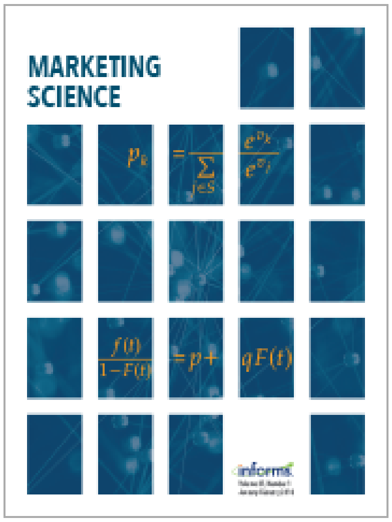

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Price Reactions to Rivals' Local Channel Exits

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In this paper, we study the effect of a firm's local channel exits on prices charged by incumbents remaining in the marketplace. Exits could result in higher prices due to tempered competition or lower prices due to reduced colocation or agglomeration benefits. The net effect of these two countervailing forces remains unknown. In addition, little is known about how this effect could change depending on incumbents' geographic locations. We address this research gap by examining new car price reactions by incumbent multiproduct automobile dealerships who experience the exit of a Chrysler dealership in their local markets. We find evidence that the competition effect exceeds the colocation effect: prices increase by about 1% ($318) following an exit relative to the price change in the absence of an exit. More important, we find that the price increase is lower at dealerships more proximate to the exiting dealership than dealerships farther away for the same set of cars available across these locations. This finding suggests differences in the extent of the two forces (competition and agglomeration) at different distances from the closed dealership. We assess the generalizability of our results by looking at the impact of GM's closure of Pontiac dealerships. Taken together, our results inform consumers, firms, and policymakers about possible implications of an exit.

Data, as supplemental material, are available at http://dx.doi.org/10.1287/mksc.2015.0952.

Keywords: competitive reaction; market exit; pricing; agglomeration; automobiles; bailout
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1. Introduction

In this paper, we seek to understand the impact of a firm's local channel exit on prices charged by the incumbents that continue to serve the market. When a firm exits the local market, intuition suggests that the decrease in the number of firms will lead to a less competitive market, and thus *higher* prices (the "market power" hypothesis). However, in markets where consumer search costs are important, firms colocate with their rivals so as to lower consumer search costs (Rosenthal 1980, Samuelson and Zhang 1992). In such a setting, exit by a proximate firm could lead to *lower* prices as the remaining firms need to compensate for the loss in colocation benefits (the "agglomeration" hypothesis). In the presence of these two opposing forces, ceteris paribus, the impact of an exit on prices of surviving incumbents is ambiguous.

Three broad literatures inform us about the price consequences of a change in the number of firms in a market. The literature on price-concentration and mergers (see Weiss 1989 or Newmark 2006 for a review of these studies) investigates the association between market concentration (measured by the number of firms) and prices. Relying on cross-sectional analyses (either at the market or firm level), early studies found that concentration is positively associated with prices, but the presence of unobserved demand and cost shocks could lead to severe biases in their estimated effects of the number of firms on prices (Bresnahan 1989, Schmalensee 1989, Orhun 2012). Since the mid-2000s, using firm- or market-level panel data, researchers have been able to account for time-invariant unobservables, and have found no effect of geographic concentration or mergers on retail prices (Davis 2005, Jiménez and Perdiguero 2014). A potential limitation of these panel-data-based studies is that they rely on aggregate firm-level price indices or investigate industry settings where prices do not vary within a firm (Cotterill 1986, Manuszak and Moul 2008). Therefore, it is unclear if the predictions from these studies hold in multiproduct firms settings (such as those in the automobile industry we consider), where prices vary quite significantly within and across firms' product lines.

The empirical entry literature also offers inconclusive predictions on the impact of a new competitor on prices (e.g., Simon 2005; Basker and Noel 2009;

Ailawadi et al. 2010; Singh et al. 2006; Gielens et al. 2008; Jia 2008; Singh and Zhu 2008; Zhu and Singh 2009; Zhu et al. 2009, 2011). While several studies have shown incumbent price reductions up to 3% with entry (Basker 2005, Hausman and Leibtag 2007, Basker and Noel 2009), other studies find that no reaction is also common (Ailawadi et al. 2010). Yet another set of studies finds a positive effect of entry on prices (Thomas 1999, Yamawaki 2002). At the same time, it is unclear whether the predictions from this literature translate directly to the case of exits (the focus of our study) due to inherent differences between entering and exiting firms. For example, consumers with high habit persistence may not visit a new store that opens in their local market, but they will have to change their patronage if their favorite store closes (Rhee and Bell 2002). With some notable exceptions, incumbent reactions to exits have received scant attention in the marketing and economics literature. The studies by Joskow et al. (1994) and Daraban and Fournier (2008) (using market-quarter-level data) look at exits in the airline industry and show an increase in fares of around 10% following an exit. The magnitude of competitive reaction to exits in these airline-industry studies is much higher in absolute terms compared to the findings from entry studies in other industries (Basker 2005, Hausman and Leibtag 2007, Basker and Noel 2009), suggesting that a change in the number of firms in a market could have a different effect depending on whether the change is due to a market entry or exit. Since the exit analyses are conducted at the market level, consequences for individual firms/products are less clear. At the same time, predictions from the analysis of exits in the airline industry with little spatial differentiation within a market may not directly translate to industries with potentially high levels of spatial differentiation (e.g., automobile retailing).

A third literature stream, on agglomeration, suggests that firms colocate to take advantage of heightened demand (e.g., Marshall 1920, Stahl 1982, Wernerfelt 1994, Vitorino 2012, Datta and Sudhir 2013). Marshall (1920) makes the observation that consumers are willing to incur a substantial investment of time and travel when buying high-priced products. Thus, he implicitly brings up the role of consumers' search costs in stores' decisions to cluster due to potential demand-side benefits. Stahl (1982) and Wernerfelt (1994) provided formal models that suggest search costs as a motivation for firm agglomeration. According to Stahl (1982), a retailer might choose to locate close to its rivals if increased demand due to colocation (i.e., market area effect) exceeds reduced demand due to substitution. If the retailer decides to join a cluster of competitors, under high consumer search costs due to imperfect information, that retailer will enhance the likelihood of visitation for the other retailers in that location. This positive externality is especially relevant when consumers have a limited knowledge related to the characteristics of the products available in different locations. Wernerfelt (1994), on the other hand, argues that, by locating close to each other, retailers might commit to a strategy that does not take advantage of consumers' initial investment. In other words, retailers can use colocation instead of price advertising especially when consumers need to inspect the product at the store before purchasing (e.g., consumer durables such as cars). This assertion could be more applicable for cars as dealers cannot advertise prices for every unit available for sale. Although scholars have examined the impact of these agglomeration benefits on firm entry decisions, their impact on product-level prices in the context of firm exits has not received much attention.

Taken together, these three streams of literature suggest that in many industries including automobile retailing, where both the competitive effect and the agglomeration effect are at work, extant research offers limited to no guidance on how a firm's exit impacts incumbents' pricing decisions. On one hand, Davis (2005) shows that the price effect of competition attenuates with distance in the motion picture industry. On the other hand, in their analysis of births of new establishments across six U.S. industries, Rosenthal and Strange (2003) find that agglomeration externalities decline swiftly in the first couple of miles but more gradually after that. Unfortunately, how the net effect changes with distance is not known.

In this paper, we seek to complement the above literatures in two ways. *First*, we investigate how incumbent firms' *product-level* pricing decisions respond (direction and magnitude) to a local competitor's exit. *Second*, since competitor exits can result in either gains (due to increased market power) or losses (due to decreased agglomeration benefits) to incumbent firms, and since these losses and gains can change with the distance from the exiting firm, we characterize differential price responses as a function of the geographic distance between the exiting firm and the incumbent.

To accomplish these objectives, we compile a unique data set around an important and unprecedented event in the U.S. automobile retailing industry. In 2009, as part of the much publicized and debated Federal Troubled Asset Relief Program (TARP), also referred to as the "automobile industry bailout program," two of America's three car manufacturers, i.e., Chrysler and General Motors (GM), exercised their last-ditch option and filed for Chapter 11 bankruptcy (Lafontaine and Morton 2010). These were the first auto manufacturer bankruptcies in U.S. history. The move gave Chrysler the freedom to immediately terminate 789—or 25%—of its 3,181 dealerships in May 2009, which it completed in the June–July 2009 period (one dealership in our final data sample closed in August 2009). Such quick and large-scale changes are unparalleled in the U.S. auto industry, but also afford new opportunities to research distribution-channel-related questions that were previously not addressable. Although our main focus is on the exit of Chrysler dealerships, we assess the robustness of our results to the closure of GM's Pontiac dealerships as well.

Specifically, our data and approach provide the following advantages that alleviate the concerns raised in the context of the previous literature as discussed above. First, because of the extent of an unprecedented number of Chrysler and GM closings, we observe a large number of exits spanning numerous geographic markets. Second, our data consists of a comprehensive dealer-VIN-level monthly panel for new cars between October 2008 and September 2010.[1] Our unit of analysis is a car as defined by the 10-digit subset of its VIN that reflects most vehicle characteristics.[2] We seek to understand how the price of a car changes with the exit of a neighboring Chrysler dealership (relative to the price change in the absence of an exit). Thus, we try to mitigate the concerns related to the market- or firm-level analysis. Third, although the number of closed Chrysler dealerships varies across geographic markets, most Chrysler dealerships are terminated in a relatively short period (June–July 2009). So the exit variation in our setting is mainly cross-sectional. This, combined with the panel structure of the data, allows us to use dealer fixed effects to take into account strategic selection (if any) of the terminated dealer as well as other dealer-specific unobservables. Finally, the automobile retailing industry provides an appropriate context to assess agglomeration-related benefits, as previous studies show that agglomeration effects are particularly important in industries where product heterogeneity is high and consumers need to personally inspect goods (Fischer and Harrington 1996, Simon 2005).

We find that, on average, retail prices increase by 1.006% ($318) following an exit, suggesting that the overall gains from tempered competition offset a reduction in agglomeration benefits (if any). To examine if both the "market-power hypothesis" and "agglomeration-benefits hypothesis" are at work in our empirical context, we study how prices of cars vary across three distance bands—within 10 miles, between 10 and 20 miles, and between 20 and 30 miles. As there could be differences in the cars available across these distance bands because of endogenous dealership locations, we use a subsample of our data including a set of car models that are available across all three distance bands. For this subsample, we find that the average price increase due to the exit of Chrysler dealerships within 30 miles is $562. Cars located closest to the exiting dealer (within 10 miles) stand to gain the most from reduced competition, but also experience the most reduction in demand-side agglomeration benefits, compared with cars at less proximate dealers. As a result, prices of cars at proximate dealers rise by $265. By contrast, dealers that are located farther away from the exiting dealer (more than 10 miles) also face tempered competition and do so with limited to no erosion in agglomeration benefits. This affords distant dealers more room to raise prices following an exit. For example, cars located at dealers within 10–20 miles (20–30 miles) raise prices on average by $1,034 ($780) following a Chrysler dealer exit. Since agglomeration benefits erode much faster than gains from tempered competition, proximate dealers have a lower ability to raise prices than extremely distant ones ($265 versus $780).

These results have several implications for consumers, firms, and policymakers. For consumers, our results suggest that if search costs are such that shopping is restricted to the geographic area where the exit occurs, then going to a dealership closest to the exited dealership will likely yield the lowest price because of the biggest loss in agglomeration benefits for the nearest dealerships. From the firm perspective, our results have different implications for product categories in which search costs may or may not play a role in consumers' shopping behaviors. In the presence of search costs, we show that firms need to consider the trade-offs between competitive and agglomeration effects when locating retail outlets. Since the exits in our setting were only realized by forced bankruptcy protection, our findings have direct and immediate appeal to policymakers. First, our findings inform them to the extent to which prices change as a consequence of the closures. This has potential implications for consumer welfare and calls for a more complete analysis of the demand and pricing consequences of the exits. Our findings also inform policymakers of the potential consequences of relaxing franchise laws if such laws lead to closure of additional dealerships. A third implication has to do with evaluating the consequences of mergers in such markets. If mergers lead to fewer dealerships, then policymakers need to evaluate not just the competitive effects of these mergers but also the consequences for consumers in terms of potentially increasing search costs due to lower distribution intensity. These implications transcend the auto industry and are relevant

[1] A VIN is the abbreviation for a 17-digit vehicle identification number that is used to identify individual motor vehicles. As discussed in §2.2.1, we define a "car" as every combination of the digits 1–8 and 10–11 of VINs to capture information on most vehicle characteristics. The availability of data at this level allows us to control for factors such as manufacturer, make, model year, assembly plant, restraint system, body type, engine type, and transmission.

[2] Although we refer to each unit as a car, our products span all vehicle types, including convertibles, coupes, hatchbacks, minivans, sedans, SUVs, trucks, and wagons; and all fuel types including gasoline, diesel, hybrid, or electric.

in other forms of retailing where the competitive and agglomeration forces are at play.[3]

2. Empirical Context

2.1. Background Information: The U.S. Automobile Industry Crisis, Bailout, and Chrysler Dealer Network Consolidation

The automotive industry is an important contributor to the U.S. economy. U.S. auto dealers (new and used car dealerships) account for 7.9% of total retail employment, directly providing jobs for an estimated 1.2 million American workers.[4] Given its economic significance and rich institutional features, the automobile industry has had natural appeal to marketing and management scholars. Academic research examining this industry has generated rich insights around pricing (Bresnahan 1981, Berry et al. 1995, Sudhir 2001), consumer-directed price promotions (Bruce et al. 2006), trade promotions (Bruce et al. 2005), buyer-supplier links (Martin et al. 1995), channel pass-through (Busse et al. 2006), information search (Punj and Staelin 1983), leasing versus selling (Desai and Purohit 1998, 1999), new versus used-car competition (Purohit 1992), consumer-adoption decisions (Schiraldi 2011), dealer-consumer negotiations (Desai and Purohit 2004), product obsolescence (Levinthal and Purohit 1989), and hybrid-car adoption (Gallagher and Muehlegger 2011).

However, since the early 2000s, the Detroit Three have seen their share of the domestic market drop from 64.5% in 2001 to 47.5% in 2008. The decline in U.S. motor vehicle sales accelerated in late 2008, with monthly sales running more than 30% lower than the same month the year before. Americans bought 13.2 million cars and light trucks in 2008, below the 16.1 million units sold in 2007, and well below the peak of the 17.8 million sold in 2000. For the full year, the Detroit Three were the hardest hit, with 2008 sales falling by 30.3%, 22.7%, and 20.3% for Chrysler, GM, and Ford, respectively.[5] Until 2009, the United States was the world's largest car market, but a recession-led decline in U.S. sales and a parallel surge in Chinese purchases made China the world's largest single auto market. In 2008 alone, the U.S. industry shed 50% of its sales volume and slashed 400,000 jobs.

Despite declining sales, union contracts severely restricted manufacturers' ability to shut down production facilities (SIGTARP 2010), and state franchise laws curtailed them from terminating their contractual obligations with their downstream channel dealer distribution network (Lafontaine and Morton 2010). By the end of 2008, GM and Chrysler were unable to secure the day-to-day funding needed to remain in business. On December 19, 2008, President George W. Bush announced a plan to lend $17.4 billion from TARP to GM and Chrysler to prevent any near-term bankruptcy and to help them restructure as more viable and competitive companies over the longer term. GM and Chrysler were advised to use the terms of bankruptcy to quickly eliminate dealers from their dealer network, an action that the two manufacturers could not otherwise pursue, given state-level, dealer-leaning franchise laws. As per recommendations from the U.S. Treasury Department, both Chrysler and GM filed for Chapter 11 bankruptcy protection on April 30, 2009, and June 1, 2009, respectively. The move gave Chrysler the opportunity to immediately shut down 789 (or 25%) of its 3,181 dealerships.

Chrysler's project to consolidate dealerships to sell all three brands under one roof was a major driver when choosing which dealerships to close. The project was planned to be completed in 2014; but Chrysler expedited the schedule during the bankruptcy. According to the SIGTARP report,

> Chrysler used the following primary criteria to select dealerships to retain or terminate: whether the dealer's location was a desirable one targeted by Chrysler; which brands were offered; the number of new vehicle sales; and the Minimum Sales Responsibility ("MSR"). Chrysler also considered customer convenience, financial stability of the dealership's company, condition of the dealership's buildings and lots, and capacity of the facility's buildings and lots. (SIGTARP 2010, p. 21)

2.2. Data

The data we analyze come from three sources. Our primary new-car data set, which is provided by a major marketing research firm, includes dealer/VIN-specific monthly retail prices between October 2008 and September 2010. Apart from retail prices, the data set provides information about dealer characteristics such as the dealer's name and address along with vehicle characteristics in each dealer's inventory. Vehicle information that we observe include make, model, category (e.g., sedan, truck, etc.), and model year. Since each vehicle is associated with a unique VIN, we also have information about the restraint system (e.g., airbags, belts), passenger car identifier, body series (e.g., trim level, wheel base, number of doors, hardtop, body type, premium/sport), engine type (e.g., cylinders, displacement, turbo, fuel type), transmission (FWD, AWD, manual/automatic), and assembly plant.

[3] Since we focus on retail prices, our analysis cannot directly speak to either retailer or manufacturer profits or the long-run survival of bankruptcy-court-protected domestic manufacturers.

[4] The employment statistics are based on annual data from 2008 reported by the U.S. Department of Labor, Bureau of Labor Statistics, Quarterly Census of Employment and Wages program. It includes all employees who work at automobile dealers included in North American Industry Classification System category 4411 (this category covers new and used car dealers).

[5] Ward's, Motor Vehicle Facts and Figures, 2009.

Table 1 Descriptive Statistics

Variables	No. of Obs.	Mean	Std. dev.	Min	Max
Chrysler estimation data for the main effect of closings					
Price (in USD)	437,040	31,723	10,756	7,990	121,130
Time since listed (months)	437,040	3.475	2.803	1	32
Zip-code-level median household income (in USD)	433,037	44,842	9,714	21,254	90,148
Zip-code-level population	433,037	20,752	11,288	814	74,902
Number of closings within 30 miles	437,040	0.131	0.402	0	3
Categorical variables (percentage occurrence)					
Category: Convertible (0.25), Coupe (4.13), Hatchback (3.03), Sedan (30.23), SUV (21.20), Truck (36.83), Van (2.58), Wagon (1.75). Country/region of origin: Domestic (70.83; Chrysler 7.78), Asian (28.80), European (0.37).					
Chrysler estimation data for spatial heterogeneity in price effects					
Price (in USD)	252,375	31,384	10,594	7,990	98,540
Time since listed (months)	252,375	3.532	2.801	1	29
Zip-code-level median household income (in USD)	252,375	45,120	9,484	21,254	75,103
Zip-code-level population	252,375	19,270	10,882	814	74,902
Number of closings within the primary distance band	252,375	0.064	0.259	0	2
Number of closings within the secondary distance band	252,375	0.011	0.104	0	1
Number of closings within the tertiary distance band	252,375	0.047	0.234	0	2
Categorical variables (percentage occurrence)					
Category: Convertible (0.27), Coupe (5.38), Hatchback (1.54), Sedan (29.96), SUV (17.86), Truck (42.86), Wagon (2.14). Country/region of origin: Domestic (84.12), Asian (15.88).					
GM estimation data for the main effect of closings					
Price (in USD)	449,987	31,063	10,606	7,990	149,900
Time since listed (months)	449,987	3.628	2.879	1	32
Zip-code-level median household income (in USD)	449,987	46,320	10,494	24,819	81,489
Zip-code-level population	449,987	19,699	11,819	1,263	58,175
Number of closings within 30 miles	449,987	0.172	0.487	0	3
Categorical variables (percentage occurrence)					
Category: Convertible (0.24), Coupe (4.91), Hatchback (3.98), Sedan (27.31), SUV (20.55), Truck (37.37), Van (3.77), Wagon (1.86). Country/region of origin: Domestic (72.84; GM 29.96), Asian (27.16).					
GM estimation data for spatial heterogeneity in price effects					
Price (in USD)	226,147	32,141	10,431	8,340	103,075
Time since listed (months)	226,147	3.684	2.835	1	26
Zip-code-level median household income (in USD)	226,147	46,794	11,048	24,819	81,489
Zip-code-level population	226,147	19,097	11,928	1,263	58,175
Number of closings within the primary distance band	226,147	0.093	0.337	0	2
Number of closings within the secondary distance band	226,147	0.049	0.216	0	1
Number of closings within the tertiary distance band	226,147	0.043	0.244	0	3
Categorical variables (percentage occurrence)					
Category: Convertible (0.35), Coupe (4.82), Hatchback (3.61), Sedan (19.97), SUV (22.38), Truck (43.98), Van (4.41), Wagon (0.49). Country/region of origin: Domestic (85.32), Asian (14.68).					

Note that our unit of analysis is not the VIN itself but the limited VIN. This way we can control for most vehicle characteristics while at the same time pooling across all cars that share those characteristics.

We complement this primary data set by collecting information on the identities of dealerships slated for closure using the bankruptcy filings and press releases of Chrysler and GM. This second data set includes the name, address, and majority owner of each of the closed dealers as well as the franchises (i.e., brands) carried by each of them.[6] Using the addresses of the dealers in our two data sets, we generate precise latitude and longitude coordinates. Then we compute the distance in miles from each dealer's location to each of the closed Chrysler and GM dealers. Accordingly, we calculate the number of closings within different distance bands (i.e., 0–10 miles, 10–20 miles, and 20–30 miles). Table 1 provides the descriptive statistics for the key variables used in the estimation procedures as well as the observed car categories in our data set.

Finally, to control for the demographic characteristics (e.g., median household income and population) of local markets, we collected zip-code-level demographic information that varies annually from the SimplyMap database (http://geographicresearch.com/simplymap).

2.2.1. Car Definition and Selection. We define a car as every combination of the digits 1–8 and 10–11 of VINs.

[6] A comparison of exit and nonexit dealers of Chrysler across various observable characteristics is provided in Online Appendix 1 (available as supplemental material at http://dx.doi.org/10.1287/mksc.2015.0952).

For instance, for a given VIN = 1FDBP05FXBA100001, our car ID is 1FDBP05FBA. The reason for using these digits for defining cars is that each VIN is composed of several sections. The first three digits identify the manufacturer, country of origin, and type of vehicle. Digits 4 to 8 identify vehicle attributes such as engine type, vehicle line, drive type, restraint system, and body type. Digits 10 and 11 identify the model year and the assembly plant for that vehicle. As such, every combination of the digits 1–8 and 10–11 of VINs provides detailed information about vehicle characteristics. Our limited VIN-based definition of cars resulted in 4,121 different cars associated with 119 different models in our data set. These 119 models are among the top 150 car models by sales and account for more than 80% of sales of these 150 models. Note that using the common "make-model-model year" based car definition results in only 468 different cars. This suggests that the limited VIN-based fixed effects provide substantially stronger controls for various car characteristics. Our final sample comprises a total of 437,040 observations, which approximately corresponds to 248 dealers per month, 73 vehicles per dealer/month over a 24-month period.

2.2.2. Retail Market Definition. We use a fixed mileage (i.e., 30 miles) distance radius to define the *local* retail markets (see, for example, Ailawadi et al. 2010 and Davis 2005 for a similar approach). In other words, a specific dealer is assumed to be "treated" by a local market exit if there is a closed dealer within its retail market. This definition aims at capturing the effect of market exits on dealers that are directly competing with closed dealers.[7] We do not expect significant changes to our results if we enlarge the retail market coverage because Albuquerque and Bronnenberg (2012) show that the effect of distance from dealers on consumer utility is marginally decreasing. In other words, most of the utility reduction happens within short distances. Thus, they find that cross-price elasticities are negligible when the distance between two dealers is more than 30 miles. Although we use the 30-mile radius to define the market, as we note above, we look for differences across various distance bands, for a fixed set of car models, within that radius to look at agglomeration versus competition effects.

Based on the above mentioned local market definition, we eliminate dealers that are within the local market of (i) an inconsistently observed closed Chrysler dealer, (ii) a consistently observed closed Chrysler dealer that switches to another brand after the closing, and (iii) a GM dealer that has been slated for closure. In addition, some of the closed Chrysler dealerships continued to carry other brands (e.g., Toyota). Although these dealerships appear as incumbent dealerships, they cannot be designated as rivals to the closed Chrysler dealerships. As our analysis focuses on rival responses, we excluded such dealerships from our analysis. From the remaining list of dealers, we dropped those with very few observations (e.g., less than two cars per month) to have a sample of 385 dealers, 69 of which are surviving Chrysler dealerships. Out of these dealers, 60 (14 of which are Chrysler dealerships) are located in the markets affected by a Chrysler exit ("treated" dealers).[8] The locations of the treated and control dealers in our analysis are shown in Figure 1. It is important to note that the closures took place within a small time interval. More precisely, around 94% of the Chrysler exits within the local market of these affected dealers happened between June and July 2009. There is only one exit that occurred in August 2009.

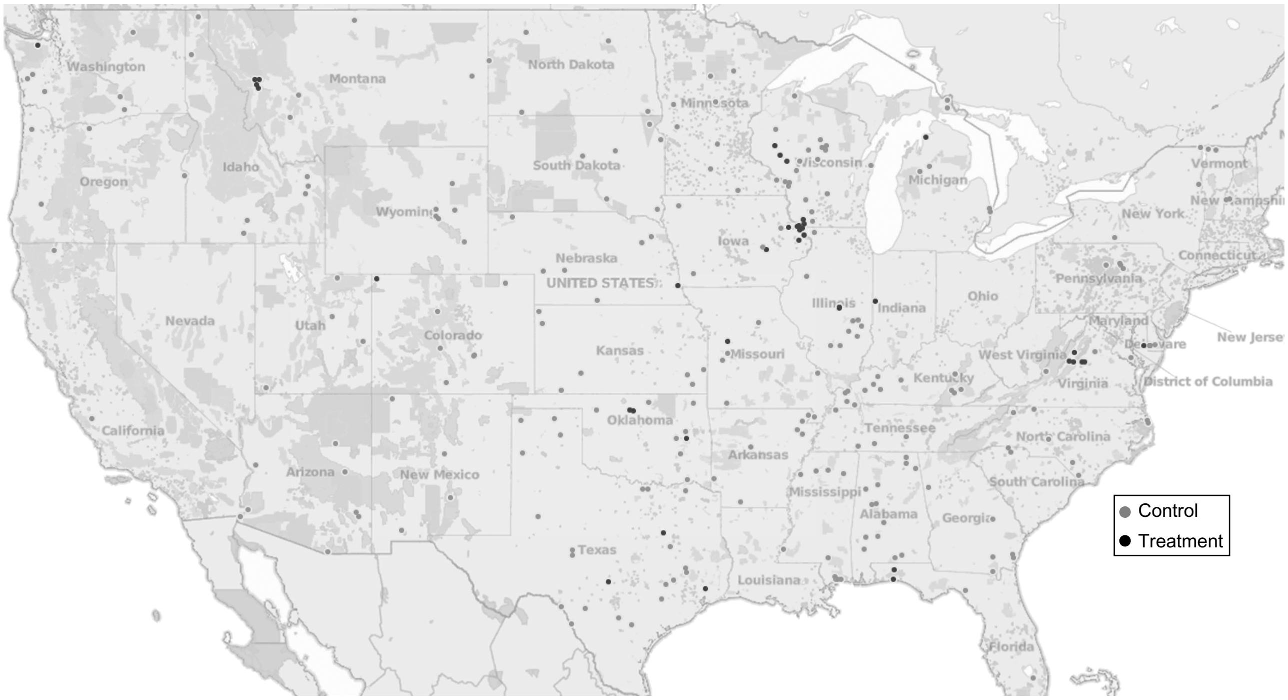
A preliminary comparison of the dealers that are affected by Chrysler closures with those that are not (i.e., control dealers) in the periods *before* Chrysler closings suggests that these two groups are relatively similar on various observed characteristics (see Online Appendix 1 for the comparison statistics). More precisely, the affected and control groups are comparable in terms of population, average household size, economic stability, and percent urban and female population in the pre-exit period.[9] In addition, these dealers have a similar number of makes, models, prices, and categories on average. On the other hand, dealers with Chrysler exits are in markets with higher income levels, greater Caucasian population share, and higher numbers of local competitors. Possibly as a result of

[7] In §4, we conduct a more detailed analysis by breaking up the local market into three distance bands.

[8] A comparison of the dealerships we finally included in our sample and those we dropped from our sample (because they met conditions (i) through (iii)) is provided in Online Appendix 1. The difference between included and dropped dealers is not economically and statistically significant in terms of stability, number of vacant units, percent female population, or average prices. The difference between the two groups is statistically significant but economically modest in terms of average household size, average number of makes, average number of models, and average number of categories. The dropped dealers are different from the included dealers in that they operate in more urban markets with higher populations, median household incomes, and average number of cars sold. This is not unexpected, given the criteria we used to ensure clean identification. More precisely, we dropped dealers that are within the local market of (i) an inconsistently observed closed Chrysler dealer, (ii) a consistently observed closed Chrysler dealer that switches to another brand after the closing, and (iii) a GM dealer that has been slated for closure. Essentially, the third criterion means that affected dealers that are dropped experienced both Chrysler and GM closings in their market. In other words, these markets should have had a larger population to sustain both Chrysler and GM dealerships to start with compared to the markets where included dealers operate.

[9] We report the p-values of the Wilcoxon two-sample test along with the t-test for group differences because the box plots for the variables (provided in Online Appendix 1) exhibit outliers, thus casting doubt on the distributional assumption of the t-test.

Figure 1 **Locations of Treatment and Control Dealers**



being in markets with higher demand potentials, their monthly average sales are higher than those of control dealers. Using only the preclosure period monthly time-series data, we regressed log prices at the car-month-dealer level on the treatment dummy (for all those dealerships that subsequently saw an exit) along with all of the observables that we subsequently use in our analysis such as population and income at the zip-code level, number of local competitors, etc., and identified unobservable factors reflected in various fixed effects (discussed later). Our recovered coefficient for the treatment dummy is not significant at the 10% significance level (p-value of 0.37). This regression result supports the idea that the treatment and control group dealers are similar in the pretreatment period after controlling for observables.[10]

Another issue related to the comparability of treatment and control dealers is the similarity in terms of local market time trends. Our subsequent analysis explicitly accounts for such trends at the designated market area (DMA) (where the dealership is located) level via DMA-month fixed effects. However, to assess whether more micro trends exist, we look at data for zip codes in which treatment and control group dealers are located. Specifically, we report the change in population, employment, household income, number of vacant units, and stability (% in current residence 5+ years) between 2008 and 2009 in Table 2. The changes in these demographic variables, except for median household income, show no significant difference between the two groups. Since the data show that household income increases more in zip codes where the treated dealers are located, an increase in price (if any) associated with cars at these dealerships could be explained by the concomitant increase in incomes. Consequently, in our subsequent analysis we (i) include this as a control variable, and (ii) include a dealer-specific time trend in the analysis. In addition, we look at changes in the number of gas stations and number of electronics and appliance stores in Table 2 to see if there is any differential trend for businesses related to cars and durable goods. Again the comparison suggests that the local time trends are not significantly different between control and treatment groups.[11]

3. Empirical Framework and Identification Strategy

In this section, we assess the effect of local market exits on retail prices. Specifically, we estimate the impact of the number of Chrysler dealer closings on the retail price of a given car (as defined previously). Our empirical strategy is closest in spirit to Ailawadi et al. (2010), who examine the effect of Wal-Mart entry on incumbent retailers' marketing mix reactions and sales outcomes.

Ideally, given a set of markets with Chrysler dealerships, if we could randomly assign a subset of markets

[10] We also provide several graphical analyses that show examples of treatment and control group similarity before treatment as well as differential price changes between the two groups after the treatment in Online Appendix 2.

[11] We thank an anonymous reviewer for the suggestion.

Table 2 Local Market Trends: Treatment versus Control Dealer Comparison

	Control		Treatment		Differences	
Change between 2008 and 2009	Mean	Std. dev.	Mean	Std. dev.	Control mean minus treatment mean	Wilcoxon two-sample test p-value
Population	−28.328	2,003	61,513	486	−89.840	0.479
Total civilian employment	−266.1	908.4	−229.2	260.1	−36.943	0.778
% median household income	3.585	9.242	5.851	6.520	−2.266*	0.083
Number of vacant units	−20.127	185.6	-29.333	74.470	9.207	0.115
Stability (% in current residence 5+ years)	−0.012	0.444	-0.080	0.814	0.068	0.601
Number of gas stations	0.061	1.844	0.256	1.094	−0.195	0.273
Number of electronics and appliance stores	−0.096	1.108	-0.205	0.923	0.109	0.275

Note. Demographic variables show zip-code-level census data with $N(\text{control}) = 229$ and $N(\text{treatment}) = 39$.
$^{*}p < 0.01$.

to a "treatment" condition, i.e., the exit of the Chrysler dealership, and the remaining to a "control" condition, i.e., no dealer exit, we could then look at the "difference-in-differences" for prices across the treatment and control groups before and after the Chrysler exit. As Chrysler likely chooses the treatment strategically rather than randomly, unobserved factors that are correlated with the decision to shut down a Chrysler dealership might also be correlated with, say, Toyota's prices; so a simple comparison of prices across rival dealers that do and do not face an exit may not suffice. Furthermore, one might be concerned about exits that occur for reasons other than the bankruptcy filing. Two key institutional features help alleviate these concerns. First, unlike other industry settings where exits are permitted and frequently occurring, federal and state franchise regulations limit the ability of automobile manufacturers to terminate their franchised dealerships at will other than via Chapter 11 bankruptcy court protection (Lafontaine and Morton 2010). Therefore, apart from the variation in dealer exits introduced by Chrysler's dealer network consolidation event, there is limited additional variation in dealer exits in our setting.[12] Second, although the number of terminated Chrysler dealerships varies across geographic markets, these dealership closings happened (i.e., treatment occurred) largely in the (short) June–July 2009 time frame across all geographic markets. Note that once a dealership closes, it remains shut—so the treatment effect on incumbent dealerships is in force from that month on. Hence, much of the exit variation in our setting is cross-sectional (i.e., varies across geographic markets). Moreover, as we discussed in §2.1, the selection criteria for Chrysler closings are dealer-specific characteristics. Our panel data, therefore, allow us to account for strategic selection of the exiting dealer and for other dealer-specific unobservables, via the inclusion of dealer fixed effects.[13]

The next concern is regarding the market where the exit takes place. In certain states, e.g., Connecticut, Georgia, Michigan, and New York, state franchise laws (enacted decades before Chrysler's decision to drop dealers) strongly favor automobile dealers at the expense of automobile manufacturers (Smith 1982). Only under bankruptcy court protection can Chrysler override current state franchise laws, a flexibility not afforded to other nonbankrupt manufacturers. So dealers in certain states (e.g., "monopoly" states) are more likely to face termination than others.[14] Since a dealership can only belong to a particular local geographic market (DMA, e.g., Detroit), our included dealer fixed effects will also account for these geographic differences. Third, since our analysis is at the car level, differences in dealer reactions for different cars need to be accounted for. For this, we include car (as previously defined) fixed effects.

Next, given the temporal nature of our data, it is important to control for various trends in prices that might exist (see Kalnins 2004 and Busse et al. 2006, who discuss the importance of controlling for trends). We include model/month-year (e.g., Toyota Camry/July 2009) and DMA/month-year (e.g., Detroit/July 2009) fixed effects in the estimation. Including these fixed effects helps us control for unobserved factors like manufacturer incentives, advertising, and local gas-price shocks. For example, demand conditions for cars

[12] As we discussed earlier, we exclude markets with GM dealer exits.

[13] Since the timing of exits is the same (up to being either in June or July) across all treated dealerships, the issue of strategic timing of exits across dealerships is moot; the absence of a time-varying component of the potentially endogenous exit decision obviates the concern of any residual endogeneity and the need for instruments. We address the June versus July exit issue in §3.3.

[14] Model-free evidence does lend support to our ex ante belief about "monopoly" states. Following Smith (1982), we define a monopoly state as a state where the following policies are in place: (i) a manufacturer cannot force dealers to accept unordered vehicles, (ii) franchises cannot be canceled by manufacturers without cause, and (iii) there is a restriction on the entry of new franchises within the exclusive territory of an existing dealer. Only 54% of our dealers who do not face a Chrysler exit are located in monopoly states, as compared to 85% of the dealers who do experience a local market Chrysler exit.

may change as a result of increasing DMA-level gas prices, which in turn might affect the pricing policy of the dealers. DMA/month-year fixed effects also control for differential trends across markets; this effect is identified separately from the treatment effect since there are multiple dealerships in a given geographic area. Furthermore, if one is concerned about trends more local to a dealership area (e.g., prices in certain neighborhoods of Detroit change over time differentially from other neighborhoods), we control for monthly dealer time trends as well as zip-code-level demographics like household income that might change over time (see Table 2).[15] Our identifying assumption is that conditional on these controls, dealership closures represent an exogenous shock to market structure and consequently, dealership exits are orthogonal to any residual unobservables in prices. To allay any further concerns regarding endogeneity, we conduct a "falsification" test; we run our specifications to see whether there is a "treatment" group effect in a period when there is no closing (i.e., before June 2009). If we were to find a significant impact of this "placebo treatment" on the outcomes of interest, this would suggest that unobservable differences that are correlated with Chrysler's closings are contributing to our estimated effects.

To see if both the competitive and agglomeration effects are at work, we then look at how prices for a given car (or a set of cars) are affected as the dealership(s) selling that car(s) is (are) located farther away from the exiting Chrysler dealer—within 10 miles, between 10 and 20 miles, and between 20 and 30 miles. The idea behind such a comparison is that agglomeration effects and competitive effects change differentially as one moves farther away from the focal dealership. So a change in relative effects with distance will be reflected in the prices of the same set of cars across these distance bands. Furthermore, by focusing on the same car (or set of cars) across the various distance bands, we alleviate the concern that different car brands might have located differently relative to one another and the Chrysler dealership (within the 30-mile radius) when opening their dealerships.

3.1. The Price Model

To evaluate the influence of Chrysler exits on car prices, we regress the natural logarithm of the retail price for vehicle v at month-year t on the number of Chrysler closings within 30 miles of the dealer carrying that vehicle ($NClos_{vt}$), with the variable taking the value 0 when there is no exit, a vector of dummies indicating the dealer that carries the vehicle (I_{vt}^{dealer}), a vector of dummies indicating the limited VIN (i.e., specific car type) for the vehicle (I_{vt}^{LimVIN}), a vector of dummies denoting car model/month-year-specific fixed effects ($I_{vt}^{model/t}$), a vector of dummies denoting DMA/month-year-specific fixed effects ($I_{vt}^{DMA/t}$), the number of months since the vehicle was first listed ($TSListed_{vt}$), the quadratic term for the number of months since the vehicle was first listed ($TSListed_{vt}^2$), zip-code-level median household income ($ZIP\ Inc_{vt}$), and population ($ZIP\ Pop_{vt}$)

$$\begin{aligned} \ln(Price_{vt}) &= \kappa + \alpha\, NClos_{vt} + \Theta_d' I_{vt}^{dealer} + \Theta_{LimVIN}'\, I_{vt}^{LimVIN} \\ &\quad + \Theta_{model/t}' I_{vt}^{model/t} + \Theta_{DMA/t}' I_{vt}^{DMA/t} + \lambda_1\, TSListed_{vt} \\ &\quad + \lambda_2 TSListed_{vt}^2 + \zeta_1 ZIP\ Inc_{vt} + \zeta_2 ZIP\ Pop_{vt} + \varepsilon_{vt}. \end{aligned} \tag{1}$$

The coefficient of interest in this specification is α, which represents the average effect of a one-unit change in Chrysler closings on retail prices.[16] In other words, for a one-dealership increase in the number of closings, we expect to see a $100 \times [\exp(\alpha) - 1]\%$ increase in retail price while all other variables in the model are held constant. Note that if α is small (e.g., less than 0.2, which will be the case in our estimations), one can say that a one-unit increase in the number of closings will result in a $100 \times \alpha\%$ increase in retail price. Dealer fixed effects (Θ_d) allow us to control for time-invariant dealer characteristics such as location, size, and chain affiliation. In addition, since the number of local competitors is very stable during the span of our data set, dealer fixed effects effectively control for local market structure except for the Chrysler closings (i.e., dealer fixed effects account for 99% of the variation of the number of local competitors excluding exited Chrysler dealers).[17]

Limited VIN fixed effects (Θ_{LimVIN}), on the other hand, control for car-specific factors such as manufacturer, brand, model, model year, body type, and trim level, among others. Model/month-year ($\Theta_{model/t}$) and DMA/month-year ($\Theta_{DMA/t}$) fixed effects permit us to control for demand conditions (e.g., popularity of the model, gas prices, etc.), and seasonality, as well as month-year-varying model- or DMA-level marketing activity (for example, manufacturer promotions

[15] We thank K. Sudhir for this suggestion. Note that we cannot include dealer/month-year fixed effects since that is the level of variation of the treatment in our data.

[16] Note that the dummy variables and the interaction of them in a usual difference-in-differences specification are subsumed in the various fixed effects included in Equation (1). Specifically, the treatment dummy in a usual difference-in-differences specification is controlled for by our dealer fixed effects, and the after-period dummy is accounted for by our DMA/month-year fixed effects. The interaction of the treatment dummy and the after-period dummy is captured by our number-of-closings variable. Thus, the coefficient of the number-of-closings variable (α) shows the difference-in-differences estimate.

[17] In §3.3.3, we check for robustness to the inclusion of the number of local competitors explicitly in the analysis.

Table 3 Fixed-Effects Estimation: The Impact of Chrysler Closings on ln(*Price*)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of closings within 30 miles	−0.581***	−0.558***	−0.420***	1.430***	1.404***	1.037***	1.083***	1.006***
	(0.214)	(0.175)	(0.105)	(0.201)	(0.201)	(0.206)	(0.093)	(0.093)
Time since listed	—	—	—	—	−0.683***	−0.686***	−0.231***	−0.230***
					(0.020)	(0.021)	(0.010)	(0.010)
Time since listed^2	—	—	—	—	0.026***	0.029***	0.007***	0.007***
					(0.002)	(0.002)	(0.001)	(0.001)
Zip-code-level median household income	—	—	—	—	—	—	—	0.144***
								(0.015)
Zip-code-level population	—	—	—	—	—	—	—	0.412***
								(0.066)
Category fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
DMA/month-year fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes
Model/month-year fixed effects	No	No	No	No	No	Yes	Yes	Yes
Car fixed effects	No	No	No	No	No	No	Yes	Yes
Adjusted R^2	0.292	0.526	0.831	0.834	0.834	0.837	0.968	0.968

Notes. All estimations include dealer, brand, and month-year fixed effects unless already subsumed by finer fixed effects. Estimations for columns (1)–(7) are based on 437,040 observations across 119 car models. The estimation for column (8) uses 433,037 observations since median household income and population data are not available for some zip codes. All coefficients and standard errors are scaled up by a factor of 100. Standard errors are given in parentheses. Similar estimations with robust standard errors did not change the results given the very large number of observations used in the estimations.

***$p < 0.01$.

to dealers and consumers[18]). We include the number of months since the vehicle was first listed (λ_1) to take into account the pressure for the dealer to sell the vehicle. We also include its quadratic term (λ_2) to be able to capture a possible curvilinear effect of the number-of-months-since-listed variable on prices. For example, a significant and positive coefficient for the quadratic term will indicate that the effect of time-since-listed on retail prices is convex.

As we discussed in §2.2.2, there was a differential change between treatment and control groups in terms of zip-code-level median household income between 2008 and 2009. To account for such local time-varying demand factors, we control for zip-code-level median household income (ζ_1) and population (ζ_2), which vary annually.

3.2. Fixed-Effects Estimation Results

Table 3 presents results from eight different fixed-effects specifications where we estimate the effect of dealer closings on car prices. All eight specifications include dealer, brand, and month-year fixed effects unless already subsumed by finer fixed effects. Column (1) provides the results from a specification where we only control for dealer, brand, and month-year fixed effects. We find that the average effect of a Chrysler dealer closing is significantly negative (−0.581%, i.e., −$184). However, column (1) also suggests that only 29% of the variation in log prices is explained by the current specification. In other words, one might worry about potential omitted factors that determine the log prices of the cars and that may or may not be correlated with our focal closing variable.

Column (2) presents results from the specification where we control for category-specific factors in addition to the previous controls. The average effect of a closing is a significant reduction in prices (−0.558%, i.e., −$177). Still, only around 53% of the variation of log prices is accounted for.

Columns (3) through (7) control for additional fixed effects, including model, DMA/month-year, model/month-year, and car fixed effects, as well as the number of months since the vehicle was first listed. The results show that as we control for more factors, the average effect of closings becomes significantly positive and then stays positive, despite some attenuation of the effect. The inclusion of DMA/month-year fixed effects in column (4) changes the closing coefficient from negative to positive (1.43%, i.e., $454). This can be explained by the fact that a possible DMA-level decline in the demand for cars that is omitted in column (3) could lead to a downward bias on the closing coefficient. This might be the case because a decrease in the DMA-level car demand is expected to be positively correlated with competitor closings and negatively correlated with incumbent prices. Therefore, the closing coefficient in column (3) partially reflects the omitted effect of

[18] Previous literature on manufacturer promotions in the automobile industry suggests that manufacturers usually make dealer and consumer promotion decisions by nameplate, model, and model year (Busse et al. 2006). We include model/month-year-level fixed effects, which will control for the effect of promotions and other unobservables, including advertising. We also estimated our main specification using model/model year/month-year fixed effects instead of model/month-year fixed effects. The resulting coefficient for the number-of-closings variable did not change.

DMA-level demand shocks. When we further add the time-since-listed variables in our model (specification 5), we find a significant curvilinear effect of pressure to sell the vehicle (despite a negligible change in the explanatory power of the model). On the other hand, the inclusion of model/month-year (specification 6) and car fixed effects (specification 7) suggests an upward bias on the closing coefficient. This is expected because model/month-year and car fixed effects mainly control for month-year-varying model popularity and time-invariant product quality, respectively. The popularity and quality of a car sold by an incumbent is positively correlated with competitor closings (due to tougher competition) and with price. Thus the omission of these factors will lead to an upward bias on the closing coefficient, as the positive effect of better incumbent product quality on price will incorrectly be attributed to competitor closings.

Similarly, our final fixed effect specification shown in column (8) suggests that the omission of zip-code-level median household income and population results in an upward bias on the closing coefficient. This is because we observe larger increases in household income and population in zip-codes where the treated dealers are located, as we discussed in §2.2.2. Accordingly, when we omit these variables, their potentially positive effect on prices is attributed to competitor closings. Thus, our final specification shows that, holding other relevant factors constant, incumbent dealers increase their prices, on average, by 1.006% in response to a local Chrysler exit.[19] This average effect amounts to an increase of $318 for an average car. This final specification explains 97% of the variation in log prices, which leaves little room for any omitted variables.[20]

3.3. Robustness Checks

3.3.1. Narrow Temporal Windows Around Closings. The difference-in-differences approach we took in our fixed-effects estimation uses the prices of cars that are not affected by dealer closings as the counterfactual prices for cars that are "treated" by dealer exits. In addition, that approach exploits the entire duration of data (October 2008–September 2010) for estimating the average price effect of closings. Although we include a detailed set of time-varying fixed effects, one concern with this approach is potential unobservable local market events that might happen in the analysis periods. One way to see if such unobservables could change our previous result, i.e., a modest positive effect of closings, is to examine the effects using narrow temporal windows around closings (see Busse et al. 2006 for a similar analysis).[21] The idea behind that approach is that as the analysis window around the focal event gets shorter, the possibility of other events affecting prices is lower. Accordingly, we analyze only observations within a specific temporal window (e.g., ±1 month, ±2 months, etc.) around the closing events. The underlying assumption here is that the only discontinuous change that affects prices of the incumbent dealer is the exit of a Chrysler dealership. In addition, analyzing temporally local treatment effects helps us answer the question of whether our price-reaction results are short lived. The effects of closings across various narrow temporal windows are reported in Table 4. They show that the effect of dealer closings on prices is positive and significant for all of the windows we consider. So it is unlikely that an unobserved event was the main reason behind our dealer closings effect.

3.3.2. Differential Trends Across Dealers. We already included zip-code-level median household income and population that vary annually in our main specification to account for local time trends. To further check concerns regarding more granular time trends local to a dealership area (such as the different areas of Detroit example that we gave before), we estimate another specification where we include dealer/monthly trend interaction terms to our main specification. Trend interactions are significant for only eight of our analysis dealers. This indicates that dealer and DMA/month-year fixed effects already capture much of the variation. Accordingly, we reran the model with only significant dealer/monthly trend interactions and report the results in column (1) of Table 5. The results show that the treatment effect is stable (0.961%, i.e., $304), which

[19] We also estimated the specification shown in column (8) using a categorical (instead of continuous) measure for the number-of-closings variable. The percentage of observations for the four categories we observe for the number-of-closings variable is as follows: no exit −89%, one exit −9.08%, two exits −1.69%, and three exits −0.24%. Using the no-exit category as the base category, the coefficients associated with the three other category dummies, which are significantly different from zero, are 0.76%, i.e., $241 (one exit); 2.91%, i.e., $921 (two exits); and 2.69%, i.e., 851 (three exits). These results suggest that the marginal effect of an exit for the one-exit and two-exit cases (which collectively represent 98% of the exit observations) are 0.76% ($241) and 1.45% ($460), respectively. As the marginal effect of 1.006% ($318) based on the continuous measure of the number-of-closings variable summarizes these estimates accurately, the estimation with the categorical closing variable does not improve the model fit despite estimating more parameters. Thus, we proceed with the continuous measure of the number-of-closings variable in the rest of the paper.

[20] We also checked whether there is any evidence of endogeneity of the exit-timing decision despite the short period of closings. For the vast majority (87%) of treated dealers, the exits happened either in July, or both in June and July (i.e., some Chrysler dealers exited a local market in June and others exited the same market in July). So we estimated the full specification with all controls using only those markets with June–July (July only) exits and controls. The average closing effect was found to be 1.22%, i.e., $386 (1.50%, i.e., $474). Thus, there is no substantial evidence of exit-timing endogeneity. We thank Yeşim Orhun for this suggestion.

[21] We thank an anonymous reviewer for the suggestion.

Table 4 Narrow Temporal Windows Around Closings

Variable	One-month window	Two-month window	Three-month window	Five-month window	Seven-month window
Number of closings within 30 *miles*	0.483*	0.698***	0.634***	0.792***	1.110***
	(0.289)	(0.211)	(0.180)	(0.143)	(0.123)
Time since listed	−0.302***	−0.317***	−0.331***	−0.344***	−0.331***
	(0.030)	(0.024)	(0.021)	(0.017)	(0.014)
Time since listed^2	−0.001	0.002	0.005***	0.009***	0.010***
	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Zip-code-level median household income	n/a	n/a	n/a	0.279***	0.174***
				(0.028)	(0.017)
Zip-code-level population	n/a	n/a	n/a	0.239	0.772***
				(0.199)	(0.092)
DMA/month-year fixed effects	Yes	Yes	Yes	Yes	Yes
Model/month-year fixed effects	Yes	Yes	Yes	Yes	Yes
Car fixed effects	Yes	Yes	Yes	Yes	Yes
Dealer fixed effects	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.971	0.970	0.970	0.969	0.968
Number of observations	74,295	105,287	136,784	205,929	274,982

Notes. All coefficients and standard errors are scaled up by a factor of 100. Standard errors are given in parentheses. Zip-code-level median household income and population vary annually, so they are not identified in the first three columns where we have only observations in 2009.
$^{*}p < 0.10$; $^{***}p < 0.01$.

is consistent with the fact that 97% of the variation in prices is accounted for using our full fixed effects specification.

3.3.3. Number of Local Competitors. Another potential issue is that the number of local competitors may confound the estimates. For instance, if Chrysler closed dealerships where the number of local competitors is high and we do not control for its effect, this could lead to a downward bias on the closing coefficient. This is because we expect that the number of local competitors is negatively correlated with incumbent prices. Since DMAs could span a market area much greater than the 30-mile radius around a focal dealer, our current DMA-month fixed effects specification does not completely control for the number of local competitors. To see if our results are robust to the inclusion of the number of local car dealerships in a 30-mile radius of a focal dealer, we collected additional monthly data from R. L. Polk for the census of local competitors.[22] Column (2) in Table 5 reports the results of this robustness check. When we control for the number of local competitors, we find that the main effect goes up slightly to 1.019%, or \$322 (specification 2 in Table 5), from 1.006%, or \$318 (specification 8 in Table 3), showing that the coefficient for the number-of-closings variable is robust. In addition, the coefficient for the number of local competitors is insignificant. This is because (as we stated previously) there is not much temporal variation in this measure. Most of the variation in this measure is cross-sectional and therefore its effect is already subsumed in the dealer fixed effects we include in our main specification.

Table 5 Robustness Checks

Variable	(1) Trend interactions	(2) Number of local competitors	(3) Falsification
Number of closings within 30 miles	0.961***	1.019***	0.318
	(0.093)	(0.094)	(0.206)
Time since listed	−0.225***	−0.230***	−0.429***
	(0.010)	(0.010)	(0.025)
Time since listed^2	0.007***	0.007***	0.012***
	(0.001)	(0.001)	(0.002)
Zip-code-level median household income	0.151***	0.145***	0.193***
	(0.015)	(0.015)	(0.020)
Zip-code-level population	0.449***	0.415***	0.892***
	(0.066)	(0.066)	(0.126)
Number of local competitors	—	−0.031	0.140
		(0.033)	(0.134)
Adjusted R^2	0.968	0.968	0.967

Notes. All coefficients and standard errors are scaled up by a factor of 100. The dependent variable in all estimations is ln(Price). Estimations for columns (1) and (2) use 433,037 observations. The estimation in column (3) is based only on preclosings period (135,424 observations) to run a falsification test with a placebo treatment variable. Standard errors are given in parentheses. Similar estimations with robust standard errors did not change the results given the very large number of observations used in the estimations. All estimations include dealer, car, DMA/month-year, and model/month-year fixed effects. Specification in column (1) additionally includes significant dealer/monthly trend interactions.
$^{***}p < 0.01$.

3.3.4. Falsification Exercise. To allay any remaining concerns related to endogeneity, we run a "falsification" test. We estimate our final fixed-effects specification above to see whether there is a "treatment" group effect, i.e., placebo effect, in a period when there is no closing (i.e., before June 2009). To do this, we use all of the pre-exit data and divide that into two halves, namely, before and after the placebo treatment. Then we test whether the placebo-closing effect is significant.

[22] We thank the associate editor for the suggestion.

Note that if we were to find a significant impact of the "closing" variable on prices in a period when no closing happens, this would suggest that unobservable differences that are correlated with Chrysler closings are contributing to our estimated price effects. The result from the falsification exercise in Table 5 column (3) shows that the estimate for the placebo-closing variable is insignificant. In other words, the pricing effects are not driven by the correlation of closing variables with some unobserved shocks that are not controlled for in our final specification. Although in general, one might attribute such a result to the smaller sample size, it is not an issue here because the error degrees of freedom is 131,588. Thus, these results further bolster the argument that the price effects of closings are true closing effects.

The falsification test discussed above also tests the existence of differential trends between treatment and control groups in the pretreatment period. As a result, the failure to find a significant coefficient for the placebo-closing variable suggests that the preexisting trends are not different enough to bias our treatment effect. We also check if there are differential price trends for control and treatment groups by using the pretreatment period data and testing an interaction term between a monthly time trend and a dummy variable for treated units. We find that the estimated interaction coefficient is very small (0.026% with a p-value of 0.55), supporting the results from the falsification exercise.

4. Spatial Heterogeneity in Price Effects

The average local market exit effect we find in §3 is considerably smaller than the 10% increase that earlier studies looking at the airline industry have found (Joskow et al. 1994, Daraban and Fournier 2008). One possible explanation for the small average price increase could be the magnitude of potential externalities among dealers compared to those among airlines. Specifically, since the spatial structure of the auto retail market is different from the airline market, the small effect we find might be a result of averaging among competitors with less spatial differentiation (those within 10 miles of a closing dealer) and more distant competitors (those within a 20–30-mile distance band from a closing dealer). For example, one could expect that competitors with less spatial differentiation increase their prices more in response to Chrysler exits than distant competitors. However, if agglomeration benefits erode much faster than gains from spatial spillovers, then we might find the opposite. Accordingly, in this section, we advance empirical evidence that both the "market-power hypothesis" and "agglomeration-benefits hypothesis" are indeed at work in our empirical context.[23] To demonstrate this, we estimate a specification where we count the number of closings within three separate distance bands, namely, 0–10 miles, 10–20 miles, and 20–30 miles, representing various levels of spatial differentiation.[24] If firms colocate around their more substitutable competitors, then firms that are nearest to the closed dealership are most affected by the competition effect. Intuition suggests that firms in the 0–10 miles band are likely to raise prices the most, with the effect diminishing as we move away from the focal dealership.

On the other hand, if agglomeration benefits erode as a result of an exit, then the dealerships located nearest to the closed dealership suffer the most and need to trade off lower prices to compensate for the increased consumer search costs. As we move farther away from the closed dealership, those other dealerships do not benefit from the lower search costs as much in the first place, so they feel less need to keep prices low to attract consumers. In the absence of agglomeration, only the market power hypothesis will be at work.

What we will see in the data at different distance bands is the net effect of the two forces. Since the rates at which these two forces erode (as we move farther away from the closed dealership) could be different, the net effect may not be a monotonic relationship in distance. Specifically, if going from the first band to the second band agglomeration benefits diminish faster than gains from reduced competition and if going from the second band to the third band the opposite happens, then the net effect will be an inverted-U shaped function of the distance from the closed dealership. As a result, which pattern exists in our data is an empirical question.

To alleviate the concern noted earlier that different cars may be sold in different distance bands, we use a subset of our original data set that involves a set of car models that are available across all three distance bands to investigate agglomeration effects. In addition, because we do not have a sufficient number of surviving Chrysler dealers across the three bands, we drop Chrysler dealers from the distance-bands analysis. This data set contains 252,375 observations across 44 car models. The price of an average vehicle is $31,384. The majority of observations are associated with domestic cars (84.12%), whereas the rest of the observations come from Asian cars (15.88%). The car categories with the highest number of observations are truck (42.86%), sedan (29.96%), and sport utility (17.86%). Additional details about this data set are provided in Table 1. The average increase in car prices across the three distance bands is $562.

[23] We also investigated other dimensions of heterogeneity such as closed Chrysler dealership size, car popularity, and inventory costs. They indicated small effects on prices (see Online Appendix 3 for details).

[24] Davis (2006) and Zhu and Singh (2009) use similar parameterizations to assess the distance-varying impact of local market structure changes.

Table 6 Spatial Heterogeneity in Price Effects—Competing Brands

Variable	(1) Chrysler closings		(2) GM closings	
Number of closings within the 0–10 miles band	0.845***	(0.263)	1.229***	(0.124)
Number of closings within the 10–20 miles band	3.295***	(0.581)	1.836***	(0.195)
Number of closings within the 20–30 miles band	2.485***	(0.252)	1.394***	(0.205)
Time since listed	−0.266***	(0.014)	−0.252***	(0.017)
Time since listed^2	0.010***	(0.001)	0.008***	(0.001)
Zip-code-level median household income	0.112***	(0.024)	−0.030	(0.023)
Zip-code-level population	−0.169*	(0.100)	−0.549***	(0.071)
Number of local competitors	−0.062	(0.053)	−0.148**	(0.062)
Adjusted R^2	0.965		0.951	

Notes. All coefficients and standard errors are scaled up by a factor of 100. The dependent variable in all estimations is ln(Price). Estimations for column (1) are based on 252,375 observations. Estimations for column (2) are based on 226,147 observations. Similar estimations with robust standard errors did not change the results given the very large number of observations used in the estimations. All estimations include dealer, car, DMA/month-year, and model/month-year fixed effects as well as significant dealer/monthly trend interactions.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

The results using the same set of cars across the three distance bands are reported in column (1) of Table 6. We find that, holding other relevant factors constant, the average increase in prices due to a local market exit in the primary distance band (0–10 miles) is 0.85% ($265), in the secondary distance band (10–20 miles) is 3.30% ($1,034), and in the tertiary distance band (20–30 miles) is 2.49% ($780). These findings are in contrast with the intuition that incumbent dealers that are geographically closer to the exiting Chrysler dealers would benefit more from the lower competition due to exits, and as a result increase their prices more than their distant surviving peers. However, our results are consistent with price reactions in an environment where demand-side agglomeration benefits erode via exits. According to this explanation, proximal dealers might have to cut their prices to offset lost agglomeration efficiencies. Moreover, the lost agglomeration benefits will affect proximate dealers the most, which is in line with the smallest price increase for incumbents in the primary distance band. Our results, therefore, reveal a more nuanced effect of market exits than predicted by either the competition effect or the agglomeration effect on its own.

5. GM Closings

To test the generalizability of our recovered price reaction effects, we investigate, in this section, the price reactions of surviving dealerships to GM's bankruptcy-protected local-channel exits.

5.1. Data

Our data set for the GM analysis includes dealer/VIN-specific monthly retail prices between October 2008 and September 2010. As GM closed its dealerships in a phased-out manner, there is the concern that the timing of the closures may be endogenous. To alleviate such endogeneity concerns, we use a subset of GM exits that happened in a relatively short time interval. Because there are a sufficient number of closings in a small time interval for Pontiac dealership closings, we analyze Pontiac exits between October 2009 and January 2010.

Following the same procedure we used for our Chrysler analysis, we eliminate dealers that are within the local market of (i) an inconsistently observed closed GM dealer, (ii) a consistently observed closed GM dealer that switches to another brand after the closing, and (iii) a Chrysler dealer that has been slated for closure. From the remaining list of dealers, we dropped closed Pontiac dealerships that continued to carry other brands and dealerships with very few observations to have a sample of 265 dealers, 93 of which are surviving GM dealerships. Out of these dealers, 64 (25 of which are GM dealerships) are located in the markets affected by a Pontiac exit. Our final sample includes 449,987 observations across 3,660 cars associated with 98 car models. The final sample corresponds to 220 dealers per month and 85 cars per dealer/month over a 24-month period. The bottom half of Table 1 provides the descriptive statistics for the key variables used in the estimation as well as the observed car categories in our GM analysis data set. Using data only prior to closures, we ran a regression of log prices on the treatment dummy along with all of the observables. The coefficient for the treatment dummy is not significant at the 10% significance level, which suggests that, in the case of GM, too, the treatment and control group dealer prices are similar in the pretreatment period after controlling for observables.

5.2. Results

5.2.1. Main Effect. Table 7 presents results for various fixed-effects specifications that test the effect of Pontiac dealer closings on car prices. The final specification (8) replicates our main effect specification for Chrysler. The estimation for that specification shows that incumbent dealers increase their prices, on average, by 1.472% ($457) in response to a local GM exit. This effect is slightly larger than that for the Chrysler analysis (1.006%, or $318), and it suggests that our main effect result can be extended to the case of GM exits. Similar to the Chrysler case, the final specification explains around 96% of the variation in log prices. In Table 11 in Online Appendix 3, we report robustness checks regarding differential trends across dealers and the number of local competitors. These robustness

Table 7 Fixed-Effects Estimation: The Impact of GM (Pontiac) Closings on ln(*Price*)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of closings within 30 miles	1.121***	1.069***	0.971***	1.683***	1.603***	1.547***	1.304***	1.472***
	(0.131)	(0.107)	(0.064)	(0.126)	(0.125)	(0.127)	(0.059)	(0.061)
Time since listed	—	—	—	—	−0.655***	−0.669***	−0.249***	−0.248***
					(0.021)	(0.021)	(0.010)	(0.010)
Time since listed^2	—	—	—	—	0.026***	0.030***	0.007***	0.007***
					(0.002)	(0.002)	(0.001)	(0.001)
Zip-code-level median household income	—	—	—	—	—	—	—	0.019
								(0.015)
Zip-code-level population	—	—	—	—	—	—	—	−0.513***
								(0.038)
Category fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
DMA/month-year fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes
Model/month-year fixed effects	No	No	No	No	No	Yes	Yes	Yes
Car fixed effects	No	No	No	No	No	No	Yes	Yes
Adjusted R^2	0.215	0.481	0.814	0.816	0.817	0.820	0.962	0.962

Notes. All estimations include dealer, brand, and month-year fixed effects unless already subsumed by finer fixed effects. Estimations are based on 449,987 observations across 98 car models. All coefficients and standard errors are scaled up by a factor of 100. Standard errors are given in parentheses.
***$p < 0.01$.

checks show that the exit effect for Pontiac closings does not change much in response to various additional controls. In addition, the falsification exercise in column (3) indicates that the placebo-closing variable is insignificant.

5.2.2. Spatial Heterogeneity in Price Effects. For the agglomeration-effects analysis, we used a sample of our data with a set of car models that we observed across all three distance bands. In addition, we restricted the sample to competing brands for a meaningful comparison with our Chrysler analysis. The final sample covers 226,147 observations across 48 car models. The estimated coefficients for the agglomeration analysis are shown in column (2) of Table 6. The coefficients indicate that the average increase in prices due to a local Pontiac exit in the primary distance band (0–10 miles) is 1.23% ($395), in the secondary distance band (10–20 miles) is 1.84% ($591), and in the tertiary distance band (20–30 miles) is 1.39% ($447). Again, we find that the smallest price increase for incumbent dealers is in the nearest distance band. This result further supports the idea that the agglomeration effect is at work, although the relative differences across bands are smaller in this case than in the Chrysler situation. In summary, our GM analysis results are qualitatively similar to our Chrysler analysis findings.

6. Conclusion

Firms use local market exits as a strategic way to cope with significant declines in demand, especially in times of economic crisis. Given the prevalence of local market exits, it is useful for policymakers and managers to have a better understanding of the effect of exits on prices, and on product competition in general. This paper investigates the price effects of Chrysler's dealer closings in the U.S. auto industry. Using a unique and extensive auto dealer panel data set on new cars that includes monthly observations at the dealer/vehicle level, we provide some empirical evidence on the price effects of local market exits in a setting with durable goods, high product heterogeneity, and a complex spatial market structure.

We find that retail prices on average increase by 1.006% ($318) following an exit, suggesting that incumbent dealers realize higher pricing power from tempered competition. To separate out the competition and agglomeration effects, we also investigate the spatial heterogeneity of price reactions. We find that agglomeration benefits erode much faster than gains from tempered competition. Accordingly, proximate dealers (within 10 miles) have an even lower ability to raise prices than extremely distant (20–30 miles) firms ($265 versus $780). This result supports consumers' search costs motivation for store clustering put forth by previous agglomeration studies (Marshall 1920, Stahl 1982, Wernerfelt 1994). Our GM analysis suggests that the Chrysler closing results are qualitatively generalizable to other firm exits. Collectively, our results inform consumers, firms, and policymakers about possible implications of a firm exit regarding changes in the market power of incumbent firms and heterogeneous spatial outcomes.

Although this study makes several contributions to the empirical literature on market exits and distribution channels, it has several limitations. Consistent with the vast majority of the extant studies on competitive reactions, this study, too, investigates the impact of exits on one aspect of the marketing mix, i.e., retailer pricing. Expanding the investigation to other elements of the marketing mix will nicely augment the current study. Our distance-bands analysis shows that the relationship between the distance from the exiting dealership and price increases due to exits follows an inverted-U shape. Although this finding indirectly suggests that the inverted-U pattern emerges as a result of two countervailing forces (i.e., competition and agglomeration), we do not model these forces explicitly. Our analysis is limited by our data, which contain dealer-vehicle-specific retail prices. By focusing on these prices, we are able to study the incumbent dealers' reactions across their entire inventory. Understanding how dealers' reactions to Chrysler exits manifest in transacted prices is another valuable direction of future research inquiry. However, this will come at the expense of studying price reactions only on a subset of the dealer inventory. Our results are based on the exits of two firms in a single industry critical to the success of the U.S. economy. Future research can investigate and contrast similar large-scale distribution channel changes in other industry settings. In the current study, our distribution channel exits are rooted in the financial distress of the upstream manufacturer. We also hope that future research will investigate whether and how incumbent retailers differentially respond to market structure changes induced by the financial distress of the upstream manufacturer versus the financial woes of the downstream competing retailer.

Supplemental Material

Supplemental material to this paper is available at http://dx.doi.org/10.1287/mksc.2015.0952.

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