruptcy category. Panel A for Idiosyncratic Firm-Level highlights terms such as "legal," "petition," "claims," "creditor," "lender," and "parent," pointing towards legal and regulatory entanglements or disputes with lenders specific to the

[14]To do so, the textual data from the AI-generated summary for these observations must go through several standard natural language preprocessing steps. This involved converting all text to lowercase, removing punctuation, special characters, and numerical digits to standardize the vocabulary. Subsequently, the text was tokenized into individual words. Standard English stop words (e.g., "the", "is", "are") were removed, thus ensuring that the analysis captured broader thematic elements.

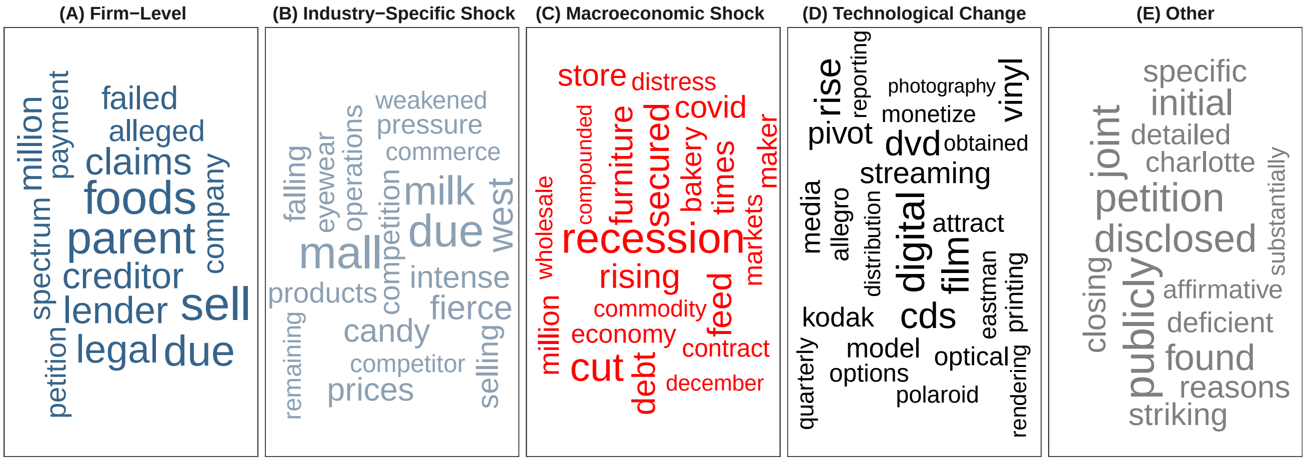

Figure 3. Word Clouds of AI-Generated Bankruptcy Reason Summaries by Mapped Category. This figure displays word clouds for the broad bankruptcy categories, representing the most distinctive terms identified via Term Frequency-Inverse Document Frequency (TF-IDF) analysis. The source text comprises 157 AI-generated summaries (max 300 characters each) detailing the primary causes of U.S. company bankruptcies, based on AI-powered web searches of court documents, SEC filings, and news articles. These summaries were then categorized by the AI into the broader categories of each panel. The text underwent preprocessing steps — tokenization, cleaning, removal of standard and custom stop words — before the calculation of the score. The font size of each word is proportional to its TF-IDF score, highlighting the most characteristic terms for each bankruptcy narrative.

firm's ownership. Panel B, representing "Industry-Specific Shock," prominently features words like "mall," "fierce," "competition," "prices," and "pressure;" indicating import competition and the challenges faced by traditional brick-and-mortar retail during our sample period, a phenomenon often referred to as "retail apocalypse," largely driven by the rise of e-commerce. Panel C for "Macroeconomic Shock" is characterized by terms including "recession," "economy," "distress," "financial," "markets," and notably, "covid," clearly reflecting broad adverse economic conditions. In Panel D, "Technological Change" underscores transformative shifts with terms like "digital," "eastman," "kodak," along with "cds," "vinyl," and "streaming;" directly reflecting narratives such as the decline of physical media like CDs due to digital streaming services, and the disruption faced by companies like Eastman–Kodak as traditional photographic film was supplanted by digital photography. Panel E shows miscellaneous words that do not fit into the other broad categories.

Finally, we employ a data-driven approach to examine the predictive power of different variables and factors with respect to bankruptcy filings. Specifically, we estimate survival models in a monthly panel of all our sample firms, in which the outcome variable of interest is an indicator that equals one in the month a firm files for bankruptcy, and zero otherwise. The coefficients in these regressions estimate how the variables are associated with the timing of the filing, and the goodness-of-fit measures inform us of the model's explanatory power in predicting bankruptcies. We include several firm-level variables of product market performance, indicators for industry growth quartiles as defined previously in this section, and indicators

Table 1. Bankruptcy Predictive Models. This table reports goodness-of-fit measures of multiple specifications and estimation models. The sample consists of firm-month observations where the dependent variable is one in the month a firm files for bankruptcy — after which the firm is excluded from the sample — and zero otherwise. In the models reported in the first row, the independent variables are logarithm of sales, number of products, number of stores, and number of counties. Sales is total firm revenue. Number of products is the count of unique different products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sells products from the firm. We also include the annual growth rate of each of these variables — the log difference between a variable in a given month and in the same month of the previous year. We also include indicators for industry quartiles based on industries' total sales growth between the sample's three final years (2018–2020) and early years (2006–2008). We define industry as Nielsen's product groups, and assign a firm to the group that represents the largest share of its sales. See Section 2.2 for details on industry classification. Each column specifies the estimation method and goodness-of-fit measure reported, in percentage points. See Section A.1 for more details on sample construction and estimation. The rows report the fixed effects included in the specification, with the first row including control variables only.

	OLS R^2	Logit Pseudo R^2	Probit Pseudo R^2	Cox Hazard Concordance
Base Control Variables	0.01	3.51	3.51	69.5
Year Effects	0.01	4.42	4.43	68.6
Industry Effects	0.01	4.57	4.56	74.9
Firm Effects	5.82	19.94	19.66	99.8
Industry-Year	0.08	5.59	5.60	79.5
Firm-Year Effects	20.12	15.85	15.23	100

for the periods of the GFC and COVID-19 pandemic.[15] In addition — and most importantly for our purposes at this stage — we gradually add fixed effects to capture the explanatory power of different factors of interest.

We report goodness-of-fit measures for several specifications and models in Table 1, in percentage points. Each column describes the estimation method and measure reported, while the rows report the fixed effects included in the specification, with the first row including control variables only. Overall, the measures show that product market performance of firms and industries and aggregate shocks have very limited explanatory power for bankruptcy events. For instance, even accounting for industry-specific yearly shocks, only 0.08% of the variation in bankruptcies is explained in an OLS model, with roughly 5.6% explained in a Logit or Probit model's Pseudo R^2.[16] Further supporting our discussion around Figure 2, firm-specific factors considerably improve the explanatory power of the models, with the regression R^2 increasing to roughly 6% when firm fixed effects are included, and to more than 20% if firm-specific yearly shocks are included.[17]

In sum, our comprehensive analysis of bankruptcy cases provides crucial context for understanding the product market consequences we examine. Our sample's anatomy reveals cases dominated by firm-specific factors rather than broader sectoral or economic trends, providing firm-level heterogeneity in bankruptcy causes across industries with different growth trajectories. Combined with our transaction-level data, this heterogeneity composes an ideal empirical setting to study how bankrupt firms and their competitors adjust their product market strategies while controlling for confounding industry-wide dynamics.

[15] We provide added details about variables and sample construction in Appendix A.1.

[16] Industries correspond to Nielsen's groups, therefore absorbing industry-level growth quartile indicators.

[17] The Pseudo R^2 of the Logit and Probit models with Firm-Year fixed effects is smaller than with Firm-only fixed effects because of the restrictions the former models impose on the sample. See Appendix A.1 for a detailed discussion.

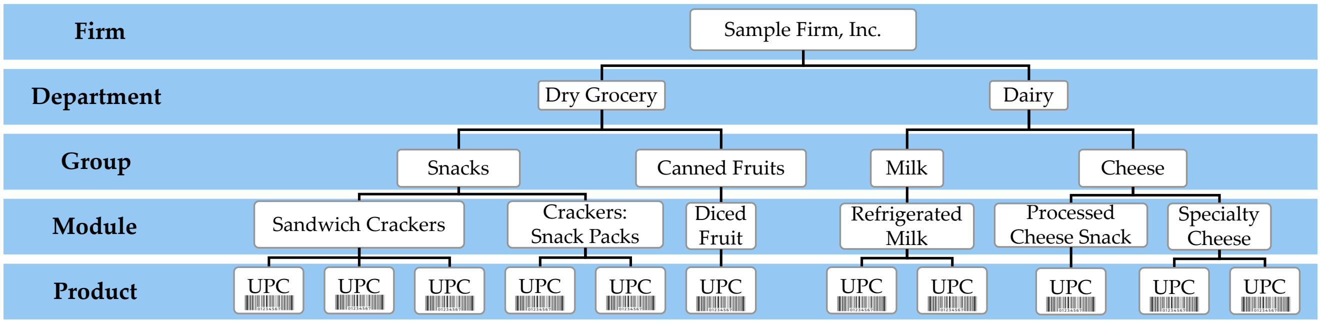

Figure 4. Hierarchy in the Nielsen Retail Scanner Data. This figure shows the hierarchy structure observed in the Nielsen Retail Scanner Data. At the top of the hierarchy is a firm whose products are divided by department, group, module, and finally by product (represented by a unique product code, UPC). UPCs are popularly known as bar codes.

3 Data and Sample Construction

3.1 Nielsen Retail Scanner Data

We compile and integrate data from multiple sources. Our product market data comes from the Nielsen Retail Scanner database, made available by the Kilts Center for Marketing Data Center at the University of Chicago Booth School of Business. This large-scale dataset reports product-level purchase information captured by point-of-sale scanners at participating U.S. retail stores at a weekly frequency. Our sample covers purchases made between January 2006 and December 2020.

The Nielsen dataset classifies products hierarchically. The most granular category is the individual *product*, identified by its Universal Product Code (UPC) — or *barcode*. Products are grouped into one of about 1,075 *modules*, which are the most granular product lines — similar to a 10-digit NAICS classification. These modules are then further aggregated into 125 *groups*, which form 10 *departments*, the coarsest product categories. We illustrate this hierarchy with an example in Figure 4, which depicts a part of the product portfolio of a dairy producer in our sample. As the figure shows, the sample firm produces goods from both the "Dry Goods" and "Dairy" departments. Each department encompasses two product groups in which the firm operates. The product group "Cheeses" further includes the modules "Processed Cheese Snack" and "Specialty Cheese." Finally, each of these modules might contain multiple individual products, identified by their UPCs.

We observe weekly quantities sold and average price for each product and store location, allowing us to compute sales value. The data track information from approximately 30,000 to 50,000 individual stores from around 90 retail chains across more than 2,700 counties, covering over 6 million unique products across all years.[18] This corresponds to roughly 53% of total food sales, 55% of drug store sales, and 32% of mass mer-

[18]Due to a confidentiality agreement with retailers, Nielsen does not disclose exact store addresses and chain names. Available geographic variables include 3-digit zip code, county, and designated market area (DMA).

chandise sales in the U.S., providing a rich and comprehensive outline of local consumer product markets in the economy (see Argente et al. (2017, 2024) for a detailed description of the data).

To identify the producers of each UPC, we leverage information from GS1 U.S. Data Hub. GS1 is the official organization responsible for issuing and managing UPCs. Any producer that wishes to obtain barcodes for its products must purchase a company prefix from GS1 and report its name and address. The prefix is a five- to ten-digit number that is placed at the beginning of any UPC belonging to its respective firm. This allow us to identify the manufacturer of the products in the Nielsen dataset. Using a list of prefixes issued by GS1, we are able to match over 80% of all UPCs in the retail scanner data to a firm name.

3.2 Nielsen Homescan Data

We supplement the Retail Scanner dataset with the Nielsen Homescan dataset, which reports product-level purchases captured at participant households' scanners. Besides information on products' UPCs, prices, and quantities purchased by households, the data reports coupon usage and deals for each purchase, as well as store visits by participants. Notably, the dataset also reports household socioeconomic and demographic characteristics such as income level, employment status, education, and 5-digit ZIP code of residence. Stores in the Homescan data can be matched to stores in the Retail Scanner data.

The Homescan data tracks approximately 40,000 households between 2004 and 2006 and 60,000 from 2007 onward. Nielsen uses a stratified sampling procedure to recruit and maintain panel households that match pre-selected demographic characteristics. In addition, the data report projection factors that allow researchers to extrapolate demographic characteristics to broader geographic locations, ensuring the sample's representativeness (see Einav et al. (2010) and Butler et al. (2023)). We leverage Nielsen Homescan data to track socioeconomic characteristics of the geographic locations where firms sell their products and coupon usage and deals in household purchases. For sales and other product-level information, we rely on Retail Scanner.

3.3 Bankruptcy Filings

We collect cases of corporate bankruptcy filings from four different sources: The Florida–UCLA–LoPucki BRD, SDC Platinum, the Federal Judicial Center's Integrated Database at Wharton Research Data Services (FJC–WRDS), and Capital IQ. The first three datasets report only bankruptcies of public firms. Capital IQ, however, also reports bankruptcies by private companies. From these sources, we collect the filer's name and the exact filing date. For all public firms and most private ones, we also observe whether the bankruptcy case was filed under Chapter 11 (restructuring) or Chapter 7 (liquidation), as well as the duration and outcome of the court process. We incorporate data from Bloomberg Terminal on a case-by-case basis for private firms with missing case information. Observing case outcomes is crucial

Table 2. Summary Statistics: Bankruptcy Cases. This table reports summary statistics of the 227 bankruptcies included in our sample. Panel A reports the number of product categories in which each firm operates, where product categories are Nielsen's modules, groups, and departments (see Section 3.1 for a detailed description of these categories). Panel B describes the number of bankrupt firms operating in each product category. We include only modules and groups for which we have at least one bankrupt firm operating. Our sample covers all departments in Nielsen data.

Panel A. Product Categories by Bankrupt Firm

Statistic	Min	Pct(25)	Median	Mean	Pct(75)	Max
Modules by firm	1	7	15	22.3	25	112
Groups by firm	1	3	7	8.6	10	29
Departments by firm	1	2	3	2.7	3	7

Panel B. Number of Bankrupt Firms by Product Categories

Statistic	Min	Pct(25)	Median	Mean	Pct(75)	Max
Firms by module	1	2	4	5.2	7	23
Firms by group	1	7	13	13.6	18	29
Firms by department	8	55	65	66.2	93	93

as firms that initially enter Chapter 11 can have their process converted to Chapter 7 and (very rarely) vice-versa. In addition, firms might also be liquidated under Chapter 11.

We restrict attention to the cases initiated between 2007 and 2020, as we require available product market data for at least one year before bankruptcy proceedings begin since we construct our main dependent variables as yearly growth rates. We match bankruptcy information to our Nielsen–GS1 dataset using producer names by following the string matching algorithm proposed by Schoenle (2017) and Argente et al. (2017). We manually inspect each match and thoroughly search the internet for names and addresses to discard likely false positives. This procedure leaves us with 215 bankruptcy cases, 19 of which involve public firms. To account for subsidiaries when a parent firm enters bankruptcy, we incorporate information from WRDS, implementing the algorithm by Schoenle (2017). This adds 12 more firms to our sample, all of which are subsidiaries of public companies. As a result, our final sample consists of 227 bankruptcy cases, of which 31 are by public firms or their subsidiaries and 196 by private ones. For all but three small private firms, we have information on the outcome and other details of the case.

Table 2 reports summary statistics of the bankruptcy cases in our sample. Panel A describes the operational breadth of firms across product categories. On average, a firm that files for bankruptcy within our sample operates in roughly 22 modules, 9 groups, and 3 departments, with large heterogeneity across firms. While some firms specialize in a single module, such as "Video Products Prerecorded" or "Water - Bottled." One firm (Spectrum Brands) has products in 112 modules.

Panel B of Table 2 shows our sample's coverage of product categories. All 10 departments in the Nielsen data are covered in our sample. The department with the least number of bankrupt firms is in "Packaged Meat" with 8 firms, while 93 bankrupt firms had products in "General Merchandise." A total of 110 groups and 628 modules have products from at least one firm that entered bankruptcy.

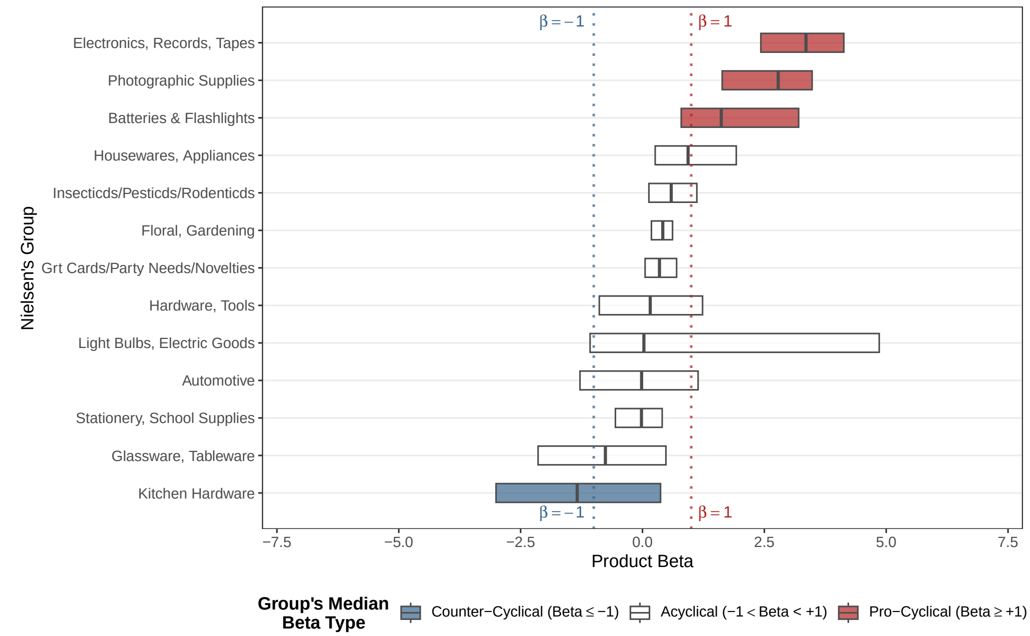

Figure 5. Distribution of Product Betas: Groups in Department "General Merchandise". This figure shows the distribution of product betas across various Nielsen groups within the "General Merchandise" department. The graph includes only groups with at least 50 unique products. Each boxplot summarizes the distribution of betas within a product group. The box represents the interquartile range (IQR), capturing the middle 50% of the data, with the horizontal line inside the box indicating the median. Product beta measures the sensitivity of a product's sales to consumption at the department level, i.e., a more aggregate level of consumption at the firm's main line of business. The beta is estimated as the coefficient from a regression of log product sales on log department-level sales. Products are categorized into three groups based on their beta: *Counter-Cyclical* ($\beta \leq -1$, shown in blue), *Acyclical* ($-1 < \beta < 1$, shown in white), and *Pro-Cyclical* ($\beta \geq 1$, shown in red). The boxplots are ordered by their median beta. The red and blue vertical lines are reference points for the pro-cyclical ($\beta \geq 1$) and counter-cyclical ($\beta \leq -1$) thresholds.

3.4 Product Beta: A New Measure of Consumption Sensitivity

Corporate finance theory suggests that firms in distress may strategically adjust their risk profiles. Early theoretical work by Brander and Lewis (1986) argues that firms with high leverage may pursue riskier strategies in product markets (see also Campello (2006)). More recently, Aretz et al. (2019) show empirical evidence that distressed firms shift operations towards riskier business segments. The underlying theory is that those firms rationally shift toward higher-risk investments with asymmetric payoff profiles offering greater upside potential (Campello and Kankanhalli (2024) provide a related argument). To empirically test whether bankrupt firms systematically retain riskier products in their portfolios — consistent with a "gambling for resurrection" strategy — we introduce a measure that captures risk at the product level. In short, building on established measures of product attributes such as quality and durability, our novel metric quantifies a product's sensitivity to aggregate consumption fluctuations.

For each product i, we estimate a regression of the log of monthly sales on the log of total sales in its department — the most general description of the product's segment. Formally:

$$\log(\mathit{Product\ Sales}_{i,t}) = \alpha_i + \beta_i \cdot \log(\mathit{Nielsen\ Department\ Sales}_t) + \varepsilon_{i,t}, \tag{1}$$

where the coefficient β_i from Equation (1), which we call *Product Beta*, captures the elasticity of product i's sales to broader consumption trends in that line of business. To wit, a product beta greater than one ($\beta_i > 1$) indicates the product's sales are more volatile than its department's overall consumption. In contrast, a beta lower than minus one ($\beta_i < -1$) indicates a counter-cyclical behavior relative to broader consumption trends. Firms with a high-beta product portfolio are riskier in that their revenues are more sensitive to aggregate cycles in their lines of business.

Figure 5 focuses on our sample's largest department "General Merchandise" and reveals substantial heterogeneity in consumption sensitivity across product groups. We classify products into three categories based on within-group median betas: "Counter-Cyclical" ($\beta \leq -1$, shown in blue), "Acyclical" ($-1 < \beta < 1$, shown in white), and "Pro-Cyclical" ($\beta \geq 1$, shown in red). Let's briefly discuss the patterns in the figure:

- Discretionary and durable goods show the highest *pro-cyclical* sensitivity: *"Electronics, Records, and Tapes"* lead with the highest median beta, followed by *"Photographic Supplies"* and *"Batteries and Flashlights."* Consumers seem prone to postponing these purchases during states of lower consumption. *"Batteries and Flashlights"* also display high cyclicality ($\beta \geq 1$), likely due to bulk-buying during higher-income periods.

- Household necessities cluster in the *acyclical* range. For example, *"Insecticides, Pesticides, and Rodenticides"*, as well as *"Floral and Gardening"*, show stable demand regardless of economic conditions. Narrow interquartile ranges for *"Stationery and School Supplies"* highlight their reliability as resilient consumption staples. While *"Hardware and Tools"* and *"Light Bulbs and Electric Goods"* are also acyclical, they show a wide distribution, suggesting category heterogeneity.

- The *counter-cyclical* group includes *"Kitchen Hardware,"* possibly reflecting increased home cooking in challenging times. Relatedly, *"Glassware and Tableware"* also trends toward counter-cyclicality, suggesting more purchases of these items during recessions.

In addition to our Product Beta measure, we incorporate metrics of product attributes and importance to comprehensively assess firms' strategic decisions during bankruptcy. Following Argente et al. (2017), we proxy for product quality with the relative distance between a product's unit price and its respective module's median price. We also compute each product's share of (1) total firm sales and (2) total module sales separately, thereby capturing the product's importance to both its producer and within its segment. By combining these metrics, we create a multidimensional framework for analyzing how product characteristics influence firms' operational decisions during bankruptcy.

3.5 Price Patterns: Large Retailers and "Product Deserts"

A key advantage of product-level data is allowing us to trace product price patterns across retailers and locations. Since the price of a product can vary across regions, even within the same retail chain (Butters et al. (2022)), these price differentials are relevant for firms' operational decisions and competitive environment. In this section, we document two stylized facts that we will later show are key drivers of bankrupt firms and their competitors' behavior.

First, larger retail chains typically offer lower prices than local, smaller retailers, primarily through economies of scale. Large retailers obtain discounts from suppliers for purchasing in bulk and have more efficient operations (e.g., integrated warehouse distribution systems, reducing transportation and inventory costs) that translate into lower per-unit costs. Second, areas that are underserved by retailers typically exhibit lower prices. Naturally, areas with low retail presence tend to be relatively isolated, sparsely populated, and characterized by populations facing more challenging socioeconomic conditions. These constraints translate into price-sensitive consumer bases with higher demand elasticity. In addition, the retail landscape in these locations differs from affluent areas for being served primarily by discount chains and dollar stores. On the other hand, higher transportation costs to serve isolated areas could translate into higher product prices. While quantifying these multiple margins is beyond the scope of this paper, we document that product prices are lower at large retailers and isolated areas in our retail scanner sample.

To establish these empirical patterns, we construct two indicator variables. First, we classify *Large Retailer* as those retail chains that are above mean total sales across all retailers during the sample period.[19] Second, we define *Product Desert* as an indicator of low availability of wide ranges of products in a county. Following Alcott et al. (2019), we compute the county-level number of stores of large retailers per capita and classify as product deserts those counties in the bottom quartile of the yearly distributions.[20] Next, we construct a product-store-month level sample of our matched products and regress the log of price on these two indicators including the interaction term. We include product and month-year fixed effects to ensure within-product price comparisons across locations and control for overall trends in product prices.[21]

We summarize the results of this regression in Table 3. The reference (omitted) category corresponds to products in small chains in non-product deserts. The cells in the upper-right and lower-left of the table report the coefficients for each indicator variable separately, while the lower-right cell reports the total differential; i.e., the sum of the individual and interaction term coefficients. The estimates show that, within non-product deserts, a product is 3.15% cheaper in large retail chains, on average. Within small chains, a product is 2.09% cheaper in product deserts. Finally, products for sale in large retail chains located in product deserts are 4.84% cheaper. These simple tests suggest that products in large retailers and low-retail access locations are cheaper, and these differences compound.

To illustrate these price patterns across product deserts more concretely, we focus on Florida as a representative case study. Figure 6 highlights our key stylized fact that product deserts systematically exhibit lower price levels than non-deserts. Florida offers an ideal setting to demonstrate this pattern due to its

[19]We note that this corresponds to the top quintile of the distribution of retail chains by total sales within the sample, naturally reflecting a right-skewed distribution of retail sales.

[20]Although we define *Product Desert* as time varying in our baseline analysis, counties rarely change status within the sample.

[21]See Section 4.2 for details on product-level sample construction.

Table 3. Price Differences by Retailer Size and Product Desert Status. This table reports coefficients from the estimation of a regression of product-level log prices on indicators for large retail chains and product deserts. The sample consists of product-store-month observations , where "Large Retailer" is an indicator variable that equals one if the retail chain is above mean share of total retail sales throughout the sample period. "Product Desert" is an indicator that equals one if the county is in the bottom quartile of the distribution of number of stores from large retailers per capita in a given month, as in Alcott et al. (2019). The estimates reported correspond to price differentials relative to the baseline category (small retailers in non-product deserts). The regression includes product and month-year fixed effects. t-statistics are computed using robust standard errors double-clustered by product and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Non-Product Desert	Product Desert
Small Retailer	–	−2.09 (−11.53)***
Large Retailer	−3.15 (−14.70)***	−4.84 (−20.98)***

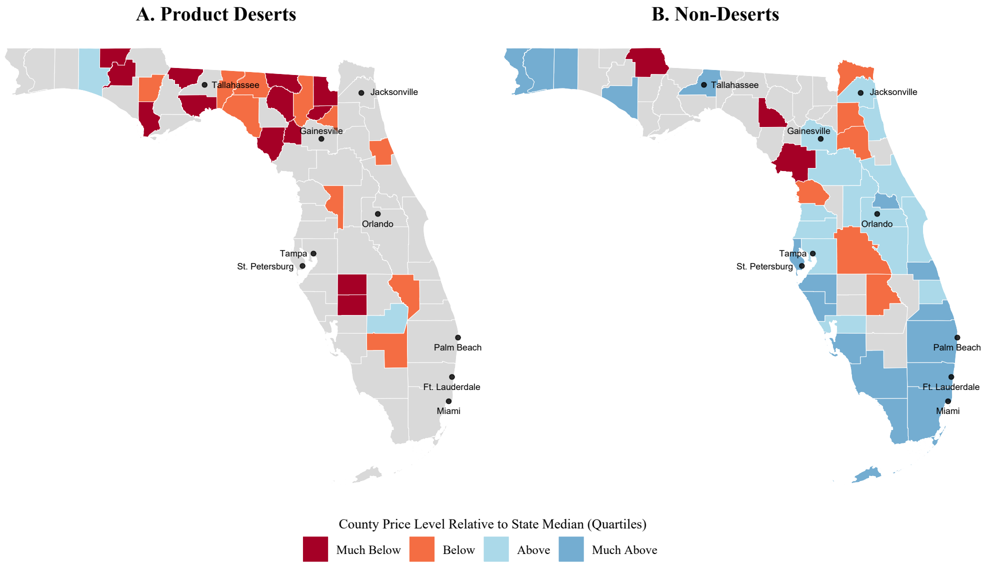

Figure 6. Product Deserts and Price Levels: Florida Counties. This figure illustrates a key stylized fact: product deserts systematically exhibit lower price levels than non-deserts. Counties are classified as product deserts (Panel A) or non-deserts (Panel B) based on retail store availability per capita, i.e., areas in the bottom quartile of the distribution of the number of stores from large retailers per capita. Large retailers are defined as being above the average total revenues within the sample period. Colors represent county price levels relative to the Florida state median, categorized into quartiles: Much Below (dark red), Below (orange), Above (light blue), and Much Above (dark blue). Product desert counties are predominantly located in rural North Florida and inland areas, exhibiting systematically lower price levels. Non-desert counties are concentrated in major metropolitan areas along the coast, with price levels above the state median. Major cities are marked for reference.

substantial geographic and demographic heterogeneity. The state encompasses major metropolitan areas (Miami–Dade, Tampa Bay, Jacksonville, Orlando) alongside sparsely populated rural counties in the Panhandle and the Everglades, generating stark contrasts in retail infrastructure. Panel A displays counties classified as product deserts, which are predominantly concentrated in North Florida — particularly in rural areas surrounding Tallahassee and Gainesville — as well as select inland counties in Central and South Florida. These areas exhibit systematically lower price levels relative to the state median, with most desert counties falling into the bottom two quartiles ("Much Below" or "Below"). In contrast, Panel B shows non-desert counties, which are primarily clustered in urban coastal regions and display price levels above the state median.

4 Empirical Strategy

4.1 Variable Construction

We aggregate the data from weekly to monthly frequency for processing feasibility and to smooth within-month consumption fluctuations. For each product within a store, we sum the quantity of units sold and revenue across weeks within a month. Next, we divide the total revenue by the quantity to obtain a sales-weighted average monthly price. We use the resulting data to build our variables of interest both at the *firm*×*month* and *product*×*store*×*month* levels. We follow Hyun and Kim (2025) and measure our dependent variables in annual growth rates (reported in percentage points). They are computed as the log difference between a given month and in the same month of the previous year.[22] To alleviate the effect of outliers, we winsorize all growth measures at the 2.5% tails.

4.1.1 Firm-Level Analysis

At the firm level, we build four dependent variables: sales growth, growth in the number of products sold, growth in the number of stores, and growth in the number of counties where products are sold. Sales is a firm's total revenue across all its products and retailers in a month. This measures firms' overall sales performance, encompassing both the quantity and price margins. The number of products is the count of unique UPCs that a producer firm sells across all U.S. stores, thus measuring the size of a firm's portfolio. The number of stores is the count of unique stores that sell any product from the firm, thus measuring product availability at retailers. Our fourth variable is the number of counties with stores that sell any product from the firm, thus measuring geographic availability of products throughout the U.S. Contrasting changes in the number of stores and counties allow us to better characterize responses in product availability by bankrupt firms. For instance, firms might discontinue operations in some stores uniformly across counties — in which case we would not see significant changes in the number of counties with products — or stop selling at specific locations completely, leaving entire areas with no company products available.

4.1.2 Product-Level Analysis

At the product level, we construct three variables of interest. The first one is sales growth, which is analogous to its firm-level version, but computed for each particular product in each store where it is sold. One major advantage of the Retail Scanner data is that they allow us to decompose revenues into local prices and quantities, which is critical to determine the strategic margins at which firms and their competitors respond to bankruptcy events. Accordingly, our other two product variables are quantity growth and price growth, where quantity is the number of units of a product sold at a given store in a month, and price is

[22] Results are qualitatively similar if we measure our variables in log levels instead. See Appendix B.

its average price during that month. Conveniently, owing to the properties of logarithms, quantity growth plus price growth approximately equals sales growth.

4.2 Matching Procedure

Entering bankruptcy proceedings is not a randomly assigned treatment across firms. Instead, it signals that past performance has been poor enough to hamper a firm's capacity to meet its financial obligations. As such, simple comparisons between firms that filed for bankruptcy and others may be misleading, as they fail to control for the confounding factors of poor performance. To account for such factors and alleviate concerns about endogeneity biases in our tests, we follow the matching strategy of Fracassi et al. (2022). Specifically, we match each bankrupt firm and its product–store pairs to competitors who never filed for bankruptcy throughout the sample period, but that are similar along multiple observable characteristics before the bankruptcy event. After matching treated units to their closest controls, we follow and compare their trajectories thereafter. We refer to bankrupt firms and their products as "treated" units. We discuss the details of our matching in turn.

In our firm-level tests, each bankrupt firm is matched to three counterfactual competitors. In doing so, we first scale each attribute used in the matching by its sample standard deviation and compute the absolute value of the standardized differences between each treated firm and the firms from the pool of potential controls. Then, for each pair of treated–control firms, we sum these standardized absolute differences to get an overall distance measure; i.e., the "Manhattan distance" between pairs of firms. Specifically, suppose that x_i^a is the value of attribute a for firm i. The overall distance between firms i and j is given by

$$D_{i,j} = \sum_a \frac{| x_i^a - x_j^a |}{\sigma^a} \tag{2}$$

where σ^a is the sample's standard deviation of attribute a. We compute Manhattan distances between each bankrupt firm and all other never-bankrupt firms in the sample based on total monthly sales, sales growth, number of unique products sold, and number of stores with products as of the month prior to the event. Finally, for each bankrupt firm, we select the three controls with the smallest overall distances to serve as the counterfactuals. We refer to each group of treated and counterfactual firms as a "cohort."

At the product level, we follow similar steps. However, we restrict our sample to stores and modules with treated and control products ("shelf neighbors"). Next, we compute the distance between each treated product and its shelf neighbors following Equation (2) based on total units sold, price, growth in units sold, and price growth. Finally, we select the three closest products as counterfactuals for each treated product. This procedure results in over 1.7 million matches between products from firms in bankruptcy and their shelf neighbors.

Consider the following example as an illustration of our product-matching method. Assume the dairy producer depicted in Figure 4 enters bankruptcy in July 2014. Suppose it sells specialty cheeses in chain A's store located in city X. We look for other products from the same module (i.e., "Specialty Cheese") sold in the same store. From these products, we select the three closest products to the bankrupt firm's product based on the aforementioned variables, considering their values as of June 2014. The four-unit cohort in this example consists of the bankrupt product and its three non-bankrupt matches in that particular module-store.

Consider the following example as an illustration of our product-matching method. Assume the dairy producer depicted in Figure 4 enters bankruptcy in July 2014. Suppose it sells specialty cheeses in chain A's Store #12345 located in city X. We look for other products from the same module (i.e., "Specialty Cheese") sold in the same store. From these products, we select the three closest products to the bankrupt firm's product based on the aforementioned variables, considering their values as of June 2014. The four-unit cohort in this example consists of the bankrupt product and its three non-bankrupt matches in that particular module-store. Critically, Nielsen assigns unique store identifiers to each retail location, allowing us to distinguish, for example, between Store #12345 in city X and Store #67890 in city Y — even within the same retail chain. Thus, when we match a bankrupt firm's specialty cheese to its "shelf neighbors," we are comparing products sold at the *same individual store*, not aggregating across all stores of a retail chain in a given area. In most cases, this means our matched products are plausibly sitting on the same store shelves, competing for the same local consumers. This level of precision allows us to control for store-specific demand shocks and isolate the effects of bankruptcy on product performance with remarkable granularity.

4.3 Summary Statistics

We report summary statistics of multiple samples in Table 4. Panels A and B report our firm- and product-level samples, respectively. In Panel A, we present the full firm sample, comprising 123,411 firm-month observations. We observe heterogeneity as firm averages are substantially larger than medians across all measurements, consistent with a right-skewed firm size distribution. For instance, the median (average) firms sell 10 (50.6) unique products sold at 342 (3,188) stores, located in 112 (685) counties. We also report summary statistics for our samples of bankrupt and matched counterfactual firms at the time of matching (the month before the bankruptcy filing) separately. Finally, we report differences in means between treated and control firms and the p-values of their respective t-statistics. We observe no significant differences in any of the variables presented, confirming the validity of our firm matching.

Panel B of Table 4 describes our sample of products. The total product-level sample consists of over 412 million product-store-month observations of sales, quantities, and prices. The median (average) products sell 8 (21.2) units per store month at 3.2 (5.4) dollars and generate over 27 (64) dollars in revenue. The fat-tailed distributions of both firms' and products' characteristics are consistent with the results by Argente et al. (2017). We also report summary statistics for products of bankrupt firms

Table 4. Summary Statistics: Firm- and Product-Level Samples. This table reports summary statistics of the firm- and product-level samples. The samples span from January 2006 to December 2020. Each panel reports the number of observations, mean, standard deviation, median, and interquartile range (IQR) for the observations, conditional on their treatment status. The last two columns report the differences in means between the treated and control groups, as well as the p-values of their respective t-statistics. On Panel A, observations are at the firm-month level. Sales are firms' monthly total sales, expressed in thousands of dollars. The number of products is the count of unique products that a firm sells. The number of stores is the count of unique stores that sell products from the firm. The number of counties is the number of unique counties with stores that sell products from the firm. Treated firms are those that filed for bankruptcy within the sample period, and controls are firms of similar performance that did not file for bankruptcy within our sample period. The matching is based on monthly sales, sales growth, the number of unique products sold, and the number of stores with products from the firm in the month preceding the bankruptcy event. On Panel B, observations are at the product-store-month level. Sales refer to the monthly total sales of the products, expressed in dollars. Quantity refers to the total number of product units sold in a month. Price is the monthly average sale price of the product, in dollars. Treated products are those produced by firms that filed for bankruptcy within the sample period. Control products belong to the same module and store as treated products and exhibit similar performance to their respective treated products before the bankruptcy filing (See Section 4.2 for details of the matching procedure).

Panel A. Firm-Level Matched Sample

	Full Sample					Treated Firms					Matched Controls					Mean Differences	
Statistic	N	Mean	St. Dev.	Median	IQR	N	Mean	St. Dev.	Median	IQR	N	Mean	St. Dev.	Median	IQR	Diff.	p-values
Sales (in thousands)	123,411	882.5	4,660.9	24.9	252.2	227	691.0	4,155.1	20.2	140.5	681	730.4	3,997.6	12.6	145.6	−39.4	0.90
Number of Products	123,411	50.6	119.0	10	39	227	43.8	106.9	8	30	681	43.4	106.7	8	31	0.4	0.96
Number of Stores	123,411	3,188.3	6,340.1	342	2,744.0	227	2,718.4	5,807,9	302	1,995	681	2,657.3	5,830.3	260	2,026	61.1	0.89
Number of Counties	123,411	487.5	685.3	112	722	227	464.5	644.4	123	624	681	413.7	620.9	91	593	50.8	0.30

Panel B. Product-Level Matched Sample

	Full Sample					Treated Products					Matched Controls					Mean Differences	
Statistic	N	Mean	St. Dev.	Median	IQR	N	Mean	St. Dev.	Median	IQR	N	Mean	St. Dev.	Median	IQR	Diff.	p-values
Sales	412,877,765	64.2	136.3	27.2	56.5	1,740,513	45.1	65.0	19.4	43.0	4,955,006	45.2	62.8	20.4	44.5	−0.1	0.22
Quantity	412,877,765	21.2	52	8	18	1,740,513	13.5	20.8	5	12	4,955,006	12.9	19	5	13	0.6	0.00
Price	412,877,765	4.6	5.4	3.2	2.6	1,740,513	4.8	4.8	3.3	2.8	4,955,006	4.8	4.6	3.5	2.6	0.0	0.06

and their matched counterfactuals, respectively, at the time of the matching. We also report mean differences across treated and control products and the p-values of their respective t-statistics. Differences across groups are minimal in terms of economic magnitude.

4.4 Empirical Specifications

4.4.1 Firm-Level

Our baseline empirical specifications consist of stacked cohort difference-in-differences (DIDs). At the firm level, we estimate the following regression:

$$y_{f,c,t} = \beta \cdot Bankrupt_{f,c,t-1} + \mu_{f,c} + \mu_{t,c} + \epsilon_{f,c,t} \tag{3}$$

where $y_{f,c,t}$ is the outcome of interest for firm f, cohort c. and month t. $Bankrupt_{f,c,t-1}$ is an indicator that equals one if firm f from cohort c entered bankruptcy in the previous month. The firm-cohort fixed effect $\mu_{f,c}$ implies an outcome comparison within the same set of treated–control firm groupings. The time-cohort fixed effect $\mu_{t,c}$ ensures that each bankrupt firm is compared only with its matched control set each month before and after bankruptcy. The coefficient of interest, β, estimates the impact of bankruptcy on the dependent variable relative to similar control firms around the period of the event. We use two different clustering schemes to account for correlation in the error term within firms and points in time. First, we cluster standard errors by firms. Second, we double-cluster standard errors by firm and month-years. We report the two respective t-statistics below each coefficient estimate in our tables.

Together with tabulated results, we conduct an event study to capture the dynamic effects of bankruptcy on the matched sample. We restrict observations of bankrupt firms and their matches to the 12 months before and 24 months after the bankruptcy event. We estimate the following specification:

$$y_{f,c,t} = \sum_{\substack{k=-12 \\ k\neq -1}}^{24} \beta_k \cdot \left[Bankrupt_{f,c,t-1} \times \mathbb{1}_k\right] + \mu_{f,c} + \mu_{t,c} + \epsilon_{f,c,t} \tag{4}$$

The coefficients of interest, β_k, estimate the differential effect of bankruptcy relative to matched counterfactuals over months around the event, compared to the omitted category, which is the month before the event. Standard errors are double clustered at the firm and month-year levels.

4.4.2 Product-Level

The dependent variables in Section 4.4.1 encompass firms' whole operations. In particular, we stress that "firm sales" aggregates revenues across several products and stores. This is a commonly used measure of firm performance in the product markets literature. Critically, however, firm sales *cannot differentiate* between changes in the product portfolio or availability at points of sale and lower revenues of continuing products (i.e., extensive and intensive margins of product sales are conflated). Moreover, it *cannot identify* revenue movements stemming from changes in product prices or quantities sold. To disentangle the effects observed at the firm level, we also perform a product-level analysis where we decompose sales into prices and quantities of each UPC at each store where it is sold. Specifically, we estimate the following specification:

$$y_{i,c,t} = \beta \cdot Bankrupt_{i,c,t-1} + \mu_{i,c} + \mu_{t,c} + \epsilon_{i,c,t} \tag{5}$$

where y is the dependent variable for product i, cohort c, and month t. All other terms in the regression model are analogous to those of our firm-level regressions. In Equation (5), β measures how products from firms in bankruptcy perform relative to similar products from healthy peers after the bankruptcy event. Again, we use two different clustering schemes. First, we double-cluster standard errors by product and month-year. Second, we double-cluster standard errors by firm and month-year. We report two respective t-statistics below each coefficient estimate.

5 Firm-Level Results

5.1 Baseline

We report estimation results of Equation (3) in Table 5. While financial distress is naturally associated with prolonged poor performance, the estimates confirm that entering bankruptcy is a discrete event that leads to severe disruptions in many aspects of product market operations. Post-bankruptcy, firms experience lower growth in sales, number of unique products sold, number of stores where products are sold, and counties with products available relative to similar non-bankrupt firms. The economic magnitudes of the coefficients presented are quite significant. For instance, bankrupt firms have roughly 37 percentage points (p.p.) lower growth in sales relative to their matched counterfactuals, which corresponds to roughly 31% of the within-sample standard deviation of sales growth. Columns 2 through 4 show that there is a concurrent contraction in product offerings as well as number of stores and localities where products are sold.[23] All estimates are statistically significant at the 0.1% level under both clustering schemes.

Despite our matching procedure alleviating self-selection concerns, a causal interpretation of the coefficients in Table 5 is still challenging. For instance, bankruptcy could result from exceptionally low past performance, even *vis-à-vis* counterfactual firms. In that case, we would observe differing pretrends across treated and control firms resulting from the lack of suitable counterfactuals in the sample. To test for parallel trends before bankruptcy and assess its dynamic effects over time, we report estimates of Equation (4) on Figure 7. The results reinforce the quality of the matching: treated and control firms show no "differing pretrends" before the bankruptcy event. After the event, however, bankrupt firms experience a sharp and almost immediate decline in all outcomes relative to their matched controls. These effects are highly sta-

[23] In Table A.2 we show that our results are robust to matching firms in the quarter, semester, and year before the filing.

Table 5. Bankruptcy Effects: Firm-Level Evidence. This table reports the DID coefficient β from the estimation of Equation (3). The dependent variables are growth in sales, number of products, number of stores, and number of counties. Growth rates are defined as the log difference between a variable in a given month and in the same month of the previous year in percentage points. Sales is total firm revenue. Number of products is the count of unique different products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sells products from the firm. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. The sample consists of firm-month-level observations of bankrupt firms matched to counterfactual controls. Each bankrupt firm is matched to three non-bankrupt counterfactual firms. The matching is based on monthly sales, sales growth, number of unique products sold, and number of stores with products from the firm in the month before the bankruptcy filing. A treated-controls group is defined as a *cohort*, and the sample is a stack of cohorts. All specifications include firm-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] parentheses, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt	−36.81	−14.74	−30.49	−24.15
	(−6.00)***	(−7.67)***	(−6.62)***	(−6.22)***
	[−5.95]***	[−7.45]***	[−6.62]***	[−6.25]***
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	107,502	107,502	107,502	107,502
Adjusted R^2	0.23	0.19	0.22	0.21

Note: * $p<0.05$; ** $p<0.01$; *** $p<0.001$

tistically significant and persist for most of the 24 months following entry into bankruptcy, consistent with the fact that most firms emerge from bankruptcy court within that window.[24]

5.2 Operational Decisions

Our results in Section 5.1 show that firms in bankruptcy discontinue operations by withdrawing products and ceasing to supply specific stores and whole geographic locations. While it is expected that firms in bankruptcy narrow their operational breadth, a relevant question is how these firms choose which operations to discontinue. As bankruptcy can trigger redeployment of assets across firms in an industry (Maksimovic and Phillips (1998)), it can also induce relocation of capacity within a firm. Our data offer a rich setting to study such sorting decisions by allowing us to define operations in terms of both products and locations. In this section, we examine the operational restructuring of firms in bankruptcy along these two dimensions. First, we trace the characteristics of products that are more likely to be withdrawn from all shelves across the country. Second, we identify the factors that predict the likelihood of bankrupt firms ceasing to sell their products at a specific location.

[24] We report dynamic estimates in levels instead of growth rates in Figure A.3. The results are qualitatively similar.

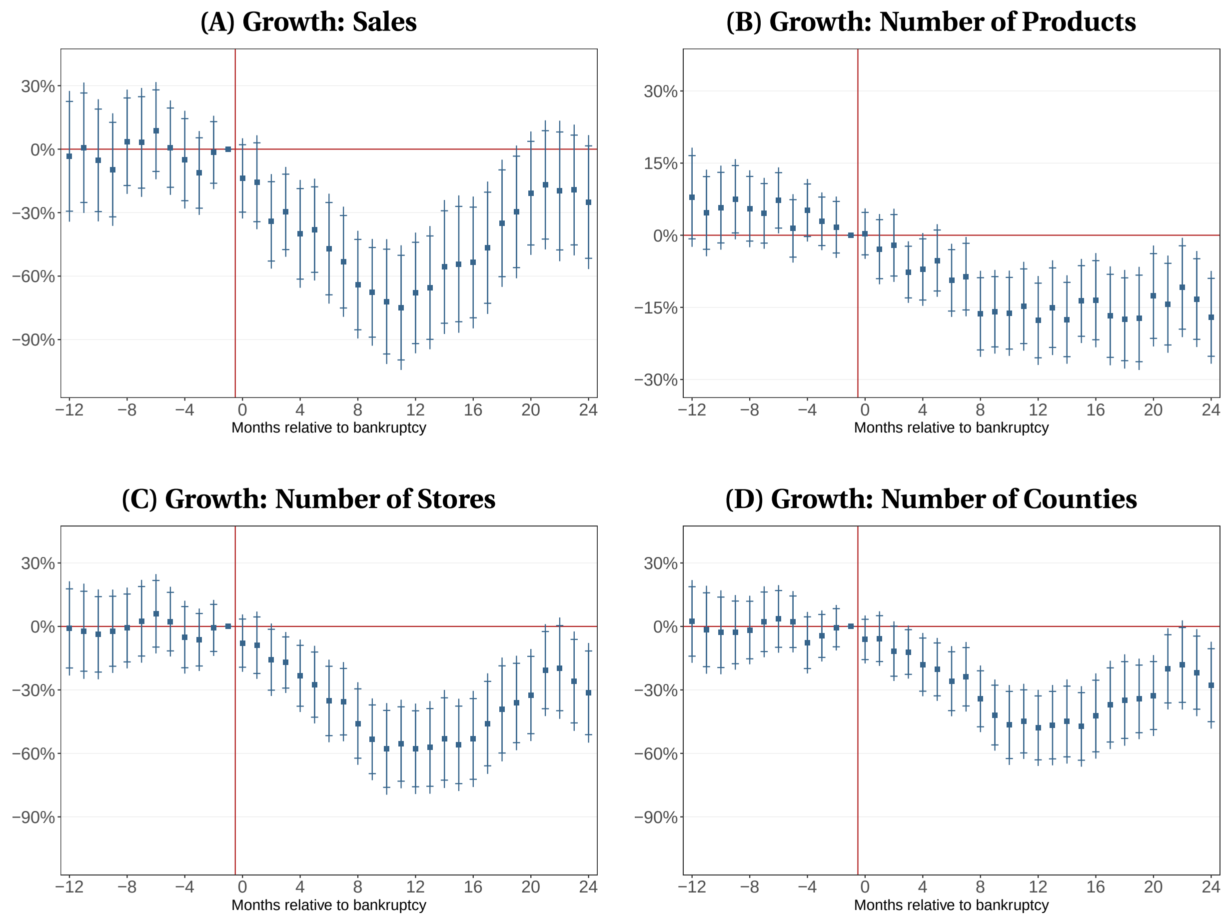

Figure 7. The Dynamic Effects of Corporate Bankruptcy on Firm-Level Outcomes. This figure plots the coefficient estimates of Equation (4) on four firm-level outcomes: sales growth, growth in the number of products, growth in the number of stores with products available, and growth in the number of counties with products available. The sample period runs from −12 to +24 months around the bankruptcy event month ($t = 0$). The coefficient estimates measure the difference in the outcome variable between treated firms and matched controls relative to the reference period, the month before the bankruptcy filing. Standard errors are double clustered at the firm and month-year levels. The bars indicate 90% and 95% confidence intervals.

5.2.1 Discontinued Products

We conduct a survival analysis to estimate the factors that correlate with the timing of withdrawal of products by firms in bankruptcy relative to their matched, similar products. We consider a product "discontinued" when it vanishes from *every* store in our sample in a given month and no sales are observed thereafter. For consistency in our sample, we focus on the same products of Section 4.2 while aggregating across the store dimension from our observations. For each product from a firm in bankruptcy, we form cohorts based on the competitor products matched in any store, forming a sample of product-month observations.[25] The dependent variable $\mathbb{1}\{Exit\}$ equals zero while the product is present in the data and equals one in the last month where sales are observed. As usual in survival analysis, the product is excluded from the panel after its "death."

[25]The same sample used in Section 3.5.

We perform a stacked cohort DID where the independent variables, fixed effects, and clustering schemes are as in Equation (5). The coefficient of *Bankrupt* estimates the change in the probability of a product being discontinued after a firm enters bankruptcy relative to matched counterfactuals. Added interaction terms estimate how product cross-sectional factors affect the *timing* of the bankrupt firms' decisions to withdraw a product (Bowen et al. (2016)). We focus on four product characteristics. *Revenue Share* is the product's share of the total monthly sales of the firm, reflecting the product's importance in the firm's portfolio. *Module Share* is the product's share of the total monthly sales generated by all products of that module and across all firms, thus reflecting the product's standing among similar products. *Quality* is the log difference between a product's unit price and its respective module's median value in a given month (as in Argente et al. (2017)). This variable captures relative price premiums within narrowly defined categories, reflecting both perceived quality and market positioning. *Revenue Share*, *Module Share*, and *Quality* are computed in the month before the filing to avoid contaminating the estimates with post-bankruptcy changes in sales and prices. Finally, *Product Beta* is our measure of product cyclicality, which proxies for the product's riskiness, as described in Section 3.4.

We report the results of our survival analysis in Table 6. To facilitate interpretation, we scale the coefficients to represent percentage points. First, the coefficients obtained for *Bankrupt* (in row 1) confirm that the probability of a product being discontinued significantly increases after its producer enters bankruptcy across all specifications. The economic magnitude is substantial. For instance, in column (1), we estimate that the probability of product withdrawal increases by 1.81 p.p. relative to similar, non-bankrupt products, which corresponds to 492% of the sample's unconditional probability of termination.

The estimates in Table 6 further reveal that firms in bankruptcy are less likely to discontinue products that account for larger shares of their revenues, suggesting that these firms prioritize retaining core products that contribute most to their top line. In addition, popular products within the category — or "superstar" products, as measured by large shares of sales within the module — are also less likely to be discontinued. Naturally, high-sales products tend to represent larger shares oh both the firm's and overall module sales in the data. Nevertheless, our results show that both these dimensions are meaningful and captured by our estimates. Higher-quality products are also less likely to be discontinued, indicating that firms aim to maintain their reputation and competitive edge even while in bankruptcy court. Finally, the finding that products with higher betas — *riskier products* — are less likely to be discontinued is particularly noteworthy. This suggests bankrupt firms are inclined to keep products with higher upside potential as part of a strategy to recover from distress during economic upturns..

5.2.2 Discontinued Locations

We conduct a similar survival analysis at the firm-county level. We consider that a firm exits a county when all its products disappear from all the stores at that location, thus representing the termination of local op-

Table 6. Product Discontinuation: Survival Analysis. This table reports coefficients of an OLS survival regression at the product level. The dependent variable is an indicator that equals one in the last month (prior to December of 2020) where we observe sales of a product in any store, and zero otherwise. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. "Revenue Share", "Module Share", and "Product Beta" are measures of a product's importance to the firm's revenues, a product's position among similar products, quality relative to similar products, and riskiness, respectively. See Section 3.4 and Section 5.2 for details on variable construction. The coefficients reported are in percentage points. The sample comprises product-month observations of products from firms that entered bankruptcy within the sample period matched to their counterfactual controls — products of similar performance produced by non-bankrupt firms. The matching is based on the average price and quantity sold in the month before the filing, as well as price growth and quantity growth in the previous 12 months. See Section 4.2 for details of the matching procedure. A treated-control group is a *cohort*, and the sample is a stack of cohorts. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors double-clustered by product and month-year. Second, in [square] brackets, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:					
	𝟙(Exit)					
	(1)	(2)	(3)	(4)	(5)	(6)
Bankrupt	1.81	1.88	1.88	1.51	0.97	0.65
	(11.71)***	(11.51)***	(11.71)***	(9.97)***	(13.54)***	(5.43)***
	[5.74]***	[5.64]***	[5.82]***	[4.89]***	[4.69]***	[2.14]*
Bankrupt × Revenue share		−0.03				−0.02
		(−4.46)***				(−3.94)***
		[−2.74]**				[−2.29]*
Bankrupt × Module Share			−0.29			−0.14
			(−7.16)***			(−5.58)***
			[−7.71]***			[−5.47]***
Bankrupt × Quality				−0.62		−0.54
				(−5.46)***		(−8.07)***
				[−3.07]**		[−4.04]***
Bankrupt × Product Beta					−0.09	−0.10
					(−6.00)***	(−6.29)***
					[−3.11]**	[−4.37]***
Product-Cohort FE	Y	Y	Y	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y	Y	Y	Y
Observations	47,268,643	47,268,643	46,855,264	47,268,357	47,268,643	46,769,532
Adjusted R^2	0.04	0.04	0.04	0.04	0.04	0.04

Note: * p<0.05; ** p<0.01; *** p<0.001

erations. Therefore, the dependent variable $\mathbb{1}\{Exit\}$ equals zero while we observe sales of a firm in a county and equals one in the last month where these sales are observed. A cohort consists of the bankrupt firm and its counterfactuals based on the firm matching procedure in Section 4.2. As in Section 5.2.1, the interactions of *Bankrupt* with cross-sectional variables estimate how these county-level characteristics predict the timing of disruption of local operations by bankrupt firms relative to their similar peers.

We focus on several factors that encompass both the importance of a location to the firms' operations and sociodemographic factors. *Firm Revenue Share* is a county's share of total firm revenues in the month before the bankruptcy. *Product Desert* is our indicator of low availability of wide ranges of products in a county (See Section 3.5 for details). To create socioeconomic indicators, we leverage household-level information from Nielsen Homescan and project it to the county level using Nielsen's sampling weights.[26] We build three variables that correspond to shares of the county's population with a socioeconomic trait. *Low Income Share* consider households in the county with income in the first tercile of the overall sample's distribution. *Unemployed Shares* corresponds to residents of working age that are not employed. Finally, *Low Education Share* reports residents with less than a high school degree.

We report results in Table 7, where the coefficients represent p.p. changes in the probability of exit from a county. Across all models, we find that filing for bankruptcy substantially increases the likelihood of discontinuing sales at a given location. The estimate in column (1) implies that firms in bankruptcy increase the probability of exit by 1.65 p.p. relative to their matched counterfactuals, which corresponds to 185% of the unconditional sample mean.

The results in this survival analysis further reveal several important patterns in how bankrupt firms exit counties. First, bankrupt firms are less likely to exit areas that represent a larger share of revenues, in consonance with our results in Section 5.2.1. In addition, bankruptcies disproportionately impact economically vulnerable areas: counties with higher shares of low-income residents, unemployed individuals, and less-educated populations all face a greater likelihood of firm exits following bankruptcy. Notably, a strong effect is observed in areas already underserved by large retailers.

5.2.3 Product Deserts and Retail Access Inequality

Our findings imply that bankrupt firms systematically withdraw from economically disadvantaged and underserved areas, raising concerns that bankruptcies may aggravate social inequalities. As highlighted by Alcott et al. (2019), limited access to diverse food options in certain areas can contribute to poorer nutritional outcomes and widening health disparities. We take this analysis further by considering a full array of consumer products, not only food products. As such, our tests examine how inequality in access to general consumer products may be widened by bankruptcy events.

To visualize the geographic distribution of these effects, we present Figure 8, which maps the differential impact of bankruptcy on product availability across counties classified as product deserts versus non-deserts. The map displays the average growth rate in the number of products per capita between 12 months before and 12 months after bankruptcy events. As in Figure 6, the growth rates are categorized into impact severity quartiles ranging from least severe to most severe. Following the definition in Alcott

[26] Butler et al. (2023) document that Homescan participating households are broadly representative of the U.S. demographic distribution across geographic areas as granular as 5-digit ZIP codes.

Table 7. County Discontinuation: Survival Analysis. This table reports coefficients of an OLS survival regression at the firm-county level. The dependent variable is an indicator that equals one in the last month (prior to December of 2020) where we observe sales of a firm in any store of a county, and zero otherwise. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. "Firm Revenue Share" is the share of a firm's revenues that comes from that county in the month before the bankruptcy. "Pop. Low Income Share", "Pop. Unemployed Share", and "Pop. Low Education Share" are the shares of the population that are on the bottom tercile of the sample income distribution, are unemployed, and have less than high school education, respectively. "Product Desert" is an indicator that equals one if the county is in the bottom quartile of the distribution of number of stores from large retailers per capita in a given month, as in Alcott et al. (2019). Large retailers are defined as those above the mean of the distribution of total revenues through the sample period. To facilitate interpretation, the coefficients are scaled to represent percentage points. The sample consists of firm-month-level observations of bankrupt firms matched to counterfactual controls. Each bankrupt firm is matched to three non-bankrupt counterfactual firms. The matching is based on monthly sales, sales growth, number of unique products sold, and number of stores with products from the firm in the month before the bankruptcy filing. See Section 4.2 for details of the matching procedure. A treated-control group is a *cohort*, and the sample is a stack of cohorts. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] brackets, t-statistics are computed using robust standard errors double-clustered by firm and month-years. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:						
	$\mathbb{1}$(Exit)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bankrupt	1.65	1.71	1.42	1.44	1.33	1.44	0.99
	(5.36)***	(5.49)***	(4.84)***	(4.89)***	(4.58)***	(4.86)***	(3.35)***
	[5.17]***	[5.29]***	[4.63]***	[4.69]***	[4.38]***	[4.66]***	[3.19]**
Bankrupt × Firm Revenue Share		−0.38					−0.35
		(−5.89)***					(−5.85)***
		[−5.79]***					[−5.75]***
Bankrupt × Pop. Low Income Share			0.55				0.44
			(4.14)***				(4.20)***
			[4.09]***				[4.14]***
Bankrupt × Pop. Unemployed Share				0.41			0.16
				(3.85)***			(3.34)***
				[3.77]***			[3.16]**
Bankrupt × Pop. Low Education Share					0.64		0.41
					(4.14)***		(3.87)***
					[4.09]***		[3.83]***
Bankrupt × Product Desert						0.86	0.79
						(5.96)***	(5.95)***
						[5.62]***	[5.58]***
Product-Cohort FE	Y	Y	Y	Y	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y	Y	Y	Y	Y
Observations	50,309,999	50,309,999	50,309,999	50,309,999	50,309,999	50,300,737	50,300,737
Adjusted R^2	0.11	0.11	0.11	0.11	0.11	0.11	0.11
Note:							* p<0.05; ** p<0.01; *** p<0.001

et al. (2019), Panel A focuses on counties classified as product deserts (areas in the bottom quartile of store availability per capita). Panel B shows non-desert areas.

Panel A of Figure 8 shows that product desert areas display severe bankruptcy impacts, with many counties experiencing the most severe category of product availability decline following bankruptcy events. Panel B, in contrast, suggests that non-desert areas experience generally less severe impacts. These results sug-

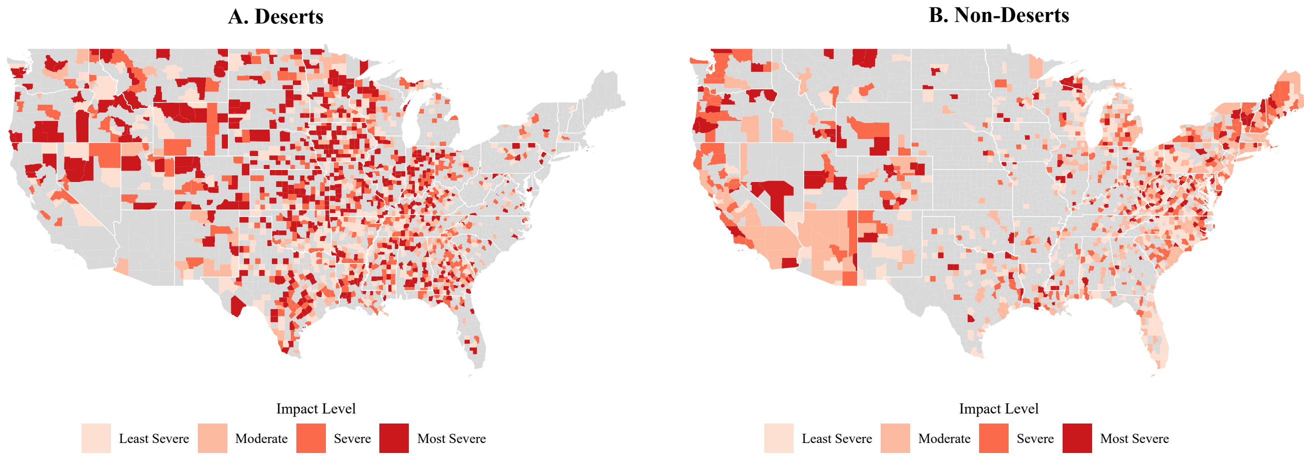

Figure 8. Bankruptcy and Product Per Capita Availability: Product Deserts vs. Non-Deserts. This figure maps the differential impact of corporate bankruptcies on product availability across U.S. counties. The analysis compares the growth rate of products per capita between 12 months before and 12 months after bankruptcy filings. Counties are classified into impact severity quartiles based on the magnitude of product availability changes, with colors ranging from least severe (light pink) to most severe (dark red). Panel A displays counties classified as product deserts, i.e., areas in the bottom quartile of the distribution of the number of stores from large retailers per capita. Large retailers are defined as being above the average total revenues within the sample period. Panel B shows non-desert areas.

gest that bankruptcy-induced supply disruptions disproportionately affect areas already facing limited retail access, compounding existing disadvantages and potentially contributing to consumption inequality.

Going beyond visual inspection, we confirm the above patterns with the *product desert* indicator in our regression specification in column (6) of Table 7. The estimates imply that bankrupt firms are 0.86 p.p. more likely to exit product deserts, which corresponds to a 92% increase in the unconditional probability of exit in any given month.[27] This effect remains highly statistically and economically significant in column (7), which controls for other population demographics. The price differentials between product deserts and non-product deserts reported in Section 3.5 rationalize these results. Lower price levels and possibly higher costs of supplying these areas compress profit margins, making it economically challenging for bankrupt firms to operate profitably and restructure, leading them to strategically withdraw from these markets. Overall, our findings in this section highlights how corporate financial distress can exacerbate consumption inequality, particularly affecting communities that are already vulnerable to limited product access and worse socioeconomic conditions in general.

5.3 Restructuring versus Liquidation

While our analysis thus far has examined bankruptcy cases collectively, it is important to recognize that the specific characteristics of each case, such as the filing chapter or court outcome, may significantly influence the product market outcomes of the bankrupt firm. In particular to our firm-level tests, the results docu-

[27] Our results are robust to defining product deserts based on overall stores per capita instead of stores from large retailers, or to using other thresholds instead of bottom quartile.

mented in Table 5 could mechanically reflect the presence of liquidating firms in our sample as they leave the market gradually after declaring bankruptcy. Therefore, separating restructuring from liquidating firms is a crucial test to validate our baseline firm-level results and tease out differential effects of court outcomes.

To explore how case characteristics shape the product market consequences of bankruptcy, we perform a heterogeneity exercise to differentiate liquidated firms from those that eventually emerged. We estimate the following firm-level specifications:

$$y_{f,c,t} = \beta \cdot Bankrupt_{f,c,t-1} + \gamma \cdot [Bankrupt_{f,c,t-1} \times Liquidated_f] + \mu_{f,c} + \mu_{t,c} + \epsilon_{f,c,t}, \tag{6}$$

where $Liquidated_f$ is an indicator that equals one if the bankrupt firm in cohort c at time *t–1* either: (i) entered Chapter 7 Bankruptcy, (ii) entered Chapter 11 and later converted into 7, or (iii) liquidated under Chapter 11. The coefficient γ estimates the differential impact of liquidation relative to restructuring on our firm-level dependent variables.

Table 8 reports the results of our heterogeneity analysis described in Equation (6). The estimates have two important implications. First, the coefficients of *Bankrupt* show that filing for bankruptcy leads to a sharp deterioration of product market performance even when the firm eventually emerges, demonstrating that our results in Table 5 are not driven solely by firms that cease operations. Second, the interaction with *Liquidated* shows that liquidation indeed entails even more severe effects across all outcomes.[28] The estimates reported help disentangle the magnitudes of the effects of temporary financial distress in the form of restructuring from actual product market exit. Since such differences are expected, these estimates also serve as a sanity check of our information on case outcomes.

5.4 Involuntary Filings

Although the decision to file for bankruptcy is inherently endogenous to the firm, our analysis thus far treats all filings uniformly. In turn, we exploit case-level information and focus on the distinction between voluntary filings (initiated by the debtor) and involuntary filings (initiated by creditors). Involuntary bankruptcies apply to 12% of the cases in our sample and provide useful quasi-exogenous variation in the timing of entry into court proceedings — the filing occurs independently of management's strategic calculations about optimal timing. For the cases we have information on the initiating party, we estimate a specification similar to Equation (6), interacting *Bankrupt* with *Involuntary*, and an indicator that equals one when the filing was initiated by the firm's creditors. Therefore, the interaction coefficient estimates differential effects of involuntary filings, relative to voluntary ones.

Table 9 reports the results. The coefficients on *Bankrupt* confirm that voluntary bankruptcies lead to significant deterioration across all performance metrics, with firms experiencing 33 p.p. lower sales growth and substantial contractions in product offerings and geographic presence. Notably, the interaction co-

[28] We report similar results in levels instead of growth rates in Table A.5.

Table 8. Liquidation Effects: Firm-Level Evidence. This table reports DID coefficients β and γ from the estimation of Equation (6). The dependent variables are growth in sales, number of products, number of stores, and number of counties. Growth rates are defined as the log difference between a variable in a given month and the same month of the previous year, in percentage points. Sales is total firm revenue. Number of products is the count of unique different products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sells products from the firm. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. "Liquidated" is an indicator that equals one if the firm either (i) filed for Chapter 7 Bankruptcy, which was not converted to Chapter 11, (ii) filed for Chapter 11, but it was later converted to 7, or (iii) was liquidated under Chapter 11. Otherwise, "Liquidated" equals zero. The sample consists of firm-month-level observations of bankrupt firms matched to counterfactual controls. Each bankrupt firm is matched to three non-bankrupt counterfactual firms. The matching is based on monthly sales, sales growth, number of unique products sold, and number of stores with products from the firm in the month before the bankruptcy filing. A treated–control group is defined as a cohort, and the sample comprises a stack of cohorts. All specifications include firm-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. See Section 5.3 for details. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] parentheses, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt	−22.74	−9.63	−17.70	−13.90
	(−3.16)**	(−4.32)***	(−3.36)***	(−3.07)**
	[−3.12]**	[−4.26]***	[−3.37]***	[−3.07]**
Bankrupt × *Liquidated*	−45.53	−16.34	−40.86	−32.75
	(−3.46)***	(−4.04)***	(−4.13)***	(−3.97)***
	[−3.43]***	[−3.85]***	[−4.13]***	[−3.98]***
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	106,075	106,075	106,075	106,075
Adjusted R^2	0.23	0.19	0.22	0.21

Note: * p<0.05; ** p<0.01; *** p<0.001

efficients are consistently negative, although these differential effects are imprecisely estimated given the limited number of involuntary filings in our sample. Overall, the estimates show that, if anything, involuntary filings lead to even more severe disruptions.

These findings alleviate concerns that our main results could reflect strategic timing by distressed firms, as the negative effects persist even when the filing decision is externally imposed. In addition, the marginally more severe outcomes for involuntary cases suggest that creditor-initiated bankruptcy may occur when firms have exhausted their strategic options, leading to more abrupt operational adjustments. This set of results provides additional evidence that our main findings reflect fundamental product market disruptions rather than selection effects.

Table 9. Involuntary Filings: Firm-Level Evidence This table reports DID and triple-differences coefficients β and γ from the estimation of a specification similar to Equation (6) where we interact "Bankrupt"with "Involuntary" indicators. The dependent variables are growth in sales, number of products, number of stores, and number of counties. Growth rates are defined as the log difference between a variable in a given month and the same month of the previous year, in percentage points. Sales is total firm revenue. Number of products is the count of unique different products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sells products from the firm. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. "Involuntary" is an indicator that equals one if the filing was initiated by creditors, zero if initiated by the firm, and missing if this case information is not available. The sample consists of firm-month-level observations of bankrupt firms matched to counterfactual controls. Each bankrupt firm is matched to three non-bankrupt counterfactual firms. The matching is based on monthly sales, sales growth, number of unique products sold, and number of stores with products from the firm in the month before the bankruptcy filing. A treated–control group is defined as a cohort, and the sample comprises a stack of cohorts. All specifications include firm-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. See Section 5.4 for details. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] parentheses, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt	−32.99	−15.22	−26.42	−20.09
	(−4.58)***	(−6.68)***	(−4.98)***	(−4.51)***
	[−4.49]***	[−6.58]***	[−4.89]***	[−4.46]***
Bankrupt × *Involuntary*	−27.87	−8.07	−32.75	−32.39
	(−1.21)	(−1.24)	(−1.90)	(−2.27)*
	[−1.20]	[−1.23]	[−1.89]	[−2.25]*
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	85,784	85,784	85,784	85,784
Adjusted R^2	0.21	0.18	0.21	0.20

Note: * p<0.05; ** p<0.01; *** p<0.001

6 Product-Level Results

6.1 Performance of Continuing Products

Our previous analyses show that bankrupt firms decrease the size of their product portfolios and availability at locations, which constitute the extensive margin of sales and might partially explain the concurrent drop in revenues. We dig deeper into the drivers of bankrupt firms' revenues by switching our focus to the sales of *continuing products*, where we can disentangle prices and quantities. In doing so, we consider products for which we observe sales for at least one year following the bankruptcy filing. We estimate Equation (5) on this sample of product-stores and report the results in Table 10.

Column (1) shows a significant drop in sales growth of bankrupt firms' products of about 5.08 p.p. relative to similar shelf neighbors produced by non-bankrupt firms, which corresponds to roughly 8.6% of the standard deviation of product sales growth. Columns (2) and (3) report changes in the growth of quanti-

Table 10. Bankruptcy Effects: Product-Level Evidence. This table reports DID coefficients from the estimation of Equation (5) on product-level outcomes. Dependent variables are sales growth, quantity growth, and price growth. Growth rates are measured as the log difference between the value in a given month and the same month in the previous year, in percentage points. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. The sample consists of product-store-month-level observations in a matched sample. Each product of a firm that entered bankruptcy is matched to at most three other similar products in the same store and module (product category), produced by a non-bankrupt firm. The matching is based on the average price and quantity sold at the time of filing, as well as price growth and quantity growth in the previous 12 months. A cohort is a group of a bankrupt product and its matches. The sample is composed of a stack of cohorts. All specifications include product-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors double-clustered by product and month-year. Second, in [square] brackets, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:		
	Sales Growth	Quantity Growth	Price Growth
	(1)	(2)	(3)
Bankrupt	−5.08	−5.18	0.22
	(−4.13)***	(−4.03)***	(0.61)
	[−3.27]**	[−2.75]**	[0.43]
Product-Cohort FE	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y
Observations	356,145,035	356,145,035	356,145,035
Adjusted R^2	0.10	0.10	0.16

Note: * $p<0.05$; ** $p<0.01$; *** $p<0.001$

ties sold and prices, respectively. Remarkably, the estimates show that reduced sales of distressed firms' products are entirely driven by lower quantities, with virtually no differential price change. The coefficients reported are economically meaningful: the 5.18 percentage point decrease in quantity growth corresponds to almost twice the sample average and about 8.5% of the standard deviation.

6.2 Liquidation and Fire Sales

A key consideration is how the eventual fate of the bankrupt firm — whether it emerges from bankruptcy or is liquidated — shapes its pricing and inventory strategies. This distinction is particularly relevant in light of recent work by Kim (2020), who examines how credit supply shocks affect firms' pricing decisions and concludes that credit-constrained firms cut prices to liquidate inventory and generate short-term cash flows. In contrast, our disruptive event of bankruptcy allows us to explore how the "fire sale" behavior may differ based on the expected post-bankruptcy outcome.

At the product level, we are able to observe price responses from liquidating and reorganizing firms separately. We estimate a specification analogous to Equation (6) where *Liquidated* refers to products of liquidated firms, where γ captures differences in sales, price and quantity growth rates between products from liquidated firms relative to products from firms that emerged.

Table 11. Liquidation Effects: Product-Level Evidence. This table reports DID and triple-differences coefficients from the estimation Equation (6) on product-level outcomes. Dependent variables are sales growth, quantity growth, and price growth. Growth rates are measured as the log difference between the value in a given month and the same month in the previous year, in percentage points. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. "Liquidated" is an indicator that equals one if the firm either (i) filed for Chapter 7 Bankruptcy and was not converted to Chapter 11, (ii) filed for Chapter 11, but it was later converted to 7, or (iii) was liquidated under Chapter 11. Otherwise, "Liquidated" equals zero. The sample consists of product-store-month-level observations in a matched sample. Each product of a firm that entered bankruptcy is matched to at most three other similar products in the same store and module (product category), produced by a non-bankrupt firm. The matching is based on the average price and quantity sold at the time of filing, as well as price growth and quantity growth in the previous 12 months. A cohort is a group of a bankrupt product and its matches. The sample is composed of a stack of cohorts. All specifications include product-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. See Section 6.2 for details. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors double-clustered by product and month-year. Second, in [square] brackets, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:		
	Sales Growth	Quantity Growth	Price Growth
	(1)	(2)	(3)
Bankrupt	−5.20	−5.59	0.50
	(−3.99)***	(−4.12)***	(1.37)
	[−3.19]**	[−2.91]**	[1.10]
Bankrupt × Liquidated	1.90	6.32	−4.43
	(0.85)	(2.44)*	(−5.81)***
	[0.61]	[1.56]	[−3.67]***
Product-Cohort FE	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y
Observations	356,144,840	356,144,840	356,144,840
Adjusted R^2	0.10	0.10	0.16

Note: * p<0.05; ** p<0.01; *** p<0.001

We report the results by case outcome in Table 11. The estimates show that products from firms that emerge from bankruptcy generate less revenue via lower quantities sold when compared to similar products from non-bankrupt firms. The coefficient of the interaction term in column (1) implies that products from firms under liquidation generate marginally higher revenues, although the coefficient is not statistically significant. Decomposing this revenue effect into prices and quantities reveals that firms undergoing liquidation slash the prices of their products significantly relative to emerging firms, an action that is associated with an increase in quantities sold.

The results in Table 11 reveal fundamentally different incentives for product market strategies depending on whether the bankrupt firm eventually emerges or is ultimately liquidated. Whereas firms undergoing restructuring may discontinue less profitable products, they might want to ensure the long-term pricing of their continuing products remains on par with shelf neighbors, even when facing lower sales. On the other hand, firms under liquidation engage in fire sale strategies as described in Kim (2020), whereby distressed firms decrease prices to alleviate short-term cash flow losses. Our results restrict this strategy to dissolving firms, as they have little incentive to maintain their long-term pricing strategies when heading toward the end of operations.

Our findings cast new light on the conclusions drawn by Kim (2020), who finds that credit supply shocks lead firms to engage in fire sales. In contrast, our analysis of bankruptcy events reveals that such aggressive pricing strategies are limited to extreme cases of firms facing liquidation and going out of business. Reorganizing firms that eventually emerge, on the other hand, keep their prices on par with competitors, adhering to long-run pricing policies. The new evidence we bring underscores the importance of distinguishing between different levels of distress even in extreme events when analyzing firms' product market strategies.

6.3 Disentangling Producer and Retailer Reactions

One limitation of the Nielsen data is that we only observe final prices and cannot disentangle manufacturer prices from retailer markups. Therefore, a potential alternative explanation for our results is that retailers move away from products of firms in bankruptcy and slash prices of products from liquidating manufacturers by promoting fire sales. While we cannot directly test this hypothesis without the wholesale prices that retailers pay, the Nielsen datasets offer ways to test some possible types of retailer reactions.

Retailers can exert sales efforts such as issuing discount coupons and offering deals, for which Nielsen Homescan reports two variables of interest. "Coupon Value" records whether a participant household used a coupon for a product's purchase and the total discount given by the coupon. "Deal Flag" reports if the product was perceived by the consumer to be on a deal. Thus, these variables provide information on whether any observed changes in prices are due to adjustments in standard shelf price or changes in deal offerings.

There are two caveats to using Homescan's coupons and deals in our framework. First, this information is captured through participant panelists' purchases rather than store sales. To be consistent with our product-level sample from Section 6.1, we include only product-store combinations that appear in the original sample, which decreases the testing sample size. Second, coupons and deals may be offered by manufacturers instead of retailers. Ideally, we would distinguish these cases to test promotions by producers and stores separately. Unfortunately, Nielsen no longer reports deal types, and this information was retroactively dropped from the datasets, being available only in vintage versions that are no longer accessible to researchers. However, Cha et al. (2015) report a breakdown of deal types between 2006 and 2009. The authors show that only about 10% of deals are manufacturer coupons, while roughly 87% are either store features or store coupons. Based on the shares of each deal type, we deem it plausible to assume that analyzing coupons and deals is effectively testing for retailer-initiated sales efforts.

Following Butler et al. (2023), we construct two variables at the product-store-month level.[29] *Coupon* is the the dollar value of coupon discounts as a percentage of total expenditures by households on a product,

[29] Butler et al. (2023) define these variables at the household level, relative to total expenditures across all purchased products.

and *Deal* is the percentage of total units purchased that involved a deal. In our sample, the mean values of *Coupon* and *Deal* are 3.22% and 32.78%, respectively, which are consistent with Butler et al. (2023).

In addition to offering coupons and deals, retailers can increase the visibility of particular items across different locations. The Retail Scanner dataset reports whether a product is "featured" in a given store and week. An item is featured if it appears in retailer advertisements in local newspapers, free-standing circulars, or online advertisements on the retailer's website, and typically involves a price discount. As a sales effort entirely determined by retailers, we can check whether stores react by changing how products from firms in bankruptcy are featured in localities where they are sold.

The dataset reports feature information only for a subset of stores audited by Nielsen in a given week, and these stores can change over time. However, the data manual states that if a particular store is audited, it is reasonable to assume that all other stores of the same retailer in that same week will feature the same items within a designated market area (DMA). We follow this recommendation and build a product-store-month indicator of whether an item was featured in any week of the month and any store of that retail chain and DMA.[30]

We estimate Equation (5) with *Coupon*, *Deal*, and *Feature* as dependent variables to capture retailer-initiated sales efforts for bankrupt products relative to their shelf neighbors. These results are reported in Table 12 and show no significant effects across all specifications. In column (3), although positive, the coefficient of *Bankrupt* is not statistically significant and represents only 2% of the average feature variable. Although we cannot completely rule out the role of retailers in adjusting standard shelf prices, we show that sales efforts directly controlled by stores remain unaffected. Overall, these findings strengthen the case that the lower quantities sold reported in Section 6.1 are producer-driven and not due to retailers decreasing sales efforts or engaging in other pricing strategies.[31]

7 Reactions of Product Market Rivals

The results in our product-level analysis in Section 6.1 rely on comparisons between products from firms in bankruptcy and their non-bankrupt shelf neighbors. Due to this close competition between products, it is likely that our estimates capture reactions by the product market strategies of other firms in response to a company's bankruptcy. While the non-significant results in column (3) of Table 10 could mean that neither bankrupt firms nor their close competitors change pricing strategies, these results are also consistent with *both* types of firms changing prices in the same direction, such that we do not see differential effects when comparing their products. At the firm level, we compare bankrupt firms with other firms of similar performance, but not necessarily close competitors (e.g., firms that operate in different

[30] Therefore, our sample excludes observations for which no store was audited in a given retail chain–DMA–week combination.

[31] In unreported results, we also test for differential effects for products from firms in liquidation, which could explain our fire sales results in Table 11. We find no significant estimates.

Table 12. Bankruptcy and Sales Efforts: Coupons, Deals, and Featured Products This table reports DID coefficients from the estimation of Equation (5) on product-level outcomes. "Coupon" is the dollar value of discounts obtained from coupon usage as a percentage of total purchases. "Deal" is an indicator of some type of deal in the purchase of the product, in percentage points. "Feature" is an indicator of whether a product is featured in a given store and month, in percentage points. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. The sample consists of product-month-level observations in a matched sample. Each product of a firm that enters bankruptcy is matched to at most three other similar products in the same store and module (product category), produced by a non-bankrupt firm. The matching is based on the average price and quantity sold at the time of the filing, as well as price growth and quantity growth in the previous 12 months. A cohort is a group of a bankrupt product and its matches. The sample is composed of a stack of cohorts. All specifications include product-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022, Section III.B). For details, see Section 4.2 and Section 6.3 . Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors double-clustered by product and month-year. Second, in [square] brackets, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% level, respectively.

	Dependent variable:		
	Coupon	Deal	Feature
	(1)	(2)	(3)
Bankrupt	−0.22	−1.45	0.36
	(−0.98)	(−1.42)	(0.50)
	[−0.74]	[−0.99]	[0.37]
Product-Cohort FE	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y
Observations	2,403,981	2,404,657	60,742,881
Adjusted R^2	0.46	0.48	0.44

Note: * p<0.05; ** p<0.01; *** p<0.001

areas of the country or sell different products), as such competitive spillover effects are not present. At the product level, however, our matching procedure focuses on products that are close competitors, potentially capturing strategic reactions by product market rivals at the store aisle level. In other words, "treated" and "control" units may be affected by the treatment. Fortunately, the granularity and wide scope of our data critically allow us to assess these competitive responses explicitly.

7.1 Price Wars

We build on the notion that rivals of a bankrupt firm only have incentives to implement changes in sales strategies in localities where competition effectively takes place. For instance, if a rival deviates from its long-run optimal pricing policy to engage in a price war with the bankrupt firm, it need not have to change prices across all points of sales and products uniformly. Given that demand for consumer products is segmented across categories and locations, a rival might change prices only where it sells products similar to those of the bankrupt firm.

We perform two tests to isolate the strategies employed by non-bankrupt rival firms. In these tests, we compare changes in prices and quantities between "*close*" and "*far*" competitors of products from firms in bankruptcy. The first necessary step is to identify the products used as counterfactuals to the bankrupt

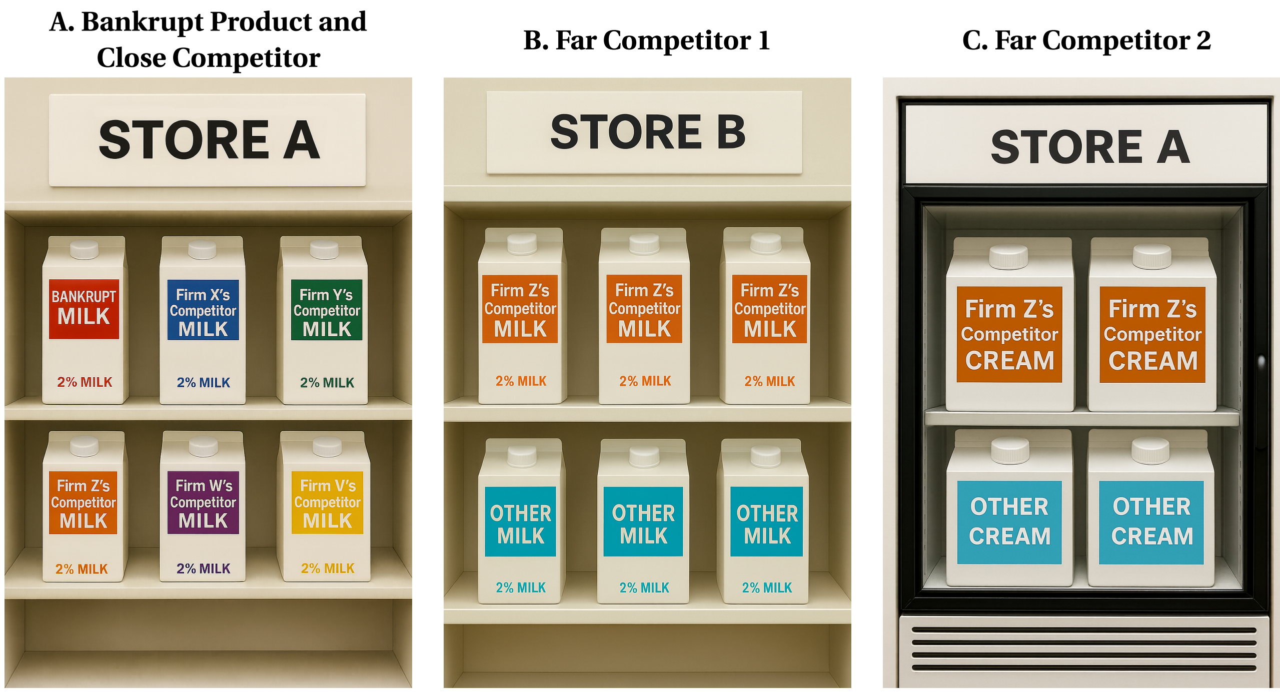

Figure 9. Close and Far Competitors of Bankrupt Products. This figure illustrates how we define close and far competitors of bankrupt products. Panel A shows a product from a bankrupt firm ("Bankrupt Milk") and its shelf neighbors in Store A. Panel B shows "Firm Z's Competitor Milk" for sale in Store B, where the bankrupt product is not for sale. Panel C shows another product by the same competitor firm ("Firm Z's Competitor Cream") in Store A. Therefore, "Firm Z's Competitor Milk" in Store A the *close* Competitor of "Bankrupt Milk"; "Firm Z's Competitor Milk" in Store B is the *far* competitor in our first definition; and "Firm Z's Competitor Cream" in Store A is the *far* competitor in our second definition.

products in Section 6.1. Since these products are in the same store and module as the bankrupt ones, we classify them as *close* competitors. We want to understand the sales strategies non-bankrupt rivals apply to these products, and they will now be the "treated" units of the tests we perform belw. As we detail next, our two tests differ on the definition of *far* competitors.

In our first test, we follow Fracassi et al. (2022) and, for each of the *close* competitor products in our sample, identify stores where they are sold but their bankrupt match is *absent*. To make consistent comparisons, we require that the stores belong to the same retail chain, so as to alleviate concerns about different trends across retailers or different contracts between suppliers and retail chains. These products constitute the *far* competitors forming the pool of controls in this exercise. While this test compares outcomes of the same products with similar sales performance, it still relies on comparisons across different stores. To address concerns of different local shocks to prices, our second test defines *far* competitors as different consumer products manufactured by the non-bankrupt competitor within the same store. Simply put, for each *close* competitor of a bankrupt product, we look for products in the same store, produced by the same non-bankrupt firm, but that belong to a different module to form the pool of controls. In both tests, we match each *close* competitor to the three most similar *far* competitors based on price, quantity sold, price growth, and quantity growth, as described in Section 4.2. It is important to stress that none of the products used in these tests are from bankrupt firms.

Figure 9 illustrates our definitions of *close* and *far* competitors. Suppose that both a bankrupt firm and non-bankrupt competitor Z sell 2% milk in store A (Panel A). As a shelf neighbor of the bankrupt product, firm Z's 2% milk in store A is a *close* competitor of the bankrupt firm's 2% milk in store A. In our first exercise, firm Z's 2% milk in store B — where the bankrupt firm does *not* sell its 2% milk — is a *far* competitor of the bankrupt firm's 2% milk in store A (Far Competitor 1 in Panel B). In our second exercise, firm Z's heavy cream in store A serves as a *far* competitor, provided that the bankrupt firm does not sell heavy cream in store A (Far Competitor 2 in Panel C).

Finally, we estimate a specification similar to Equation (5), where our variable of interest now is *Close Competitor*, an indicator that equals one if a product is a close competitor of that produced by a firm in bankruptcy. In this specification, the cohorts are formed by the *close* competitor and its three most similar *far* competitors under the two definitions just described. Accordingly, in our first matching approach, we control for any shock that is specific to each product and uniform across stores. By contrast, in our second matching approach, we control for any shock that is specific to each store and uniform across products. As a result, the identification of the DID coefficient β stems solely from variation in the outcomes of bankrupt products' shelf neighbors relative to their counterfactuals in the absence of direct competition.

We report the results of these exercises in Table 13. Panels A and B report results from our *within product, across store* and *within store, across product* tests, specifically. In both cases, column (1) reveals a strong, significant decrease in the price of a product when it is closely competing with products from a firm in bankruptcy. Taken together, the results from Table 10 and column (1) of Table 13 are consistent with price wars in which bankrupt firms and their close competitors slash the prices of their products at points of sale in which they directly compete, *after* the bankruptcy filing. Notably, there are no pricing differentials before the bankruptcy event; that is, we find no evidence of rival price-predatory pre-emptive behaviors (no pricing pre-trends). Our evidence shows that these price wars are localized: competitors only cut prices of products that are of the same type of the bankrupt products *and* sold at the same stores. Price wars appear to be a consequence —– not a cause —– of bankruptcy in the consumer goods industry.

7.2 Price Wars and Sales Shares

Our analysis reveals that competitors engage in localized price wars against bankrupt firms' products. A natural question is whether these competitive responses vary based on the strategic importance of the targeted products. Products that represent larger shares of a bankrupt firm's revenues are particularly valuable assets for maintaining operations and generating cash flows. In particular, our results in Section 5.2.1 show that firms in bankruptcy tend to retain products that account for a larger shares of sales. As such, competitors have stronger incentives to aggressively undercut prices precisely on these flagship products.

To test this hypothesis, we extend our price wars analysis by exploring heterogeneity across bankrupt products' revenue importance. Specifically, we re-estimate our specifications from Section 7.1 interacting

Table 13. Close and Far Competitor Products This table reports DID and triple-differences coefficients on product-level price growth. Price growth is measured as the log difference between the value in a given month and the same month in the previous year, in percentage points. "Close Competitor" is an indicator that equals one when a product is in the same store and module of a product from a firm that entered bankruptcy (the "bankrupt product") and after the bankruptcy event. "Bankrupt Product Sales Share" is the share of the bankrupt product for which the close competitors were matched, to the bankrupt firm's total sales. "Bankrupt Product Beta" is the riskiness measure of the bankrupt product. See Sections 3.4, 7.1, and 7.2 for variable construction. The sample consists of product-store-month-level observations in a matched sample. Each close competitor is matched to at most three similar far competitors. In Panel A, far competitors consist of the same product sold at stores where the bankrupt product is absent. In Panel B, far competitors consist of products from the same firm, sold at the same store, but of a different module (product category) than the bankrupt product. See Section 7 for sample construction. The matching is based on the average price and quantity sold in the month before the bankruptcy filing as well as price growth and quantity growth in the previous 12 months. A cohort is a group of a close competitor and its matches. The sample is composed of a stack of cohorts. All specifications include product-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors double-clustered by product and month-year. Second, in [square] brackets, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% level, respectively.

Panel A. Within Products, Across Stores

	Dependent variable:		
	Price Growth		
	(1)	(2)	(3)
Close Competitor	−0.10	−0.03	−0.10
	(−3.99)***	(−1.14)	(−3.36)***
	[−3.62]***	[−1.02]	[−3.29]**
Close Competitor × *Bankrupt Product Sales Share*		−0.55	
		(−2.22)*	
		[−2.21]*	
Close Competitor × *Bankrupt Product Beta*			−0.02
			(−3.31)**
			[−3.62]***
Product-Cohort FE	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y
Observations	5,079,601	5,079,601	5,079,601
Adjusted R^2	0.52	0.52	0.52

Panel B. Within Stores, Across Products

	Dependent variable:		
	Price Growth		
	(1)	(2)	(3)
Close Competitor	−0.69	−0.41	−1.52
	(−3.05)**	(−1.91)	(−7.28)***
	[−2.32]*	[−1.42]	[−6.29]***
Close Competitor × *Bankrupt Product Sales Share*		−2.84	
		(−2.75)**	
		[−5.25]***	
Close Competitor × *Bankrupt Product Beta*			−0.28
			(−4.67)***
			[−3.20]**
Product-Cohort FE	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y
Observations	30,906,007	30,906,007	30,906,007
Adjusted R^2	0.19	0.19	0.19

Note: * p<0.05; ** p<0.01; *** p<0.001

Close Competitor with *Bankrupt Product Sales Share*, which measures the product's share of the bankrupt firm's total sales. Therefore, the interaction coefficient captures how competitors' pricing strategies respond to the relative importance of products in the bankrupt firm's portfolio. Column (2) in Table 13 reports these results. In both panels, the estimates show negative, statistically significant interaction terms, especially in our within-stores, across-products specification in Panel B. Overall, the evidence suggests that competitors cut prices more aggressively against products that represent larger revenue shares for the bankrupt firm.

7.3 Price Wars and Product Beta

Our results in Section 5.2.1 reveal that firms in bankruptcy tend to retain riskier products with the potential to generate significant revenues in good states of the market. In addition, as our results in Section 7.2 suggest, rivals have incentives to intensify price competition precisely against the products that represent the bankrupt firm's best chance at survival. In this section, we further test this hypothesis by exploring heterogeneity of our results in Section 7.1 in response to the product beta of firms in bankruptcy's products. Specifically, we re-estimate our specifications for close and far competitors, interacting *Close Competitor* with *Bankrupt Product Beta*. In these models, the interaction coefficient estimates how product market rivals' reactions respond to the riskiness of the *continuing products of the firms in bankruptcy*.

We report these results in column (3) of Table 13. In both panels, the estimates show a strong, negative price response by close competitors against higher-beta products from firms in bankruptcy. Together with our results in Section 5.2.1, this finding suggests that while firms in bankruptcy tend to retain their with products with greater upside potential during economic recoveries, these products also attract more aggressive price-cutting reactions from competitors.

Overall, the results in Table 13 provide evidence that competitors engage in price wars by targeting precisely the products which firms in bankruptcy concentrate efforts across the importance and risk dimensions. Rather than competing uniformly across all products, rivals strategically target key markets, contributing to the sharp deterioration in performance we document following entry into bankruptcy.

7.4 Do Large Retailers Curb Price Wars?

In addition to product characteristics driving price wars, rival strategic interactions may vary across different retail environments as well, particularly due to heterogeneity in pricing policies. We examine whether the presence of large retailers mitigates aggressive competition, likely due to downside price rigidity in their stores. Major retail chains may dampen price-cutting behavior for having significant bargaining power with suppliers due to their pivotal role in providing access to a vast consumer base, influencing in-store pricing decisions more strongly.

As in Section 5.2.2, we classify large retail chains as those in the top quintile of the distribution of total revenues through the sample period. We interact an indicator *Large Retailer* in our DID

Table 14. Product-Level Bankruptcy Effects: Heterogeneity by Retail Chain Size. This table reports DID and triple-differences coefficients from the estimation of a product-level specification analogous to Equation (6), where we interact "Bankrupt" with "Large Retailer". Dependent variables are sales growth, quantity growth, and price growth. Growth rates are measured as the log difference between the value in a given month and the same month in the previous year, reported in percentage points. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. "Large Retailer" is an indicator variable that equals one if the retail chain is above mean share of total retail sales throughout the sample period. The sample consists of product-store-month-level observations in a matched sample. Each product of a firm that enters bankruptcy is matched to at most three other similar products in the same store and module (product category), produced by a non-bankrupt firm. The matching is based on the average price and quantity sold one month before the filing as well as price growth and quantity growth in the previous 12 months. A cohort is a group of a bankrupt product and its matches. The sample is composed of a stack of cohorts. All specifications include product-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors double-clustered by product and month-year. Second, in [square] brackets, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% level, respectively.

	Dependent variable:		
	Sales Growth	Quantity Growth	Price Growth
	(1)	(2)	(3)
Bankrupt	−6.13	−5.16	−0.97
	(−3.67)***	(−2.95)**	(−2.38)*
	[−2.96]**	[−2.25]*	[−2.27]*
Bankrupt × Large Retailer	1.55	0.25	1.42
	(1.74)	(0.26)	(4.99)***
	[1.55]	[0.09]	[3.33]**
Product-Cohort FE	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y
Observations	298,822,012	298,822,012	298,822,012
Adjusted R^2	0.10	0.09	0.15
Note:			* p<0.05; ** p<0.01; *** p<0.001

specifications to capture differential effects of bankruptcy in large retail chains relative to small ones. We assess these effects for the bankrupt products relative to their close competitors. We report our retail size test results in Table 14. The coefficients reported point to an attenuation of sales losses by bankrupt-firm products in large retailers, although this result is not statistically significant. Most notably, products from bankrupt firms show a price decrease at smaller retailers relative to their competitors. This price reduction, however, is muted in large retailers.

8 Robustness

8.1 Alternative Matching Periods

Our analysis matches bankrupt firms to similar counterfactuals in the month before the filing. However, this short time frame may imply that a distressed firm and its competitors are aware of the potential bankruptcy at the time of matching. If that is the case, a firm's performance before the event might reflect both its own product market adjustments as well as its competitors' reactions in anticipation. To help ensure that our results in Table 5 do not reflect these potential confounders, we estimate Equation (3) by matching bankrupt firms to other firms that exhibit similar performance several months before the event.

We report the results of this exercise in Table A.2. In Panels A, B, and C, we match firms in the quarter, semester, and year before the bankruptcy, respectively. Whereas the magnitudes of the estimates vary slightly across specifications, the results are qualitatively similar to our earlier analysis. Therefore, it's unlikely that our firm-level results are driven by fluctuations in the performance of distressed firms right before they enter bankruptcy.

8.2 Alternative Product Market Proximity Matching

By following the firm matching procedure described in Section 4.2, we ensure that each firm in bankruptcy is matched to counterfactuals that exhibited similar performance before the filing. However, this does not imply that the firms are necessarily direct competitors. For instance, a bankrupt firm that distributes dairy products in Florida may be matched to a producer of personal hygiene products in upstate New York simply because they have similar sizes and trends in sales. Thus, this performance-based matching does not allow us to assess the effects of bankruptcy relative to *close product market competitors*.

We employ an alternative matching procedure to compare bankrupt firms to their close rivals by constructing a firm pairwise measure of product market overlap. Specifically, for each pair of firms, we count the number of store-modules where both firms have products for sale — roughly the number of retailer aisles with products from both firms. Therefore, a higher score means that the firms compete in more locations. We match each bankrupt firm to three others with the highest product market overlap as of the month before the event. We then re-estimate Equation (3) on this new set of firm cohorts. We report the results of this exercise in Table A.3.

Our firm-level results hold strongly, with point estimates larger than those in Table 5. These larger effects might nonetheless reflect possible downsides of this matching procedure. Specifically, while we ensure comparisons with control firms that operate in the same product markets, the matches no longer represent counterfactuals that are similar in pre-bankruptcy performance. Nevertheless, the economic inferences remain qualitatively unchanged.

The results of this exercise reveal a trade-off in designing a matching procedure. While the matching based purely on past performance ensures cleaner comparisons to similar counterfactuals, it is silent on product market competition aspects. On the other hand, matching through the lenses of market rivals might suffer more from identification issues, exaggerating estimates. For instance, by comparing the performance of firms in bankruptcy with their potentially healthy close competitors, we might overestimate results due to competitive spillover effects in local product markets, as discussed in Berg et al. (2021). In this case, our performance-based matching procedure in Section 4.2 alleviates such issues by prioritizing similar performance rather than selecting counterfactuals that are necessarily direct competitors.

8.3 Resolution Dates

In our baseline specifications, we treat bankruptcy as a permanent state change, with the bankruptcy indicator remaining active throughout the post-filing period. However, this approach potentially conflates immediate disruptions with longer-term consequences that persist after resolution. To isolate the effects specifically attributable to bankruptcy proceedings, we modify our analysis to focus exclusively on the period during which firms are in bankruptcy court.

We redefine our treatment variable as *During Bankruptcy*, which equals one only during the months between the filing and case resolution, and zero otherwise. We exclude cases for which resolution dates are unavailable and remove all post-resolution observations, substantially reducing the sample size. In this more conservative approach, the DID coefficient estimates the changes in product market performance directly associated with the bankruptcy process relative to the pre-filing period.

We report the results of this specification on Table A.4. The estimates remain remarkably consistent with our baseline findings. Specifically, firms experience 37.8 p.p. lower sales growth during bankruptcy proceedings, compared to 36.8 p.p. in our main specification. Similarly, the contractions in product offerings, store presence, and geographic reach closely mirror our baseline results. These findings provide direct evidence of operational disruptions associated with the bankruptcy process itself. Although we employ the broader treatment window in our main analysis to maximize statistical power and account for long-term operational adjustments, this robustness check further validates our main results by showing they hold when focusing strictly on the bankruptcy period.

9 Concluding Remarks

Entering bankruptcy indicates a firm's inability to meet its financial obligations. Although processes like Chapter 11 can protect distressed firms from creditors while exploring restructuring options, the public display of financial distress may lead to further deterioration, especially in the face of competition. This paper dissects the product market consequences of corporate bankruptcy filings in the U.S. Our empirical analysis reveals that firms that enter bankruptcy experience severe disruptions in their product market operations. These firms suffer from lower sales, reduce the number of products they offer, and discontinue their supply to retailers and entire counties.

Beyond these aggregate effects, our paper carefully characterizes firms' operational adjustments following bankruptcy. These firms tend to discontinue their less important and lower-quality products. Notably, products with a higher *Product Beta* — our measure of a product's riskiness based on the sensitivity of its sales to larger aggregate consumption fluctuations — are less likely to be discontinued. These results suggest that distressed firms follow a growth-option strategy that bets on upside potential for recovery during economic upswings. In addition, we show that firms in bankruptcy are more likely to withdraw their products from economically disadvantaged locations that are already underserved by retailers.

Leveraging the granularity of our retail data, we show that continuing products from bankrupt firms generate lower revenues relative to similar shelf neighbors produced by non-bankrupt competitors. The lower revenues are driven entirely by lower quantities sold, whereas relative prices seem unaffected. Notably, firms undergoing liquidation significantly decrease prices relative to those that eventually emerge, implying that bankrupt firms engage in fire sales only when moving towards the end of their operations. We also assess the role of competitor responses in driving these effects on bankrupt firms. We show that market rivals cut product prices only *where* and *when* they closely compete with the products of bankrupt firms. Overall, our work highlights the multifaceted consequences of bankruptcy, particularly by characterizing firms' operational adjustments along the product and locality dimensions and by empirically assessing the role of competitor responses and the impact on customers.

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Appendix A Additional Results

A.1 Predictive Regressions

All specifications in Table 1 include the following firm-month level variables: total sales, number of unique products, number of stores with products for sale, and number of counties with products for sale, as defined in Section 4, as well as the yearly growth rates of all these variables. We add firm-level indicators of industry growth quartiles as defined in Section 2, and monthly indicators of the great recession (December of 2007 to June of 2009) and the COVID-19 recession (January 2020 to the end of our sample, December 2020).

To account for unobserved heterogeneity and capture its explanatory power, we include fixed effects in the OLS, probit, and logit specifications. For the survival analysis, we employ a Cox proportional hazards model with frailty, which is analogous to a random effects model. Since each firm in our sample files for bankruptcy only once, we cannot use stratified Cox models (the fixed effects equivalent), which require multiple events per group. Instead, frailty serves an analogous role by controlling for unobserved firm-level factors that affect the baseline hazard.

For the logit and probit models, we report the Pseudo R^2 to assess explanatory power. As opposed to the OLS model, in which the full panel is always used for estimation, these models' estimation relies on "switchers" within the fixed effect groups, which can modify the estimation sample. For instance, by including firm fixed effects, the estimation leverages only the data on firms that filed for bankruptcy, which considerably limits the sample. For this reason, the Pseudo R^2 does not always increase when we add more granular fixed effects, in contrast to the R^2 from OLS models. For the Cox proportional hazards model, we report the concordance statistic which measures how well the model ranks firms according to their risk of filing for bankruptcy. As such, it ranges from 50% (same as random ranking) to 100% (perfect discrimination). Similarly to binary models, controlling for unobserved heterogeneity does not necessarily increase concordance. For instance, controlling for time effects may reduce concordance by removing cross-temporal variation that aids discrimination (e.g., the GFC years), even though such controls improve model specification.

A.2 Results by Industry Trends

In Section 2, we provide an extensive description of the bankruptcy cases covered in our sample and stress the role of idiosyncratic firm-level factors. In particular, we document that only about 18% of the sample cases occurred in declining industries, following the methodology by Maksimovic and Phillips (1998) to classify industries. Still, our firm-level baseline results could be driven by these few cases if poor post-bankruptcy product market performance is particularly severe in declining industries. In such a case, our results would reflect industry secular trends instead of firm-level bankruptcy shocks that can be generalized across different industry conditions. In this section, we address this concern directly.

As we discuss in Section 8.2, our matching procedure in Section 4.2 ensures that we find counterfactual firms with similar performance at the cost of possibly selecting firms from different industries. In contrast, our matching approach of Section 8.2 finds the closest counterfactual firms in terms of product market overlap, thus ensuring industry proximity. In part, these within-industry comparisons alleviate concerns that our results are primarily capturing industry-level dynamics. Nevertheless, to assess the role of industry trends explicitly, we revisit our results in Table A.3 by estimating heterogeneous effects across industry growth quartiles as defined in Section 2. Specifically, we interact *Bankrupt* with indicators for the bankrupt firm's industry quartile and report all four coefficients in Table A.1. Across all quartiles and specifications, we find consistently negative and statistically significant coefficients.[32]

Our findings in Table A.1 further rule out concerns that our baseline results reflect declining industries. Specifically, bankrupt firms perform worse than their industry peers regardless of determinants of industry trends, such as widespread excess capacity and technological change. Finally, note that our product-level analysis controls for industry trends by design, leveraging comparisons between similar products.

[32] In addition, specifications estimating differential effects between the first industry quartile and the others yield statistically insignificant coefficients. These results are available upon request.

Table A.1. Bankruptcy Effects: Firm-Level Evidence with Product Market Overlap Matches Across Industry Growth Quartiles. This table reports the DID coefficient β from the estimation of Equation (3). The dependent variables are growth in sales, number of products, number of stores, and number of counties. Growth rates are defined as the log difference between a variable in a given month and in the same month of the previous year in percentage points. Sales is total firm revenue. Number of products is the count of unique different products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sells products from the firm. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. Industry quartile indicators are based on industries' total sales growth between the sample's three final years (2018–2020) and early years (2006-2008). We define industry as Nielsen's product groups, and assign a firm to the group that represents the largest share of its sales. See Section 2.2 for details on industry classification. The sample consists of firm-month-level observations of bankrupt firms matched to counterfactual controls. Each firm that filed for bankruptcy is matched to three counterfactual firms that did not file within our sample period. The matching is based on monthly sales, sales growth, number of unique products sold, and number of stores with products from the firm in the month before the bankruptcy event. A treated-controls group is defined as a *cohort*, and the sample is a stack of cohorts. All specifications include firm-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] parentheses, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt × Industry Quartile 1	−38.40	−16.24	−44.18	−39.46
	(−3.54)***	(−4.01)***	(−5.15)***	(−5.15)***
	[−3.49]***	[−3.93]***	[−5.16]***	[−5.11]***
Bankrupt × Industry Quartile 2	−55.00	−15.25	−44.53	−35.76
	(−3.95)***	(−3.54)***	(−4.36)***	(−4.07)***
	[−3.87]***	[−3.45]***	[−4.22]***	[−3.90]***
Bankrupt × Industry Quartile 3	−40.98	−14.38	−37.99	−30.92
	(−4.05)***	(−4.34)***	(−4.78)***	(−4.84)***
	[−3.91]***	[−4.29]***	[−4.62]***	[−4.53]***
Bankrupt × Industry Quartile 4	−54.99	−16.60	−42.94	−35.87
	(−5.62)***	(−5.69)***	(−6.02)***	(−5.93)***
	[−5.34]***	[−5.55]***	[−5.77]***	[−5.72]***
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	135,323	135,323	135,323	135,323
Adjusted R^2	0.25	0.24	0.25	0.23

Note: * p<0.05; ** p<0.01; *** p<0.001

Appendix B Additional Tables and Figures

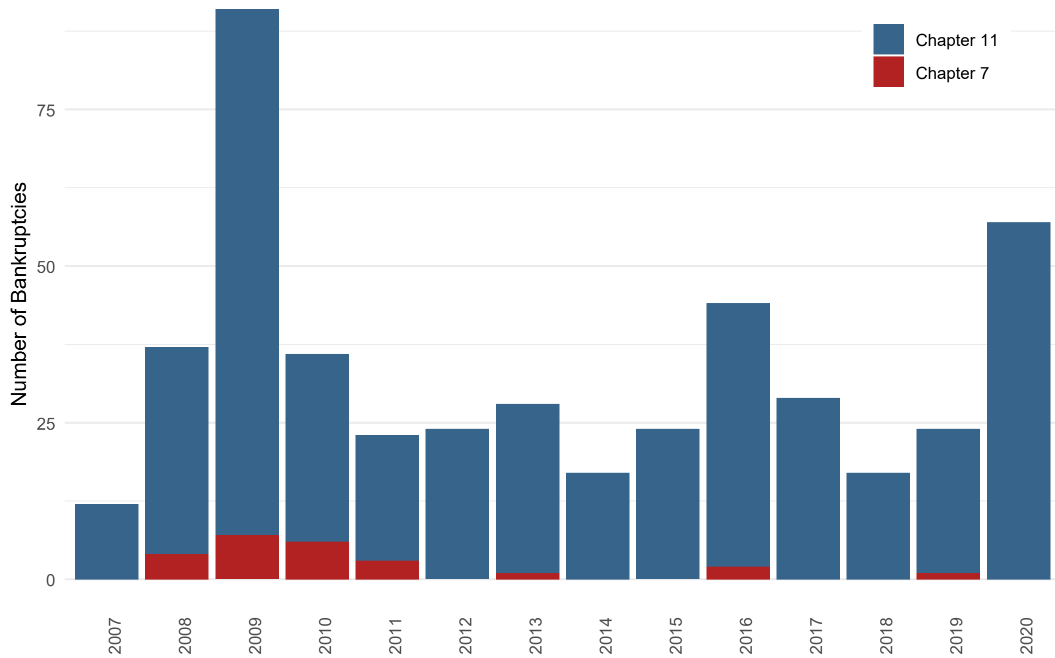

Figure A.1. BRD Sample: Filing Chapters This figure presents the bankruptcy filings in the Florida–UCLA-LoPucki Bankruptcy Database by chapter of the filing across the years 2007–2020. The figure plots annual counts of bankruptcy filing, distinguishing between Chapter 11 (blue) and Chapter 7 (red).

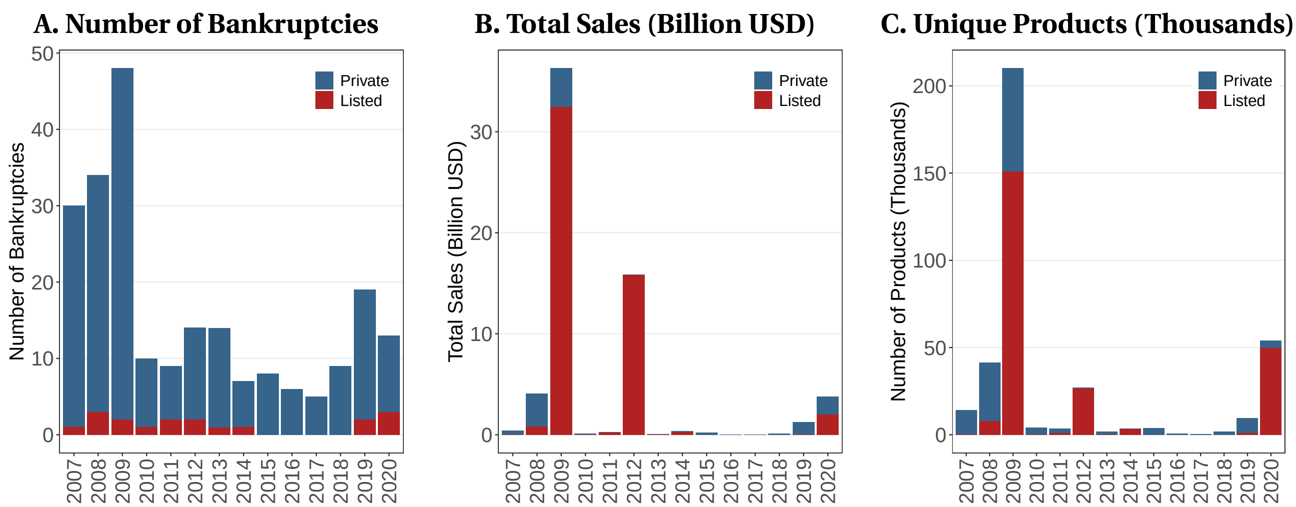

Figure A.2. Bankruptcies by Listing Status. This figure presents three key metrics characterizing bankruptcies in our sample by public listing status. Panel A displays the annual count of bankruptcy filings, distinguishing between publicly listed (red) and privately held (blue) firms. Panel B shows the total annual sales (in billion USD) generated by these bankrupt firms, highlighting the economic magnitude of market disruption caused by financial distress. Panel C illustrates the number of unique products (in thousands) offered by bankrupt firms, demonstrating the extensive product variety affected by corporate bankruptcies. Together, these panels reveal how public and private firms differ in their bankruptcy patterns and market impact.

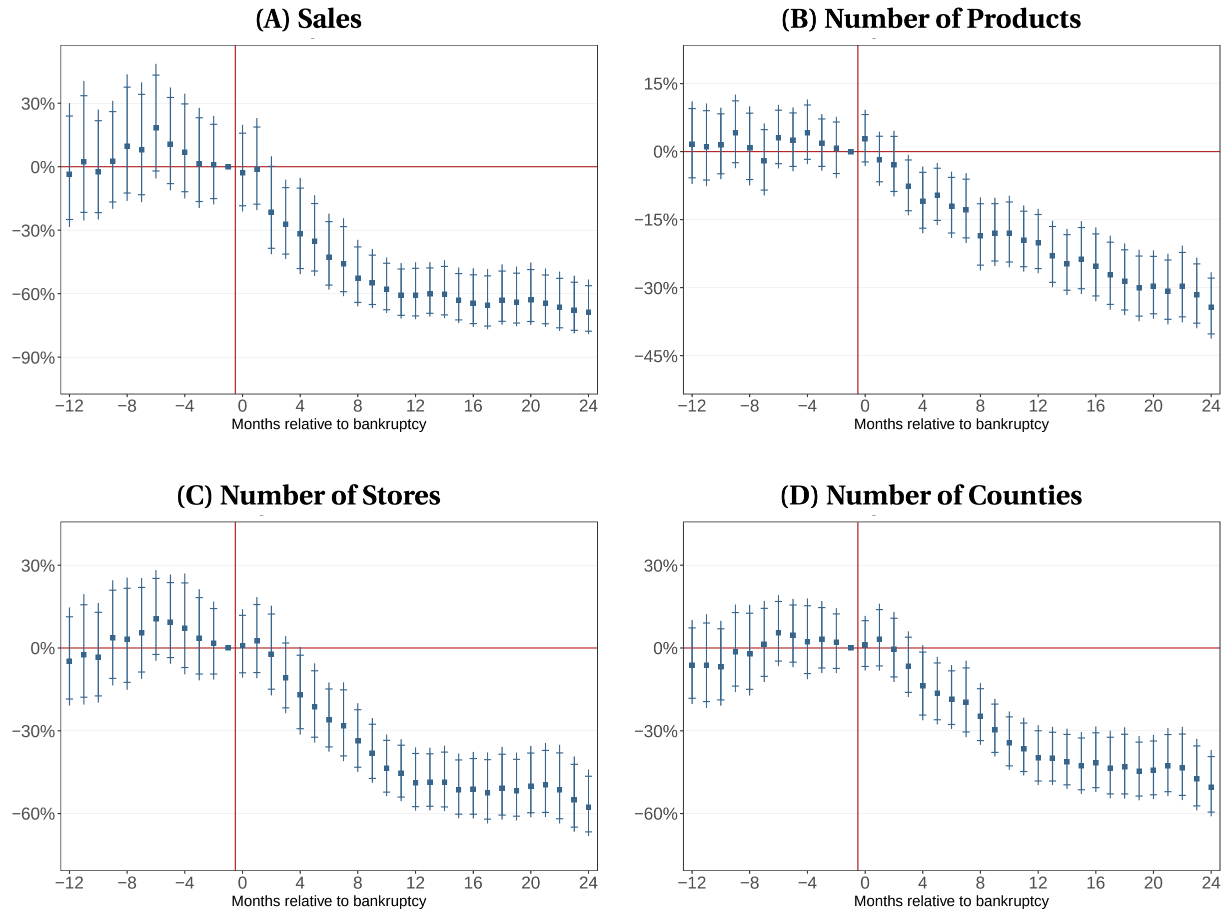

Figure A.3. The Dynamic Effects of Corporate Bankruptcy on Firm-Level Outcomes in Levels. This figure plots the coefficient estimates of Equation (4) on four firm-level outcomes: sales, number of products, number of stores with products available, number of counties with products available. The sample period runs from −12 to +24 months around the bankruptcy filing month ($t = 0$). The estimates measure the difference in the outcome variable between treated firms and matched controls relative to the reference period, the month before the bankruptcy filing. The plotted figures correspond to percentage changes in the outcome of interest, obtained by the transformation $100(e^{\beta} - 1)$ where β is the coefficient of Equation (4) with the outcome variable in log levels. Standard errors are double clustered at the firm and month-year levels and corrected for the coefficient transformation using the Delta method. The bars indicate 90% and 95% confidence intervals.

Table A.2. Bankruptcy Effects: Firm-Level Evidence with Alternative Matching Periods. This table reports DID coefficient β from the estimation of Equation (3). The dependent variables are growth in sales, number of products, number of stores, and number of counties. Growth rates are defined as the log difference between a variable in a given month and the same month of the previous year, in percentage points. Sales is total firm revenue. Number of products is the count of unique products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sells products from the firm. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. The sample consists of firm-month-level observations of bankrupt firms matched to counterfactual controls. Each bankrupt firm is matched to three similar non-bankrupt counterfactuals based on monthly sales, sales growth, number of unique products sold, and number of stores with products from the firm in the quarter (Panel A), semester (Panel B), or year (Panel C) before the bankruptcy event. A treated-controls group is defined as a *cohort*, and the sample is a stack of cohorts. All specifications include firm-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] parentheses, t-statistics are computed using robust standard errors double-clustered by firm and year-month. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

Panel A. Match in the Quarter Prior to Filing

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt	−38.14	−12.95	−32.91	−27.58
	(−6.57)***	(−6.79)***	(−7.37)***	(−7.30)***
	[−6.49]***	[−6.57]***	[−7.30]***	[−7.20]***
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	109,695	109,695	109,695	109,695
Adjusted R^2	0.24	0.21	0.23	0.22

Panel B. Match in the Semester Prior to Filing

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt	−38.74	−13.60	−41.81	−28.41
	(−6.56)***	(−6.87)***	(−7.29)***	(−7.55)***
	[−6.49]***	[−6.61]***	[−7.16]***	[−7.44]***
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	106,807	106,807	106,807	106,807
Adjusted R^2	0.23	0.20	0.21	0.23

Panel C. Match in the Year Prior to Filing

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt	−32.87	−12.64	−28.68	−23.61
	(−5.25)***	(−6.74)***	(−5.93)***	(−5.83)***
	[−5.20]***	[−6.43]***	[−5.80]***	[−5.69]***
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	100,977	100,977	100,977	100,977
Adjusted R^2	0.22	0.19	0.21	0.21

Note: * p<0.05; ** p<0.01; *** p<0.001

Table A.3. Bankruptcy Effects: Firm-Level Product Market Overlap Matches. This table reports DID coefficient β from the estimation of Equation (3). The dependent variables are growth in sales, number of products, number of stores, and number of counties. Growth rates are defined as the log difference between a variable in a given month and in the same month of the previous year, in percentage points. Sales is total firm revenue. Number of products is the count of unique different products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sell products from the firm. "Bankrupt" is an indicator that equals one in the months after a firm enters bankruptcy. Observations are at the firm-month level. The sample consists of firm-month-level observations of bankrupt firms matched to close product market competitors. Each firm bankrupt firm is matched to three other non-bankrupt firms with the highest number of common store-modules where they sell products. A treated-controls group is defined as a *cohort*, and the sample is a stack of cohorts. All specifications include firm-cohort and time-cohort fixed effects. For details, see Section 8.2. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] parentheses, t-statistics are computed using robust standard errors double-clustered by firm and year-month. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt	−46.85	−15.62	−41.25	−34.55
	(−8.45)***	(−8.83)***	(−9.79)***	(−9.63)***
	[−8.34]***	[−8.55]***	[−9.66]***	[−9.35]***
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	135,323	135,323	135,323	135,323
Adjusted R^2	0.25	0.24	0.24	0.22

Note: * p<0.05; ** p<0.01; *** p<0.001

Table A.4. Bankruptcy Effects: Firm-Level Evidence from the Duration of the Bankruptcy Process. This table reports DID coefficients on firm-level outcomes for the duration of the bankruptcy proceedings. The dependent variables are growth in sales, number of products, number of stores, and number of counties. Growth rates are defined as the log difference between a variable in a given month and in the same month of the previous year in percentage points. Sales is total firm revenue. Number of products is the count of unique different products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sells products from the firm. "During Bankruptcy" is an indicator that equals one in the months after a firm enters bankruptcy and before the resolution date. The indicator is zero in the months before to the filing, and is missing in the period after resolution and for cases with no resolution date. The sample consists of firm-month-level observations of bankrupt firms matched to counterfactual controls. Each bankrupt firm is matched to three counterfactuals based on monthly sales, sales growth, number of unique products sold, and number of stores with products from the firm in the month before the bankruptcy event. A treated-controls group is defined as a *cohort*, and the sample is a stack of cohorts. All specifications include firm-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. See Section 8.3 for details. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] parentheses, t-statistics are computed using robust standard errors double-clustered by firm and month-year. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:			
	Sales Growth	Growth in the Number of Products	Growth in the Number of Stores	Growth in the Number of Counties
	(1)	(2)	(3)	(4)
During Bankruptcy	−37.79	−18.15	−24.84	−18.44
	(−4.09)***	(−6.55)***	(−3.47)***	(−3.10)**
	[−4.02]***	[−6.18]***	[−3.43]***	[−3.06]**
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	46,682	46,682	46,682	46,682
Adjusted R^2	0.27	0.24	0.28	0.27

Note: * $p<0.05$; ** $p<0.01$; *** $p<0.001$

Table A.5. Liquidation Effects: Firm-Level Evidence in Levels. This table reports DID coefficient β from the estimation of Equation (3). The dependent variables are sales, number of products, number of stores, and number of counties. The reported estimates correspond to $100(e^{\beta}-1)$ where β is the coefficient of Equation (3) with the outcome variable in log levels. Sales is total firm revenue. Number of products is the count of unique different products that a firm sells. Number of stores is the count of unique stores that sells products from the firm. Number of counties is the number of unique counties with stores that sells products from the firm. "Bankrupt" is an indicator that equals one in the months after a firm files for bankruptcy. "Liquidated" is an indicator that equals one if the firm either (i) filed for Chapter 7 Bankruptcy, which was not converted to Chapter 11, (ii) filed for Chapter 11, but it was later converted to 7, or (iii) was liquidated under Chapter 11. Otherwise, "Liquidated" equals zero. TThe sample consists of firm-month-level observations of bankrupt firms matched to counterfactual controls. Each firm that filed for bankruptcy is matched to three counterfactual firms that did not file within our sample period. The matching is based on monthly sales, sales growth, number of unique products sold, and number of stores with products from the firm in the month before the bankruptcy filing. A group of treated–control units is defined as a cohort, and the sample is composed of a stack of cohorts. All specifications include firm-cohort and time-cohort fixed effects. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 4.2. Two sets of t-statistics are reported below the coefficient estimates. First, in (regular) parentheses, t-statistics are computed using robust standard errors clustered by firm. Second, in [square] parentheses, t-statistics are computed using robust standard errors double-clustered by firm and month-year level. Standard errors are corrected for the coefficient transformation using the Delta method. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% levels, respectively.

	Dependent variable:			
	Sales	Number of Products	Number of Stores	Number of Counties
	(1)	(2)	(3)	(4)
Bankrupt	−69.29	−30.79	−50.35	−38.06
	(−9.49)***	(−5.49)***	(−6.11)***	(−4.76)***
	[−9.43]***	[−5.51]***	[−6.09]***	[−4.75]***
Bankrupt × *Liquidated*	−75.27	−27.11	−68.64	−60.99
	(−8.19)***	(−2.77)**	(−8.45)***	(−7.47)***
	[−8.15]***	[−2.76]**	[−8.42]***	[−7.45]***
Firm-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort	Y	Y	Y	Y
Observations	116,823	116,823	116,823	116,823
Adjusted R^2	0.83	0.89	0.82	0.82

Note: * p<0.05; ** p<0.01; *** p<0.001