Do Retail Mergers Affect Competition? Evidence from Grocery Retailing

Daniel S. Hosken

Luke M. Olson

Loren K. Smith*

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Abstract

This study estimates the price effects of horizontal mergers in the U.S. grocery retailing industry. We examine fourteen regions affected by mergers, including mergers in both highly concentrated and relatively unconcentrated markets. We identify price effects by comparing markets affected by mergers to unaffected markets using difference-in-difference estimation with three different comparison groups, propensity score weights, and by using the synthetic control method. Our results are robust to the choice of control group and estimation technique. We find that mergers in highly concentrated markets are most frequently associated with price increases, while mergers in less concentrated markets are most often associated with price decreases.

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I. Introduction

Economists have long believed that, all else being equal, increases in market concentration reduce competition, and reductions in competition lead to higher consumer prices and reduce consumer welfare. This belief provides the basis for much of the world's antitrust policy. The U.S., U.K., and E.U., for example, review mergers prospectively. Although each agency operates in a different legal environment, the economic logic underlying merger review is the same. Horizontal mergers can create or enhance market power by combining firms producing substitute products.[1] The problem for regulators is determining which mergers are likely to result in reduced competition. Unfortunately, there is remarkably little reliable systematic evidence linking measures of market concentration such as the Herfindahl-Hirschman Index (HHI) to manufacturer markups or consumer prices.[2]

Empirically identifying a causal relationship between price and market concentration is extremely difficult because market concentration is rarely exogenously determined. Firms that attain large market shares through organic growth are likely those that are most efficient, and markets where scale economies are important will tend to be dominated by a small number of efficient firms (Demsetz (1973)). As a result, studies that simply estimate the price/concentration relationship without controlling for the endogeneity of market structure are unlikely to accurately measure the causal relationship between price and market concentration (Evans et al. (1993), Bresnahan and Reiss (1989, 1990)).

In this paper, we develop evidence on the price-concentration relationship by estimating how prices change following significant changes in market structure resulting from a relatively

[1] See Section 1 of the 2010 U.S. Department of Justice (DOJ)/Federal Trade Commission (FTC) *Horizontal Merger Guidelines* for a clear description of the economic logic underlying U.S. horizontal merger policy.
[2] The Herfindahl-Hirschman Index is defined as the sum of the squared market shares of market participants, where firm's market shares are typically measured as percentage points.

large number of mergers in the supermarket industry. By focusing on how changes in market structure induced by mergers affect consumer prices, we can implicitly control for endogenous factors that determined the pre-merger market structure. We estimate the casual effect of mergers on supermarket prices using two related empirical techniques. We begin by following the literature and estimate merger price effects using a difference-in-difference analysis: we compare prices in markets experiencing a merger to those in similar markets not experiencing a major change in market structure resulting from entry, exit, or a horizontal merger. The major criticism of this method is that the decision to merge itself may be related to market participants' expectations about future prices in an industry resulting in biased price effects. To address this concern, we first make use of two variants of the difference-in-difference estimator that use the relative *a priori* likelihood of a comparison market experiencing a merger to either limit the comparison group (as suggested by Crump et al. (2009)) or by weighting comparison markets by their estimated propensity score (Hirano et. al (2003) and Imbens (2004)). Second, we estimate merger price effects using the synthetic control method developed by Abadie et al. (2010).

The supermarket industry is particularly interesting to study because it has experienced a relatively large number of mergers during our sample period, and has been subject to significant antitrust scrutiny despite having market characteristics that should cause entry and expansion to be relatively easy. Assuming a retailer has an existing supply network, it need only identify an effective location and obtain permission from local regulators to open an establishment. Moreover, the industry appears to be quite competitive with estimated net post-tax profit margins of only 1.37 percent over the last decade.[3] Notwithstanding the perceived ease of entry and expansion in the industry and relatively low profit margins, mergers in retail markets are often

[3] Food Marketing Institute estimates of grocery store chain's net profits, available at: http://www.fmi.org/docs/facts-figures/net-profit-percent-sales-2011.pdf?sfvrsn=2, last visited November 6th, 2012.

subject to material antitrust review. Between 1998 and 2007, for example, the FTC investigated supermarket mergers affecting 153 antitrust markets and challenged mergers in 134 of those markets.[4]

In analyzing horizontal mergers, antitrust agencies view the level and change in market concentration associated with a merger as a predictor of competitive harm. In this study, we estimate the price effects of eight mergers in highly concentrated markets and six mergers in moderately concentrated or unconcentrated markets. Our results tend to confirm the presumptions of antitrust regulators as stated in the *Horizontal Merger Guidelines*.[5] We find that five mergers resulted in estimated price increases of more than 2 percent, and that four of those mergers were in highly concentrated markets. Five mergers resulted in estimated price decreases of more than 2 percent, and only one of those occurred in a highly concentrated market. The remaining four mergers were associated with relatively little change in price. These findings are robust to the choice of comparison group and estimation technique.

Our paper contributes to the literature that examines how prices change following the merger of competing firms. The goal of most papers in this literature is to measure the efficacy of antitrust enforcement. In the typical study, researchers identify mergers that were likely on the antitrust margin; that is, those mergers that the antitrust authority seriously considered challenging but allowed to go forward. If the merger resulted in a price increase, the researchers conclude that policy was too lax while a price decrease indicates the regulator may be too strict (Ashenfelter et al. (2009)). Roughly sixty published studies have estimated the price effects of

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[4] Horizontal Merger Investigation Data, Fiscal Years 1996-2007, Federal Trade Commission, Table 4-2. Available at http://www.ftc.gov/os/2008/12/081201hsrmergerdata.pdf.

[5] The 2010 *Horizontal Merger Guidelines* predict that mergers in highly concentrated markets (mergers in markets with an HHI greater than 2500 and an increase in HHI of more than 200) will likely enhance market power, while mergers in unconcentrated markets (with an HHI of less than 1500) resulting in a small change in market concentration are unlikely to be anticompetitive.

mergers, with the majority finding that mergers have resulted in price increases.[6] The ability to draw general conclusions regarding the efficacy of horizontal merger policy from the published literature, however, is limited. First, only a tiny fraction of the thousands of mergers filed with the U.S. antitrust agencies have been studied. Second, the majority of studies estimate the price effects of mergers taking place in one of only four industries: banking, airlines, hospitals, and petroleum.[7,8] Third, studies in this literature are case studies. Although the case study methodology is often essential to credibly identify the price effects of mergers (learning and controlling for factors that affect industry pricing), the case study approach makes the generalization of findings from any one study to other situations difficult. A strength of our study, in contrast to much of the literature,[9] is that we estimate the price effects of many mergers affecting different geographic markets with different levels of market concentration at roughly the same time. In particular, we estimate the price effects of mergers in already highly concentrated markets as well as in less concentrated markets. By also studying mergers in less concentrated markets, we are able to examine mergers that are likely to be competitively benign but that could result in efficiencies that lower consumer prices. Our approach follows Carlton's (2009) suggestion that researchers should examine the price effects of all mergers (those likely and unlikely to result in price effects) to more fully understand how mergers affect the competitive process.

[6] See Kwoka (2013) and Ashenfelter et al. (2014) for recent surveys.

[7] Most studies find that prices increased following horizontal mergers in the airline (Borenstein (1990), Kim and Singal (2003), Kwoka and Shumilkina (2010), and Peters (2006),), banking (Sapienza (2002), Focarelli and Panetta (2003), and Praeger and Hannan (1998)), and hospital industries (Dafny (2009), Haas-Wilson and Garmon (2012), and Tenn (2012)). In contrast, the evidence in the petroleum industry is quite mixed, see Silvia and Taylor (2013) and cites therein.

[8] Other studies have found evidence of anticompetitive mergers in consumer products industries (Ashenfelter and Hosken (2010), Ashenfelter et al. (2013)), academic publishing (McCabe (2002)), intermediate goods industries (Barton and Sherman (1984)), and health insurance (Dafny et al. 2012).

[9] Prager and Hannan's (1998) study of banking mergers , Kim and Singal's (2003) study of airline mergers, and Dafny's (2009) study of hospital mergers are notable exceptions.

Of particular interest to our paper are two recent studies that each estimate the price impact of a single merger in the supermarket industry. Huang and Stiegert (2009) found that the merger of grocery retailers Kohls and Copps in Madison, Wisconsin did not raise prices in the months immediately following the merger, but did increase prices relative to a control market two years after the merger. Allain et al. (2013) examine a large merger of French supermarket retailers that affected many cities, and found that prices increased in markets directly affected by the merger relative to a comparison group of unaffected markets.

The remainder of this paper is organized as follows. Section II describes our data sources, and Section III presents the methodology used to construct our merger and comparison markets. Section IV describes our estimation strategy and presents the empirical findings of the study. Section V concludes.

II. Data

Our study uses three data sources. The first is A.C. Nielsen's Trade Dimensions retail database. Each year Trade Dimensions creates a census of retail outlets operating in the U.S. for a number of retailing industries, including supermarkets, club stores, liquor stores, convenience stores, and restaurants. In this study, we focus on the primary formats used for grocery retailing: conventional supermarkets, supercenters, and club stores.[10] Our dataset consists of annual observations, including the location, size, estimated sales, the store's banner (the name the store operates under), and corporate ownership of each supermarket, supercenter, and club store in the U.S. from 2004 through the fall of 2009. An additional feature of the dataset is that every store location has a unique identification number that allows us to track stores over time.

[10] We exclude other retail formats in the Trade Dimensions Grocery dataset – limited assortment, natural/gourmet food, warehouse, and military commissary – because they are so differentiated from traditional supermarkets

The price data we use consists of the prices used to construct the ACCRA Cost of Living Index, produced by the Council for Community and Economic Research (CCER). The ACCRA price index is designed to compare the cost of living for moderately affluent professional and managerial households in different U.S. metropolitan areas at a point in time.[11] The price data assembled by CCER are collected by the staff of roughly 350 local U.S. Chambers of Commerce.[12] In this study, our primary dataset consists of the prices collected for the 26 grocery products in the ACCRA sample. These prices typically correspond to a distinct food product, such as a pound of T-Bone steak or a 2 liter bottle of Coca-Cola, sold at a specific retail outlet on a given day.

CCER reports its data at the level of the Core Based Statistical Area (CBSA). A CBSA is defined as a set of adjacent counties connected by commuting ties to a common urban core of at least 10,000 residents and is designed to capture the political jurisdictions in a region that are closely connected by commerce.[13] The population of the markets (CBSAs) included in the CCER data varies dramatically from medium sized markets such as Lima, Ohio (106,000) to the largest U.S. CBSA of New York City (19,800,000). Smaller markets in the CCER data tend to have fewer price quotes per item than large markets.[14] Since we observe the retail banner that a price quote corresponds to but not the specific retail location that was visited, we treat the CBSA in which the price was collected as the geographic unit of observation.

[11] See the Council's web page for more details http://www.coli.org/.
[12] In the first, second, and third quarter of each year, staff of participating Chambers of Commerce collect price quotes for 60 distinct products corresponding to broad categories of consumer expenditures, including housing, energy, food, transportation, and health care.
[13] The U.S. Office of Management and Budget has devised the methodology used to construct CBSAs. For a detailed discussion of this methodology see, http://www.whitehouse.gov/sites/default/files/omb/fedreg/metroareas122700.pdf.
[14] In our data we observe markets with as few as 1 retailer surveyed within a quarter while others have more than thirty. In the median market/quarter 5 retail outlets and 4 retailers are surveyed; i.e., in the median market prices two outlets of a single retailer have been visited.

The CCER data is particularly well suited to our study. First, it contains prices on a broad set of supermarket products designed to measure the typical "market basket" of consumers' food purchases. Second, the data covers more geographic regions within the U.S. than any other publicly accessible pricing data set of which we are aware. This allows us to study many mergers and gives us a great deal of flexibility in identifying potential comparison cities to use in both our difference-in-difference analysis and in constructing a synthetic control. Third, we were able to collect a relatively long panel of data (5 years).

There are two key relative weaknesses of the CCER data. The first is that CCER's price collection method is more informal than other organizations such as the U.S. Bureau of Labor Statistics (BLS). Although surveyors are given a detailed set of instructions to follow in collecting prices,[15] CCER does not enforce a formal sampling scheme. The second is that the products contained in the CCER sample are, by construction, composed of frequently purchased supermarket products. As will be discussed in more detail in Section III, the prices of frequently purchased products are likely to be more strongly affected by changes in competition than a randomly selected grocery product. In spite of these shortcomings, the CCER data is the best publicly accessible data we are aware of for our study because of its broad geographic coverage of a variety of grocery products' prices over time.[16]

Finally, we have also obtained annual data from the Census describing the demographic characteristics of the geographic markets in which the firms compete. Demographic variables describing a region's population, income, and racial composition were collected at the county level and then aggregated to the CBSA level to correspond to our pricing data.

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[15] The instruction manual given to participants can be found at: http://www.coli.org/surveyforms/colimanual.pdf (last visited 7/17/2012).

[16] Data sets with more detailed price data have much more limited geographic coverage. For example. the IRI Marketing Dataset, only provides data for 47 relatively large geographic markets (Bronnenberg et al. 2008).

III. Market and Price Construction

Retailers are differentiated by location, by the types and quality of items they sell, and by the level of service they offer consumers. As a result, market definition – identifying the geographic region in which retailers compete and the set of firms (or products) that constitute a market – can be difficult.[17] Supermarkets, club stores, supercenters, convenience stores, mass-merchandisers (non-supercenter outlets operated by firms Target, Kmart, or Walmart), and drug stores, for example, all carry some food items. However, it is unlikely that all of these retail formats are similarly substitutable to one another. Convenience stores offer a very limited selection of food products in small stores at relatively high prices, while supermarkets and supercenters offer a broad selection of food products (including meat and produce) at relatively low prices in large stores. We limit our attention to the set of retail formats most likely to affect the pricing of supermarkets – large grocery retailers that sell a sufficient variety of food and other household goods such that consumers can purchase all of their food for a week at the retail outlet.[18] This limitation results in a set of retailers employing three retail formats: traditional supermarkets, club stores, and supercenters.

Identifying the geographic region that contains the competitors that determine the prices at a specific store location is complicated because of spatial differentiation. Large chain grocery retailers develop common marketing and pricing strategies for the broad geographic markets in which they operate to differentiate themselves from rivals. At the same time, these retailers face

[17] In this paper, we do not formally define the antitrust markets retailers compete in, see, for example, the discussion of market definition (Section 4) of the *2010 Horizontal Merger Guidelines*. Instead, our goal is to identify the geographic region and set of competitors most likely affected by a horizontal merger of supermarket retailers.
[18] In the industry, this is often referred to as one-stop-shopping. Recent empirical work shows that supermarkets change their prices in response to competition from supercenters and possibly club retailers suggesting that these retail formats compete with one another, see, e.g., Hausman and Liebtag (2007), Basker and Noel (2009), and Courtemanche and Carden (2014). We are unaware of empirical work that directly measures substitution between supermarkets, supercenters, club stores and other types of food retailers.

an incentive to exploit highly localized market power. Although some retailers do engage in some localized pricing (charging different prices at stores using the same retail banner within the same broad geographic market), it is unclear how empirically important this within market retailer price variation is. Volpe and Li (2012), for instance, found that two large chain retailers charged essentially the same prices at their outlets in many locations throughout the Los Angeles metropolitan area.

Ultimately, because our price data is reported at the CBSA level, we cannot measure both how prices change across the broad geographic markets affected by mergers and within the local neighborhoods potentially most affected by a given merger. Like previous researchers studying competition in U.S. retail food markets (Hausman and Liebtag (2007), Basker and Noel (2009), Huang and Stiegert (2010)), we restrict our empirical analysis to measuring how prices change across the broad geographic markets in which retailers compete.

Market Classification

To implement our difference-in-difference and synthetic control estimators we must identify those regions that experienced a significant change in market structure as the result of a horizontal merger (treatment markets) and those markets that experienced no significant change in market structure as the result of entry, exit, or horizontal mergers (comparison markets). We define a market as experiencing a significant change in market structure if it experiences a horizontal merger, entry, or exit affecting at least five percent of the retail outlets in the market. We limit our analysis to markets whose only significant change in market structure is the result of a single merger during our sample period.[19]

[19] Most medium-sized and all large CBSAs experience some entry and exit during our sample period. As a result, the large markets affected by mergers also experience some entry and exit. However, as shown in Hanner et al. (2015), the aggregate effect of this entry and exit activity is much smaller (less than 1% of market sales) than the changes in market structure caused by the mergers in the large markets studied here.

We define entry as occurring in a market when a new firm begins operations as a grocery retailer with a new retail brand. We do not consider expansion by incumbent firms within the market or the sale of a local retail chain to a firm not previously operating in that market to be entry. We define exit as an event where a retail firm ceases operations in a market and at least one retail brand is removed from that market. Parallel to entry, we do not view the sale (and continued operation) of a retail brand to another corporate entity or the contraction of a retailer's operations within a market to be exit.

In our data, we observe two types of transactions that we refer to as horizontal mergers. The most common type of merger we observe occurs when one firm decides to exit a market by selling its existing operations to a current market participant. For example, in exiting the San Francisco, California market in 2007, Albertsons sold its stores to incumbent grocery retailer Save Mart Supermarkets. Save Mart then operated those store locations using a new name, Lucky. The second type of transaction is a traditional merger where an incumbent buys all of the assets of a rival. In this scenario, the acquiring firm may or may not continue to operate the acquired firm's stores under their prior store name. We identify mergers using the Trade Dimensions data by identifying all instances where an incumbent firm begins operating stores that had previously been operated by a rival in a given market. We then searched the trade press and local newspapers to confirm that this observed change in store ownership was the result of either a horizontal merger or acquisition. For all but one of the mergers we study, we have been able to identify at least one press article identifying the merger.[20]

Our dataset contains price data for 357 different geographic regions (CBSAs). However, only 248 of the markets meet our inclusion criterion of having at least 10 quarters of data. Of

[20] We have been unable to find an article documenting the merger which took place in Fort Smith, Arkansas.

these 248 markets, 27 experience at least one significant horizontal merger, 42 experience at least one significant entry event, and 64 experience at least one significant exit event. Many of the markets experiencing significant entry, exit, or a merger experience multiple changes in market structure during our sample period, or experience a change in market structure at the beginning or end of our sample period. Given our identification strategy, we cannot estimate the price effects of a merger for markets experiencing multiple changes in market structure within our sample period. When we limit attention to those markets that 1) experienced only one significant merger, and 2) experienced mergers in either 2007 or 2008, we are left with our estimation sample of 14 markets experiencing horizontal mergers.

We next define two sets of CBSAs that we use as potential comparison markets. The first consists of CBSAs that experienced *no* change in market structure; that is, during the sample period the CBSA experienced no entry, exit, or merger of competing firms. We observe 75 CBSAs that meet this criterion. Because all large CBSAs in our data experience some change in market structure (most often the entry or exit of an independent or small chain retailer), there are no large CBSAs in the narrow comparison group. For this reason, we consider a second set of comparison markets that consists of markets that experienced a likely *di minimis* level of entry, exit, or mergers: collectively entry, exit, and horizontal mergers affected fewer than 2 percent of stores within the CBSA during our sample period. This less strict restriction increases the number of markets to 117 and adds a number of very large CBSAs to the broad comparison group such as Los Angeles, California; Washington, DC; and Dallas, Texas.[21]

Measuring Grocery Prices

We measure a retailers' grocery price at a point in time by constructing a price index designed to correspond to the market basket purchased by a consumer during a shopping trip.

[21] A list of the comparison group CBSAs used in the study is available from the authors by request.

We use a price index (rather than the price of specific grocery items) because it likely corresponds to the "price" consumers consider when choosing which grocery retailer to shop at in a time period. This assumption follows the retailing literature, which typically views consumers as trying to minimize the total costs of shopping (both grocery expenditures and travel costs). In these models, consumers choose an optimal retailer by determining the price of the entire bundle of products they will purchase rather than the price of any single item in the bundle (see, e.g., Bliss (1988)).[22]

CCER's grocery sample is constructed to correspond to a typical manager's food consumption bundle. To approximate this bundle, CCER has constructed expenditure weights using data extracted from the 2006 U.S. Consumer Expenditure Survey. We use these weights (w_k) to construct a price index for a retailer/market/quarter (p_{ijt}) as shown in equation (1) below

$$p_{ijt} = \sum_{k=1}^{26} w_k * p_{ijkt} \qquad (1)$$

where p_{ijkt} is the price of product k sold by retailer i in market j in time quarter t, and w_k is the expenditure share associated with product k.[23]

One shortcoming of our price data is that they are likely disproportionately composed of items that are especially sensitive to retail competition. Retailers likely offer low prices and have frequent sales on commonly and frequently purchased products (the products about which consumers are most informed) as a cost-effective mechanism to communicate a store's price level to consumers.[24] Ashenfelter et al. (2006), for example, found that the office supply retailer

[22] In preliminary work, we also estimated models where the unit of observation was a price quote; that is, a product/retailer/market/quarter/price. The results obtained using the individual product prices as the price measure were very similar to those estimated using the bundle prices reported in the paper.
[23] A list of the grocery products and the expenditure weight assigned to each product is available from the authors by request.
[24] Lal and Matutes (1994) describes how offering low prices on a subset of popular items can be a profitable pricing strategy and MacDonald (1998), Chevalier et al. (2003), Hosken and Reiffen (2004b) provide empirical evidence

Staples was more likely to change the prices of frequently purchased items, such as copier paper or pens, in response to changes in competition (entry or exit of a close rival) than the prices of less frequently purchased items (staplers). Unfortunately, we do not know how the pricing strategies used by grocery retailers vary across the items included in (and excluded from) the CCER price data. We do, however, suspect that many of the products included in CCER's basket are commonly purchased items (such as 2-liter bottles of Coca Cola) that are likely to be more sensitive to the level of retail competition than the average product. As a result, our price index will likely be more sensitive to changes in retail competition than an index that included all products sold by a grocery retailer (weighted appropriately by a product's relative expenditures). Nonetheless, our price index should correctly estimate the sign and relative magnitude of a merger's effect on market pricing.

Table 1 provides some information describing the individual mergers.[25] Table 1 shows that there is significant heterogeneity in the size and estimated market concentration of the markets experiencing mergers. Our sample consists of a number of medium-sized U.S. markets, with less than 100 retail outlets, and some massive markets, including New York, Philadelphia, and Detroit, with hundreds of retail outlets. Over half of our merger sample consists of highly concentrated grocery markets (with estimated HHIs greater than 2,500), while the remaining markets are relatively unconcentrated. New York and Philadelphia, for example, both have HHIs below 1,000. This variability in market concentration provides us with an opportunity to

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showing that when grocery items become more popular (experience a seasonal demand spike) average retail prices fall.

[25] We use the estimated grocery revenues of all club, supercenter, and supermarket retailers within the broad geographic regions affected by mergers in calculating market concentration (HHI). Our measure of market concentration could differ from that calculated by an antitrust agency in a merger investigation. An antitrust agency may define the product or geographic market differently and will have access to different revenue data (typically from subpoena responses rather than Trade Dimensions) to calculate market concentration.

determine if there is a systematic relationship between market concentration and the price effects resulting from consummated mergers.

Table 2 presents average demographic characteristics of the markets affected by horizontal mergers, and the broad and narrow comparison groups. Not surprisingly, because the merger markets include some of the largest and wealthiest U.S. CBSAs, on average, the merger markets tend to be larger and have higher pre-merger grocery prices than CBSAs in either the broad or narrow comparison group. In addition, as discussed earlier, the average CBSA in the narrow comparison group is much smaller than the average merger market because all large US CBSAs experience some entry, exit, and/or mergers by chain grocery retailers; that is, there are no major metropolitan areas in the narrow comparison group. When we weaken the requirement to include those markets that experience small levels of entry, exit, or horizontal mergers, the average market in the broader comparison group becomes much larger and more similar to the average merger market. However, the large variation in market characteristics of comparison CBSAs and merger CBSAs suggests that some comparison markets are likely to be more appropriate than others in providing a forecast of the counterfactual. In the next section, we describe in detail how we construct different comparison groups and estimators to examine the robustness of our findings to the choice of comparison group.

IV. Empirical Model and Results

The goal of our study is to determine how consumer prices are affected by the changes in market structure resulting from horizontal mergers within a retail market (CBSA). The major issue faced by any study attempting to measure the effect of a change in market structure on retail prices is to develop a reasonable estimate of the counterfactual change in price. Like most studies that estimate the price effects of mergers, we use a difference-in-difference estimator to control for the counterfactual change in price.[26] That is, the estimated price impact of a merger is identified as the change in price in the market experiencing a merger less the change in price in similar markets not experiencing a merger. For this approach to be valid, however, it must be the case that the change in price in the comparison markets closely approximates how prices would have changed in the merger market "but for" the merger. It is not possible to directly test this assumption because the counterfactual price is unobserved. However, because we observe a large number of markets that are unaffected by horizontal mergers or other large changes in market structure, we can test the robustness of our findings to different plausible comparison groups. In addition, given the large number of comparison group markets in our data set, we can estimate the *a priori* probability that a comparison group market will experience a merger (its propensity score) to either further limit the comparison group (as suggested by Crump et al. (2009)) or to weight comparison group markets in estimation.

To further evaluate the robustness of our findings, we also estimate merger price effects using the synthetic control estimator (Abadie et al. (2010)).[27] Like the difference-in-difference estimator, the synthetic control estimator uses information from markets unaffected by mergers to construct a counterfactual price for the merger market. However, instead of comparing how prices in the merger market changed relative to prices in all unaffected regions (where all regions receive equal weight in the comparison), the synthetic control estimator identifies a subset of the control markets that most closely "match" the merger market in the pre-merger period. The technique defines the counterfactual price for the merger market as a weighted average of the prices in this subset of control markets (the synthetic control). The weights are constructed using

[26] Most studies that exploit geographic variation in how mergers affect pricing using an identification similar to ours. Examples include studies of mergers in the airline industry (Borenstein (1990) and Kim and Singal (1993), banking (Prager and Hannan (1998) and Focarelli and Panetta (2003)), gasoline refining and distribution (Taylor and Hosken (2007) and Simpson and Taylor (2008)), and hospitals (Haas-Wilson and Garmon (2011) and Tenn (2011)).
[27] To our knowledge, the synthetic control estimator has not been used to estimate the price impact of mergers.

a data-driven technique that uses the levels of pre-merger prices and demographic characteristics in the merger and control markets to determine which control markets most closely match the merger market in the pre-merger period.

Difference-in-Difference Estimator

We estimate price effects using equation (2) below, where the (log) of retailer i's price index in market j in quarter t is regressed on a retailer/market specific fixed-effect (γ_{ij}), a time fixed-effect to control for idiosyncratic factors affecting grocery prices in all markets in a given quarter (δ_t), an indicator set equal to one in the post-merger period for the market affected by the merger, and (in some specifications) controls for time-varying market specific factors (x_{ijt}) which may affect grocery pricing.

$$\log(p_{ijt}) = \gamma_{ij} + \delta_t + \theta(\text{Post-Merger}_{ijt}) + \beta x_{ijt} + e_{ijt} \qquad (2)$$

Because the price impact of mergers may vary, we estimate equation (2) separately for each merger relative to a (potentially different) comparison group. We estimate standard errors by clustering by both the market (CBSA) and quarter using Cameron, Gelbach, and Miller's (2011) multiway clustering procedure.[28]

To estimate equation (2), we must first specify the timing of the event; that is, determine when we think the merger could begin having an effect on grocery pricing. We are somewhat constrained in our ability to determine precisely when mergers took place. Although we can identify the year in which a merger took place in the Trade Dimensions data, we cannot identify precisely the quarter in which all of the mergers occurred.[29] To avoid contamination bias, we

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[28] We use the Stata code developed by Cameron, Gelbach, and Miller (cgmreg.ado) available at http://old.econ.ucdavis.edu/faculty/dlmiller/statafiles/ (last visited August 14, 2013).

[29] For the larger mergers we have been able to identify the dates the transactions closed, e.g., A&P's merger with Pathmark. For smaller mergers, such as the transfer of ownership of a handful of stores in a small CBSA, we have not been able to identify the precise date the merger became effective.

have dropped data corresponding to the year in which the event took place, so that the pre-event and post-event periods are clearly defined.

We next describe the various methods we have used to construct comparison groups for each of the markets experiencing a merger. Ideally, the comparison group would consist of a set of grocery markets experiencing similar demand and supply conditions to the merger market "but for" the merger, such that any change in relative prices occurring after the merger has taken place can be attributed to the merger. As noted in the previous section, markets that have experienced economically important exit, entry, or horizontal mergers during our sample period are poor candidates for a counterfactual, because prices in those markets may have changed as the result of changes in market structure.[30] We have constructed two candidate comparison groups. The narrow comparison group consists of 75 relatively small CBSAs that did not experience any change in market structure as the result of entry, exit, or a horizontal merger during our sample period. The broad comparison group includes all CBSAs in the narrow group and 42 additional larger markets that experienced only small changes in market structure as the result of entry, exit, or mergers (affecting fewer than 2 percent of stores within the CBSA).

The key assumption underlying the validity of the difference-in-difference estimator is that the comparison markets and the merger market experience common trends in pricing "but for" the merger. Although it is not possible to directly test the validity of this assumption post-merger, it is possible to test this assumption using pre-merger price data. We implement this test by estimating equation (3) using pre-merger prices for all retailers in each merger

[30] A market affected by entry, for example, would be a poor candidate for the comparison group, because entry in food markets typically causes prices to fall. Moreover, the amount by which prices fall depends on the magnitude of market entry as well as how the entrant's product compares to those offered by incumbent retailers. Basker and Noel (2009), Hausman and Leibtag (2007), and Hosken, Olson, and Smith (2015), for example, find that the estimated price decline following entry varies from roughly 1% to as much as much as 7%.

market/comparison market combination, and determine if the interaction of the time trend and the merger city indicator (α_2) is statistically different than zero.

$$\log p_{ijt} = a_{ij} + \alpha_1 t + \alpha_2 t^{*}\text{MergerCity}_j + e_{ijt} \qquad (3)$$

To ensure that grocery prices in the comparison group CBSAs are likely to track those in the merger market, we limit the estimation sample to those comparison group CBSAs whose pre-merger price trends are not statistically different from the merger market at the 10 percent level.

Even after eliminating candidate comparison markets that either experienced significant changes in market structure or statistically different pre-merger trends in pricing, it is still possible that price trends in the remaining candidate comparison markets could differ systematically from the merger markets and thus provide poor forecasts of the counterfactual change in price. In particular, it is possible that a firm's decision to engage in merger activity within a market may be influenced by its expectations about future pricing within that market. For example, mergers may systematically take place in markets with growing (or shrinking) demand for grocery services. To address this concern, we also estimate merger price effects using two alternative difference-in-difference estimators that make use of information on the relative likelihood of a market experiencing a merger.[31]

The first method is a two-step estimator suggested by Crump et al. (2009). The motivation behind this estimator is that comparison markets that are highly unlikely to experience a merger are unlikely to be "similar" to those markets experiencing mergers. Crump et al. propose limiting the comparison markets to those whose probability of experiencing a merger (referred to as the propensity score) is not too extreme. To justify this approach, Imbens

[31] For both estimators, we use the broad comparison group. However, we limit the CBSAs in the broad comparison group to those markets with pre-merger price trends that are not statistically different than the merger market at the 0.1 level of significance.

(2014) notes that when the observations used in estimation have a propensity score that is either equal to one or zero that linear regressions are very sensitive to model specification, and estimates are likely to be biased.[32] Because the true propensity score is unknown and must be estimated, it is not possible to determine if a comparison market has a zero (or one) probability of experiencing a merger. Due to this uncertainty, Crump et al. propose trimming markets in the tails of the estimated propensity score distribution. In our implementation of the Crump et al. method, we first estimate the propensity score using an algorithm proposed by Imbens (2014) and Imbens and Rubin (2015). This algorithm is a stepwise regression that systematically selects linear and second order functions of market characteristics that could plausibly predict whether a market will experience a merger during our sample period.[33] Once the propensity score has been estimated for each comparison market, we use the method suggested by Crump et al. (2009) to select the optimal propensity score cutoffs to trim the sample, and keep comparison markets with estimated propensity scores roughly between 0.081 and 0.919.

We also estimate merger price effects using the propensity score as a weight in the difference-in-difference estimation. The intuition underlying this estimator is that those comparison markets most likely to experience mergers (based on observable characteristics) should receive more importance in the estimation than those relatively less likely to experience a merger. However, instead of dropping markets with very low probabilities of experiencing a

[32] In our data, this is a potentially important concern. While no market in our data is predicted to have a probability of experiencing an economically significant merger near 1 (the largest estimated propensity score in our data is 0.767), we observe many markets with very low estimated probabilities of experiencing mergers.

[33] Candidate variables included in this analysis are the levels of demographic variables from the first time period of our data (2004), including population, population density , number of households, income, school age population, average people per household, percent of population in poverty, percent of school age population in poverty, percent Hispanic, percent black; and percentage changes (from 2000 to 2004) in population growth, median income, the poverty rate, the school age poverty rate, black population, Hispanic population, and pre-merger market characteristics (measured in 2004) including the price level, HHI, grocery sales, presence of supercenters, presence of club stores, and market concentration. We implement this estimator using a logit model. Results from this estimation are are available from the authors by request.

merger as in Crump et al., we use a smooth function of the propensity score to weight observations based on their relative likelihood of experiencing a merger. We implement a propensity score estimator proposed in Hirano et. al (2003) and Imbens (2004),[34] where we re-estimate equation (2) where an observation in a market experiencing a merger receives a weight of "1" and an observation from comparison market j receives a weight equal to the ratio of its propensity score to one minus its propensity score $\left(\frac{\text{Probability Merger}_j}{(1\text{-(Probability Merger}_j)}\right)$.[35] In estimating this model, we use the propensity score generated in the implementation of the Crump et al. technique described above.

Results

We begin by providing some graphical evidence describing how well the average prices in the comparison group corresponding to a merger market track the prices in that merger market in the pre-merger period, and how prices diverge in the post-merger period. Specifically, for each merger market/comparison group combination we estimate equations (4) (for the merger market) and (4') (for the corresponding comparison markets) below, where the log of the price index of retailer i in market j in quarter t is regressed on a retailer/market fixed effect and an indicator for the quarter. Each pair of regressions is run using price data from either the merger market or the broad comparison group corresponding to that merger.

$$\log(p_{ijt}^{M}) = \gamma_{ij} + \delta_t^{M} + e_{ijt} \tag{4}$$

$$\log(p_{ijt}^{C}) = \gamma_{ij} + \delta_t^{C} + e_{ijt} \tag{4'}$$

[34] Our implementation uses the Stata code from Nichols (2007, 2008).

[35] Allain et al. (2013) use this propensity score estimator in estimating the effect of a major merger of French food retailers on consumer prices.

In these regressions, the coefficient corresponding to the quarter (δ_t^M, δ_t^C) corresponds to the average difference in log price between the current quarter (t) and the quarter observed just prior to the merger for the merger market and the comparison markets, respectively.[36] We then plot the coefficients corresponding to the quarter indicators in Figure 1 separately for each comparison group/merger market combination.[37] Because we have demeaned the data through retailer/market fixed-effects, an approximate overlap in the pre-merger period indicates that the comparison group does a good job of tracking pricing in the merger markets. In Figure 1, we see that prices in nine of the fourteen merger markets very closely match the comparison group in the pre-merger period, suggesting that for these markets, the average grocery prices in the comparison markets closely track those in the merger market.[38] Although average prices in the five remaining merger and comparison markets do not appear to diverge pre-merger, the imperfect pre-merger fit suggests that it is particularly important to check the sensitivity of the estimated price effects to the choice of comparison group for these merger cities. Finally, the divergence in (log) prices between the merger market and the average comparison market in the post-merger period corresponds to the estimated merger price effect. From Figure 1, it appears that post-merger prices fell significantly in San Francisco, San Jose, and Philadelphia, and rose significantly in Oklahoma City, Topeka, New Orleans, Fort Smith, and Fresno.

We next quantify the effect of each merger on grocery prices by estimating the various difference-in-difference estimators described above. The results are presented in Table 3. Each

[36] We have forced the coefficient corresponding to the time-period just prior to the merger to be zero. That is, the estimated quarter dummies are all measured relative to the prices in the quarter prior to the merger year.

[37] Recall, because we have dropped markets with statistically different pre-merger price trends from the comparison group, each merger market can have a different comparison group. Moreover, because some merger markets were not observed in all time-periods, we have not plotted price data for the comparison markets for the missing time-period. For example, CCER did not collect price data for Detroit in the third quarter of 2005. As a result, we do not plot a price for either Detroit or its comparison group in the third quarter of 2005.

[38] Prices in Detroit, Fort Smith, New Orleans, New York, Oklahoma City, Philadelphia, San Francisco, San Jose, and Topeka almost exactly track prices in the average comparison markets.

row in Table 3 corresponds to a distinct region affected by a horizontal merger, and each column corresponds to a different specification of the estimating equation and/or comparison group. Entries in the table correspond to the estimated effect of the merger on grocery prices in the region affected by the merger (θ from equation 2). The first column of results corresponds to regressions estimated using the narrow comparison group (markets experiencing no change in market structure as the result of entry, exit, or horizontal mergers) and includes retailer/market fixed-effects and time indicators as controls. The second column differs from the first by using the broad comparison group (including markets experiencing only small changes in market structure). The fourth column implements the Crump et al. (2009) estimator that eliminates markets from the broad comparison group that have a very high or very low probability of experiencing a merger. Finally, the fifth column contains the results from propensity score weighted difference-in-difference estimator. To facilitate interpretation, we have also included the post-merger HHI corresponding to the CBSA in which the merger took place.

The estimated price effects are robust to both the choice of comparison group and the use of propensity score weights. The estimated sign of the price effect for a merger does not change across specifications and, in many cases, the estimated merger price effects vary by less than a percentage point across specifications. The predicted effect of the merger in Philadelphia, for example, varies by less than a percentage point (between -4.6 percent to -4.4 percent) across specifications (comparing columns 1 to 5 of Table 3). For roughly half of the merger cities the use of propensity score weights results in larger estimated merger price effects (on the order of 1-3 percent) than the other model specifications.

Although the estimated price effects corresponding to any single merger are stable across specifications and comparison groups, there is remarkable variability across markets in the estimated price effects of a merger. That is, there does not appear to be a common "price effect" of a merger. Overall, the results in Table 3 show that five of the markets experienced estimated price increases of more than 2 percent, five experienced estimated price decreases of more than 2 percent, and the remaining four markets experienced little change in retail markets post-merger. Most of the estimated price changes that we view as economically significant (greater than 2 percent in absolute value) are also statistically significant at conventional levels. Moreover, prices tend to increase post-merger most frequently in highly concentrated markets and decrease most frequently in the least concentrated markets.

Some of our estimated price effects are very large in absolute value. As we noted in Section III, many of the grocery items in our price index are likely to be more strongly affected by changes in the level of retail competition than a randomly selected item. For example, while we estimate that the price of the CCER bundle fell by between 10-13 percent in San Francisco and San Jose following the purchase of Albertson's by Save-Mart, we strongly suspect that the reduction in a price index including all grocery items (appropriately weighted) would be considerably smaller. For this reason, we interpret our estimated price effects as being a relative measure of how much the overall price level changed as the result of a change in market structure. That is, we conclude that the Save-Mart/Albertsons transaction in San Francisco and San Jose led to the relatively large price reductions, while the merger of A&P and Pathmark led to more modest price reductions in Philadelphia.

Synthetic Control Groups

We now further assess the robustness of the study's findings to the choice of comparison group by using the synthetic control group estimator developed by Abadie et al. (2010). The synthetic control method was developed to identify treatment effects in studies like ours that use macro (market level) data where identification of the treatment effect comes from comparing a region that experienced treatment to regions that did not. Like the difference-in-difference estimator, the goal of the synthetic control method is to build a forecast of how the variable of interest (grocery prices) would have evolved but for treatment (a merger) using information on how the variable of interest (grocery prices) evolved in markets unaffected by treatment. However, rather than compare prices in the merger market to all markets unaffected by mergers, the technique determines which regions unaffected by mergers (the control group) are most similar to the merger market, and only uses prices from this subset of regions to forecast how prices would have evolved but for the merger. The counterfactual price (synthetic control) is a weighted average of this subset of the comparison group's prices. The weights are determined using an algorithm that minimizes the distance between pre-merger market characteristics including markets' price levels, of the merger market and the synthetic control market.[39] The estimated price effect of a merger is calculated by computing the average difference in the observed post-merger price of the merger city and the average post-merger price of the "synthetic control." Below we provide the details of how we implement the synthetic control estimator, and then present the results.

Let $t = 1, \ldots, T$ be the time-periods covered by the data and let t_M be the period in which the merger of interest occurred. Define $i = 1$ to be the geographic market in which the merger

[39] For example, the algorithm predicts that the best comparison price (synthetic control) for Oklahoma City, is the sum of 0.20 times the price index of Providence, RI; 0.19 times the price index of Tampa, FL; 0.16 times the price index of Paducah, KY; 0.12 times the price index of Cedar City, UT; 0.10 times the price index of Tuscaloosa, AL; and smaller proportions of 10 additional CBSA or CSAs.

occurred, and let $i = 2, \dots, I$ be the $I - 1$ potential comparison markets. P_{it} is the observed average price in market *i* at time *t*, and define $P_{\widetilde{it}}$ to be the average price that would obtain if no merger had occurred. The relationship between P_{it} and $P_{\widetilde{it}}$ in markets $(1, \dots, i, \dots, I)$ is given by

$$P_{it} = P_{\widetilde{it}} + \alpha_{it} D_{it} \qquad (5)$$

where

$$\begin{cases} D_{it} = 1 \; if \; i = 1 \; and \; t > t_M \\ \quad D_{it} = 0 \; otherwise. \end{cases}$$

The variable of interest, the effect of the merger on average prices, is α_{it} for periods $t = t_M + 1, \dots, T$. To construct an estimate of α_{1t}, the unobserved $P_{\widetilde{1t}}$ are estimated for periods following the merger by taking the difference between the observed average price in market 1 and a weighted average of the average prices in the control markets. These weights are found by matching the observed attributes of control markets to those of the merger market in the pre-merger period. Specifically, we estimate the set of weights $w = (w_2, w_3, \dots, w_I)$ that minimize the difference between $P_{\widetilde{1t}}$ and $\sum_{i=2}^{I} w_i P_{\widetilde{it}}$ for periods $t = 1, \dots, t_M - 1$, where $\sum_{i=2}^{I} w_i P_{\widetilde{it}}$ is specified as a function of observed market attributes.[40] The weighted sum, $\sum_{i=2}^{I} w_i P_{\widetilde{it}}$, has the following form:

$$\sum_{i=2}^{I} w_i P_{\widetilde{it}} = \sum_{i=2}^{I} w_i X_i + \sum_{i=2}^{I} \varepsilon_{it}, \qquad (6)$$

where each $k \times 1$ X_i vector includes market-specific attributes – population, population density, median per capita income, percentage of population that is black, percentage of population that is Hispanic, percentage of population below the poverty level, and price-levels – averaged across time periods 1 to $t_M - 1$, as well as the change in each of these variables from period 1 to period $t_M - 1$. The ε_{it} are idiosyncratic unobserved shocks to demand and/or costs in market *I* at time

[40] Recall that we drop data from the year a merger took place. For example, in estimating the price effects of a merger that took place in 2007, the pre-merger period includes data from 2005 and 2006 and post-merger data from 2008 and 2009.

t. The unknown parameters and weights in equation (5) are estimated by iteratively choosing the $w = (w_2, \dots, w_I)$ and V that minimize

$$(X_1 - \sum_{i=2}^{I} w_i X_i)' V (X_1 - \sum_{i=2}^{I} w_i X_i), \qquad (7)$$

where V is a $k \times k$ symmetric positive semidefinite matrix.[41] The optimal weights, $w^* = (w_2^*, \dots, w_I^*)$, are then used to estimate the desired $P_{1t}^{\sim}$ and α_{1t}.

We use the Stata code developed by Abadie et al. (2010) to estimate the synthetic control model.[42] Abadie et al.'s program requires that there be a single time series for the treatment group being analyzed. Thus, we need to aggregate the data to the level of a market/quarter from a market/retailer/quarter. However, we cannot simply construct a simple average of the retailers' prices in a market, because not all retailers are observed in a market in every time-period; that is, the composition of retailers observed in a market varies over time. Instead, we construct a price index that controls for retailer/market effects. Specifically, we regress retailer i's (log) price in market j at time t on a retailer/market fixed-effect (α_{ij}) and a series of time indicators. We estimate these regressions at the retailer/market level.

$$\log(p_{ijt}) = \alpha_{ij} + \sum_t \delta_{jt} + e_{ijt} \qquad (8)$$

The time indicator $\left(\delta_{jt}\right)$ from equation (8) estimates market j's average price at time t, holding retailer effects constant. We use the estimated δ_{jt} as prices in the synthetic control group estimator.[43] Abadie et al.'s Stata programs also require a balanced panel. Hence, for a given

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[41] We begin each synthetic regression at three different initial V matrices. For each initial V, we employ a fully nested optimization routine that searches over all diagonal positive definite matrices V and weights w for the control that minimizes (6). Finally, we choose the control that produced the smallest value of (7) among the three starting V matrices.

[42] The Stata programs implementing the synthetic treatment estimator are available at: http://www.mit.edu/~jhainm/synthpage.html.

[43] Prices are all normalized relative to the first quarter of 2006 $\left(\delta_{j,Q_1 2006} = 0 \text{ for all regions j}\right)$. All included treatment and comparison markets report a price in the first quarter of 2006.

merger, we limit the potential set of controls to comparison markets that report prices for each period reported by the merger market.

Abadie et al. suggest testing the validity of the synthetic control estimator by plotting both the prices in the merger (treatment) region and that region's synthetic control. If the synthetic control does not track prices in the merger city well pre-merger, it is unlikely to provide a good forecast of the counterfactual price. Figure 2 provides a plot of each merger region's observed (log) price index and its corresponding synthetic control price pre- and post-merger. [44] In all but one market (New Orleans), the synthetic control very closely fits the merger market's price in the pre-merger period. In the post-merger period, the average deviation between the merger market's price and the synthetic control's price provides our estimated merger price effect.

Abadie et al. do not calculate conventional standard errors for the estimated effects of treatment using their estimator. The authors argue that in market level studies like ours, the most important source of uncertainty is not the estimated precision of the price change within the affected region (which is typically estimated with a high degree of precision) but in the uncertainty of the methodology itself. To understand the importance of this uncertainty, the authors suggest that researchers conduct placebo studies; that is, compare the magnitude of the estimated merger effect for a market that experienced a merger to the estimated price impact of "placebo mergers." A placebo merger price effect is constructed by treating a comparison market as if it experienced a merger in the same year as the (true) merger market, and then using the synthetic control algorithm to construct a "placebo" price effect.[45] We implement our

[44] Recall that to avoid contamination bias, the year the merger occurred was excluded from the empirical analyses (and is also removed from Figure 2).

[45] In implementing the synthetic control group estimator, we have used all regions in the broad comparison group rather than limiting the estimation sample to those with similar pre-merger trends as was the case in the difference-

placebo tests as follows. For every merger/comparison group combination, we treat each comparison region as if it experienced a merger and then calculate the estimated merger price effect using the synthetic control estimator. This generates a distribution of up to 116 placebo treatment effects (one effect corresponding to each member of the comparison group).[46] We then rank these effects from smallest to largest and report the percentile corresponding to the (true) estimated merger price effect. Table 4 presents the synthetic control estimates of the price effect of the merger and the percentiles of the counterfactual pricing distribution generated by the placebo study in columns 3 and 4 respectively. For example, the estimated price effect of the supermarket merger in Oklahoma City is 6.2 percent. This price effect falls in the 94th percentile of the counterfactual pricing distribution. One can interpret this percentile as analogous to a p-value; that is, using this method, 94 percent of the markets not experiencing a merger were predicted to experience price increases less than Oklahoma City, and 6 percent of markets not experiencing mergers were predicted to experience price increases larger than Oklahoma City.

To facilitate comparison of the synthetic control estimates to the difference-in-difference estimates, we have re-estimated the difference-in-difference model using the same data used in the synthetic control analysis (the market-level prices generated by equation 5). We also generate an analogous measure of where the difference-in-difference estimate falls in the counterfactual distribution. Specifically, for each year in which a merger event can take place (2007 or 2008), we estimate how much the price changed following that year for each comparison market and the market that experienced a merger in that year using equation 9 below.

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in-difference estimation shown in Table 3. We have done this to increase the number of regions we can use in the placebo studies. This change will not affect the synthetic control estimate because regions with different pre-merger price trends will not be chosen to be included in the synthetic control estimator (i.e., their weight would be zero).

[46] As noted above, if a comparison market does not have data for the same time-periods as the treatment market, it is dropped from the synthetic control model.

$$\log(p_{ijt}) = \gamma_{ij} + \theta_j(\text{Post-Event}_{ijt}) + e_{ijt} \qquad (9)$$

We then sort the estimated price effects (θ_j) from smallest to largest for the comparison group and record which percentile a given merger market's estimated price effect corresponds to. Columns 1 and 2 of Table 4 contain the estimated price effect and the percentile of the counterfactual pricing distribution to which a price effect corresponds. For example, the difference-in-difference model estimates the price effect of the merger in Oklahoma City increased price by 7 percent. That price effect was larger than 93 percent of the price changes taking place in the comparison group for Oklahoma City following the merger.

The difference-in-difference estimates in Table 4 are very similar to those estimated with retailer/market level data (Table 3) suggesting that the data aggregation used in equation (5) does not result in a meaningful change in our estimated price effects. Although the difference-in-difference and synthetic control model estimates are not identical, they are very similar both qualitatively and quantitatively. The robustness of the estimated merger price effects to both model specification and choice of control group suggests that mergers are likely exogenous to the time path of prices within the market affected by the merger.

Discussion

The estimated merger prices are broadly consistent with the price concentration hypothesis. To illustrate this finding more clearly, we have plotted both the estimated price effects (using the difference-in-difference estimates in column 1 of Table 4) and the corresponding predicted post-merger HHI (which is calculated as the sum of the pre-merger HHI and the change in HHI shown in Table 1).[47] In addition, each point on Figure 3 is depicted by a

[47] This likely differs from the realized post-merger HHI. For example, if the merger were anticompetitive (that is, the merging firms increased price), then we would expect the size of the merging firms to decrease and the realized post-merger HHI to be less than the simple prediction we use in Figure 2.

marker, which varies based on the magnitude of the change in market concentration $(\Delta\text{HHI}<100,\ 100<\Delta\text{HHI}<200,\ \Delta\text{HHI}>200)$. For example, the merger in Topeka resulted in a predicted post-merger HHI of 4100, a change in HHI of about 600, and a change in price of about 7.7 percent. We have constructed vertical lines classifying the markets as defined in the 2010 *Horizontal Merger Guidelines*: markets with a post-merger HHI of less than 1500 are defined as unconcentrated, between 1500 and 2500 defined as moderately concentrated, and greater than 2500 highly concentrated. Finally, the figure includes the regression line from the simple linear regression of the level of the predicted post-merger concentration on the change in price caused by the merger.

While not all mergers in highly concentrated (unconcentrated) markets resulted in price increases (decreases), Figure 3 shows that, on average, most of the economically significant price increases occurred following mergers in highly concentrated markets. Similarly, although there is one significant price decrease resulting from a merger in a highly concentrated market, most mergers resulting in price decreases are in either moderately concentrated or unconcentrated markets. The relationship between the change in market concentration and the change in price resulting from a merger appears to be less closely related, although positively correlated. Thus, for this sample of mergers, market concentration provides a useful screen for identifying which mergers are most likely to increase market prices.

V. Conclusion

Antitrust enforcement agencies must determine how many competitors are necessary to maintain competition within a market. The answer to this question depends on market specific supply and demand factors such as the degree of product differentiation, ease of entry and expansion, and the model of competition that best fits the industry. By examining a relatively large number of mergers taking place in the same industry at roughly the same time we can draw some conclusions about how changes in market structure caused by a merger affect prices. Despite the relative ease of entry and expansion and low aggregate profit margins in the supermarket industry, we find evidence that horizontal mergers can result in significant increases in consumer prices and thereby harm consumers. The mergers that result in higher consumer prices are largely those that we would expect, *a priori*, to be problematic. When market concentration increases in highly concentrated markets as the result of a horizontal merger, we frequently – but not always – observe significant increases in grocery prices. Our results are consistent with the broader merger retrospective literature: mergers on the enforcement margin are, on average, associated with price increases.

Because the *ex post* merger evaluation literature has focused on estimating the price effects of mergers on the enforcement margin, there is little empirical evidence describing how presumably benign mergers affect consumer prices. Our study helps fill this gap. We find that mergers in unconcentrated or moderately concentrated markets are often associated with reductions in consumer prices. This result supports the presumption that competitively benign mergers can confer significant efficiencies that are passed on to consumer in the form of lower prices. Overall, our study's findings support the use of market concentration as a screen (as employed by the *Horizontal Merger Guidelines*) to aid antitrust agencies in efficiently deploying scarce enforcement resources.

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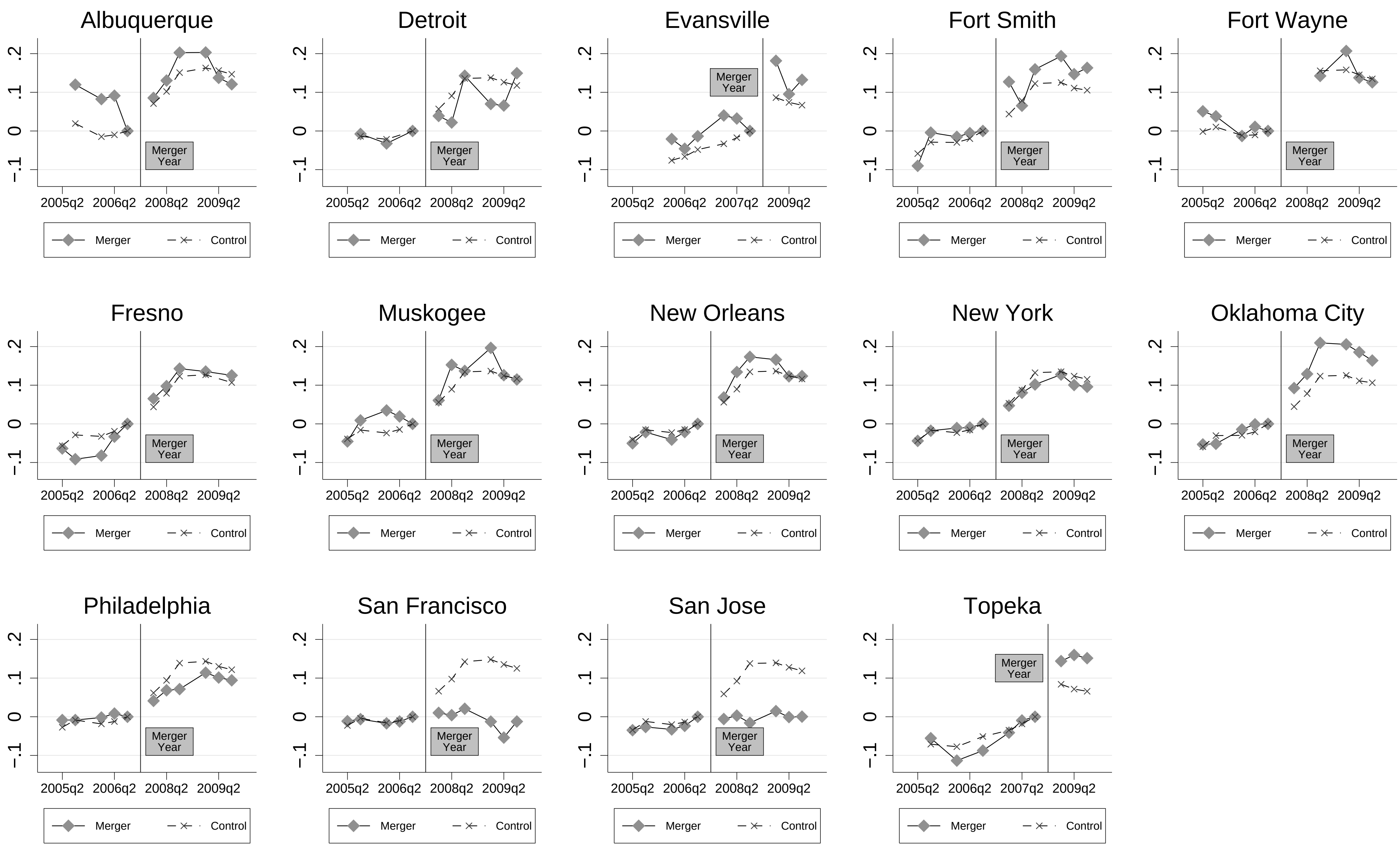
Figure 1: Time Series Plot of Scaled Log Price in The Merger Market and the Average Comparison Market

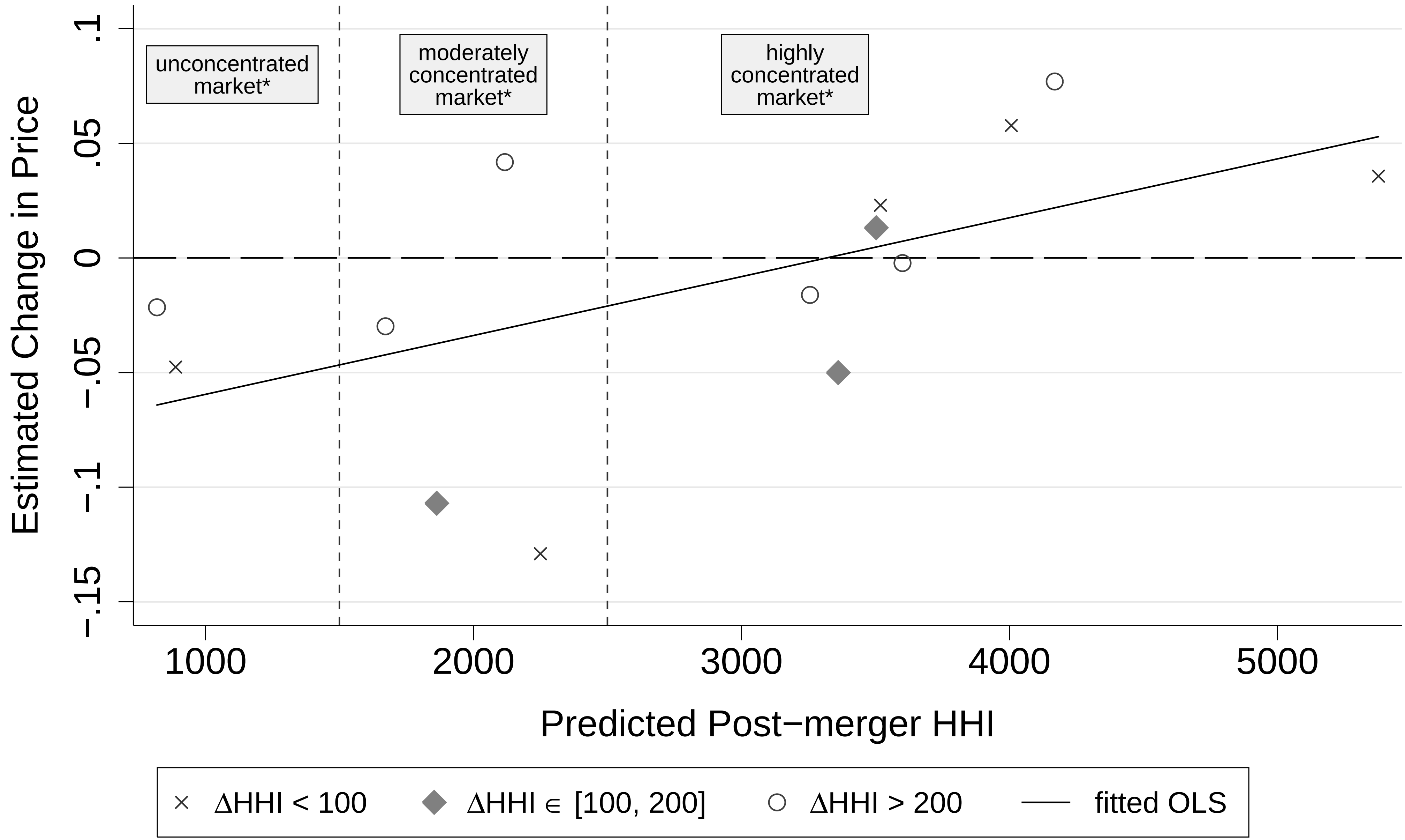


Plot of δ_t^M and δ_t^C from equations 4 and 4'. The coefficient correspond to the quarter prior to the merger is scaled to 0.
Data from the merger year − 2007 or 2008 − is excluded from the graph

Figure 2: Time Series Plot of Scaled Log Price in Merger Market and Synthetic Control Market

For the Merger Markets, we plot $\delta_{j,t}$ from equation 8, where $\delta_{j,Q1-2006}$ is set to zero.
Data from the merger year − 2007 or 2008 − is excluded from the graph

Figure 3: Relationship Between Post−merger Market Concentration, Change in Concentration, and Estimated Change in Price



* Section 5.3 of the 2010 *Horizontal Merger Guidelines* categorizes market concentration as follows: highly concentrated −− HHI > 2500; moderately concentrated −− HHI ∈ [1500, 2500]; unconcentrated −− HHI < 1500.

Table 1: Description of Mergers Studied

Market	Merger Year	Merger Description	Aquiring Firm		Aquired Firm		Pre-Merger Firms In Market		Market		
			Stores	Revenue Share	Stores	Revenue Share	Chains	Independents	Stores	Pre-Merger Revenue HHI	Change in HHI
Albuquerque, NM	2007	Albertsons buys 8 Raleys stores, 6 continue to operate; more stores in purchase, Raleys continued operation in N. Nevada and N. California.	10	0.09	8	0.06	7	14	72	3251	110
Detroit-Warren-Livonia, MI	2007	Kroger acquires roughly 20 Farmer Jack Supermarket locations from Great A & P Tea Co.	73	0.15	63	0.14	19	171	409	1260	412
Evansville, IN-KY	2008	Houchens Industries bought all Buehler Foods locations including 11 stores here.	5	0.07	11	0.13	8	9	47	3331	172
Fort Smith, AR-OK	2007	C V Foodliner buys 7 stores from CVM Inc.	10	0.08	7	0.06	4	7	42	5278	99
Fort Wayne, IN	2007	Kroger buys 11 stores from SuperValu Inc.	7	0.10	13	0.15	6	11	40	2943	313
Fresno, CA	2007	Save Mart Super Markets buys 5 stores from Albertsons.	24	0.36	5	0.06	11	34	86	1705	412
Muskogee, OK	2007	Assoc Wholesale Grocers Inc buys one store from Albertsons	3	0.13	1	0.08	5	3	11	3375	226
New Orleans-Metairie-Kenner, LA	2007	Rouse Enterprises buys 15 stores from Great A & P Tea Co	4	0.02	18	0.12	7	43	109	3462	57
New York-Northern New Jersey-Long Island, NY-NJ-PA	2007	Great A & P Tea Co buys 111 stores from Pathmark.	197	0.13	112	0.09	69	769	1755	597	222
Oklahoma City, OK	2007	Assoc Wholesale Grocers Inc buys 12 stores from Albertsons	13	0.04	12	0.06	11	24	113	3961	46
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	2007	Great A & P Tea Co buys 26 stores from Pathmark;.	38	0.07	26	0.05	33	96	452	817	72
San Francisco-Oakland-Fremont, CA	2007	Save Mart Super Markets buys 42 stores from Albertsons.	13	0.05	42	0.11	23	73	317	2152	98
San Jose-Sunnyvale-Santa Clara, CA	2007	Save Mart Super Markets buys 8 stores from Albertsons.	8	0.06	19	0.11	21	27	145	1729	134
Topeka, KS	2008	Kroger buys 3 stores from Assoc Wholesale Grocers Inc.	7	0.27	6	0.11	4	11	30	3572	597

Table 2: Market Characteristics by Market Type Prior to Treatment

Market Characteristics	Market Type		
	Narrow Comparison*	Broad Comparison**	Merger
Price Index	2.300	2.318	2.578
	(0.241)	(0.223)	(0.421)
Total Weekly SuperMarket Revenue ($ 000)	9460	30365	71700
	(18254)	(47403)	(111407)
Market Concentration (HHI)	3208	2773	2147
	(1171)	(1136)	(995)
Store Concentration (HHI of store ownership shares)	2182	1961	1153
	(1008)	(920)	(393)
Count of Firms with More than 100 Establishments	4.197	5.805	6.500
	(2.169)	(3.234)	(3.757)
Median Household Income	40120	42877	45459
	(7130)	(7637)	(10272)
Population	308203	1019988	2880174
	(595551)	(1711163)	(4916621)
Population Density (persons per square mile)	155.1	282.4	624.8
	(227.0)	(363.2)	(787.2)
Percentage of Population Living in Poverty	13.7	13.0	13.3
	(4.7)	(4.0)	(3.5)
Percentage of School Age Population Living in Poverty	17.0	16.1	16.9
	(6.8)	(5.8)	(5.0)
Percentage of Population African American	7.6	10.3	12.3
	(10.2)	(10.6)	(9.9)
Percentage of Population Hispanic	14.1	13.4	14.2
	(20.1)	(17.6)	(14.9)
Number of People per Household	2.4	2.4	2.4
	(0.2)	(0.2)	(0.2)
Income Growth (2000-2004)	4.4	4.2	2.8
	(4.0)	(3.7)	(4.5)
Poverty Growth (2000-2004)	16.9	21.1	18.5
	(11.9)	(12.9)	(11.2)
School Age Poverty Growth (2000-2004)	10.2	14.5	15.2
	(13.2)	(13.8)	(15.7)
Percentage Growth in Hipanic Population (2000-2004)	20.1	23.3	17.9
	(11.2)	(11.4)	(10.3)
Percentage Growth in African American Population (2000-2004)	23.2	17.6	3.0
	(34.2)	(28.2)	(7.7)
Number of Markets in Group	75	117	14

The price index corresponds to the premerger time period for merger markets. Prices come from the first year of available data (either 2005 or 2006). All other statistics are calculated using 2004 or the difference between 2004 and 2000. Standard deviations are shown in parentheses.

*The narrow comparison group contains markets that do not experience entry, exit, or a horizontal merger during the sample period (2005-2009).

**The broad comparison group contrains markets that do not experience any one entry, exit, or horizontal merger that affects more than 2% of stores in a market.

Table 3: Estimated effects of Mergers on Price: Difference in Difference

Region	Predicted Post-Merger HHI	1	2	3	4
New York City, NY-NJ-PA	819	-0.0195***	-0.0177***	-0.0155**	0.00333
		(0.007)	(0.006)	(0.008)	(0.022)
Philadelphia, PA-NJ-DE-MD	889	-0.0443***	-0.0438***	-0.0459***	-0.0446***
		(0.010)	(0.009)	(0.008)	(0.009)
Detroit, MI	1672	-0.0297	-0.0274	-0.0262	-0.00561
		(0.020)	(0.022)	(0.019)	(0.028)
San Jose, CA	1683	-0.105***	-0.105***	-0.105***	-0.106***
		(0.018)	(0.017)	(0.016)	(0.016)
Fresno, CA	2117	0.0416***	0.0441***	0.0484***	0.0711**
		(0.016)	(0.015)	(0.016)	(0.032)
San Francisco, CA	2250	-0.136***	-0.134***	-0.135***	-0.133***
		(0.021)	(0.020)	(0.024)	(0.021)
Fort Wayne, IN	3256	-0.0189	-0.0156	-0.0182	-0.0163
		(0.017)	(0.016)	(0.021)	(0.021)
Albuquerque, NM	3361	-0.0536*	-0.0559**	-0.0651**	-0.0653**
		(0.029)	(0.022)	(0.027)	(0.030)
Evansville, IN-KY	3503	0.0162	0.0188	0.0227	0.0488
		(0.024)	(0.025)	(0.024)	(0.033)
New Orleans, LA	3519	0.0272*	0.0299**	0.0335**	0.0536**
		(0.014)	(0.014)	(0.014)	(0.026)
Muskogee, OK	3601	-0.00326	-0.000437	0.000785	0.0197
		(0.018)	(0.018)	(0.024)	(0.029)
Oklahoma City, OK	4007	0.0570***	0.0611***	0.0671***	0.0955***
		(0.010)	(0.010)	(0.012)	(0.030)
Topeka, KS	4169	0.0856***	0.0870***	0.0903***	0.118***
		(0.018)	(0.019)	(0.022)	(0.030)
Fort Smith, AR-OK	5377	0.0339*	0.0388**	0.0432*	0.0697**
		(0.019)	(0.019)	(0.022)	(0.029)
Specification					
Broad or Narrow Control Group		Narrow	Broad	Broad	Broad
Market/Retailer Fixed-Effects		x	x	x	x
Quarter Indicators		x	x	x	x
Propensity Score Trimmed Control Group				x	
Propensity Score Weights					x

Dependent variable is the log of a retailer's price index in a CBSA/quarter. Each entry in columns 1-4 of the table corresponds to the estimated effect of entry on market prices in the specified region. Each estimate comes from a separate difference-in-difference regression. All regressions include market/retailer fixed-effects and separate indicator variables for time periods (quarters). Control markets that experience pre-event price trends different from the merger markets at the 0.1 level are excluded from the analysis. Column 1 includes as control markets those markets that experienced no significant merger, entry or exit event during our time period (narrow comparison). Column 2 adds control markets that experienced small events (broad control group). Column 3 further limits the comparison group to stores in markets whose estimated probability of experiencing a merger (propensity score) is within (0.081, 0.919). Column 4 weights observations in the control markets by a function of the propensity score for the market experiencing a merger: (PS / (1-PS)). Standard errors (in parentheses) are clustered by census division/quarter using the method of Cameron et al. (2011). * Statistically significant at the 10% level, ** Statistically significant at the 5% level, *** Statistically significant at the 1% level.

Table 4: Estimated Price Effects Mergers
Comparison of Difference-in-Difference and Synthetic Control Estimates

		Difference-in-Difference		Synthetic Control	
Merger Market	**Predicted Post-Merger HHI**	**Coefficient**	**Percentile Of Counterfactual Distribution**	**Coefficient**	**Percentile Of Counterfactual Distribution**
New York	819	-0.022	30.2%	-0.018	32.1%
Philadelphia	889	-0.048	10.9%	-0.045	9.9%
Detroit	1672	-0.030	18.1%	-0.038	12.3%
San Jose	1863	-0.107	2.6%	-0.075	3.7%
Fresno	2117	0.042	89.7%	0.062	98.8%
San Francisco	2250	-0.129	1.7%	-0.105	1.2%
Fort Wayne	3256	-0.016	50.9%	-0.048	9.9%
Albuquerque	3361	-0.050	7.8%	-0.065	3.7%
Evansville	3503	0.013	54.8%	0.008	70.3%
New Orleans	3519	0.023	75.9%	0.027	91.4%
Muskogee	3601	-0.002	44.8%	-0.005	51.9%
Oklahoma City	4007	0.058	94.0%	0.056	98.8%
Topeka	4169	0.077	96.5%	0.052	98.9%
Fort Smith	5377	0.036	85.3%	0.057	98.8%

The dependent variable is the log of a region's price index in a CBSA/quarter. Each entry in the columns labelled "Coefficient" corresponds to the estimated effect of a merger on prices in that market. The difference-in-difference models include time indicators and market fixed-effects. Each estimate comes from a separate difference-in-difference regression or synthetic control estimation. Each entry in the columns labeled "Percentile of Counterfactual Distribution" corresponds to the percentile of the counterfactual distribution in which the merger coefficient is located.