The Product Market Consequences of Corporate Bankruptcy: New Evidence from 300 Million Retail Transactions*

MURILLO CAMPELLO†
University of Florida & NBER

GUSTAVO S. CORTES‡
University of Florida

SERGIO H. ROCHA§
Monash University

March 2025

Abstract

Poor market conditions can drive firms into distress, but a public signal of insolvency, such as filing for bankruptcy, can further impact performance via required operational adjustments and actions taken by competitors. Leveraging *big data* on millions of retail transactions, this paper traces the multifaceted product market consequences of bankruptcy, disentangling the role of prices and quantities, as well as that of products offered, in the performance of bankrupt firms and their competitors. We show that firms that file for bankruptcy alter their portfolio of products, retaining those with higher "product-beta" — our novel measure of a product's riskiness based on the sensitivity of its sales to consumption — suggesting an option-like strategy that bets on upside potential in good states. In the process, those firms discontinue the supply of products to entire geographies, withdrawing from economically disadvantaged and underserved locations, creating "product deserts." Our product–location–time matching approach shows that rivals engage in price wars only *after* a firm files for bankruptcy and *where* they compete with similar products. Only firms undergoing liquidation engage in product fire sales, while firms that eventually emerge from bankruptcy maintain their prices on par with that of local competitors throughout the process.

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

---

*We thank our discussants Ting Dai, Diogo Mendes, Peter Pham, Gordon Phillips, David Sovich, and Ikuo Takei for their valuable suggestions. We also thank Kevin Aretz, Arseny Gorbenko, Evgeny Lyandres, Nadya Malenko, Lakshmi Naaraayanan, Michaela Pagel, Woochoel Shin, Wei Wang, and conference and seminar audiences of the Finance Down Under, AFBC, Australasian Meeting of the Econometric Society, FIRN, Lubrafin, FSU–Truist Beach Conference, Sydney Banking and Financial Stability Conference, UFPE, and Monash University for their useful comments. Hassan Ilyas and Guilherme Junqueira provided excellent research assistance. Disclaimer: *Researchers' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ data sets at the Kilts Center for Marketing Data Center at the University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researchers and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.*

†Corresponding Author. Warrington College of Business, University of Florida, and NBER. 327 Stuzin Hall, PO Box 117168, Gainesville, FL 32611. Email: campello@ufl.edu. Website: murillocampello.com.

‡Warrington College of Business, University of Florida. 306 Stuzin Hall, PO Box 117168, Gainesville, FL 32611. Email: gustavo.cortes@warrington.ufl.edu. Website: sites.google.com/site/cortesgustavos.

§Department of Banking and Finance, Monash University. 900 Dandenong Road, Caulfield East, VIC 3145, Australia. Email: sergio.hr@monash.edu. Website: www.shrocha.com.

1 Introduction

Poor product market performance often precedes financial distress, leading firms to seek Chapter 11 protection from creditors while restructuring operations. The market's reaction to bankruptcy filings is both immediate and severe. Take the example of Eastman Kodak's Chapter 11 filing. The firm was underperforming for several years before filing for a much-anticipated bankruptcy in January 2012. While management assured normal operations would continue, Moody's immediately downgraded the firm, and the stock price dropped 55%.[1][2] Such reactions trigger broader market dynamics, affecting product pricing and availability, competitors' strategies, and consumer welfare.

This paper traces out a number of unexplored product market consequences arising from corporate bankruptcy. It does so addressing pivotal questions for consumer markets: How does a firm's filing for bankruptcy shape its market operational strategies and performance? How do these dynamics impact product portfolios, pricing, and availability in stores and geographic locations? What role do market characteristics and competitor responses play in the outcomes of the distressed firm? Leveraging *big data* from NielsenIQ's Retail Scanner, we address these questions going beyond conventional product market performance metrics, speaking to firm operational choices and its impact on consumers and competitors, while disentangling — often confounded — price and quantity dimensions.

We complement our retailer-level data with NielsenIQ's Homescan panel, which tracks the purchases of approximately 60,000 households across the U.S., providing rich socioeconomic and demographic information on consumers. This dataset allows us to connect product dynamics to household characteristics such as income, education, and employment status, enabling us to determine how bankruptcy-induced supply changes affect different consumer segments. The combination of retailer-level and household-level data creates a comprehensive view of both sides of consumer markets, allowing us to map firms' operational decisions to local socioeconomic characteristics and assess their implications, particularly for underserved communities.

The granularity of our data allows us to perform comparisons that, although not randomized, provide several new insights to the literature. First, we show that firms report steep, significant declines in sales

[1] The WSJ published two articles on Jan 5, 2012. *"Kodak Teeters on the Brink"* quoted insiders saying that *"Kodak would continue to pay its bills and operate normally while under bankruptcy."* Another piece, titled *"Moody's Lowers Eastman Kodak on Probability of Bankruptcy Filing,"* explained Moody's decision to downgrade it into junk after bankruptcy *"The ratings firm noted Kodak's deteriorating liquidity position poses additional challenges to consummating the sale or licensing agreements of its patents."*

[2] By way of comparison, Figure A.1 shows abnormal stock returns of −60% around bankruptcy filings of firms in consumer markets. This estimate aligns with the −55% returns in the literature (Shumway and Warther (1999)).

and product availability after they file for 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 characterize these adjustments along different dimensions of a firm's operations. We show that firms in bankruptcy are more likely to discontinue their lower quality products. They also disproportionately discontinue their less risky products. They further withdraw from economically disadvantaged locations already underserved by rival firms ("product deserts").

Next, we show that the continuing products of bankrupt firms generate less revenue at retail stores due solely to lower quantities sold — not prices — relative to similar products from solvent competitors. Notably, the prices of rival products are cut where they compete with the products of distressed firms, but not elsewhere. Our results are consistent with firms strategically engaging in price wars against rivals going through formal bankruptcy at the points of sale where they directly compete. In addition, we find that firms undergoing liquidation cut the prices of their products relative to bankrupt firms that eventually emerge. In all, firms going through restructuring keep their prices on par with their rivals, while only those in liquidation generate short-term cash flows by aggressively undercutting competitors.

Naturally, filing for bankruptcy results from deteriorating prior performance. As such, simple differences in product market outcomes between a firm in bankruptcy and its rivals should not be interpreted causally. To alleviate this issue, we follow the approach of Fracassi et al. (2022), who also study the consequences of a salient, selected corporate event using our data — the acquisition of consumer goods manufacturers by private equity firms. Specifically, we leverage the granularity of the Retail Scanner data to implement firm and product matching procedures. Under this approach, we match bankrupt firms to non-bankrupt peers that exhibit similar performance in various measures of sales and product availability before filing. At the product-store level, we match each product from a bankrupt firm to similar products of competing firms sold at the same store ("shelf neighbors"). We then estimate changes in performance between bankrupt firms and their products relative to product–location–time-matched controls by employing a stacked-cohort differences-in-differences (DID) method.[3] While this strategy may not fully eliminate endogeneity biases, as we later illustrate, our specifications seem able to rule out confounders along multiple dimensions, such as pre-bankruptcy performance trends and shocks to a local product market as granular as a store aisle.

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

Our firm-level estimates show that firms filing for bankruptcy experience lower growth in gross sales, number of unique products sold, stores ("points of sale"), and locations (counties) of products sold. The economic magnitude of the coefficients is significant. For example, firms observe 36.5 percentage points (p.p.) lower sales growth rates relative to matched counterfactuals following bankruptcy filings.

As bankrupt firms discontinue products and reduce their availability at stores and entire geographic areas, we set out to characterize these operational adjustments. We conduct two complementary survival analyses to shed light into how distressed firms restructure their operations along product and geographic dimensions. 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 all their products from specific counties.

We find that firms in bankruptcy retain products and local operations that represent larger shares of their revenues, indicating a strategic focus on maintaining flagship products and presence in key markets. These companies are also more likely to keep higher-quality, less durable, and riskier products. Our measures of quality and durability follow Argente et al. (2017) and Granja and Moreira (2022), respectively. To measure a product's riskiness, we develop a novel measure of *Product Beta* that captures the sensitivity of a product's sales to more aggregate consumption cycles. The preference for products with higher betas suggests bankrupt firms pursue a "growth option strategy." That is, by retaining products with higher upside potential, these firms better position themselves for recovery during economic upturns. This behavior aligns with theoretical predictions that firms might optimally choose riskier investments when facing uncertainty (Campello and Kankanhalli (2024)). At the same time, discontinuing low-quality, less durable products reflects efforts to streamline operations.

Our county-level survival results reveal how socioeconomic factors influence the exit decisions of bankrupt firms. These firms are more likely to exit counties with higher shares of low-income residents, unemployed individuals, and less-educated populations. Notably, bankrupt firms are substantially more likely to exit"product deserts" — areas already underserved by retailers. Our results show that bankrupt firms' exit strategies disproportionately affect geographic areas with challenging socioeconomic conditions further hurting consumer welfare.

We also uncover significant effects of bankruptcy in the performance of *continuing products*. Our estimates point to a joint, proportional decline in sales and quantities for products associated with bankrupt firms: product sales growth drop by 5 p.p., largely explained by a drop of 5 p.p. in quantity sold, while relative prices stay constant. Our evidence shows that bankrupt firms' sales are affected (1) at the extensive and intensive margins through lower offering of products at points of sales and (2) by lower quantities sold of continuing products.

Our product-level results rely on comparisons between products from bankrupt firms and their non-bankrupt shelf neighbors. However, these estimates may also capture competitive local spillover effects (Berg et al., 2021). To address this potential threat and 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 this direct competition ("far competitors"). The first approach compares prices within products and across stores — effectively controlling for common shocks to the same product. The second approach compares prices within stores and across similar product categories — wiping out common shocks affecting the same point of sale. To illustrate these comparisons, consider a gallon of whole milk from a bankrupt dairy producer sitting beside a competitor's milk in a grocery store's refrigerated section. Our first test compares the competitor's whole milk price at this store with the identical whole milk sold in stores where the bankrupt brand is absent. Our second test compares this whole milk gallon with the same competitor's heavy cream sold elsewhere in the same store. These counterfactuals isolate local competitive responses by exploiting the intuition that manufacturers would primarily adjust strategies for products directly competing against bankrupt offerings rather than their entire product portfolio or store network.

In both exercises, we find that the price of a competitor product decreases when it is "close" to the bankrupt firm's product after the filing relative to when it is "far", revealing that strategic price adjustments occur precisely where direct rivalry exists. Our results are consistent with a product market rival engaging in price wars to drive the distressed firm out of the markets where they *both* sell their competing products. These novel findings show remarkably localized evidence of price wars while sweeping out shocks to particular products or stores.

We also show how retail chain size modulates the competitive dynamics following bankruptcy filings by establishing that aggressive price competition is only found on the shelves of smaller retailers. This finding can be rationalized by large chains operating with thin profit margins and benefiting from bargaining power with suppliers, leaving little space for price-cutting strategies. As a result, large retailers act as a buffer against extreme price competition during periods of financial distress.

Finally, we investigate whether pricing policies differ based on whether firms recover from bankruptcy under Chapter 11 or cease operations under liquidation. Arguably, firms undergoing restructuring betting on resurrection may maintain longer-term pricing policies despite poor sales, while those permanently discontinuing operations may seek to liquidate inventory quickly. Indeed, we find that the price of products from firms under liquidation decreases by 3.9 p.p. relative to products of restructuring firms. Our estimates suggest that only firms nearing the end of their operations adjust prices to quickly meet creditors' obligations in court, while firms planning on reemerging show no price change relative to their market rivals.

We provide fresh, new contributions to a well-established literature on firm financing and product markets. Brander and Lewis (1986) argue that leverage can serve as a commitment device to engage in aggressive output strategies (see also Maksimovic (1988)). Bolton and Scharfstein (1990), in contrast, argue that firms can explore their rivals' distress by increasing output and driving them into insolvency. Early empirical studies largely support the "deep pocket" hypothesis, whereby financially healthy firms use war chests to finance predatory strategies against distressed rivals (see, e.g., Chevalier (1995) and Phillips (1995) for early papers). Campello (2006) reconciles opposing arguments by showing that the effect of debt financing on competitive performance is nonmonotonic: it can boost performance to some extent, after which further leverage and investment become value-destroying.[4]

More recently, access to big data allowed researchers to examine product markets more granularly (see Dichev and Qian (2022) for a discussion). Kim (2020) and Granja and Moreira (2022) report evidence that credit-constrained firms reduce prices to liquidate inventories and innovate less. Fracassi et al. (2022) show that firms targeted by private equity acquisitions increase their sales by introducing new products and expanding geographically. In a regulatory paper, Bhattacharya et al. (2023) evaluate U.S. antitrust guidelines by examining the effects of a series of mergers on retail markets.

By departing from commonplace measures of firm performance, such as market shares and profits, we show that the consequences of financial distress go well beyond lower sales, shaping firms and product market outcomes across various dimensions. Our data allow us to provide new insights into the product market consequences of corporate bankruptcy by mapping firms operational decisions as a function of local economic conditions and product characteristics while developing a new product-level measure of riskiness. By decomposing revenues into prices and quantities, we are able to assess the role of each of these

[4]More recent work investigating various interactions between firm financing and product markets include Frésard (2010), Frésard and Valta (2016), Cookson (2017, 2018), Clara et al. (2021), and Hajda and Nikolov (2022).

margins in the performance of firms' continuing products. Finally, we document how the consequences of bankruptcy are not restricted to bankrupt firms, generating incentives for their rivals to respond at the locations where they directly compete. By observing product markets more accurately than usual industry classifications such as SIC and NAICS, we reveal the two-fold nature of granular competition: product and location. This comprehensive approach ensures the validity of our results.(Frésard and Phillips (2022)).

Our paper is also related to the literature on corporate bankruptcy. Benmelech et al. (2019) and Bernstein et al. (2019) assess the spatial spillovers of bankruptcy. They show that the liquidation of establishments imposes negative externalities on their locations, depressing employment and leading to further closures. Using randomized experiments, Antill and Hunter (2021) find that consumers are less willing to pay for products of firms that file for bankruptcy. He et al. (2024) show that filing firms must divert resources away from maintaining valuable customer relationships during restructuring, suggesting that filing for bankruptcy can be viewed as sacrificing investment in customer capital. Finally, Chava et al. (2024) show that large, publicly listed firms' bankruptcies increase municipal bond yields at the counties where the firms are headquartered.

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 a previously undocumented dimension: bankrupt firms disproportionately exit from economically disadvantaged areas, aggravating "product deserts" that widen consumption inequality. Furthermore, our identification strategy isolates competitive price adjustments that occur exclusively where direct product rivalry exists, revealing localized dynamics invisible to conventional industry-level analyses. Complementing Antill and Hunter's (2021) experimental approach measuring consumer attitudes, we empirically document that retail sales effects manifest primarily through quantities, not prices. Our product-level analysis reveals strategic patterns impossible to detect with establishment-level data, showing precisely which products firms retain or discontinue when distressed. Finally, related to He et al.'s (2024) definition of customer capital, our evidence that firms strategically retain high-beta products suggests an active management of their product portfolios to preserve upside potential. Our findings complement theirs showing that firms might want to avoid a complete dismantling in customer relationship during the filing process, investing in strategic products as a major source of the firms' intangible capital value.

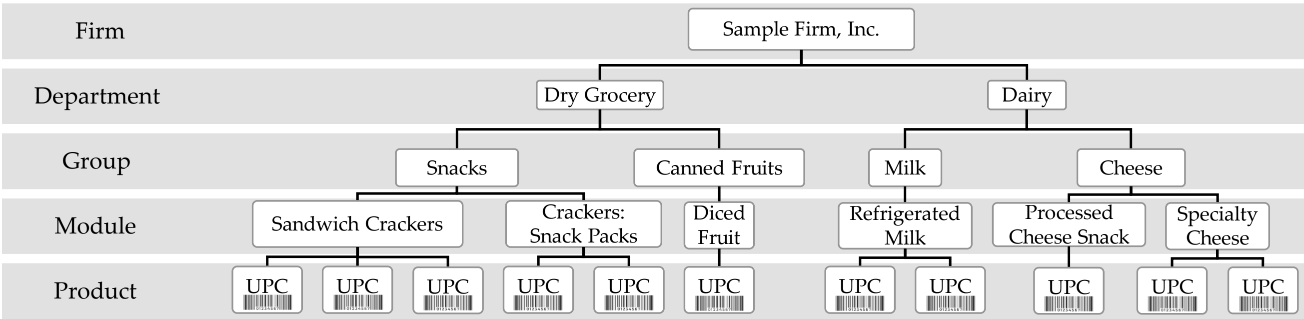

Figure 1. Hierarchy in the Nielsen Retail Scanner Data. This figure shows the hierarchy structure observed in the Nielsen Retail Scanner Data. At the top of the hierarchy is a firm whose products are divided by department, group, module, and finally by product (represented by a unique product code, UPC). UPCs are popularly known as bar codes.

2 Data and Sample Construction

2.1 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 data set 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 data 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 (Granja and Moreira (2022)). These modules are then further aggregated into 125 groups, which form 10 departments, the most coarse product categories. We illustrate this hierarchy with an example in Figure 1, 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. 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 as well. The data tracks information from approximately 30,000 to 50,000 individual stores from around 90 retail chains across more than 2,700 counties yearly,[5] covering over 6 million unique products across all years. This corresponds to roughly 53% of total food sales, 55% of drug store sales, and 32% of mass merchandise sales in the U.S., providing a rich and fairly 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 parent company of the products in the Nielsen dataset. Using a list of prefixes issued by GS1, we are able to match nearly 80% of all UPCs in the retail scanner data to a firm name.

2.2 Nielsen Homescan Data

We supplement the Retail Scanner data with Nielsen Homescan, 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 in each purchase and store visits by participants.[6] Crucially, the data also reports household socioeconomic and demographic characteristics such as income level, employment status, education, and 5-digit ZIP code of residence.

The Homescan data track approximately 40,000 households between 2004 and 2006 and 60,000 from 2007 onwards. 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.[7] We leverage Nielsen Homescan data primarily to track socioeconomic characteristics of the geographic locations where firms sell their products and coupon usage and deals in household purchases, while we rely on Retail Scanner for product sales information.

[5]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).

[6]Where available, the stores in the Homescan can be matched to the stores in the Retail Scanner.

[7]See Einav et al. (2010) and Butler et al. (2023) for discussions on Homescan representativeness.

2.3 Bankruptcy Filings

We collect cases of corporate bankruptcy filings from four different sources: The Florida–UCLA–LoPucki Bankruptcy Research Database (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 filings by public firms only, with BRD covering cases filed by companies with assets worth at least $355 million, measured in 2022 values. Capital IQ, however, also reports filings by private companies. From all these sources, we collect information on the filers name and the exact date of the filing. 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 as firms that initially file for 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 filed between 2007 and 2020, as we require available product market data for at least one year before the filing as 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 were filed by public firms. To account for subsidiaries when bankruptcy is filed by a parent firm, 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 1 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.[8]

[8]Spectrum Brands filed for Chapter 11 on February, 2009 and emerged on August of that same year. See *NBC News*, "Spectrum Brands files for bankruptcy," Feb 4, 2009; and *Reuters*, "Spectrum Brands exits bankruptcy," Aug 29, 2009.

Table 1. Summary Statistics: Bankruptcy cases. This table reports summary statistics of the 227 bankruptcy filings included in our sample. Panel A reports the number of product categories each firm operates, where product categories are Nielsen's modules, groups, and departments (see Section 2.1 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	Pctl(25)	Median	Mean	Pctl(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	Pctl(25)	Median	Mean	Pctl(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

Panel B of Table 1 describes our sample's coverage of product categories. Importantly, all 10 departments are covered in our sample. The department with the least number of bankrupt firms operating is "Packaged Meat" with a total of 8 firms, while 93 bankrupt firms had products in the "General Merchandise" department. In addition, a total of 110 groups and 628 modules are present in our sample with products from at least one firms that filed.

2.4 Product Beta: A New Measure of Consumption Sensitivity

Corporate finance theory suggests that firms facing financial distress may strategically adjust their risk profiles. The seminal work by Brander and Lewis (1986) demonstrates that firms with high leverage may pursue riskier strategies as a competitive commitment device. More recently, Aretz et al. (2019) provide evidence that moderately distressed firms move towards riskier segments. Likewise, Campello and Kankanhalli (2024) show that firms facing uncertainty optimally shift toward higher-risk investment opportunities with asymmetric payoff profiles offering greater upside potential. 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 product-level risk. Building on established measures of product attributes such as quality and durability, our novel metric quantifies a product's sensitivity to aggregate consumption fluctuations, providing a product-level dimension of risk assessment.

For each product, we estimate a simple regression of the log of monthly sales on the log of total sales in its department, which represents the most general description of the product's segment. The coeffi-

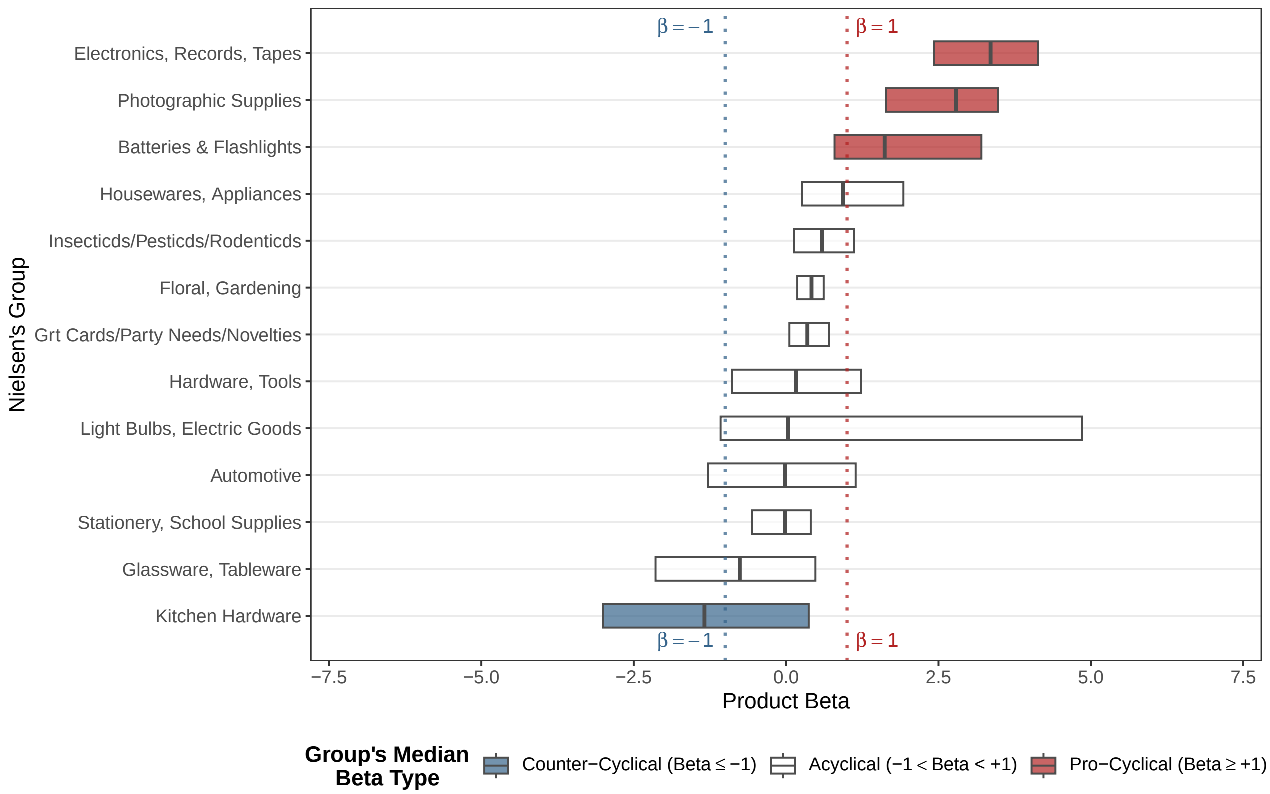

Figure 2. Distribution of Product Betas: Groups in Department "General Merchandise". This figure shows the distribution of product betas across various Nielsen groups within the "General Merchandise" department. The graph includes only groups with at least 50 unique products. Each boxplot summarizes 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.

cient from this regression, which we call *Product Beta* (β), thus represents the elasticity of a product's sales to broader consumption trends in that line of business. To wit, a product beta greater than one ($\beta > 1$) indicates the product's sales are more volatile than its department's overall consumption. In contrast, a beta lower than minus one ($\beta < -1$) indicates a counter-cyclical behavior relative to broader consumption trends. As such, firms with a high-beta product portfolio are riskier to the extent their revenues are more sensitive to consumption cycles in their lines of business.

Figure 2 focuses on the 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).

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 could be prone to postponing these purchases during states of lower consumption at the department level. *"Batteries and Flashlights"* also display high cyclicality ($\beta \geq 1$), perhaps 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.

Finally, the counter-cyclical group includes *"Kitchen Hardware,"* showing an inverse relationship to department-level consumption — possibly reflecting increased home cooking in challenging times — and *"Glassware and Tableware"* trends toward counter-cyclicality but remains acyclical, suggesting a selective purchasing of these items during recessions.

In addition to our Product Beta measure, we incorporate established metrics of product characteristics to comprehensively assess firms' strategic decisions during bankruptcy. Following Argente et al. (2017), we measure product *Quality* as the log difference between a product's unit price and its respective module's median price in a given month. This approach captures relative price premiums within narrowly defined product categories, reflecting both perceived quality and market positioning. For durability assessment, we follow Granja and Moreira (2022) and construct a *Semidurable* indicator that equals one if a product's module is in the top tercile of our durability measure. We calculate this measure as the average number of times households purchase goods from a particular module during a year, with less frequent purchases indicating higher durability. By combining these established metrics with our novel Product Beta measure, we create a multidimensional framework for analyzing how product characteristics influence firms' operational decisions during bankruptcy.

3 Empirical Strategy

3.1 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 total revenue by quantity to get average monthly prices. We use this resulting dataset to build our variables of interest both at the firm×month and product×store×month levels. All of our dependent variables are measured in annual growth rates, computed as the log difference in a given month and in the same month of the previous year, in percentage points (the same approach is used by Hyun and Kim (2023)).[9] To alleviate the effect of outliers in our estimates, we winsorize all growth measures at the 2.5% tails.

3.1.1 Firm-Level Analysis

At the firm level, we build four dependent variables: sales growth, growth in the number of products, growth in the number of stores, and growth in the number of counties where products are sold. Sales are 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 of products. The number of stores is the count of unique stores that sell any product from the firm, thus measuring product availability at retailers. Finally, our fourth variable is the number of counties with stores that sell any product from the firm, thus measuring product geographic availability through 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.

3.1.2 Product-Level Analysis

At the product-store 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.

[9] Results are qualitatively similar if we measure our variables in log levels instead.

One major advantage of the Retail Scanner data is that it allows us to decompose revenues into local prices and quantities, which is critical to determine the strategic margins at which firms and their competitors react to bankruptcy filings. 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 its average price during that month. Conveniently, owing to the properties of logarithms, quantity growth plus price growth approximately equals sales growth.

3.2 Matching Procedure

Filing for bankruptcy 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 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 underwhelming performance.

To alleviate endogeneity concerns, we use a matching strategy implemented by Fracassi et al. (2022). Specifically, we match each bankrupt firm and their product–store pairs to competitors who never filed for bankruptcy throughout the sample period that are similar along multiple observable characteristics as of the month before the filing. After matching treated units to their closest controls, we follow and compare their trajectories after the filing.[10]

In our firm-level tests, each bankruptcy-filing firm is matched to three counterfactual competitors. The matching procedure closely follows Fracassi et al. (2022). First, we 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. Finally, for each pair of treated-control firms, we sum these standardized absolute differences to get an overall distance measure. Specifically, suppose that x_i^a is the value of attribute a for firm i. The distance between firms i and j is given by

$$D_{i,j} = \sum_a \frac{| x_i^a - x_j^a |}{\sigma^a} \tag{1}$$

where σ^a is the sample's standard deviation of attribute a. We compute the distances between each bankrupt firm and all others 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 filing. Then,

[10] We refer to firms that filed for bankruptcy and their products as "treated" units.

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 at least one treated and one control product ("shelf neighbors"). Next, we compute the distance between each treated product and its shelf neighbors following Equation (1) based on total units sold, price, growth in units sold, and price growth. Finally, we select up to three closest products as counterfactuals for each treated product.

Consider the following example as an illustration of our product-matching procedure. Assume the firm depicted in Figure 1 files for bankruptcy in January 2010. Suppose it sells specialty cheeses in store XYZ in Gainesville, Florida. We look for all other products from the same module (i.e., "Specialty Cheese") sold in store XYZ. 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 December 2009. The four-unit cohort in this example consists of the bankrupt product and its three matches in store XYZ throughout the sample period.

3.3 Summary Statistics

We report summary statistics of multiple samples at Table 2. Panels A and B report our firm- and product-level samples, respectively. In Panel A.1, we consider the full firm sample, which consists of 123,411 firm-month observations. We observe large heterogeneity as firm averages are substantially larger than medians across all measurements, consistent with a right-skewed firm size distribution. For instance, whereas the median firm sells about $24.9 thousand a month, the average firm sells almost $900,000. In addition, the median (average) firms sell 10 (50.6) unique products sold on 342 (3,188) stores, located in 112 (685) counties. In Panels A.2 and A.3, we report summary stats for our samples of bankrupt and matched counterfactual firms at the time of the matching, the month before the bankruptcy filing. Finally, Panel A.4 reports 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, asserting the validity of our firm matching.

Panel B of Table 2 describes our sample of products. Like Panel A.1, Panel B.1 considers the total product-level sample, which 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). Panels B.2 and B.3 report summary statistics for

Table 2. Summary Statistics: Firm and Product Samples. This table reports summary statistics of the firm- and product-level samples. The samples spam from January 2006 to December 2020. Each panel reports the number of observations, mean, standard deviation, median, and interquartile range (IQR) conditional on treatment status of the observations. The last two columns report the differences in means between treated and control groups and 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. 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. Treated firms are those that filed for bankruptcy within the sample period. On Panel B, observations are at the product-store-month level. Sales are the products' monthly total sales, expressed in dollars. Quantity is the total number of units of the product 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 and show similar performance to their respective treated product prior to the bankruptcy filing (See Section 3.2 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

products of bankrupt firms and their matched counterfactuals, respectively, at the time of the matching. Panel B.4 reports mean differences across treated and control products, and the p-values of their respective t-statistics. Although the differences across the groups are minimal in economic magnitude, we observe statistical significance in quantities sold due to our large sample size.

3.4 Empirical Specifications

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

$$y_{f,c,t} = \beta \cdot Bankrupt_{f,c,t-1} + \mu_{f,c} + \mu_{t,c} + \epsilon_{f,c,t} \tag{2}$$

where $y_{f,c,t}$ is the outcome of interest for firm f, month t and cohort c. $Bankrupt_{f,c,t-1}$ is an indicator that equals one in the months after firm f from cohort c filed for bankruptcy. The firm-cohort fixed effect $\mu_{f,c}$ implies an outcome comparison within the same firm before and after bankruptcy. The time-cohort fixed effect $\mu_{t,c}$ ensures that each bankrupt firm is compared only with its matched control each month. The coefficient of interest β estimates the impact of the bankruptcy filing on the dependent variable relative to a similar control firm around the period of the filing. 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.

Next, we conduct an event study to capture the dynamic effects of bankruptcy filings on the matched sample. Here, we restrict observations of bankrupt firms and their matches to the 12 months before and 24 months after the filing. Therefore, we estimate the following specification:

$$y_{f,c,t} = \sum_{\substack{k=-12 \\ k\neq-1}}^{24} \beta_k \cdot \left[Bankrupt_{f,c,t-1} \times \mathbb{1}_k\right] + \mu_{f,c} + \mu_{t,c} + \epsilon_{f,c,t} \tag{3}$$

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

Note that firm sales aggregates revenues across several products and stores. As such, 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). 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 an individual UPC at each store where it is sold. Specifically, we estimate the following specification:

$$y_{i,c,t} = \beta \cdot Bankrupt_{i,c,t-1} + \mu_{i,c} + \mu_{t,c} + \epsilon_{i,c,t} \tag{4}$$

where y is the dependent variable for product i, month t and cohort c. Similar to our firm-level regressions, *Bankrupt* is an indicator that equals one in all the months after a product's producer filed for bankruptcy. Here, β measures how products from firms in bankruptcy perform relative to similar products from healthy peers after the filing. We use two different clustering schemes. First, we double cluster standard errors by products and month-year. Second, we double-cluster standard errors by firm and month-years. We report two respective t-statistics below each coefficient estimate.

4 Results

4.1 Firm-Level Evidence

We report estimation results of Equation (2) in Table 3. The estimates show that, although filing for bankruptcy is already the result of poor performance and inability to honor debts, it further leads to severe disruptions in many aspects of product market operations. Post-filing, bankrupt firms experience lower growth in sales, number of unique products sold, number of stores with products, and counties with products relative to similar non-bankrupt firms. The economic magnitude of the coefficients is significant. For instance, bankrupt firms have roughly 37 percentage points (p.p.) lower growth in sales relative to their matched counterfactuals. Columns 2 to 4 show similar figures, with all estimates statistically significant at the 0.1% level in both clustering schemes.

Despite our matching procedure alleviating self-selection concerns, a causal interpretation of the coefficients in Table 3 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

Table 3. Bankruptcy Effects: Firm-Level Evidence. This table reports DID coefficient β from the estimation of Equation (2). 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 expressed in percentage points. 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. Observations are at the firm-month level. 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 counterfactuals based on monthly sales, sales growth, and the number of unique products sold 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 3.2. 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: * $p<0.05$; ** $p<0.01$; *** $p<0.001$

treated and control firms resulting from the lack of suitable counterfactuals in the sample. To test for parallel trends before the bankruptcy filing and assess its dynamic effects over time, we report estimates of Equation (3) on Figure 3. The results reinforce the quality of the matching: treated and control firms show no pretrends before the bankruptcy filing. After the filing, however, bankrupt firms experience a sharp and almost immediate decline in all outcomes relative to their matched controls. These effects are highly statistically significant and persist for most of the 24 months following the filing.[11]

We go a step forward characterizing our findings and map our results to observe the geography of bankruptcy through the lens of product availability and sales growth. Panel A in Figure 4 depicts the growth rates of total number of unique products sold in each county. Panel B shows sales growth from dollar-value sales of bankrupt products. It shows that bankruptcy brings about a notable drop in sales and product availability in virtually all U.S. counties.

[11] We report dynamic estimates in levels instead of growth rates in Figure A.2. The results are qualitatively similar.

Figure 3. The Dynamic Effects of Corporate Bankruptcy on Firm-Level Outcomes. This figure plots the coefficient estimates of Equation (3) 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 filing 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.

4.2 Operational Decisions

Our results in Section 4.1 show that firms in bankruptcy discontinue operations by withdrawing products and ceasing to supply specific stores and whole geographic locations. Whereas 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 offers 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

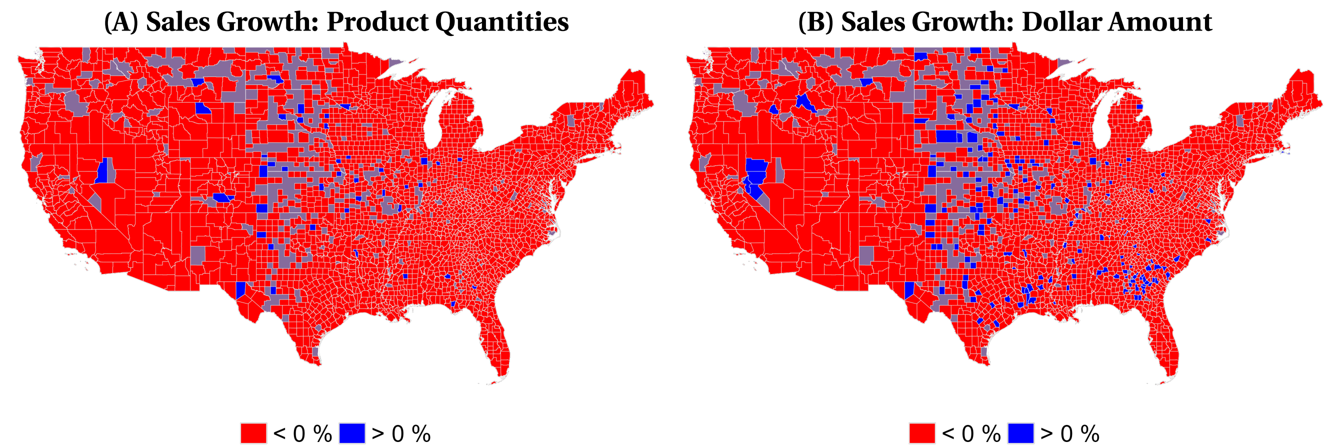

Figure 4. The Geography of Bankrupted Firms' Products: 1Y Before vs. 1Y After Bankruptcy Filing. This figure maps the growth rates of products sold by bankrupted firms in each county. Panel A shows the growth rate in total number of products sold in each county. Panel B shows each county's total nominal USD sales growth. Counties with no data are shown in gray.

shelves across the country. Second, we map the factors that predict the likelihood of bankrupt firms stop supplying all their products from a geographical location.

4.2.1 Discontinued Products

We conduct a survival analysis to estimate the factors that correlate with the withdrawal of products from 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 3.2 while subsuming the store dimension from our observations. For each product from a firm undergoing bankruptcy proceedings, we form cohorts based on all the competitor products matched in any store following our procedure in Section 3.2. Thus, our sample consists of product-month observations. 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.[12] As usual in survival analysis, the product is excluded from the panel after its "death".

We perform a stacked cohort DID where the independent variables, fixed effects, and clustering schemes are as in Equation (4). Therefore, the coefficient of *Bankrupt* estimates the change in the probability of a product being discontinued after a firm files for bankruptcy relative to matched counterfactuals, and the interaction terms estimate how product cross-sectional factors affect the timing of the bankrupt firms' decisions to terminate its production. We follow Bowen et al. (2016) and estimate the model via OLS.

[12] Prior to the end of our sample (December 2020).

We focus on four product characteristics: (*i*) *Revenue Share* is the product's share of the total monthly sales of the firm; (*ii*) *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); (*iii*) *Semidurable* is an indicator variable that equals one if the product's module is in the top tercile of our module durability measure;[13] (*iv*) *Product Beta* is our measure of product cyclicality, which proxies for the product's riskiness, as described in Section 2.4.

We report the results of our survival analysis on Table 4. To facilitate interpretation, we scale the coefficients to represent percentage points. First, the coefficient of "Bankrupt" confirms that the probability of a product being discontinued significantly increases after its producer files for bankruptcy across all specifications. The economic magnitude is substantial. For instance, in column (1), we estimate that the probability of product withdrawal increases by 1.10 p.p. more than similar, non-bankrupt products after the filing, which corresponds to 291% of the sample mean.

Overall, our results in Table 4 reveal that firms in bankruptcy are less likely to discontinue products that account for larger shares of their revenues, that are of higher quality, less durable, and riskier. Products with higher revenue shares are significantly less likely to be discontinued, suggesting that bankrupt firms prioritize retaining core products that contribute most to their top line. Higher-quality products are also less likely to be discontinued, indicating that firms aim to maintain their reputation and competitive edge even during financial distress.

Interestingly, semidurable products are more likely to be discontinued, although this result is less robust across specifications. A number of non-competing explanations can rationalize this result. First, discontinuing such products could be part of a broader strategy to simplify operations and focus on faster-moving product lines that require lower inventory investments and storage costs. Second, semidurable goods tend to be more capital-intensive and sensitive to financial frictions (Granja and Moreira (2022)), thus being particularly costly for financially impaired firms. Finally, products with longer sales cycles face more competition from used markets, making them less attractive when firms need to generate immediate cash flows.

The finding that products with higher betas (i.e., riskier products) are less likely to be discontinued is particularly noteworthy. This suggests bankrupt firms may be inclined to keep products with higher upside potential, possibly as part of a strategy to recover from financial distress during economic upturns. This behavior aligns with the theoretical predictions in Campello et al. (2022), where firms optimally choose riskier

[13] We follow Granja and Moreira (2022) and compute this measure as the average number of times households purchase goods from that module during a year. We obtain household purchase habits from Nielsen Homescan, which tracks participating households' shopping trips and product purchases.

Table 4. 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 files for bankruptcy. "Revenue Share", "Quality", "Semidurable", and "Consumption Beta" are measures of a product's importance to the firm's revenues, quality relative to similar products, durability, and riskiness, respectively. See Section 4.2 for details on variable construction. To facilitate interpretation, the coefficients are scaled to represent percentage points. The sample comprises product-month level observations of products from firms in bankruptcy matched to their counterfactual controls. See Section 3.2 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:					
	$\mathbb{1}$(Exit)					
	(1)	(2)	(3)	(4)	(5)	(6)
Bankrupt	1.06	1.11	0.91	0.91	0.97	0.68
	(14.59)***	(14.37)***	(13.06)***	(7.98)***	(13.54)***	(5.67)***
	[4.98]***	[4.89]***	[4.74]***	[2.98]**	[4.69]***	[2.24]*
Bankrupt × Revenue share		−0.02				−0.02
		(−5.03)***				(−4.24)***
		[−2.66]**				[−2.56]*
Bankrupt × Quality			−0.56			−0.59
			(−8.33)***			(−8.36)***
			[−4.03]***			[−4.23]***
Bankrupt × Semidurable				0.32		0.37
				(2.55)*		(2.91)**
				[0.90]		[1.19]
Bankrupt × Product Beta					−0.09	−0.09
					(−5.97)***	(−6.06)***
					[−3.09]**	[−4.07]***
Product-Cohort FE	Y	Y	Y	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y	Y	Y	Y
Observations	47,374,866	47,374,866	47,374,580	47,281,359	47,374,866	47,281,073
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

investments when facing uncertainty, potentially viewing the retention of high-beta products as a form of "growth option" investments that could yield significant returns if market conditions improve.

4.2.2 Discontinued Locations

We conduct a similar survival analysis at the firm-county level. Here, 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 operations. 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 3.2. As in Section 4.2.1, 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 peers.

We focus on several factors that encompass both the importance of a location to the firms' operations and social-demographic factors. *Firm Revenue Share* is a county's share of total firm revenues throughout the sample. *Product Desert* is an indicator of low availability of wide ranges of products is a county. We follow Alcott et al. (2019) and consider a location to have low availability of products when it has scarce presence of retailers. Specifically, we compute the number of stores per capita and classify as product deserts those counties in the bottom quartile of the yearly distributions. To create socioeconomic indicators, we leverage household-level information from Nielsen Homescan and project it to the county level using Nielsen's sampling weights.[14] 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 on Table 5. Again, 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 generally increase the probability of exit by 1.65 p.p. relative to their matched counterfactuals, which corresponds to 185% of the overall sample mean.

[14] 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 5. 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 files for bankruptcy. "Firm Revenue Share" is the share of a firm's revenues that comes from that county. Large retailers are defined as being above the average total revenues within the sample period. "Low Income Share", "Minority Share", "Unemployed Share", and "Low education Share" are the shares of the population that are on the bottom tercile of the sample income distribution, are black or of Hispanic origin, 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 per capita. To facilitate interpretation, the coefficients are scaled to represent percentage points. The sample comprises firm-county-month level observations of firms in bankruptcy matched to their counterfactual controls. See Section 3.2 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

The results in this survival analysis reveals 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 our results in Section 4.2.1. 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,

Table 6. Bankruptcy Effects: Product-Level Evidence. This table reports DID coefficients from the estimation of Equation (4) 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, reported in percentage points. "Bankrupt" is an indicator that equals one in the months after a firm files for bankruptcy. The sample consists of product-month-level observations in a matched sample. Each product of a firm that filed for bankruptcy is matched to at most three other similar products in the same store and module (product category). 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 3.2. For details, see Section 3.2. 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: * $p<0.05$; ** $p<0.01$; *** $p<0.001$

a strong effect is observed in areas already underserved by large retailers. Based on the estimates in column (8), we find that bankrupt firms are 73% more likely to exit product deserts. This finding indicates that bankruptcy-induced exits exacerbate the already limited availability of consumer products in these areas, potentially widening consumption disparities.

4.3 Performance of Continuing Products

Our previous analyses show that bankrupt firms decrease the size of their product portfolios and availability at locations, which compose the extensive margin of sales and might partially explain the concurrent drop in revenues. Next, we dig deeper into the drivers of bankrupt firms' revenues by switching our focus to the sales of *continuing* products, where we can disentangle the role of prices and quantities. Specifically, we consider only products for which we observe sales for at least one year after the filing and their shelf neighbors. We estimate Equation (4) on this sample of product-stores and report the results in Table 6.

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. Columns (2) and (3) report changes in the growth of quantities sold and prices, respectively. Interestingly, the estimates show that reduced sales of distressed firms' products are entirely driven by lower quantities, with virtually no differential price change. The coefficients are economically significant: the 5.18 percentage point decrease in quantity growth corresponds to almost twice the sample average.

4.4 Restructuring versus Liquidation

While our analysis thus far has examined bankruptcy cases collectively, it is crucial to recognize that the specific characteristics of each case, such as the filing chapter or court outcome, may significantly influence the product market strategy of distressed firms. 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 when comparing our setting to recent work by Kim (2020), who examines how credit supply shocks affect firms' pricing decisions and concludes that firms facing credit constraints tend to decrease prices to liquidate inventory and generate short-term cash flows. In contrast, our disruptive event of financial distress allows us to explore how the "fire sale" behavior may differ based on the expected post-bankruptcy outcome.

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

$$y_{f,c,t} = \beta \cdot Bankrupt_{f,c,t-1} + \gamma \cdot [Bankrupt_{f,c,t-1} \times Liquidated_c] + \mu_{f,c} + \mu_{t,c} + \epsilon_{f,c,t}, \tag{5}$$

where $Liquidated_c$ is an indicator that equals one if the bankrupt firm in cohort c either (i) filed for Chapter 7 Bankruptcy, which was not later converted to Chapter 11, (ii) filed for Chapter 11, which was later converted to 7, or (iii) was liquidated under Chapter 11. $Liquidated_c$ equals zero if the firm filed for Chapter 11 or filed for Chapter 7, which was later converted to Chapter 11, and is missing otherwise. Therefore, the coefficient γ estimates the differential impact of liquidation relative to restructuring on our firm-level dependent variables.

At the product level, we estimate an analogous specification where *Liquidated* is similar to that in Equation (5), but referring to product cohorts. Here, γ captures differences in sales, price and quantity growth rates between products from liquidated firms relative to products from firms that emerged. In Table 7, we report the results of our heterogeneity analysis defined in Equation (5).

Table 7. Liquidation Effects: Firm-Level Evidence. Log-Levels. This table reports DID coefficient β from the estimation of Equation (2). 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, expressed in percentage points. 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. Observations are at the firm-month level. "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 bankrupt firms matched to counterfactual controls. Each bankruptcy-filing firm is matched to three similar counterfactuals based on monthly sales, sales growth, and the number of unique products sold 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 3.2. 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	−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

The estimates in Table 7 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, showing that our results in Table 3 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.[15] While this is not surprising, it disentangles 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.

Finally, we report product-level results of our case outcome analysis in Table 8. Our 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

[15]We report similar results in levels instead of growth rates in Table A.1.

Table 8. Product-Level Bankruptcy Effects. This table reports DID coefficients from the estimation of a product-level specification analogous to 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, reported in percentage points. "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 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-month-level observations in a matched sample. Each product of a firm that filed for bankruptcy is matched to at most three other similar products in the same store and module (product category). 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), cf. Section 3.2. For details, see Section 3.2. 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	−5.20	−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	0.15

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

(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, which is associated with higher quantities sold.

The results in Table 8 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 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 condition 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 engage in fire sales. In stark contrast, our analysis of bankruptcy filings reveals that such aggressive pricing strategies are limited to firms facing liquidation. It is striking that while Kim (2020) reports substantial price reductions in response to relatively mild financial constraints, we observe this behavior only in the most extreme cases of firms that are going out of business in literal terms. This discrepancy questions the broader applicability of Kim's (2020) results and underscores the importance of distinguishing between different levels of financial distress when analyzing firms' product market strategies.

4.5 Reactions of Product Market Rivals

The results in our product-level analysis of Section 4.3 and Section 4.4 rely on comparisons between products from firms in bankruptcy and their non-bankrupt shel neighbors. Due to this close competition between products, it is possible that our estimates also capture reactions in the product market strategies of other firms in response to a rival's financial distress. Whereas the non-significant results in column (3) of Table 8 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 that are not necessarily close competitors. At the product level, however, our matching procedure focuses on products that are close competitors, possibly capturing strategic reactions by product market rivals at the store-aisle level. The granularity and wide scope of retail scanner data, however, allow us to assess these competitive responses explicitly.

4.5.1 Price Wars

We build on the notion that a competitor of a distressed firm only has incentives to react with products and 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 distressed firm, it does not have to change prices across all points of sales and products uniformly. Given that demand for consumer products is segmented across product categories and locations, a competitor might change prices only where it has products similar to those of the bankrupt firm for sale.

To test this idea, we perform two complementary exercises where we compare the changes in prices and quantities between *close* and *far* competitors of products from firms in bankruptcy. Our first step is to obtain the products used as counterfactuals to the bankrupt products in Section 4.3. Since these products are in the same store and module as the bankrupt ones, we classify them as close competitors. Our two tests differ on the definition of far competitors.

In our first test, we follow Fracassi et al. (2022) and look for these same products in stores where their respective bankrupt match is absent. Further, we impose that the stores belong to the same retail chain to alleviate concerns of different trends across retailers or different contracts between suppliers and retail chains. These products are the far competitors forming the pool of potential controls in this exercise. Whereas this test compares outcomes of same products with similar sales performance, it still relies on comparisons across different stores. To further address concerns of different local shocks to prices, our second exercise defines far competitors as different products manufactured by the same competitor within the same store. For each close competitor of a bankrupt product, we look for products in the same store, produced by the same firm, but that belongs to a different module to form the pool of potential 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 3.2.

Finally, we estimate a specification similar to Equation (4), where our variable of interest now is *Close Competitor*, an indicator that equals one if a product is a close competitor of a bankrupt one post-filing. Here, the cohorts are formed by the close competitor and its three most similar afar competitors. Hence, in our first matching approach, we control for any shock that is particular to each product and uniform across stores. In 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 9. Panel A shows our *within product, across store* results. Column (1) shows smaller revenues from close competitors, although the estimates are only marginally statistically significant. Decomposing revenues into quantities and prices in columns (2) and (3), respectively, reveals a strong, significant decrease in the price of a product when it is closely competing with products from a firm in bankruptcy. Panel B reports the estimates from our *within store, across product* test. Overall, we do not observe significant effects in revenues and quantities across close and far competitors in this specification. Again, column (3) shows a decrease in prices relative to products of different categories from the same firm and store.

Table 9. Close and Far Competitor Products. This table reports DID coefficients 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, reported 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 files for bankruptcy and after the filing. Each such close competitor is matched to at most three similar far competitors, which consist of the same product sold at stores where the bankrupt product is absent. The matching is based on the average price and quantity sold in the month prior to 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, Section III.B). For details, see Section 3.2. 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:		
	Sales Growth	Quantity Growth	Price Growth
	(1)	(2)	(3)
Close Competitor	−0.37	−0.26	−0.10
	(−2.08)*	(−1.48)	(−3.99)***
	[−2.02]*	[−1.43]	[−3.62]***
Product-Cohort FE	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y
Observations	5,056,312	5,056,312	5,056,312
Adjusted R^2	0.16	0.18	0.60

Panel B. Within Stores, Across Products

	Dependent variable:		
	Sales Growth	Quantity Growth	Price Growth
	(1)	(2)	(3)
Close Competitor	−0.14	0.74	−0.69
	(−0.15)	(0.79)	(−3.05)**
	[−0.11]	[0.58]	[−2.32]*
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	0.09	0.10	0.19

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

Taken together, the results from Table 6 and Table 9 are consistent with price wars in which distressed firms and their close competitors slash the prices of their products at points of sale. Our granular evidence suggests that these prices wars are localized: competitors only decrease prices of products that are of the same type of the bankrupt products *and* sold at the same stores. Although we find some weak evidence of lower revenues for competitors, our results suggest that bankrupt firms largely bear the cost of attrition in the form of lower sales growth relative to close competitors despite the overall lower prices.

4.5.2 Do Large Retailers Curb Price Wars?

While our results on competitors' reactions provide evidence of price wars where firms in bankruptcy sell their products, these interactions likely vary across different retail environments. We examine whether the presence of large retailers mitigates aggressive competition, potentially due to downside price rigidity in their stores. Major retail chains may dampen price-cutting behavior for two primary reasons. First, their economies of scale allow them to operate with lower profit margins than smaller, local retailers, leaving less room for price reductions. Second, large retailers often have significant bargaining power with suppliers due to their pivotal role in providing access to a vast consumer base, potentially influencing in-store pricing decisions more strongly.

We classify large retail chains as those in top quintile of the distribution of total revenues through the sample period. We interact an indicator *Large Retailer* in our DID specifications to capture differential effects of bankruptcy in large retail chains relative to small ones. We assess these effects for both the bankrupt products relative to their close competitors as well as for close competitors relative to far competitors following our two definitions in Section 4.5.1.

We report results for our sample of bankrupt products and their matched counterafctuals by retail chain size in Table 10. Notably, products from bankrupt firms show a slight price decrease at smaller retailers relative to their close competitors. However, this price reduction is strongly muted in large retailers. We also observe a slight attenuation of the lower sales of bankrupt products in large retailers, although this result is not statistically significant at the 5% level.

Next, we extend our analysis to examine how product market rivals' reactions vary by retail chain size.[16] Table 11 shows strong evidence of price decreases in small retailers, with notable attenuation in large chains and in both classifications of far competitor products. These findings suggest that the competitive landscape is not uniform across different types of stores, with larger retailers potentially serving as a buffer against extreme price competition, acting as a stabilizing force in the market during periods of financial distress.

[16]Note that, since our within product, across store analysis imposes that stores should belong to the same retail chain, the variable *Large Retailer* is defined at the cohort level.

Table 10. Product-Level Bankruptcy Effects: Heterogeneity by Retail Chain Size. This table reports DID coefficients from the estimation of Equation (4) 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, reported in percentage points. "Bankrupt" is an indicator that equals one in the months after a firm files for 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-month-level observations in a matched sample. Each product of a firm that filed for bankruptcy is matched to at most three other similar products in the same store and module (product category). 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 3.2. 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

5 Robustness

5.1 Disentangling Producer and Retailer Reactions

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

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

Table 11. Close and Far Competitor Products across Stores: Heterogeneity by Retail Chain Size. This table reports DID coefficients 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, reported 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 files for bankruptcy and after the filing. Each such close competitor is matched to at most three similar far competitors, which consist of the same product sold at stores where the bankrupt product is absent. The matching is based on the average price and quantity sold in the month prior to 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, Section III.B). For details, see Section 3.2. 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:		
	Sales Growth	Quantity Growth	Price Growth
	(1)	(2)	(3)
Close Competitor	−0.98	−0.66	−0.33
	(−2.01)*	(−1.34)	(−4.23)***
	[−1.94]	[−1.30]	[−3.96]***
Close Competitor × *Large Retailer*	0.96	0.72	0.30
	(1.91)	(1.40)	(3.60)***
	[1.87]	[1.38]	[3.47]***
Product-Cohort FE	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y
Observations	5,016,983	5,016,983	5,016,983
Adjusted R^2	0.16	0.18	0.60

Panel B. Within Stores, Across Products

	Dependent variable:		
	Sales Growth	Quantity Growth	Price Growth
	(1)	(2)	(3)
Close Competitor	0.26	2.04	−1.52
	(0.27)	(2.03)*	(−8.37)***
	[0.22]	[1.83]	[−6.95]***
Close Competitor × *Large Retailer*	−0.53	−1.70	1.10
	(−0.45)	(−1.29)	(3.47)***
	[−0.39]	[−1.12]	[3.10]**
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	0.09	0.10	0.19

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

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 features only for a subset of stores audited by Nielsen. While the stores can change over time, the data manual states that if a particular store is audited in a given week, it is reasonable to assume that all other stores of the same retailer will have the same items featured within a designated market area (DMA). We follow this recommendation and build a product-store-month indicator of whether an item was featured at any week of the month and any store of that retail chain and DMA. The sample excludes observations for which no store was audited in a given retail chain–DMA combination. We estimate Equation (4) with *Feature* as an outcome to capture if retailers tend to feature items of firms in bankruptcy relative to their shelf neighbors. We also check for differential effects of firms in liquidation, which could explain our fire sales results in Table 8.

We report these results in Table 12, where the coefficients are changes in the probability of a product being featured, in percentage points. The estimates show no evidence of differential sales efforts by retailers for products of firms in bankruptcy. Although positive, the coefficients of *Bankrupt* are not statistically significant and represent around 2% of the average feature variable. In addition, column (2) shows a null differential effect for products of liquidating firms and a non-statistically significant and negligible coefficient in the interaction with *Liquidated*.

In addition to featuring items, retailers can exert other sales efforts, such as issuing discount coupons and offering deals. 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 in using Homescan's coupons and deals in our framework. First, this information is captured by participant panelists' purchases rather than store sales. Therefore, to be consistent with our product-level sample of Section 4.3, we include only product-stores that appear in the original sample, which significantly decreases its size. We also maintain the original cohort groups for our stacked DID regressions. Second, coupons and deals may be offered by the manufacturer instead of the retailer. 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

Table 12. Bankruptcy and Sales Efforts: Featured Products. This table reports DID coefficients from the estimation of Equation (4) on product-level outcomes. Dependent feature indicators, in percentage points. "Bankrupt" is an indicator that equals one in the months after a firm files for bankruptcy. The sample consists of product-month-level observations in a matched sample. Each product of a firm that filed for bankruptcy is matched to at most three other similar products in the same store and module (product category). 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 3.2. 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:	
	Feature	
	(1)	(2)
Bankrupt	0.36	0.39
	(0.50)	(0.49)
	[0.37]	[0.36]
Bankrupt × Liquidated		−0.11
		(−0.08)
		[−0.07]
Product-Cohort FE	Y	Y
Month-Year-Cohort FE	Y	Y
Observations	60,742,881	60,742,881
Adjusted R^2	0.44	0.44

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

10% of the 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.

Akin to Butler et al. (2023), we construct two variables at the product-store-month level.[17] *Coupon* is the the dollar value of coupon discounts as a percentage of total sales and *Deal* is the percentage of total units sold that involved a deal. In our sample, the mean values of *Coupon* and *Deal* are 3.22% and 32.78%, respectively, which is consistent with Butler et al. (2023).

We report results of Equation (4) with *Coupon* and *Deal* as dependent variables in Table 13. Again, we find no significant estimates for both regardless of the outcome of the bankruptcy case. In column (4) the coefficient of the interaction term corresponds to roughly 8% of the sample's average of deal sales, which is non-negligible. Still, its negative sign goes against the notion that store deals could explain the price cuts for products of firms under liquidation.

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

Table 13. Bankruptcy and Sales Efforts: Coupons and Deals. This table reports DID coefficients from the estimation of Equation (4) on product-level outcomes. Dependent feature indicators, in percentage points. "Bankrupt" is an indicator that equals one in the months after a firm files for bankruptcy. The sample consists of product-month-level observations in a matched sample. Each product of a firm that filed for bankruptcy is matched to at most three other similar products in the same store and module (product category). 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 3.2. 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:			
	Coupon		Deal	
	(1)	(2)	(3)	(4)
Bankrupt	−0.22	−0.26	−1.45	1.08
	(−0.98)	(−0.78)	(−1.42)	(0.49)
	[−0.74]	[−1.05]	[−0.99]	[0.36]
Bankrupt × Liquidated		0.07		−2.65
		(0.17)		(−1.13)
		[0.18]		[−0.84]
Product-Cohort FE	Y	Y	Y	Y
Month-Year-Cohort FE	Y	Y	Y	Y
Observations	2,403,981	2,403,981	2,404,657	2,404,657
Adjusted R^2	0.46	0.46	0.48	0.48

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

Taken together, the results in this section provide no evidence of retailer-initiated promotions driving the price effects that we observe for products of firms under liquidation. Although we cannot completely rule out the role of retailers in adjusting standard shelf prices, we show that sales efforts that are directly controlled by stores remain unaffected. Overall, this finding strengthens the case for our results in Section 4.4 being producer-driven.

5.2 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 filing at the time of matching. If that is the case, a firm's performance before filing for bankruptcy might reflect its own product market adjustments as well as its competitors' reactions in anticipation. To help ensure that our results in Table 3 are not reflecting these potential confounders, we estimate Equation (2) by matching bankrupt firms to other firms that exhibit similar performance several months before the filing.

Table 14. Bankruptcy Effects: Firm-Level Evidence with Alternative Matching Periods. This table reports DID coefficient β from the estimation of Equation (2). 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, expressed in percentage points. 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 files for bankruptcy. Observations are at the firm-month level. 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 similar counterfactuals based on monthly sales, sales growth, and the number of unique products sold in the quarter (Panel A), semester (Panel B), or year (Panel C) 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 3.2. 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

We report the results of this exercise in Table 14. In Panels A, B, and C, we match firms in the quarter, semester, and year before the bankruptcy filing, respectively. Whereas the magnitudes of the estimates vary slightly across specifications, the results are qualitatively similar to our earlier analysis. It does not appear that our firm-level results are driven by fluctuations in the performance of distressed firms right before they file for bankruptcy.

5.3 Product Market Proximity Matching

By following the firm matching procedure described in Section 3.2, 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 filing for bankruptcy relative to *close product market competitors*.

We employ an alternative matching procedure to compare distressed firms to their close rivals. We construct 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. Here, 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 filing. We then re-estimate Equation (2) on this new set of firm cohorts. We report the results of this exercise in Table 15.

Our firm-level results hold, with point estimates larger than those in Table 3. These larger effects might reflect possible downsides of this matching procedure nonetheless. To wit, while we ensure comparisons with control firms that operate in the same product markets, we can no longer claim that the matches represent counterfactuals that are similar in pre-filing performance. Nevertheless, the economic inferences remain qualitatively unchanged.

The results of this exercise unveil a trade-off in designing a matching procedure. Whereas 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 distress with their possibly healthy close competitors, we might overestimate results

Table 15. Bankruptcy Effects: Firm-Level Product Market Overlap Matches. This table reports DID coefficient β from the estimation of Equation (2). 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 expressed in percentage points. 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 files for 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 that filed for bankruptcy is matched to three other 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. The matching procedure and specification closely follow Fracassi et al. (2022), cf. Section 3.2. For details, see Section 3.2. 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

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 3.2 alleviates such issues by prioritizing similar performance instead of selecting counterfactuals that are necessarily direct competitors.

6 Concluding Remarks

Filing for bankruptcy clearly 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 investigates the product market consequences of corporate bankruptcy filings in the U.S. Our empirical analysis reveals that following the filing, bankrupt firms experience severe disruptions in their product market operations. We demonstrate that bankrupt firms suffer from lower sales, reduce the number of products they offer, and discontinue their supply to retailers and entire counties.

We characterize firms' operational adjustments following bankruptcy. These firms tend to discontinue their less important, lower quality, and durable 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 the upside potential to recover during economic upswings. In addition, we show that firms in bankruptcy are more likely to withdraw their products from economically disadvantaged locations that retailers already underserve.

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 are virtually unaffected. However, firms undergoing liquidation significantly decrease prices relative to those that eventually emerge, implying that distressed firms engage in fire sales only when moving towards the end of their operations. Next, we assess the role of competitor responses in driving these effects on bankrupt firms. We show that market rivals decrease the price of their products only when they closely compete with the products of distressed firms but not elsewhere. However, these price responses are muted in large retailers, arguably due to downside price rigidity.

Our results contribute to the long-standing literature on how financial distress shapes product markets through the strategies of distressed firms and their competitors. Our detailed product-level data allows us to go well beyond previous empirical work by examining product markets at a granular level, unveiling localized decisions by multiproduct manufacturers. Our work illuminates the multifaceted consequences of bankruptcy, particularly by characterizing distressed firms' operational adjustments along the product and locality dimensions and by empirically assessing the role of competitor responses.

References

Alcott, Hunt, Rebecca Diamond, Dubé Jean-Pierre, Jessie Handbury, Ilya Rahkovsky, and Molly Schnell, 2019, Food deserts and the causes of nutritional inequality, *Quarterly Journal of Economics* 134, 1793–1844.

Antill, Samuel, and Megan Hunter, 2021, Consumer choice and corporate bankruptcy, *Working Paper*.

Aretz, Kevin, Shantanu Banerjee, and Oksana Pryshchepa, 2019, In the path of the storm: Does financial distress cause industrial firms to risk-shift?, *Review of Finance* 23, 1115–1154.

Argente, David, Munseob Lee, and Sara Moreira, 2017, Innovation and product reallocation in the Great Recession, *Journal of Monetary Economics* 93, 1–20.

Argente, David, Munseob Lee, and Sara Moreira, 2024, The life cycle of products: Evidence and implications, *Journal of Political Economy* 132, 337–390.

Benmelech, Efraim, Nittai Berman, Anna Milanez, and Vladimir Mukharlyamov, 2019, The agglomeration of bankruptcy, *Review of Financial Studies* 32, 2542–2586.

Berg, Tobias, Markus Reisinger, and Daniel Streitz, 2021, Spillover effects in empirical corporate finance, *Journal of Financial Economics* 142, 1109–1127.

Bernstein, Shai, Emanuele Colonnelli, Xavier Giroud, and Bnejamin Iverson, 2019, Bankruptcy spillovers, *Journal of Financial Economics* 133, 608–633.

Bhattacharya, Vivek, Gastón Illanes, and David Stillerman, 2023, Merger effects and antitrust enforcement: Evidence from US retail, *Working Paper*.

Bolton, Patrick, and David Scharfstein, 1990, A theory of predation based on agency problems in financial contracting, *American Economic Review* 93, 93–106.

Bowen, Donald E., Laurent Frésard, and Jérôme P. Taillard, 2016, What's your identification strategy? Innovation in corporate finance research, *Management Science* 63, 2529–2548.

Brander, James, and Tracy Lewis, 1986, Oligopoly and financial structure, the limited liability effect, *American Economic Review* 76, 956–970.

Butler, Alex, Irem Demirci, Umit Gurun, and Yessenia Tellez, 2023, Consuption smoothing via product markets, *Working Paper*.

Callaway, Brantly, and Pedro H.C. Sant'Anna, 2021, Difference-in-differences with multiple time periods, *Journal of Econometrics* 225, 200–230.

Campello, Murillo, 2006, Debt financing: Does it boost or hurt firm performance in product markets?, *Journal of Financial Economics* 82, 135–172.

Campello, Murillo, Gustavo S. Cortes, Fabrício d'Almeida, and Gaurav Kankanhalli, 2022, Exporting uncertainty: The impact of Brexit on Corporate America, *Journal of Financial and Quantitative Analysis* 57, 3178–3222.

Campello, Murillo, and Gaurav Kankanhalli, 2024, Corporate decision-making under uncertainty: Review and future research directions, in David Denis, and John McConnell, eds., *Handbook of Corporate Finance*, 315–332 (Edward Elgar Publishing).

Cha, William, Pradeep Chintagunta, and Sanjay Dhar, 2015, Food purchases during the great recession, *Kilts Booth Marketing Series, Paper No. 1-008* .

Chava, Sudheer, Baridhi Malakar, and Manpreet Singh, 2024, Impact of large firms' bankruptcies on local governments, *Working Paper*.

Chevalier, Judith A., 1995, Do LBO supermarkets charge more? An empirical analysis of the effects of LBOs on supermarket pricing, *Journal of Finance* 50, 1095–1112.

Clara, Nuno, Alexandre Corhay, and Howard Kung, 2021, Firm product concentration and asset prices, *Working Paper*.

Cookson, J. Anthony, 2017, Leverage and strategic preemption: Lessons from entry plans and incumbent investments, *Journal of Financial Economics* 123, 292–312.

Cookson, J. Anthony, 2018, Anticipated entry and entry deterrence: Evidence from the American casino industry, *Management Science* 64, 2325–2344.

Dichev, Ilia D., and Jingyi Qian, 2022, The benefits of transaction-level data: The case of NielsenIQ scanner data, *Journal of Accounting and Economics* 74, 101495.

Einav, Liran, Ephraim Leibtag, and Aviv Nevo, 2010, Recording discrepancies in Nielsen Homescan data: Are they present and do they matter?, *Quantitative Marketing and Economics* 8, 207–239.

Fracassi, Cesare, Alessandro Previtero, and Albert Sheen, 2022, Barbarians at the store? Private equity, products, and consumers, *Journal of Finance* 77, 1439–1488.

Frésard, Laurent, 2010, Financial strength and product market behavior: The real effects of corporate cash holdings, *Journal of Finance* 65, 1097–1122.

Frésard, Laurent, and Gordon M. Phillips, 2022, Product markets, competition and corporate finance: A review and directions for future research, in David Denis, ed., *Handbook of Corporate Finance* (Edward Elgar Publishing).

Frésard, Laurent, and Philip Valta, 2016, How does corporate investment respond to increased entry threat?, *Review of Corporate Finance Studies* 5, 1–35.

Goodman-Bacon, Andrew, 2021, Difference-in-differences with variation in treatment timing, *Journal of Econometrics* 225, 254–277.

Granja, João, and Sara Moreira, 2022, Product innovation and credit market disruptions, *Review of Financial Studies* 36, 1930–1969.

Hajda, Jakub, and Boris Nikolov, 2022, Product market strategy and corporate policies, *Journal of Financial Economics* 146, 932–964.

He, Bianca, Lauren Mostrom, and Amir Sufi, 2024, Investing in customer capital, Working Paper, University of Chicago.

Hyun, Jay, and Ryan Kim, 2023, Spillovers through multimarket firms: The uniform product replacement channel, *Working Paper*.

Kim, Ryan, 2020, The effect of the credit crunch on output price dynamics: The corporate inventory and liquidity management channel, *Quarterly Journal of Economics* 136, 563–619.

Maksimovic, Vojislav, 1988, Capital structure in repeated oligopolies, *RAND Journal of Economics* 19, 389–407.

Maksimovic, Vojislav, and Gordon Phillips, 1998, Asset efficiency and and reallocation decisions of bankrupt firms, *Journal of Finance* 53, 1495–1532.

Phillips, Gordon M., 1995, Increased debt and industry product markets: an empirical analysis, *Journal of Financial Economics* 37, 189–238.

Schoenle, Raphael, 2017, International menu costs and price dynamics, *Review of International Economics* 25, 578–606.

Shumway, Tyler, and Vincent A. Warther, 1999, The delisting bias in CRSP's Nasdaq data and its implications for the size effect, *Journal of Finance* 54, 2361–2379.

Appendix A Additional Results

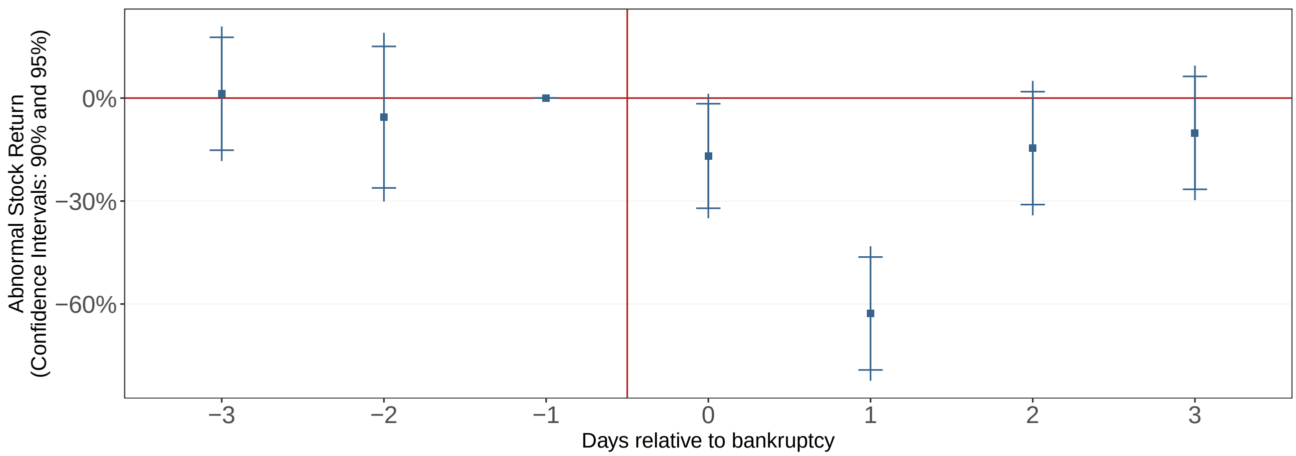

Figure A.1. Buy-and-Hold Abnormal Returns around Bankruptcy Filings. This figure plots abnormal returns in the window of $[-3, +3]$ days around bankruptcy filing dates (day 0). We first estimate expected returns using the Fama–French three-factor model over an estimation window of $[-252, -30]$ trading days before the filing. We then compute abnormal returns as the difference between actual and expected returns, and estimate a regression of these abnormal returns on event time indicators, using the day before filing (i.e., day $t = -1$) as the base period. The figure plots the daily abnormal returns relative to $t = -1$, along with 90% and 95% confidence intervals. The event study includes all publicly traded firms in our Nielsen sample that filed for bankruptcy between 2006:M1 and 2020:M12.

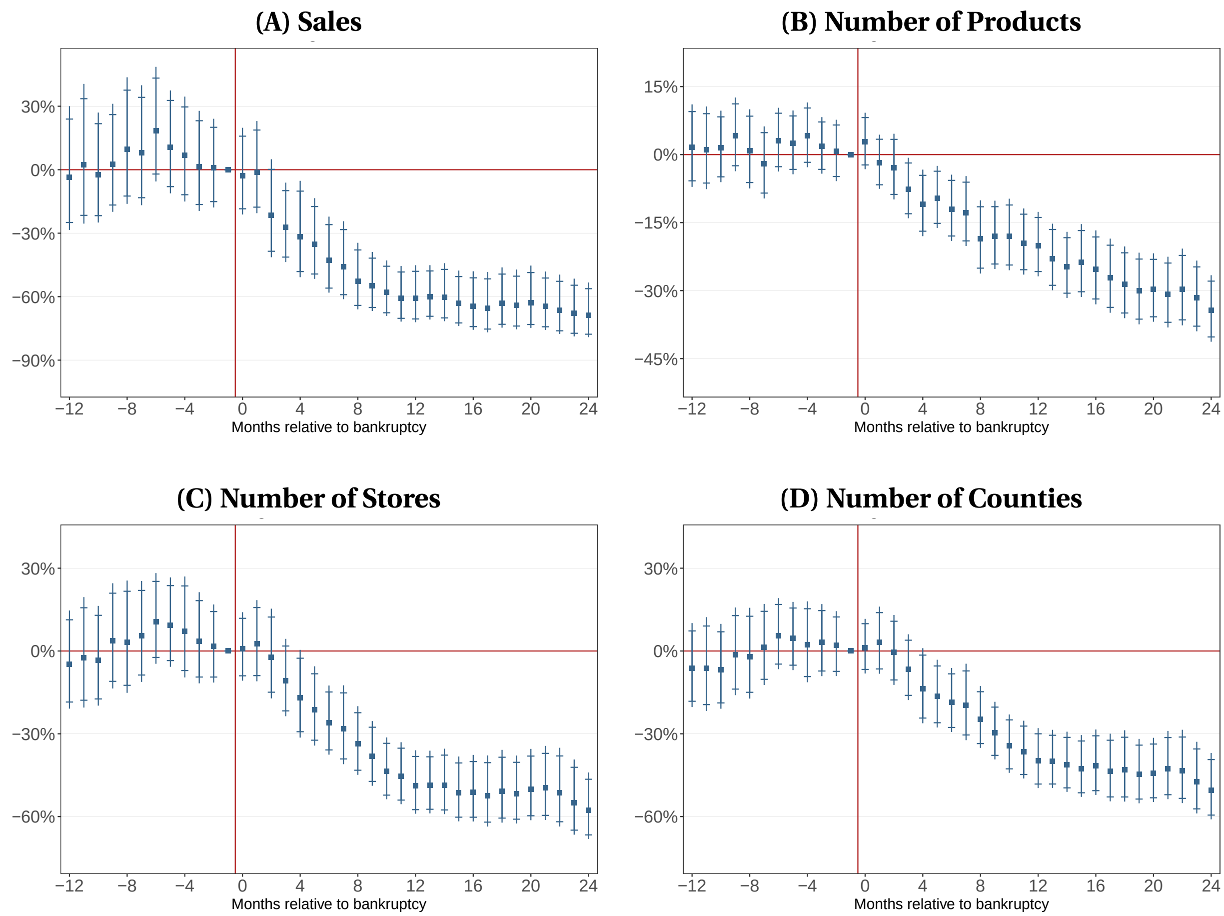

Figure A.2. The Dynamic Effects of Corporate Bankruptcy on Firm-Level Outcomes in Levels. This figure plots the coefficient estimates of Equation (3) on four firm-level outcomes: sales, number of products, number of stores with products available, number of counties with products available. The sample 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 (3) with the outcome variable in log levels. Standard errors are double clustered at the firm and month-year levels. The bars indicate 90% and 95% confidence intervals.

Table A.1. Liquidation Effects: Firm-Level Evidence in Levels. This table reports DID coefficient β from the estimation of Equation (2). 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 (2) with the outcome variable in log levels. 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. Observations are at the firm-month level. "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 firm that filed for bankruptcy is matched to three similar counterfactuals based on monthly sales, sales growth, and the number of unique products sold 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 3.2. 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. *, **, 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	−60.24	−31.20	−50.72	−38.61
	(−5.29)***	(−4.64)***	(−4.29)***	(−3.80)***
	[−5.27]***	[−4.66]***	[−4.28]***	[−3.80]***
Bankrupt × *Liquidated*	−81.18	−26.10	−68.46	−60.47
	(−4.94)***	(−2.24)*	(−4.43)***	(−4.40)***
	[−4.92]***	[−2.23]*	[−4.41]***	[−4.39]***
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