Horizontal Mergers, Prices, and Productivity*

Job Market Paper

Robert Kulick

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Abstract

I estimate the price and productivity effects of horizontal mergers in the ready-mix concrete industry using plant and firm-level data from the US Census Bureau. Horizontal mergers involving plants in close proximity are associated with price increases and decreases in output, but also raise productivity at acquired plants. While there is a significant negative relationship between productivity and prices, the pass-through rate of productivity increases is small enough that the effects of increased market power are not offset. I use a simple structural framework to assess the effects of merger activity on total welfare. At acquired plants, the consumer and producer surplus effects of mergers approximately cancel each other out, but effects at acquiring plants and non-merging plants, where prices also rise, cause a substantial decrease in consumer surplus of about $170 million (1987 dollars) leading to a loss of total welfare of around $30 million in aggregate for the sample. I also present several additional new results. For example, mergers are only observed leading to price increases after the relaxation of antitrust standards in the mid-1980s; price increases following mergers are persistent but tend to become smaller over time; and, there is evidence that firms target plants charging below average prices for acquisition.

*The results presented here have been screened to ensure that no confidential information is released in accordance with the policy of the Bureau of the Census. The results and conclusions expressed here are those of the author and do not reflect the opinions of the Bureau of the Census or the Center for Economic Studies. I am deeply indebted to John Haltiwanger and Andrew Sweeting for their guidance and support for this research. I also thank Ginger Jin, Chad Syverson, Allan Collard-Wexler, Matthew Weinberg, Nathan Miller, Einer Elhauge, Devesh Raval, Ethan Kaplan, Ryan Decker, Javier Miranda, and Emek Basker as well as seminar participants at the Department of Justice, IIOC Rising Stars Session, Loyola University Maryland, and the Federal Trade Commission for their helpful comments and suggestions. Email: kulick@econ.umd.edu.

In recent years, empirical research into the consequences of horizontal mergers has been a burgeoning area of inquiry and there has been significant progress in the retrospective analysis of price effects. A large body of research now provides systematic evidence that horizontal mergers are often associated with price increases, but research on the productivity consequences has lagged behind. Furthermore, empirical literature simultaneously examining the price and productivity effects of horizontal mergers is virtually non-existent, even though evaluation of the tradeoff between market power effects and efficiencies is one of the oldest and most important topics in the economic analysis of mergers.

Using plant and firm-level data collected by the U.S. Census Bureau for the ready-mix concrete industry, this study seeks to fill the gap in the literature by evaluating the price and productivity effects of horizontal mergers. I find that horizontal mergers involving plants in close geographic proximity are associated with significant price increases and decreases in output, but also significant increases in productivity at acquired plants. While there is a negative relationship between productivity and prices, the pass-through rate of changes in productivity is small enough that the effects of increased market power are not offset. I also find evidence of higher prices but not productivity at acquiring plants and non-merging plants located nearby to horizontally acquired plants.

I then use a simple structural model to calculate the total welfare impact of the horizontal mergers in my sample, building on the framework first suggested by Williamson (1968) to assess the tradeoff between the welfare effects of increased efficiency and higher prices. At acquired plants, the consumer and producer surplus effects of mergers approximately cancel each other out, but effects at acquiring plants and non-merging plants, where prices also rise, cause a substantial decrease in consumer surplus of approximately $170 million (1987 dollars) leading to a net decline in total welfare of approximately $30 million for the entire sample. This consumer surplus loss represents approximately 4% of ready-mix concrete revenues in affected markets.

The horizontal merger retrospective literature has been highly influential among academic economists and has even gained the attention of the general public. Numerous studies have shown across a spectrum of industries that prices have risen following approved mergers (Ashenfelter et al., 2014). The conclusions of the academic literature have influenced merger enforcement, informing regulatory efforts at the Department of Justice (DOJ) and Federal Trade Commission (FTC), and have even affected the public perception of merger policy. Yet, despite the importance and influence of the horizontal merger retrospective literature, it has at least three significant limitations that I seek to address.

First, and most importantly, almost none of this literature has addressed the question of how mergers have affected total welfare, instead focusing solely on prices. To a large extent, this gap reflects the fact that the previous literature has lacked data on establishment or plant level productivity.[1] The US Census Bureau's plant-level data allows me to construct a measure of productivity for each observation in my sample so that I can simultaneously evaluate both prices and productivity over a long time horizon (1977 to 1992).

Second, most of the literature on horizontal mergers has focused on individual mergers, or a small number of mergers. For example, one of the most well-known and recent papers, Miller and Weinberg (2015), focuses on a 2008 joint venture between SAB Miller and Coors brewing companies. Another prominent example is Ashenfelter et al. (2013), which assesses the competitive impact of the Maytag-Whirlpool merger. The focus on small samples of mergers makes it difficult to control for the possible endogeneity of which firms choose to merge. In my data, I observe over 400 plants engaged in horizontal merger activity over a 15-year time period. I also observe a large number of characteristics of both plants and markets, which makes it possible to estimate models that control for many types of selection on observables. A key finding of my paper is that both the direction and the size of my baseline price and productivity estimates are very robust to several different types of observable controls, which provides some support for a causal interpretation of the results. However, because mergers are not natural experiments, my case for a causal interpretation ultimately relies on a variety of evidence. For example, the pattern of price increases in the data is accompanied by decreases in plant level output, which is precisely what would be expected as a result of the creation of additional market power. I find significant price increases due to horizontal mergers after a relaxation in antitrust enforcement standards in the mid-1980s, but no evidence of systematic price increases before. I also find that price increases are associated solely with horizontal mergers as opposed to other types of mergers and that price increases are associated exclusively with local merger activity.

Third, much of the evidence on the consequences of horizontal mergers has come from differentiated-product industries where measuring merger effects may be made more difficult

[1] Establishments are defined by the Census as the specific location where business activity occurs while firms are defined as all establishments under common operational control. Here, all establishments in the data are plants engaged in the production of ready-mix concrete.

because products often change their physical quality, package size or how they are sold. In contrast, I look at ready-mix concrete where the physical product itself is close to being physically homogenous. There is, of course, geographical differentiation in the industry, but this is a feature that I am able to exploit in order to distinguish mergers involving local plants and mergers involving geographically distant plants, where market power effects are likely to be absent.

The literature specifically addressing the relationship between horizontal mergers and efficiencies at any level is very small and based entirely on indirect evidence. Indeed, analysis of the relationship between horizontal mergers and efficiencies is currently limited to two studies of which I am aware. The first examines the effects of changes in transportation costs associated with the Miller-Coors joint venture (Ashenfelter et al., 2015). The second examines the timing of price effects over the short and long-term in the Italian banking sector arguing that in the short-term market power effects dominate leading to higher prices, but in the long-term lower prices reflect the realization of efficiencies (Focarelli and Panetta, 2003). My study is the first within the literature that directly assesses the empirical relationship between productivity and price following merger activity. Furthermore, I observe price and productivity at five year intervals so that I can directly examine this relationship over time. Specifically, I am able to determine the precise year in which each merger takes place in my data so that I can distinguish between short-term and long-term effects.

There is a more extensive literature on the relationship between mergers and productivity, with some of the most recent literature also explicitly considering price effects or markups (Hortaçsu and Syverson, 2007; Braguinsky et al., 2015; Blonigen and Pierce, 2016). However, none of these studies have distinguished between types of mergers and have focused on mergers as a whole rather than horizontal mergers. Furthermore, with the exception of Blonigen and Pierce, these studies have not found evidence of systematic price increases and have emphasized efficiencies rather than market power effects. Conversely, Blonigen and Pierce find evidence of higher markups but not productivity increases as a result of merger activity, so there is no examination of the tradeoff between market power effects and efficiencies.

An advantage of this study is that productivity is measured directly following the recent trend of evaluating productivity in terms of total factor productivity calculated with respect to quantity or TFPQ (Hortaçsu and Syverson, 2007; Braguinsky et al., 2015). However, my results also have implications for the older literature considering the relationship between mergers and productivity, which uses total factor productivity measured with respect to revenue or TFPR (McGuckin and Nguyen, 1995; Maksimovic and Phillips, 2001). Because data on revenue is more abundant than data on quantity, the largest studies of productivity and mergers use TFPR instead of TFPQ. But, because TFPR is both a function of price and TFPQ, TFPR will provide an unreliable estimate of productivity if mergers have systematic effects on prices. This problem is well known in the literature and has been addressed by assuming that antitrust enforcement is sufficient to eliminate a systematic upward bias (McGuckin and Nguyen, 1995). Yet, to date, there has been little research directly examining the validity of this assumption.

Section 1 of this paper considers data and measurement issues and provides details about the ready-mix concrete industry, the sample of plants, the calculation of total factor productivity, and the identification of merger activity. Section 2 introduces my methodology and presents the primary regression results. Section 3 introduces a structural model to evaluate the welfare impact of the mergers in my sample, and Section 4 offers concluding remarks.

1 Data and Measurement

1.1 Ready-Mix Concrete

The ready-mix concrete industry has become popular in economic research due to its unique characteristics and because of the detailed data collected for the industry through the Census of Manufactures (CM). The CM occurs every 5 years and collects detailed data on inputs used by plants in the production process. For 1977–1982, the CM also collected product specific revenue and quantity data from plants in the ready-mix concrete industry. These data have been used extensively in the economic literature on productivity to calculate TFPQ (Syverson, 2004a,b; Hortaçsu and Syverson, 2007; Foster et al., 2008, 2016; Collard-Wexler, 2013; Backus, 2016). Here, I use the sample of ready-mix concrete plants with non-imputed product specific revenue and quantity data from Foster et al. (2016).[2]

Ready-mix concrete is a mixture of water, cement, gravel, and other chemical admixtures. The

[2]The foundation of this dataset was originally developed in Foster et al. (2008). Although this study attempted to identify all observations with imputed product specific revenue and quantity data using a variety of methods, the original impute flags in the raw Census data had been lost. White et al. (2015) recovered the missing impute flags and these recovered flags were applied in Foster et al. (2016). As approximately half of the original sample was imputed, in Appendix A of this paper, I evaluate the robustness of my conclusions applying inverse propensity score weighting to the primary results. I show that all conclusions are highly robust.

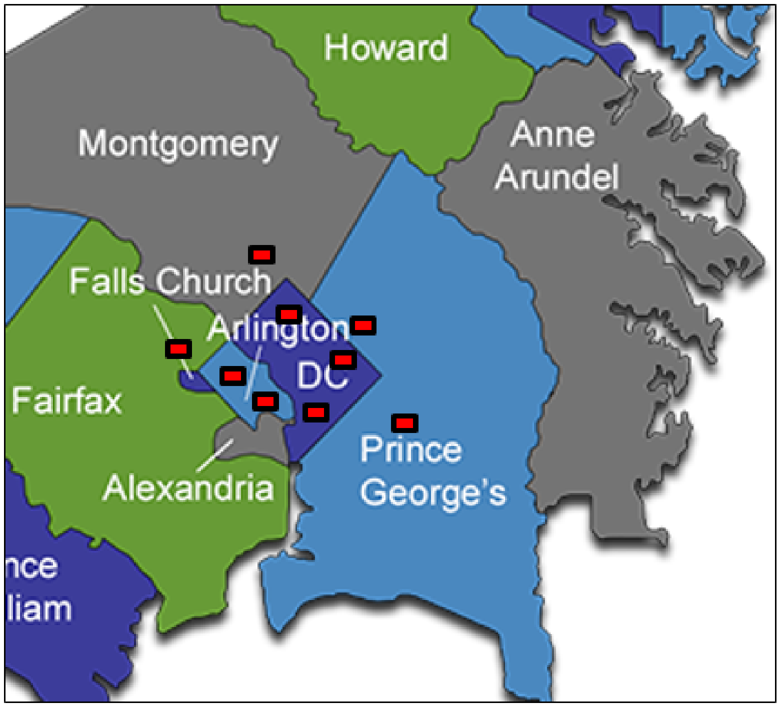
vast majority of ready-mix concrete is purchased by the construction sector (Syverson, 2004a). The ingredients of ready-mix concrete are typically mixed at a central plant and then transported to construction sites. The American Society for Testing and Materials (ASTM) standards specify that ready-mix concrete should be transported and discharged within 1.5 hours of initial mixing. Although this stipulation can be waived by the purchaser, the perishability of the product and the cost of transporting it result in a highly localized market for ready-mix concrete (Collard-Wexler, 2013). The Census' Commodity Transportation Survey indicates that ready-mix concrete plants ship approximately 95 percent of their output by weight less than 100 miles (Syverson, 2004a).

Following Syverson (2004a), ready-mix concrete markets are often defined in the economic literature in terms of the BEA's 1995 Component Economic Areas (CEAs). CEAs partition all 3,141 counties and county equivalents in the United States into 348 market areas designed to capture linked economic activity (Backus, 2016). CEAs are then combined by the BEA to form 172 Economic Areas or EAs. CEAs have the benefit of providing a contiguous, relatively compact market definition for the ready-mix concrete industry.

However, for the purposes of assessing the market power effects of horizontal mergers, CEAs are potentially problematic. First, plants on opposite ends of a CEA will often be too geographically distant to be directly competitive. Second, because CEAs partition the United States into contiguous geographic entities, two plants on the edges of different CEAs may be in much closer geographic proximity than either plant is to other plants within the CEA. Thus, for the purposes of my empirical analysis of market power, I define an alternative geographic area: the adjacent county block (ACB). For a given plant, an ACB constitutes the county in which the plant is located and the immediately adjacent counties. This strategy essentially restricts the competitive ambit of a given plant to a small surrounding geographic area. In Figure 1, I provide a map that depicts the ACB associated with the Washington, D.C. county equivalent.

The map in Figure 1 depicts Washington, D.C. and its adjacent counties Montgomery, Prince George's, Arlington, Fairfax, and Alexandria and also indicates the locations of the current major ready-mix concrete plants in the Washington metro area. All of the plants denoted with red squares are within the Washington, DC ACB as they are located either in Washington or in one of the adjacent counties. On the other hand, the plant in Prince George's County would not be in the Arlington County ACB, as Prince George's is not directly adjacent to Arlington. While CEAs contain over 9 counties on average, ACBs in my sample have an average of 6 counties.

Figure 1: The Washington, D.C. Adjacent County Block



Furthermore, because ACBs are drawn with respect to the surrounding counties, a given plant is always located centrally within its ACB. Finally, ACBs represent a convenient unit of analysis because the constituent units of CEAs and EAs are also counties, facilitating direct comparison of the different market definitions. However, because ACBs are necessarily overlapping, when structurally estimating the demand system in Section 3, I use CEAs to define markets.

1.2 Productivity

Following Foster et al. (2008), TFP is calculated using the typical index form. Specifically, for each plant i, TFP takes the form:

$$\text{TFP}_i = y_i - \alpha_l l_i - \alpha_k k_i - \alpha_m m_i - \alpha_e e_i \tag{1}$$

where the lower-case letters indicate respectively, the (log) values of gross output, labor input, capital, materials, and energy inputs, and the α_j coefficients are factor elasticities that are assumed to be invariant within the industry.

Labor inputs are measured, following Baily et al. (1992), as production-worker hours multiplied by the ratio of total payroll to payroll for production workers and the corresponding variable is denoted as *LABOR* below. Capital inputs are the book values reported by plants for their structural and equipment capital stocks deflated to 1987 levels using sector-specific deflators from the BEA. The capital variables are identified separately and are denoted as *STRUCTURE* and *EQUIPMENT*. Materials and energy inputs are plants' reported expenditures deflated using the corresponding input price indices from the NBER Productivity Database. These variables are denoted as *MATERIALS* and *ENERGY*.

The factor elasticities are calculated as industry-level cost shares. Cost shares are a widely used method for calculating factor elasticities as they avoid the classic endogeneity problem involved in estimating production functions (Syverson, 2011). However, this attractive feature requires us to rely on the following assumptions: (1) that plants are cost-minimizing, (2) that the first order conditions linking observed output shares to output elasticities hold on average eliminating the effects of idiosyncratic adjustment cost-induced misalignments in input levels,[3] and (3) that the production function exhibits constant returns to scale. The advantages and disadvantages of the various approaches to calculating productivity have been discussed at length in the literature. Van Biesebroeck (2007) shows that cost shares are particularly effective relative to other methodologies, including techniques relying on structural estimation of the production function, when changes in productivity are of interest as is the case here. Nevertheless, there has been immense progress in the structural estimation of production functions over the last decades (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Wooldridge, 2009; Ackerberg et al., 2015), and I am currently in the process of checking the robustness of my findings applying these techniques. Preliminary results indicate that the overall conclusions regarding productivity remain quite similar.

The labor, materials, and energy cost shares are calculated using reported expenditures from the CM. Capital cost shares are the reported equipment and building stocks multiplied by the capital rental rates matched to ready mix-concrete's two-digit industry code. As discussed above, I consider two measures of TFP in this study: TFPQ and TFPR. For TFPQ, y_i in the equation above is each plants' physical output of concrete measured in thousands of cubic yards. For TFPR, y_i is the nominal revenue from product sales deflated by the revenue weighted geometric

[3]Using plant plant-specific cost shares instead of industry-specific would require a much stronger assumption that the first order conditions hold for every plant. Previous research considering the use of plant-specific cost shares has found that conclusions regarding average productivity effects are quite similar to results derived from industry-specific cost shares.

mean price across the ready-mix concrete plants in the sample for a given year.[4]

1.3 Mergers

I identify merger activity by linking the CM to the Census Bureau's Longitudinal Business Database (LBD). The LBD maintains distinct identifiers for establishments (in this case plants) and firms (Firm ID) allowing researchers to observe how for a given set of plants ownership structure evolves over time. Consequently, the Firm ID variable in the LBD has been used extensively in the economic literature to track changes in ownership (Haltiwanger et al., 2013; Davis et al., 2014). I use this Firm ID variable both to identify merger activity and to distinguish horizontal mergers from other types of mergers in the ready-mix concrete industry.

Table 1 provides some basic information on the frequency of mergers within the data to help clarify the distinctions between the categories of plants involved in merger activity.[5] For now, these distinctions are defined without any geographic limitations. Later in this section, I explicitly distinguish local mergers from non-local mergers.

Table 1: Categorization of Merger Activity

	Plants
TOTAL	1,980
ACQUIRED ALL	320
ACQUIRED HORIZONTAL	200
ACQUIRING	220

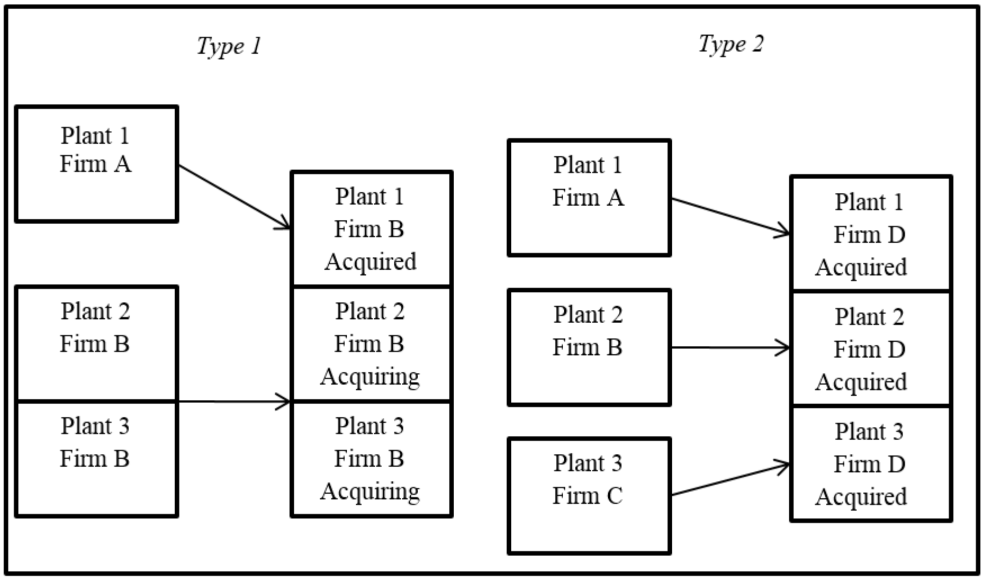
The total sample includes 1,980 plant-year observations. Since changes in price and productivity are the dependent variables of interest, the sample is limited to plants with both price and quantity in year t and year $t + 5$ (denoted as t'). The variable *ACQUIRED ALL* refers to the total number of plants undergoing an identifiable ownership change as indicated by a change in

[4]An alternative measure of productivity, labeled TFPT by Foster et al. (2008), uses plant level revenue as opposed to product specific revenue. Using this nomenclature, much of the classic literature on mergers and productivity relies on TFPT as plant level revenue is more readily available than product specific revenue. I find that both TFPR and TFPT are inflated from price increases associated with horizontal merger activity, but that the exaggeration of productivity is much larger using TFPR. Although a somewhat minor point, it is worth noting that this can be taken as additional evidence that the price increases are the result of enhanced market power. The inflation of revenue is restricted to revenue derived from the sale of ready-mix concrete as opposed to revenue related to other income sources.

[5]Given the preliminary nature of these results, to facilitate the disclosure of updated results in the future I have rounded all counts to the nearest multiple of 20.

the Firm ID variable between year t and t'. Horizontal mergers in the data take two forms which are depicted schematically in Figure 2.

Figure 2: Horizontal Mergers



In the Type 1 merger, Firm B exists both before and after the merger. When Plant 1 is purchased, it takes on the Firm ID "B," while Plant 2 and Plant 3 maintain the Firm ID "B." Thus, Plant 1 is labeled as "acquired" because its Firm ID changes. Plant 2 and Plant 3 are clearly involved in the merger but do not experience a change in Firm ID and are consequently labeled "acquiring" plants. In the Type 2 merger, no plant is labeled as an "acquiring" plant because all of the plants involved experience a change in Firm ID. The subset of *ACQUIRED ALL* plants that fit either of the patterns indicated above are labeled *ACQUIRED HORIZONTAL*. Plants that are part of firms that are involved in the acquisition of at least one plant but do not experience a change in Firm ID as indicated in the Type 1 merger are labeled as *ACQUIRING*.

A theme of this study will be assessing how the distinction between acquiring and acquired plants affects merger dynamics and outcomes. In Table 2, I begin this process examining the extent to which there are important differences between *ACQUIRED HORIZONTAL*, *ACQUIRING*, and non-merging plants pre-merger.

In Table 2, I consider the relationship between plants involved in horizontal merger activity and initial revenue, quantity, price, and TFPQ by regressing each variable against the *ACQUIRED HORIZONTAL* and *ACQUIRING* plant dummies and sweeping out EA-year effects. Each observation represents a plant-year combination. The most striking result of this table is

Table 2: Pre-Merger Characteristics of *ACQUIRED HORIZONTAL/ACQUIRING* Plants

Dep. Var.	[2.1] *REVENUE*	[2.2] *QUANTITY*	[2.3] *PRICE*	[2.4] *TFPQ*
ACQUIRED HORIZONTAL	−0.017	−0.010	−0.007	−0.007
	(0.129)	(0.133)	(0.017)	(0.028)
ACQUIRING	−0.061	−0.075	0.014	0.064***
	(0.093)	(0.095)	(0.019)	(0.024)
R-Squared	0.399	0.397	0.454	0.405
N	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for EA-year interactions. Standard errors are clustered by CEA. Dependent variables represent lagged values.

that for horizontal merger activity (defined in aggregate without geographic distinction) there are no significant pre-merger distinctions between plants except that *ACQUIRING* plants have above average productivity. This result is particularly interesting in light of the firm dynamics literature (Jovanovic, 1979, 1982; Jovanovic and Rousseau, 2002), which predicts a high productivity buys low productivity dynamic as well-managed buyers purchase poorly-managed sellers to reallocate capital. Here, I find evidence that the *ACQUIRING* plants are indeed high productivity, but that the *ACQUIRED HORIZONTAL* plants are of average, rather than low, productivity. The results presented in the next section will help shed further light on these patterns.

Because of the local nature of ready-mix concrete markets, distinguishing between local and non-local merger activity is a potentially important source of variation. I define local merger activity in terms of adjacent county blocks or ACBs. Specifically, for a given horizontally acquired plant, the plant is defined as *ACQUIRED HORIZONTAL ACB* if and only if within the ACB surrounding the plant there is at least one other acquiring or acquired plant associated with the merger. The acquiring plants that are associated with within ACB mergers according to the above definition are denoted as *ACQUIRING ACB*. Table 3 examines the geographic pattern of merger activity by comparing within ACB mergers to within CEA horizontal mergers, within EA horizontal mergers, and horizontal mergers defined with no geographic limitations.

A number of patterns are evident in Table 3. First, ready-mix concrete acquisitions are highly clustered within relatively small geographic areas such that the vast majority of acquired plants are located in at least the same EA as another plant involved in the merger. Indeed, most acquired plants are even more locally situated. On the other hand, most acquiring plants lie outside of

Table 3: Geographic Pattern of Horizontal Merger Activity

	ALL	EA	CEA	ACB
ACQUIRED HORIZONTAL	200	180	160	160
ACQUIRING	220	80	60	20

the areas where merger activity is taking place. To a large extent this distinction reflects that fact that for a given acquiring plant within a geographic area there are often multiple acquired plants. Another related issue, is that in a Type 2 merger as defined above, there need not be an acquiring plant, so that clusters of acquired plants can be assembled within a geographic area without the presence of an acquiring plant. Taken as whole, these patterns provide some initial evidence that ready-mix concrete firms engage in carefully selected, highly targeted merger behavior that involves clustering acquired plants in close geographic proximity.

2 Methodology and Results

2.1 Descriptive Results

I begin this section with an essentially descriptive analysis that relates changes in the dependent variables of interest to horizontal merger activity. Specifically, for plant i at time t in EA e, I consider the model

$$\Delta Y_{it} = \beta_0 + \beta_1\, ACQUIRED_{it} + \beta_2\, ACQUIRING_{it} + \lambda_{et} + \epsilon_{it} \tag{2}$$

restricting the acquired and acquiring variables to only within-ACB mergers (*ACQUIRED HORIZONTAL ACB* and *ACQUIRING ACB*). The only controls are a full set of EA-year interactions denoted by λ_{et}. Standard errors are clustered at the CEA level, which will also be the case in all of the analyses below.[6] Because evaluating the consumer welfare impact of mergers is the focus of this study, all results are also quantity weighted. Specifically, I use Davis et al. (1996) activity weights which are calculated as the average of the year t and year t' quantity sold for each plant. In Appendix B, I present unweighted results as a robustness check. The pattern

[6] All results and conclusions are extremely similar if clustering is done at the EA level as opposed to the CEA level. I have thus chosen to cluster at the CEA level following the previous ready-mix concrete literature.

of results in both the weighted and unweighted analyses is economically very similar, although the coefficient estimates and the level of statistical significance tend to be higher for the weighted results.

Table 4 presents the results from estimating the descriptive model with changes in prices, quantity, and TFPQ as the dependent variables.

Table 4: Descriptive Results

Dep. Var.	[4.1] $\Delta PRICE$	[4.2] $\Delta QUANTITY$	[4.3] $\Delta TFPQ$
ACQUIRED HORIZONTAL ACB	0.068*** (0.019)	−0.106 (0.069)	0.087*** (0.032)
ACQUIRING ACB	0.039 (0.066)	−0.057 (0.184)	0.097 (0.085)
R-Squared	0.377	0.541	0.347
N	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for EA-year interactions. Standard errors are clustered by CEA.

Regression [4.1] indicates a price increase of approximately 7% for *ACQUIRED HORIZONTAL ACB* plants significant at the 1% level. The estimated price increase at *ACQUIRING ACB* plants is approximately 4% but is not statistically significant. Regression [4.2] indicates a quantity decrease of over 10% approaching significance at the 10% level for *ACQUIRED HORIZONTAL ACB* plants. Regression [4.3] indicates an increase in TFPQ for *ACQUIRED HORIZONTAL ACB* plants of approximately 9% significant at the 1% level and an increase for *ACQUIRING ACB* plants of over 9% which is not statistically significant.

2.2 Causality

Moving from a descriptive to a causal analysis of merger activity is inherently challenging as there are many possible sources of selection that may induce merger activity. Thus, one way to interpret the subsequent results is simply as a series of analyses establishing a robust pattern comparing the average change in price/quantity/TFPQ for merging plants to the average change for all other plants. However, as a causal interpretation is the primary goal of merger retrospective studies, I proceed by considering how the CM data can help address sources of selection that are typically difficult to control for when studying merger activity.

The primary tool I use to address the issue of selection is the rich set of plant specific controls available through the CM. Many of these variables, including input expenditures and variables like TFPR or revenue, are endogenous to the firm's profit maximization problem. Thus, they will likely be correlated with factors that are otherwise difficult to control for, like quality, plant capacity, and financial health. To illustrate how the controls, in particular these lagged endogenous variables, can be applied to help mitigate selection, consider the following simple model. Suppose that in the absence of any changes in market structure, the level of prices for plant i at time t in geographic region m is set according to the linear model

$$p_{it} = X_{it}\gamma + Z_{mt}\theta + \eta_{it} \tag{3}$$

where p_{it} is price, X_{it} is vector of plant specific variables, and Z_{mt} is a vector of market level factors influencing demand. Since we are interested in the relationship between changes in price and merger activity, this price setting process motivates the following model relating the average price effect of merger activity to the first difference of price

$$\Delta p_{it} = \beta M_{it} + X_{it-1}\gamma + \Delta Z_{mt}\theta + \Delta\eta_{it} \tag{4}$$

where M_{it} represents a merger and X_{it-1} is now the lag of the vector of plant specific variables influencing price.[7] In using variables endogenous to the plant's profit maximization problem to identify the price effect of merger activity one would not want to control for ΔX_{it}, as including post-merger realizations of the plant specific variables could confound estimation of merger specific price effects (Wooldridge, 2010). On the other hand, because the endogenous variables in X_{it-1} are realized prior to the consummation of a merger, they will likely account for sources of unobserved heterogeneity that may create selection bias. Thus, the net effect of mergers on price will be identified if $\Delta\eta_{it}$ is conditionally independent of M_{it} after controlling for X_{it-1} and ΔZ_{mt}. Before moving on, however, it is important to note that there are specific timing assumptions implicit in this model. For instance, the model above assumes that selection into merger activity is based on the level of the lagged variables in X_{it-1}. But, if, for instance, changes in service quality are what drive selection rather than the level of service quality, controlling for the lagged

[7] For the sake of simplicity, in this section I abstract from the potential differences between acquired and acquiring plants.

differences of the endogenous variables may represent a more appropriate control than the levels of the endogenous variables. Furthermore, the model above assumes that that the plant characteristics inducing selection are fully present at time t. But, as the data are only observed at five year intervals, it is possible that the controls will not be as effective for mergers occurring later in each five-year period as there is unobserved heterogeneity in within each time period between observations. Thus, in presenting the results after applying my control strategy, I also discuss additional analyses that suggest that the results are robust to concerns about timing.

Of course, even taking the structure of this model as given, conditional independence is a very strong assumption. To see how selection may confound a causal interpretation of the results, consider the following examples. While as a physical product ready-mix concrete is quite homogenous, ready-mix concrete plants can differentiate themselves by providing superior service.[8] Suppose that high-quality plants are able to charge higher prices as a result of improved service, but that the full potential for price increases is realized with a lag as it takes time for the market to learn about quality advantages. If firms looking to make acquisitions target high-quality plants, then it is possible mergers will be associated with price increases, but not as a result of acquisitions per se. As another example, suppose that plants that have limited productive capacity are more likely to raise prices in the presence of demand shocks as their ability to increase output will be constrained.[9] If firms anticipating positive demand shocks in a region target capacity constrained plants, then post-merger prices may rise, but again for reasons unrelated to mergers themselves. Thus, in the next section I conduct a detailed analysis of the control strategy and the extent to which it helps support a causal interpretation of the results. In particular, I examine how the controls can help address selection stories like these and a host of related threats to my identification strategy.

2.3 Selection on Observables

While the controls that I have are rich relative to the previous literature, given the myriad of selection stories that are possible, arriving at a plausibly causal interpretation requires careful examination of how the underlying results are affected by the controls. I show in this section

[8]In my discussions with industry participants, service quality is typically offered as the primary differentiating factor among ready-mix concrete providers.

[9]I thank Dan Hosken for suggesting this example.

that while the controls I apply are often powerful predictors of the dependent variables, not only do all of the effects reported above remain statistically significant, but the magnitudes remain very similar as well. Indeed, to the extent adding controls has any appreciable effect, the overall results tend to become stronger.

Table 5 considers the effects of first controlling for lagged TFPR by itself and then adding controls for the lagged inputs *EQUIPMENT*, *STRUCTURE*, *LABOR*, *MATERIALS*, and *ENERGY* for each of the dependent variables from Table 4. As TFPR is a function of both revenue and efficiency, high TFPR firms will tend to be high profit firms. Accordingly, controlling for TFPR can be thought of as controlling for selection on profitability.

Table 5: Results Controlling for Lagged Endogenous Variables

Dep. Var.	[5.1] $\Delta PRICE$	[5.2] $\Delta PRICE$	[5.3] $\Delta QUANTITY$	[5.4] $\Delta QUANTITY$	[5.5] $\Delta TFPQ$	[5.6] $\Delta TFPQ$
ACQUIRED HORIZONTAL ACB	0.061*** (0.019)	0.062*** (0.019)	−0.117* (0.069)	−0.118* (0.068)	0.061*** (0.028)	0.058** (0.028)
ACQUIRING ACB	0.036 (0.064)	0.041 (0.066)	−0.063 (0.182)	−0.052 (0.160)	0.081 (0.054)	0.090 (0.055)
TFPR	−0.140*** (0.040)	−0.156*** (0.042)	−0.264*** (0.097)	−0.270*** (0.091)	−0.631*** (0.060)	−0.652*** (0.062)
EQUIPMENT		−0.002 (0.007)		−0.031 (0.034)		0.006 (0.013)
STRUCTURE		−0.012*** (0.004)		0.029 (0.020)		−0.008 (0.008)
LABOR		−0.021* (0.012)		0.012 (0.039)		−0.025 (0.017)
MATERIALS		0.023* (0.012)		−0.195*** (0.035)		0.011 (0.016)
ENERGY		0.006 (0.006)		0.012 (0.016)		−0.002 (0.008)
R-Squared	0.393	0.400	0.545	0.582	0.507	0.511
N	1,980	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for EA-year interactions and include quantity weights. Standard errors are clustered by CEA. Additional controls are lagged TFPR (*TFPR*), lagged capital equipment (*EQUIPMENT*), lagged structural capital (*STRUCTURE*), lagged labor input (*LABOR*), lagged materials input (*MATERIALS*), and lagged energy input (*ENERGY*).

Lagged TFPR is a strong predictor of each dependent variable and is significant at the 1% level in all regressions in Table 5. Nevertheless, as indicated in regression [5.1], the coefficient estimate for the price increase at *ACQUIRED HORIZONTAL ACB* plants remains over 6% and is significant at the 1% level. The economic significance of the estimated quantity decrease for

ACQUIRED HORIZONTAL ACB plants in [5.3] remains similar to that from the descriptive model, but as the coefficient is slightly larger in magnitude it is now statistically significant at the 10% level. Controlling for lagged TFPR has strongest effect when the dependent variable is the change in TFPQ. The coefficient estimate remains substantial and significant at the 1% level but is now approximately 6%. Across all regressions the coefficients on the *ACQUIRING ACB* dummies remain non-significant and of similar magnitudes to the results from Table A1. Regressions [5.2], [5.4], and [5.6] add the additional lagged endogenous input variables. As these variables are chosen as part of each plants profit maximization problem, they are set with respect to precisely the sort of unobserved factors that may induce problematic selection.[10] Yet, despite being individually significant predictors of price and quantity effects (although not TFPQ), inclusion of these variables has very little effect on the merger-related coefficient estimates.

Returning to the capacity story from the previous section, we might be concerned that the combination of capacity constraints and demand shocks could create a spurious correlation between mergers and prices. However, as structural and to some extent equipment capital will reflect plant capacity, the lack of movement in the coefficients after controlling for these observed inputs suggests that this source of selection is not driving the results. Or, in terms of the service quality story from the previous section, we might be concerned that the descriptive results attribute price increases to mergers because firms target high quality providers.[11] The idea behind the control strategy is that initial unobserved heterogeneity in quality will be reflected in the lagged endogenous variables. Specifically, using the lagged values of the input variables seems like a potentially effective strategy as firm's input choices will likely be linked to unobserved heterogeneity in quality. Furthermore, it seems highly plausible that at least some of the benefits of providing high quality service will be realized in the short-run. While this connection is less direct than the application of initial capital to control for capacity constraints, the essential point is that at least some significant proportion of unobserved product quality is likely to be reflected in these variables. As such, to the extent that this source of selection is driving the

[10] The rationale for including these variables is based on the same unobserved heterogeneity that has driven the literature on estimating production functions.

[11] In terms of addressing the question of the appropriate timing of the control variables, it is unclear from a theoretical standpoint whether it is better to take advantage of the larger amount of cross-sectional variation associated with using lagged levels or lagged differences, which require plants to have at least 10 years of data. However, as I discuss below, from a practical standpoint, the distinction is not important here as the results are very similar under either strategy.

results, one would expect to see substantial movement in the coefficient estimates.[12] But even after controlling for lags of these endogenous variables that are likely to be strongly correlated with a number of different sources of selection, the results remain strongly robust.

Table 6 continues the process of adding control variables likely to be associated with unobserved plant heterogeneity.

Table 6: Benchmark Results

Dep. Var.	[6.1] $\Delta PRICE$	[6.2] $\Delta PRICE$	[6.3] $\Delta QUANTITY$	[6.4] $\Delta QUANTITY$	[6.5] $\Delta TFPQ$	[6.6] $\Delta TFPQ$
ACQUIRED HORIZONTAL ACB	0.075*** (0.018)	0.079*** (0.019)	−0.119* (0.067)	−0.113* (0.069)	0.064*** (0.023)	0.058** (0.023)
ACQUIRING ACB	0.064 (0.057)	0.065 (0.058)	−0.081 (0.157)	−0.125 (0.148)	0.033 (0.041)	0.022 (0.040)
TFPQ	0.309*** (0.045)	0.307*** (0.045)	−0.403*** (0.114)	−0.408*** (0.112)	−0.842*** (0.074)	−0.838*** (0.074)
REVENUE	−0.240*** (0.039)	−0.237*** (0.038)	−0.066 (0.072)	−0.099 (0.075)	0.034 (0.034)	0.019 (0.035)
MU		−0.020 (0.016)		−0.029 (0.037)		0.014 (0.016)
AGE		0.001 (0.002)		−0.005 (0.008)		−0.004 (0.003)
CONSTRUCTION		0.057 (0.053)		0.470*** (0.144)		−0.028 (0.050)
DENSITY		0.002 (0.005)		0.065*** (0.019)		0.014* (0.007)
R-Squared	0.455	0.457	0.589	0.600	0.608	0.612
N	1,980	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for equipment capital, structural capital, labor input, materials input, energy input, EA-year interactions and include quantity weights. Additional controls are lagged TFPQ (*TFPQ*), lagged revenue (*REVENUE*), multi-unit status (*MU*), age (*AGE*), change in construction employment (*CONSTRUCTION*), and population density (*DENSITY*). Standard errors are clustered by CEA.

In regressions [6.1], [6.3], and [6.5], the TFPR control is removed and replaced with separate controls for lagged TFPQ and lagged revenue. Separating TFPR into supply and demand side controls allows for the possibility that selection on efficiency might be a distinct source of bias in addition to selection on financial status. Lagged TFPQ is a strong and highly significant predictor of each dependent variable, while revenue has a large and significant effect on the change in price,

[12]To frame this argument differently, had I found significant movement in the coefficients, I would not argue that I had effectively controlled for all of the unobserved heterogeneity. Rather, this would be indicative that the potential influence of the remaining unobserved heterogeneity would be too great to arrive at a plausibly causal interpretation.

but not the change in quantity or TFPQ. As far as effects on the merger variables of interest, these controls create a slight increase in the estimated price increase for acquired plants with an estimated effect of over 7%. The estimated price effect for acquiring plants increases more substantially to over 6% but remains statistically insignificant. The coefficient estimates for [6.3] and [6.5] remain very similar, with the exception of the relationship between TFPQ and acquiring plants which remains insignificant and is now also of a much smaller magnitude.

Regressions [6.2], [6.4], and [6.6] add controls for multi-unit status and age and also CEA-level demand controls for the change in construction employment and population density. Multi-unit status and age are frequently used as controls in research using Census microdata, and age has been shown to be a particularly important predictor of establishment level growth (Haltiwanger et al., 2013). Nevertheless, both variables have almost no effect on the dependent variables. It is important to note, however, that before inclusion of the lagged endogenous variables, age has a statistically significant effect on each of the dependent variables. The additional demand controls are not significant predictors of changes in price, although it bears emphasis that in the absence of the EA-year interaction, construction is a very strong and significant predictor of changes in price. On the other hand, both demand controls are strong predictors of changes in quantity and population density has a modest and significant effect on changes in productivity. Again, the conclusion remains the same. Despite the addition of these additional control variables, the estimates remain very similar across each dependent variable.

The robustness of the relationship between mergers and the dependent variables is the first piece of evidence offered in support of a causal interpretation of the results from this paper. Of course, there remain a number of potential threats to a causal interpretation that must be acknowledged. Some of these threats are addressed in additional analyses not included here for the sake of brevity. For instance, one might be concerned that the proper control variables for this analysis are changes in the lagged endogenous variables rather than levels. Implementing this strategy requires dropping a significant number of observations as it necessarily restricts analysis to a sub-sample of plants with 10 years of data and also requires that the first plant-year observation must be dropped. Thus, in my primary analysis, I employ lagged levels. Nevertheless, the results remain very similar if lagged differences are implemented with the necessarily reduced sample.[13] In fact, the estimated price effects are slightly larger.[14]

Another concern is measurement error, which could be amplified by the use of lagged endogenous control variables. However, as the results are very similar before and after adding revenue and independent variables, it is unlikely that measurement error is a major confounding factor. In addition, I have performed the analysis above instrumenting for the lagged input and revenue variables with the double lag of each variable. Again, the results remain very similar. This is unsurprising, as it is consistent with the findings of previous research using this data (Foster et al., 2008).

Even with these results, the case for a causal interpretation would be significantly stronger with evidence suggesting that the observed price increases are the result of market power. Thus, in the next section I address the question of market power using two related approaches. First, I refine my comparisons of the different categories of plants to distinguish between types of mergers likely to be associated with market power. Second, I consider the overall pattern of results and whether this is consistent with a market power interpretation. For instance, one of the most compelling pieces of evidence in favor of a market power interpretation is one I have already presented evidence for and will continue to develop: that price increases are accompanied by decreases in output at acquired plants. The benchmark results suggest that an approximately 8% increase in price is associated with an over 11% decrease in quantity sold. Because, as emphasized above, higher quality is primarily a function of superior service rather than physical attributes, offering a higher quality product will be unlikely to change the amount of ready-mix concrete necessary for a project. Consequently, evidence of price increases unaccompanied by decreases in output suggest a market power effect rather than merger specific changes in quality. In addition to this test, I examine price effects at plants not engaged in local merger activity, the initial pricing conditions that precede merger activity, and the timing of the price effects relative to when mergers are consummated.

[13]Another potential problem discussed in the previous section is that the controls may be less effective in controlling for selection the later a merger occurs in five-year period between observations. Thus, I have also conducted analysis considering the robustness of the results based on the timing of mergers. I find that regardless of when mergers take place, the magnitudes and significance levels remain very similar before and after implementation of the control strategy.

[14]The likely reason for an increase in the estimated price effects using lagged differences is that my sample is necessarily restricted to plants during the period from 1982 to 1992, which as shown in Table 10 below, are associated with higher prices when controlling for lagged levels as well.

2.4 Market Power

Table 7 assesses changes in price and quantity for within ACB mergers versus horizontal mergers lacking a horizontal component using the full set of controls from Table 6. Acquired and acquiring plants associated with non-local horizontal merger activity are denoted as *ACQUIRED HORIZONTAL OUT* and *ACQUIRING OUT* respectively.

Table 7: Local Versus Non-Local Horizontal Merger Results

Dep. Var.	[7.1] $\Delta PRICE$	[7.2] $\Delta PRICE$	[7.3] $\Delta PRICE$	[7.4] $\Delta PRICE$	[7.5] $\Delta QUANTITY$	[7.6] $\Delta QUANTITY$
ACQUIRED HORIZONTAL ACB	0.082*** (0.021)	0.100*** (0.022)	0.107*** (0.025)	0.125*** (0.025)	−0.126* (0.076)	−0.170** (0.072)
ACQUIRED HORIZONTAL OUT	0.008 (0.034)	0.009 (0.034)	0.000 (0.034)	0.000 (0.035)	−0.037 (0.180)	−0.049 (0.189)
ACQUIRING ACB	0.068 (0.059)	0.073 (0.060)	0.089 (0.061)	0.093 (0.062)	−0.135 (0.153)	−0.163 (0.146)
ACQUIRING OUT	0.011 (0.020)	0.028 (0.020)	0.012 (0.020)	0.030 (0.020)	0.011 (0.075)	−0.027 (0.075)
NON-MERGING ACB			0.030* (0.018)	0.030* (0.016)	−0.018 (0.067)	−0.015 (0.065)
$\Delta TFPQ$		−0.265*** (0.042)		−0.265*** (0.043)		0.592** (0.083)
R-Squared	0.458	0.488	0.459	0.489	0.600	0.621
N	1,980	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ or lagged change in TFPQ ($\Delta TFPQ$), lagged revenue, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

Regression [7.1] indicates an increase in price at *ACQUIRED HORIZONTAL ACB* plants of 8.5% ($e^{0.082} = 0.085$) significant at the 1% level. The estimated price increase for *ACQUIRED HORIZONTAL OUT* plants is close to zero and not significant. Equality of the coefficients is rejected at the 1% level and this holds across all regressions in Table 7, indicating that all systematic evidence of price increases at acquired plants is associated solely with local merger activity.

In regression [7.2], the control for lagged TFPQ is replaced with a control for the concurrent change in TFPQ. The purpose of this specification is to isolate the gross price increase associated with horizontal merger activity holding the effect of increased productivity constant.[15] The

[15] In employing the change in TFPQ as a control, I am assuming that productivity is not endogenous to the

coefficient on the *ACQUIRED HORIZONTAL ACB* variable indicates a gross price increase of 10.5% with almost no change in the coefficient estimate for *ACQUIRED HORIZONTAL OUT* plants. As indicated by the coefficient on the $\Delta TFPQ$ variable, the pass-through elasticity of TFPQ with respect to price is -0.265 and is highly significant. Thus, while the approximately 6% increase in productivity from [7.6] puts some downward pressure on price, the pass-through rate of productivity is small enough to leave ample room for productivity and price increases to co-exist.

In regressions [7.3] and [7.4], the net and gross price effects are re-estimated adding an additional variable representing non-merging plants located in ACBs that are characterized by within ACB merger activity (denoted as *NON-MERGING ACB*). Both regressions indicate a price increase of just over 3%, significant at the 10% level at *NON-MERGING ACB* plants. The addition of this control substantially amplifies the estimated price increase associated with *ACQUIRED HORIZONTAL ACB* plants to 11.3% and 13.3% respectively. Using the same net and gross specifications in regressions [7.5] and [7.6] indicates decreases in quantity sold of approximately -12.5% and -16% respectively. However, the standard errors for quantity are substantially higher than those for prices so that these effects are significant at the 10% and 5% levels individually, and I cannot reject the equivalence of the *ACQUIRED HORIZONTAL ACB* and *ACQUIRED HORIZONTAL OUT* coefficients. Nevertheless, estimated decreases in quantity are much smaller at *ACQUIRED HORIZONTAL OUT* plants.

This evidence supports interpreting the price effects associated with merger activity as caused by the creation of additional market power. Acquired plants associated with local mergers are associated with large and significant increases in price and decreases in output, but horizontal mergers lacking a local component indicate no evidence of such effects. Furthermore, there are small but significant price increases at non-merging plants located near merging plants which is what theory would predict in the context of differentiated Bertrand competition where mergers increase market power. The evidence for acquiring plants is more ambiguous. For instance, the estimated price increases for *ACQUIRING ACB* plants are substantially larger than the price increases for *ACQUIRING OUT* plants and the coefficient estimate for *ACQUIRING ACB* plants in regression [7.4] approaches significance at the 10% level. Yet, no point estimate for

firm's profit maximization problem or, in other words, the only merger specific price effect on plants from changes in TFPQ is through the dual relationship between TFPQ and marginal cost.

acquiring plants actually attains significance. Table 8 thus provides additional analysis to help better explain the pattern of pricing behavior at acquiring plants.

Table 8 revisits the gross and net price regressions from the previous table replacing the control for the lagged level of revenue with a control for the lagged level of price. While both are controls for plant specific demand conditions, controlling for lagged price amounts to looking at the effects of merger activity holding initial price constant and thus abstracts from the role that initial prices play in the consequences of merger activity.

Table 8: Results Controlling for Lagged Price

Dep. Var.	[8.1] $\Delta PRICE$	[8.2] $\Delta PRICE$	[8.3] $\Delta PRICE$	[8.4] $\Delta PRICE$
ACQUIRED HORIZONTAL ACB	0.067*** (0.023)	0.080*** (0.023)	0.068*** (0.025)	0.083*** (0.025)
ACQUIRED HORIZONTAL OUT			0.004 (0.029)	0.006 (0.033)
ACQUIRING ACB	0.062* (0.033)	0.076** (0.038)	0.063* (0.034)	0.078** (0.039)
ACQUIRING OUT			0.004 (0.021)	0.009 (0.019)
$\Delta TFPQ$		−0.157*** (0.028)		−0.158*** (0.028)
R-Squared	0.558	0.590	0.558	0.590
N	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ or lagged change in TFPQ ($\Delta TFPQ$), lagged price, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

As regressions [8.1] and [8.2] indicate, adding lagged price has very interesting consequences relative to the results from the previous table. Although the estimated net and gross price effects for *ACQUIRED HORIZONTAL ACB* plants remain large and highly significant at 6.9% and 8.3% respectively, the magnitudes are notably smaller than in the previous table. On the other hand, the price increases for *ACQUIRING ACB* plants of 6.4% and 7.9% are now significant at the 10% and 5% level so that after controlling for lagged price, the change in price estimated for acquiring and acquired plants converges to a very similar magnitude. Furthermore, as indicated by regression [8.3] and [8.4] the estimated price effects for both *ACQUIRED HORIZONTAL*

OUT and *ACQUIRING OUT* plants are very close to zero. And, in all cases, I can reject the equivalence of the coefficients for both acquired plants and acquiring plants. As to whether the estimates from Table 7 or Table 8 are more useful, the answer largely depends on both the underlying interpretation of the results and the context in which the results are to be applied. Thus, in Table 9, I consider an analysis of initial pricing that is helpful for interpreting the pattern of the results and framing them in terms of the consumer welfare implications.

Table 9: Initial Price Results

Dep. Var.	[9.1] *PRICE*	[9.2] *PRICE*
ACQUIRED HORIZONTAL ACB	−0.055** (0.026)	−0.050* (0.030)
ACQUIRED HORIZONTAL OUT		0.030 (0.027)
ACQUIRING ACB	0.045* (0.027)	0.052** (0.031)
ACQUIRING OUT		0.017 (0.024)
R-Squared	0.547	0.548
N	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for concurrent TFPQ, multi-unit status, age, change in construction employment, population density, EA-year interactions, and include quantity weights. Standard errors are clustered by CEA. Dependent variable is lagged price.

Regressions [9.1] and [9.2] now apply an alternative specification where the dependent variable is initial price and I restrict attention to mergers from the period from 1982 to 1992.[16] Controls are limited to concurrent TFPQ, multi-unit status, age and EA-year effects. *ACQUIRED HORIZONTAL ACB* plants are associated with statistically significant below average prices and *ACQUIRING ACB* plants are associated with statistically significant above average prices. There is no statistically significant effect for either of the *OUT* treatment groups (and in the case of acquired plants we can reject equality of the coefficients at the 5% level) even though in many cases the same firms are often involved in both the local and non-local mergers.

Ultimately, the decision of which estimates to apply comes down to what one thinks to be

[16]I restrict analysis to the period from 1982 to 1992 for this analysis as it is more informative about the variance of the data and due to disclosure concerns, I cannot report the 1977 to 1982 results for within ACB mergers.

the appropriate counterfactual. As these estimates will be used as inputs into a structural model quantifying the welfare tradeoff between efficiency and price effects, the essential question is how to interpret the consumer welfare implications of the estimates. For instance, to the extent that the prices charged by the *ACQUIRED HORIZONTAL ACB* plants would have remained below average in the absence of mergers and that the price increases are driven by market power, then the entire net price increase of 11.3% from regression [7.3] represents a loss of consumer welfare. The notion that specific firms may play a special role in exerting downward pressure on prices and, thus, may be targeted for acquisition is a well-established and prominent concern in antitrust enforcement. The 2010 Horizontal Merger Guidelines note that mergers may pose a particular threat to competition when they "lessen competition by eliminating a 'maverick' firm, i.e., a firm that plays a disruptive role in the market to the benefit of customers." The evidence of price increases at non-merging plants is particularly interesting in light of the low prices initially charged by acquired plants.

On the other hand, if prices would have risen to the average level in the absence of mergers, then the price increase of 6.9% from regression [8.1] would be the appropriate input into the structural model. For acquiring plants, there is less of an issue as the coefficient estimates are similar between Table 7 and Table 8. For acquiring plants, the main advantage provided by the analyses in Table 8 is that the standard errors are smaller leading to more precise estimates. As a precaution, I will limit my structural analysis to only statistically significant price increases, so I will use the price increases for acquiring plants from regression [8.1]. For acquired plants, I will do the analysis both ways, using the 6.9% price increase as a conservative figure and the 11.3% price increase as a more aggressive estimate.

2.5 Temporal Variation

Table 10 quantifies the price effects of horizontal mergers over the period from 1977 to 1982 versus the period from 1982 to 1992. These time periods correspond to CM years that conveniently line up with the promulgation of the 1982 Horizontal Merger Guidelines, which marked the beginning of a period of significant change in antitrust regulation. By the mid-1980s, enforcement patterns indicate that antitrust regulators became substantially more permissive of merger activity.[17]

[17]It is beyond the scope of this paper whether policy towards horizontal mergers started changing in 1982 following the promulgation of the 1982 Merger Guidelines or in the middle of the decade. Here, what is important

However, as noted above, for disclosure reasons, I am not able to report results for within ACB mergers for the period from 1977 to 1982. For the purposes of this analysis, I extend consideration to all horizontal mergers. Fortunately, the price effects of horizontal mergers are prominent enough at acquired plants that I am still able to present informative results. However, price effects at acquiring plants become insignificant when local and non-local merger activity are pooled. Accordingly, I focus on the results for acquired plants in the next two tables.

Table 10: Pre- and Post-1982 Results

Dep. Var.	[10.1] $\Delta PRICE$	[10.2] $\Delta PRICE$	[10.3] $\Delta PRICE$	[10.4] $\Delta TFPQ$	[10.5] $\Delta TFPQ$	[10.6] $\Delta TFPQ$	[10.7] $\Delta TFPQ$
ACQUIRED ALL	0.021 (0.022)			0.074*** (0.022)			
*ACQUIRED ALL**77–82	−0.012 (0.036)			−0.042 (0.041)			
ACQUIRED HORIZONTAL		0.082*** (0.019)	0.072*** (0.020)		0.064*** (0.023)	0.074*** (0.023)	0.074*** (0.023)
*ACQUIRED HORIZONTAL**77–82		−0.134*** (0.045)	−0.121*** (0.047)		−0.122*** (0.041)	−0.124*** (0.042)	−0.123*** (0.040)
ACQUIRED NON-HORIZONTAL			−0.079** (0.036)			0.073 (0.049)	0.071** (0.036)
*ACQUIRED NON-HORIZONTAL**77–82			0.110** (0.042)			−0.007 (0.054)	
R-Squared	0.448	0.459	0.465	0.616	0.613	0.617	0.617
N	1,980	1,980	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ, lagged revenue, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

In each regression in Table 10, interaction variables with suffix *77–82 are added to the treatment variables of interest. These variables indicate the interaction between the treatment variable and the period from 1977–1982. Accordingly, the coefficient on the *ACQUIRED HORIZONTAL* variable now reflects the change in price at horizontally acquired plants for the period from 1982 to 1992. The effect for the period from 1977 to 1982 is then given by the addition of the coefficients on the *ACQUIRED HORIZONTAL* and the *ACQUIRED HORIZONTAL**77–82 variables. Regression [10.1] indicates that when I examine price changes for all acquired plants regardless of the type of merger (indicated by the variable *ACQUIRED ALL*), there are no significant price

is that there is broad evidence of a change in enforcement patterns by the mid-1980s and that this change started in or after 1982.

effects for either time period. However, the results change dramatically as soon as attention is restricted to horizontally acquired plants in regression [10.2]. For the period from 1982 to 1992, the estimated price increase is 8.5% and is highly significant. The estimate for the period from 1977 to 1982 is negative but not significant, and the difference between the estimated effects for 1977 to 1982 versus 1982 to 1992 is significant at the 1% level.

Regression [10.3] builds on [10.2] by adding a direct comparison of non-horizontal acquired plants before and after 1982. While the coefficient estimates for horizontally acquired plants remain similar to the previous regression, the results for non-horizontal acquisitions display the opposite pattern. Over the period from 1982 to 1992, *ACQUIRED NON-HORIZONTAL* plants are associated with an almost 8% decline in prices significant at the 5% level. These results provide additional evidence that the observed pattern of price increases are the result of market power. Not only is all systematic evidence of price increases restricted solely to horizontal mergers and only after the relaxation of antitrust in the mid-1980s, but, in addition, non-horizontal mergers are actually associated with price decreases emphasizing that a force unique to horizontal mergers is driving the observed effects.

As indicated by regressions [10.4]–[10.7], the pattern of results is quite different when changes in productivity are considered. Regression [10.4] indicates that the *ACQUIRED ALL* plants are associated with highly significant increases in productivity over the period from 1982 to 1992 and the effect remains of a similar magnitude when attention is restricted to horizontal acquisitions in regression [10.5]. Regression [10.6] indicates that for the period from 1982 to 1992 productivity increases at *ACQUIRED NON-HORZIONTAL* plants have almost exactly the exact same coefficient estimate as *ACQUIRED HORIZONTAL* plants, but that the estimate falls just below the level of statistically significance. However, as indicated by the *ACQUIRED NON-HORIZONTAL* interaction term, the difference in the coefficient estimate for non-horizontally acquired plants is essentially zero between 1977 to 1982 and 1982 to 1992. Thus, in regression [A7.7] the *ACQUIRED NON-HORIZONTAL* variable is pooled and now indicates a statistically significant increase in productivity of almost exactly the same magnitude as the effect at horizontally acquired plants from 1982 to 1992. Interestingly, the estimated effects for horizontally acquired plants are negative and insignificant across the board for the period from 1977 to 1982, suggesting that, at least for ready-mix concrete, it is difficult from a regulatory perspective to distinguish mergers that increase price from mergers that increase productivity.

Given that much of this section has focused on the market power interpretation of the price effects, I now consider the question of what underlying forces drive my productivity results. Three findings in particular provide strong evidence in support of a mechanism where productivity increases as productive assets are put in the hands of more capable managers. First, before mergers, acquiring plants are associated with above average productivity. Second, productivity increases are restricted to acquired plants, and third, the estimated productivity effects are similar for plants engaged in horizontal mergers versus non-horizontal mergers. Thus, the fundamental mechanism driving productivity increases appears to be one where more productive managers take less productive assets and raise them to a level of productivity commensurate with their own. What is important from a productivity perspective is not whether a merger is horizontal, vertical, or conglomerate but the new management's ability to identify opportunities to reallocate inputs to more productive uses.

Further evidence for how productive efficiencies are realized in the ready-mix concrete industry can be gleaned by looking at the effects of local versus non-local merger activity using TFPQ as the dependent variable instead of price as in Table 7. The outcome of this analysis is that all evidence of productivity increases at acquired plants is restricted to *ACQUIRED HORIZONTAL ACB* plants versus *ACQUIRED HORIZONTAL OUT* plants. This result is consistent with the strategies described by large concrete producers. For instance, Lafarge, a large, international, publicly traded company explained in a 2004 SEC filing that the company aims "to place our ready-mix concrete plants in clusters" in order to "optimize our delivery, flexibility, capacity, and backup capability" (Hortaçsu and Syverson, 2007). Yet, there still remains the question of exactly how productivity increases are realized within local concrete networks. Some exploratory analysis I have performed suggests that local mergers increase efficiencies by reducing plant level expenditure on labor and equipment capital, relative to structural capital, materials, and energy, holding quantity effects constant. This finding suggests that an interesting path for future research would be to relax the constant returns to scale structure imposed on the production function here and consider a more flexible form that can accommodate these stylized facts.

As a final analysis in this section, In Table 11, I examine how the results from Table 10 for mergers occurring between 1982 and 1992 vary with the timing of merger activity.

Although the CM does not indicate when mergers take place for each five-year interval, using the LBD, I am able to identify the year in which a given merger was consummated. Thus, Table 11

Table 11: Post-1982 Merger Activity by Merger Vintage

Dep. Var.	[11.1] $\Delta PRICE$	[11.2] $\Delta PRICE$	[11.3] $\Delta TFPQ$
ACQUIRED HORIZONTAL YR1	0.128***	0.147***	0.082**
	(0.035)	(0.039)	(0.037)
ACQUIRED HORIZONTAL YR2–YR5	0.061***	0.073***	0.056**
	(0.019)	(0.020)	(0.027)
*ACQUIRED HORIZONTAL*PRE*	−0.141***	−0.166***	−0.125***
	(0.041)	(0.038)	(0.041)
$\Delta TFPQ$		−0.268***	
		(0.042)	
R-Squared	0.461	0.491	0.613
N	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ or lagged change in TFPQ ($\Delta TFPQ$), lagged revenue, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

compares mergers consummated in the year prior to a CM year to mergers consummated between years two and five. Regressions [11.1] and [11.2] indicate that the price effects associated with merger activity are largest in the first year and begin to decrease after that. In both regressions, I can reject the equality of the year one cohort versus the year two through year five cohort at the 5% level. However, after this initial drop off in the first year, the rate at which the price effects fall decreases and the price increases associated with horizontal merger activity persist over the entire five-year period. On the other hand, for productivity, I cannot reject the equality of the year one cohort versus the year two through year five cohort. These results provide further evidence of a market power effect as one would expect entry and expansion by existing plants to attenuate price increases caused by market power over time. However, the fact that the price increases persist for multiple years is not surprising in light of the evidence that non-merging plants located nearby to merging plants also raise their prices and evidence from Collard-Wexler (2014) suggesting substantial barriers to entry in the ready-mix concrete industry.

3 Demand Estimation and Welfare Analysis

The above results strongly suggest that consumer surplus decreased as a consequence of horizontal mergers. However, because there are simultaneous increases in both prices and productivity at horizontally acquired plants, total welfare cannot be assessed without evaluating the tradeoff between these countervailing forces. In quantifying this tradeoff, I apply the framework introduced by Williamson (1968) taking into account the oligopolistic nature of the ready-mix concrete industry. Consequently, I proceed by estimating a simple aggregate data multinomial logit model with unobserved product characteristics following Berry (1994) to facilitate the calculation of welfare effects based on the regression estimates from the previous section.

As is standard, it is assumed that there are $j = 0, 1, \ldots, J$ products in $t = 1, \ldots, T$ markets each with $I = 1, \ldots, I_t$ consumers. Products $j = 1, \ldots, J$ represent competing differentiated ready-mix concrete options corresponding to each plant in a market. The alternative zero, represents an outside option corresponding to not purchasing any of the J products. Markets are defined as CEA-year combinations of size M_t and are observed at five-year intervals. The non-random portion of utility is determined by a plant level fixed effect x_j^{fe} and the price charged by the plant p_{jt}. Indirect utility for consumer i is:

$$u_{ijt} = x_j^{\text{fe}} - \alpha p_{jt} + \xi_{jt} + \epsilon_{ijt} = \delta_{jt} + \epsilon_{ijt} \tag{5}$$

where ξ_{jt} represents unobserved differences in product quality, and ϵ_{ijt} is a stochastic error term. Specifying utility in this way abstracts from the full richness of substitution patterns in the ready-mix concrete industry which are ultimately based on complex interactions between competing networks. However, the large number of plant level observations in the data allow for inclusion of the plant fixed effect which accounts for the fact that some plants are located in superior locations with better access to customers. Acknowledging the simplification of complex interactions inherent in this approach suggests that the welfare estimates should ultimately be interpreted as back of the envelope in nature. Nevertheless, because the Census data provides a rich context for estimating this demand system, my approach is likely to capture some important aspects of competition in the ready-mix concrete industry, providing insight into the direction and magnitude of the welfare impacts.

Estimating α from the equation above is the critical step for calculating consumer welfare in the multinomial logit model. For products $j = 1, \ldots, J$ the market share s_{jt} is calculated based on the amount of concrete sold (in cubic yards) relative to M_t with the remainder accounted for in the share of the outside good s_{0t}. Assuming that ϵ_{ijt} is IID according to the Type I extreme value distribution gives rise to the well-following equation relating α to observed market shares,

$$s_{jt} = \frac{e^{\delta_{jt}}}{\sum_{k=0}^{J} e^{\delta_{kt}}}. \tag{6}$$

From this step, one might be inclined to estimate α directly using a procedure like non-linear least squares, but since unobserved quality will likely be correlated with price, this approach is problematic. To deal with this endogeneity, Berry (1994) inverts the equation above so that α can be estimated from the linear equation:

$$\ln(s_{jt}) - \ln(s_{0t}) = x_j^{\text{fe}} - \alpha p_{jt} + \xi_{jt} \tag{7}$$

using two-stage least squares. Following Foster et al. (2008), I use $\ln(TFPQ_{jt})$ as an instrument and also control for CEA-level average income and year effects in estimating the equation above.

The final step required to estimate α is fixing the size of the market M_t. My approach involves using merger simulation as in Nevo (2000) and calibrating the market size so that the average predicted price increase at acquired plants matches the 11.3% price increase estimated in regression [7.3]. Specifically, for each market I begin by setting M_t as the maximum quantity of concrete sold in the CEA across all years. I then simulate the price effects for all of the horizontal mergers in my sample that create a change in CEA-level market structure. This approach predicts large price effects due to merger activity. Thus, I then increase the size of each market proportionally until the average price increase at acquired plants matches my estimated price increase for acquired plants.

With the size of the market fixed, demand estimation follows as described above. Table 12 presents the results.

Table 12 indicates that the results of this estimation procedure are quite reasonable. The average share of the outside both indicates the relative importance of concrete as a building material, while still allowing for substitution to alternative construction materials like steel or asphalt. Given the structure of the model, elasticity of demand for each plant is given by the

Table 12: Demand Estimation Results

N	Average Share Outside Good	α	Average Elasticity
11,600	0.268	-0.113^{***} (0.014)	-4.755^{***} (0.824)

formula $\eta_{jt} = -\alpha p_{jt}(1 - s_{jt})$. It is interesting and reassuring to note that the average elasticity estimated here is very similar to the elasticity of demand estimated using the linear model estimated in Foster et al. (2008).

On the supply side, I estimate each plant's marginal cost which is necessary to simulate the producer surplus effects of the observed mergers. Firms set plant level prices by maximizing the firm's profit across all of the plants in a given CEA. For a given plant j at time t, this gives rise to the first order condition:

$$s_{jt}(p) + \sum_{r \in F_f} (p_{rt} - c_{rt}) \frac{\partial s_{rt}(p)}{\partial p_{jt}} = 0 \tag{8}$$

where for each firm-CEA combination f, F_f represents the set of plants associated with the firm. By defining the matrix Ω such that $\Omega_{jr}(p) = -\partial s_{jt}(p)/\partial p_r$ if $\exists\, f : \{r, j\} \subset F_f$ and zero otherwise, the J first order conditions for a market can be written in vector notation as

$$s(p) - \Omega(p)(p - c) = 0 \tag{9}$$

so that marginal cost for each plant is given by

$$c = p - \Omega(p)^{-1} s(p)\,. \tag{10}$$

Using this procedure, the estimated average marginal cost is \$34.10 (1.25) per cubic yard.

To incorporate my regression estimates into the welfare analysis, I simulate the welfare effects of mergers by adjusting price and marginal cost for the relevant plants by the average values indicated by regression estimates. Following Small and Rosen (1981), the change in consumer surplus is given by applying the "logsum" formula:

$$\Delta CS_t = \frac{M_t}{\alpha}\left\{\ln\left[\sum_{j=1}^{J_t}\exp\left(\delta_{jt}\right)\right] - \ln\left[\sum_{j=1}^{J_t}\exp\left(\delta'_{jt}\right)\right]\right\} \tag{11}$$

where δ'_{jt} represents the simulated product-level component of utility. The change in producer surplus is calculated simply by adjusting price and marginal cost following the geometry of Williamson tradeoff model. The change in welfare is then given by:

$$\Delta W = \Delta PS + \Delta CS\,. \tag{12}$$

The welfare simulation results are summarized in Table 13.

Table 13: Welfare Simulation Results (1987 Dollars, Millions)

Price Effect	PS Gain	CS Loss	Net Welfare
acquired: 6.9% acquiring: none non-merging: none efficiencies: 6.0%	62.9 M	−54.3 M	8.6 M
acquired: 11.3% acquiring: none non-merging: none efficiencies: 6.0%	87.4 M	−97.0 M	−9.6 M
acquired: 11.3% acquiring: 6.4% non-merging: 3.0% efficiencies: 6.0%	140.3 M	−169.4 M	−29.1 M

The first row in Table 13 considers the tradeoff at acquired plants using the price increase for acquired plants from regression [8.1] which controls for lagged initial price. This specification is conservative in that it assumes that below average prices at acquired plants would have rebounded to the average level in the absence of merger activity. In essence, this approach abstracts from any maverick firm effect as discussed in the previous section. The results from the first row indicate that although the percentage price increase is larger than the percentage increase in productivity, the producer surplus gain outweighs the loss of consumer surplus so that net welfare increases slightly. On the other hand, if the full 11.3% price increase associated with acquired plants is used as an input into the model, then there is a small net welfare loss at acquired plants. Overall, I infer from these results that the producer surplus gains and consumer surplus losses at acquired plants essentially cancel out. However, when price increases at acquiring plants and non-merging plants are taken into account, the loss of consumer surplus increases dramatically to approximately $170 million (1987 dollars) so that there is a net welfare loss of approximately $30 million. To put the consumer surplus loss in perspective, this figure represents about 4% of commerce in ready-mix concrete markets affected by the horizontal mergers in my sample.

4 Conclusion

Overall, my results suggest price increases of about 7% to 11% at acquired plants associated with local merger activity accompanied by productivity increases of about 6%. Controlling for changes in productivity yields an estimated gross market power effect of between approximately 8.5% and 13%. The estimated price increase at acquiring plants associated with local merger activity is over 6%, and the estimated price increased at non-merging plants located in close proximity to merging plants is approximately 3%. Examining price effects for the set of all horizontally acquired plants before and after 1982 indicates no evidence of price increases for the period from 1977 to 1982, but price increases of approximately 8% for the period from 1982 to 1992. This large increase is in stark contrast to the approximately −7.5% decrease in prices associated with vertical and conglomerate mergers over the period. There is no evidence of productivity increases at horizontally acquired plants over the period from 1977 to 1982, but the estimated productivity increase is over 7% for the period from 1982 to 1992. Unlike the pattern for prices, the estimated productivity increase for non-horizontally acquired plants of around 7% is of a very similar magnitude to the effect for horizontally acquired plants.

As far as productivity is concerned, this is one of the first studies to distinguish the productivity effects of horizontal mergers from other types of mergers. The similarity of the productivity results across merger types provides new support for the growing literature that emphasizes the potential for mergers to reallocate productive assets from lower value to higher value uses (Hortaçsu and Syverson, 2007; Braguinsky et al., 2015). This reallocation and convergence mechanism is supported by the evidence I present indicating that acquiring plants have above average initial productivity and productivity increases are restricted to acquired plants. Overall, the results suggest a story where sophisticated managers bring their expertise to less sophisticated operations increasing productivity. Furthermore, the concentration of productivity effects in local markets suggests that the gains are ultimately realized through improved coordination of logistics between plants. In future research, it would be particularly interesting to better understand how these efficiencies are realized in terms of observable plant level behavior. Some initial exploration of the data suggests the highly interesting possibility that efficiencies are realized by reducing relative expenditure on labor and equipment capital as plants within an ownership network are better able to strategically deploy these resources.

These productivity increases at acquired plants are also accompanied by large price increases. Although increased productivity exerts significant downward pressure on prices, the rate at which productivity increases reduce prices is modest, leaving room for the creation of additional market power. Unlike productivity, price increases are not limited to acquired plants but are also observed at acquiring and non-merging plants located near horizontally merging plants.

The evidence strongly suggests that these price increases are the result of market power. Price increases are associated solely with mergers involving plants in close geographic proximity, only with horizontal mergers, and only after the relaxation of antitrust standards in the mid-1980s. Furthermore, there is evidence that when firms pursue mergers of plants in close proximity, they target firms charging below average prices. To the extent that in the absence of mergers, these plants would have continued to charge low prices putting downward pressure on the prevailing price level, these results may indicate that acquirers targeted maverick firms. Concern over the acquisition of maverick firms has long been a facet of the antitrust review process at agencies like the DOJ and the FTC, but the horizontal merger retrospective literature evidence has devoted little attention to this issue.

While the regression results strongly suggest consumer surplus declined as a result of horizontal merger activity, quantifying the total welfare affect requires considering the tradeoff between the producer surplus increasing effect of enhanced productivity and the consumer surplus decreasing effect of higher prices. My simulation results suggest while these effects essentially cancel out at acquired plants, the price increases at acquiring and non-merging plants ultimately lead to decline in total welfare as a result of horizontal merger activity. Furthermore, while the total welfare effect at acquired plants is minimal, my results also suggest that for productivity increases to offset price increases entirely at acquired plants would require extremely large productivity increases on the order of 30%. In addition, while there is some attenuation of the price increases over time, my results indicate that price increases persist alongside productivity increases as long as five years after the consummation of mergers and beyond. Thus, increases in efficiencies and the operation of market forces were not ultimately sufficient to ameliorate the welfare losses to consumers and society as a whole in this case study.

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A Propensity Score Adjusted Results

The identification of imputed observations in the CM by White et al. (2015) indicates that a significant number of the Foster et al. (2008) ready-mix concrete plant observations included imputed product level revenue or quantity data. In my primary analysis presented above, as in the previous literature, I dropped all imputed observations, including the newly identified imputations. However, recent papers using Census data have employed propensity score methods to assess the validity of the missing-at-random assumption implicit in the standard approach (Pierce, 2011; Davis et al., 2014). In this section, I subject my main results to inverse probability weighting using propensity scores to examine whether the patterns observed above are robust to selection issues in the data.

I construct propensity scores by fitting logit specifications for each time period where the dependent variable is an indicator of whether the observation is in the sample of continuing ready-mix concrete plants with product revenue and quantity data. I employ five specifications of the propensity score model. Each specification includes controls for plant size, plant age, and multi-unit status as employed in Davis et al. (2014) as well as the variables *ACQUIRED HORIZONTAL* and *ACQUIRING* to control for potential selection on the variables of primary interest in this study. Both Pierce (2011) and White et al. (2015) suggest that imputed observations may be associated with smaller plants. Furthermore, because inclusion in this study requires quantity data in two consecutive CM years, missing data in either year t or t' can cause an observation to be missing in my study. To account for both of these potential sources of missing data, I employ 5 different specifications of the propensity score model where each specification is distinguished by the functional form and point in time used for the plant size control, which is measured in terms of employment.

In specification 1, I include employment in year t' in addition to the other variables. In specification 2, I include employment and the square of employment in year t'. In specification 3, I include employment in year t. In specification 4, I include employment and the square of employment in year t. In specification 5, I include employment in both year t and t'.

Table A1 applies each propensity score specification to the benchmark price results from while Table A2 and Table A3 present the propensity score adjusted results for the benchmark quantity and TFPQ results. Table A4 presents the propensity score adjusted results for the local versus non-local horizontal merger analysis and Table A5 applies propensity scores to the results controlling for lagged price. The propensity score adjusted results indicate that both the pattern and magnitudes of the estimates remain quite similar to the primary results.

Table A1: Propensity Score Adjusted Benchmark Price Results

Dep. Var.	[A1.1] $\Delta PRICE$	[A1.2] $\Delta PRICE$	[A1.3] $\Delta PRICE$	[A1.4] $\Delta PRICE$	[A1.5] $\Delta PRICE$
ACQUIRED HORIZONTAL ACB	0.074*** (0.018)	0.073*** (0.018)	0.073*** (0.018)	0.072*** (0.018)	0.073*** (0.018)
ACQUIRING ACB	0.060 (0.059)	0.061 (0.058)	0.063 (0.060)	0.056 (0.057)	0.061 (0.059)
R-Squared	0.444	0.447	0.439	0.452	0.441
N	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ, lagged revenue, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

Table A2: Propensity Score Adjusted Benchmark Quantity Results

Dep. Var.	[A2.1] $\Delta QUANTITY$	[A2.2] $\Delta QUANTITY$	[A2.3] $\Delta QUANTITY$	[A2.4] $\Delta QUANTITY$	[A2.5] $\Delta QUANTITY$
ACQUIRED HORIZONTAL ACB	−0.146* (0.078)	−0.147* (0.082)	−0.147* (0.078)	−0.148* (0.078)	−0.148* (0.079)
ACQUIRING ACB	−0.085 (0.135)	−0.085 (0.138)	−0.082 (0.129)	−0.062 (0.128)	−0.086 (0.135)
R-Squared	0.621	0.641	0.619	0.671	0.624
N	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ, lagged revenue, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

Table A3: Propensity Score Adjusted Benchmark TFPQ Results

Dep. Var.	[A3.1] $\Delta TFPQ$	[A3.2] $\Delta TFPQ$	[A3.3] $\Delta TFPQ$	[A3.4] $\Delta TFPQ$	[A3.5] $\Delta TFPQ$
ACQUIRED HORIZONTAL ACB	0.044*	0.043*	0.046*	0.046*	0.044*
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
ACQUIRING ACB	0.011	0.014	0.016	0.018	0.011
	(0.044)	(0.044)	(0.043)	(0.044)	(0.044)
R-Squared	0.598	0.610	0.599	0.619	0.598
N	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ, lagged revenue, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

Table A4: Propensity Score Adjusted Local Versus Non-Local Horizontal Merger Results

Dep. Var.	[A4.1] $\Delta PRICE$	[A4.2] $\Delta PRICE$	[A4.3] $\Delta PRICE$	[A4.4] $\Delta PRICE$	[A4.5] $\Delta PRICE$
ACQUIRED HORIZONTAL ACB	0.087**	0.086**	0.087***	0.086***	0.086***
	(0.021)	(0.020)	(0.021)	(0.021)	(0.021)
ACQUIRED HORIZONTAL OUT	0.004	0.002	0.001	0.001	0.003
	(0.033)	(0.033)	(0.034)	(0.034)	(0.034)
ACQUIRING ACB	0.061	0.063	0.064	0.059	0.061
	(0.063)	(0.061)	(0.063)	(0.061)	(0.063)
ACQUIRING OUT	0.020	0.022	0.021	0.023	0.020
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
$\Delta TFPQ$	−0.269***	−0.266***	−0.268***	−0.262***	−0.271***
	(0.040)	(0.038)	(0.042)	(0.041)	(0.040)
R-Squared	0.468	0.469	0.463	0.475	0.466
N	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for the change in TFPQ ($\Delta TFPQ$), lagged price, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

Table A5: Propensity Scored Adjusted Results Controlling for Initial Price

Dep. Var.	[A5.1] $\Delta PRICE$	[A5.2] $\Delta PRICE$	[A5.3] $\Delta PRICE$	[A5.4] $\Delta PRICE$	[A5.5] $\Delta PRICE$
ACQUIRED HORIZONTAL ACB	0.070**	0.069**	0.072**	0.070**	0.070**
	(0.028)	(0.027)	(0.028)	(0.028)	(0.028)
ACQUIRED HORIZONTAL OUT	0.006	0.004	0.002	0.003	0.005
	(0.032)	(0.032)	(0.033)	(0.032)	(0.032)
ACQUIRING ACB	0.074*	0.075*	0.079*	0.074*	0.075*
	(0.042)	(0.041)	(0.042)	(0.042)	(0.042)
ACQUIRING OUT	0.006	0.008	0.005	0.006	0.005
	(0.020)	(0.019)	(0.020)	(0.020)	(0.020)
$\Delta TFPQ$	−0.170***	−0.168***	−0.171***	−0.165***	−0.172***
	(0.031)	(0.030)	(0.032)	(0.031)	(0.031)
R-Squared	0.565	0.568	0.561	0.569	0.563
N	1,980	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for the change in TFPQ ($\Delta TFPQ$), lagged price, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, EA-year interactions and include quantity weights. Standard errors are clustered by CEA.

B Unweighted Results

In this appendix, I provide unweighted results to demonstrate the robustness of my conclusions to weighting used in the primary results. Table B1 considers the unweighted benchmark results for price, quantity, and TFPQ.

Table B1: Unweighted Benchmark Results

Dep. Var.	[B1.1] $\Delta PRICE$	[B1.2] $\Delta QUANTITY$	[B1.3] $\Delta TFPQ$
ACQUIRED HORIZONTAL ACB	0.041** (0.019)	−0.097 (0.067)	0.074*** (0.024)
ACQUIRING ACB	0.040 (0.034)	−0.120 (0.131)	0.079 (0.050)
R-Squared	0.415	0.529	0.568
N	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ, lagged revenue, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, and EA-year interactions. Standard errors are clustered by CEA.

Overall, the direction and pattern of the results is quite similar before and after quantity weighting. However, the estimated change in price for *ACQUIRED HORIZONTAL ACB* plants in Regression [B1.1] is smaller than the weighted counterpart and the estimated change in quantity for *ACQUIRED HORIZONTAL ACB* plants falls below the level of statistical significance.

In Table B2, I consider the unweighted results for local versus non-local horizontal mergers. In regression [B2.1], I consider the effects on prices using the benchmark specification controlling for lagged revenue, and in regression [B2.2] I use the specification controlling for lagged price instead of revenue. Regression [B2.3] considers the effects on quantity.

Table B2: Unweighted Local Versus Non-Local Horizontal Merger Results

Dep. Var.	[B2.1] $\Delta PRICE$	[B2.2] $\Delta PRICE$	[B2.3] $\Delta QUANTITY$
$ACQUIRED\ HORIZONTAL\ ACB$	0.058***	0.040*	−0.142**
	(0.020)	(0.021)	(0.067)
$ACQUIRED\ HORIZONTAL\ OUT$	−0.011	0.002	0.042
	(0.028)	(0.026)	(0.178)
$ACQUIRING\ ACB$	0.053	0.061*	−0.175
	(0.038)	(0.032)	(0.122)
$ACQUIRING\ OUT$	0.014	0.001	0.032
	(0.016)	(0.016)	(0.053)
$\Delta TFPQ$	−0.233***	−0.141***	0.660***
	(0.025)	(0.021)	(0.075)
R-Squared	0.437	0.551	0.564
N	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for the change in TFPQ ($\Delta TFPQ$), lagged price, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, and EA-year interactions. Standard errors are clustered by CEA.

Again, the results are quite similar, except that the estimated effects for *ACQUIRED HORIZONTAL ACB* plants are smaller. Notably, despite the change in quantity result from Table B1 falling below the level of statistical significance, the decrease in change for *ACQUIRED HORIZONTAL ACB* plants in regression [B2.3] is statistically significant at the 5% level. Finally, in Table B3 I confirm the robustness of the pre- and post-1982 results in the absence of weighting.

Table B3: Unweighted Pre- and Post-1982 Horizontal Merger Results

	[B3.1]	[B3.2]	[B3.3]	[B3.4]
Dep. Var.	$\Delta PRICE$	$\Delta PRICE$	$\Delta TFPQ$	$\Delta TFPQ$
ACQUIRED ALL	0.005		0.076***	
	(0.017)		(0.023)	
*ACQUIRED ALL**77–82	−0.003		−0.080**	
	(0.033)		(0.034)	
ACQUIRED HORIZONTAL	0.049***		0.072***	
	(0.018)		(0.027)	
*ACQUIRED HORIZONTAL**77–82	−0.119***		−0.116***	
	(0.037)		(0.044)	
R-Squared	0.412	0.417	0.569	0.567
N	1,980	1,980	1,980	1,980

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Regressions control for lagged TFPQ, lagged revenue, lagged capital equipment, lagged structural capital, lagged labor input, lagged materials input, lagged energy input, multi-unit status, age, change in construction employment, population density, and EA-year interactions. Standard errors are clustered by CEA.

Again, the coefficient estimates for the change in price are smaller, the pattern of the results is exactly the same with all evidence of price increases and productivity increases occurring in the period for 1982 to 1992.