ABSTRACT

Title of Document:	ESSAYS ON ANTITRUST AND REGULATORY ECONOMICS
	Robert Brian Kulick, Doctor of Philosophy, 2017
Directed By:	Professor John Haltiwanger Department of Economics
	Professor Andrew Sweeting Department of Economics

This dissertation consists of three essays on antitrust and regulatory economics. In chapter one, 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 rate at which productivity reduces price is modest and the effects of increased market power are not offset. I then present several additional new results of policy interest. 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.

Finally, I use a simple multinomial logit demand model to assess the effects of merger activity on total welfare. At acquired plants, the consumer and producer surplus effects approximately cancel out, but effects at acquiring plants and non-merging plants, where prices also rise, cause a substantial decrease in consumer surplus.

In chapter two, I introduce a model of anticompetitive exclusive dealing that provides a unified treatment of two of the major categories of potentially anticompetitive single-firm conduct recognized by the FTC: refusal to deal and exclusive purchase agreements. The exclusionary mechanism succeeds by turning the incentives of a pivotal buyer or a pivotal coalition of buyers against the incentives of the group when buyers attempt to coordinate on their preferred equilibrium. However, since all buyers acquiesce to the exclusionary strategy, no pivotal buyer or pivotal coalition of buyers emerges that can gain a competitive advantage and all buyers are strictly worse off. I argue that this approach provides a simple economic framework for evaluating a number of real-world antitrust cases, including the seminal cases Lorain Journal and Denstply, which do not fit neatly into the structure of the main body of economic research focused on exclusive dealing, the Naked Exclusion literature. I then show that by redefining exclusive contracts, this approach can be embedded within a Naked Exclusion style model, yielding a number of new results with implications for both the economic literature on exclusive dealing and antitrust jurisprudence.

Finally, in 1970, Congress added Section 36(b) of the Investment Company Act, which authorized suits against mutual fund managers for charging “excessive fees." In 1979, the SEC prosecuted the first case invoking this law in its enforcement action against Fundpack. Although the law and its economic consequences have been the subject to extensive debate, including the high profile case *Jones v. Harris Associates* which pitted Judge Frank Easterbrook and Judge Richard Posner against each other in the 7th Circuit before going the U.S. Supreme Court, the law has been subject to scant rigorous empirical analysis. Along with my co-author Ken Ueda, I use program evaluation techniques and the Center for Research in Security Price's mutual fund data to analyze the consequences of the onset of 36(b) enforcement on mutual fund fees, fund flows, fund returns, and exit rates before and after SEC v. Fundpack. We find that high-fee mutual funds reduced their fees substantially in response, but we find no evidence of reduced mutual fund quality or consumer choice as indicated by fund flows, returns, or exit rates.

ESSAYS ON ANTITRUST AND REGULATORY ECONOMICS

By

Robert Brian Kulick

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park, in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2017

Advisory Committee:
Professor John Haltiwanger, Co-chair
Professor Andrew Sweeting, Co-chair
Professor Ethan Kaplan
Professor Daniel Vincent
Professor Phillip Swagel

Preface

In this series of essays, I explore a number of issues related to antitrust and regulatory economics. While the subject matter of each essay is distinct, the essays are united by a focus on framing the results in terms of their regulatory, legal, and institutional context. In emphasizing these areas, the goal is to emphasize the question of how markets work in the real world. For instance, the first essay examines the effect of horizontal mergers on prices and productivity in the context of the ready-mix concrete industry, which has long been subject to extensive scrutiny by the Department of Justice. The unique nature of the U.S. Census Bureau data that I employ in this study also allows me to explore a number of issues of significant economic importance that have received sparse attention due to data limitations.

Chapter one provides the first rigorous empirical evidence I am aware of on the direct relationship between the price, productivity, and welfare effects of horizontal mergers, the effects on prices and productivity of the watershed changes in antitrust policy that occurred in the United States in the mid-1980s, and the potential targeting of “Maverick Firms”—firms that seek to disrupt markets by increasing competition—through merger activity.

In chapter two, I present a new theoretical model of anticompetitive exclusive dealing. What distinguishes this essay from the Naked Exclusion literature (Rasmusen et al., 1991; Segal and Whinston, 2000; Fumagalli and Motta, 2006; Simpson and Wickelgren, 2007; Abito and Wright, 2008; DeGraba, 2013), a family of models which serve as the primary applied theoretical framework for understanding anticompetitive exclusive dealing, is that the model is explicitly motivated by a number of real-world antitrust cases including the seminal cases *Lorain Journal v. United States* (1951) and is *United States v. Dentsply International, Inc.* (2005).

In chapter three, along with my co-author, Ken Ueda, I explore the effects on the fees charged by mutual funds of the first case brought against a mutual fund manager—*SEC v. Fundpack*—for charging "excessive fees" under Section 36(b) of the Investment Company Act. While this law has been subject to intense debate since *SEC v. Fundpack* was prosecuted by the SEC in 1979 including a major Supreme Court case, there has been little rigorous empirical analysis of the law and its potential welfare consequences.

In addition to being united by a common focus on institutional and legal context, each of these essays is ultimately about the extent to which competitive entry will quickly discipline prices in the absence of regulatory or antitrust enforcement. For instance, in chapter one, I show that despite the ready-mix concrete industry being characterized by frequent entry and exit, horizontal mergers lead to substantial long-term price increases. Although the evidence suggests that price increases are the largest in the first year after a merger, significant price increases remain up to five years after a merger is consummated. In terms of theory, the theoretical model in chapter two emphasizes that in addition to being able to essentially provide bribes to customers to prevent entry from a rival, exclusive dealing can also be used to coerce buyers into accepting exclusive contracts that preclude rival entry and lead to higher prices. Finally, in chapter three, I show that despite substantial competition and frequent entry and exit by mutual funds, high fee mutual funds reduced their prices in reaction to a regulatory change occurring in the late 1970s beyond what can be explained by the typical effect of market forces on prices.

A final aspect of these essays that I believe is essential to understanding their contribution is their heavy reliance on legal and economic history to attempt to draw policy conclusions that are relevant today. Today's Industrial Organization literature offers an impressive array of both theoretical models and empirical models of increasing sophistication. Unfortunately, as has been frequently noted, many of these tools are difficult to apply or have often been found to be unreliable in the context of real-world enforcement. By grounding analyses in a historical perspective, economics may prove more able to establish the validity of its mathematical tools and to better engage with the realms of law and policy. My hope is that these essays demonstrate the abundance of historical data available to researchers that can be used to better understand the policy implications of regulation.

Dedication

For Jess.

Acknowledgements

I am deeply indebted to John Haltiwanger and Andrew Sweeting for their guidance and support for this research. I also thank Ginger Jin, Dan Vincent, 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.

College Park, Maryland

April 27, 2017

Disclaimer

Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed.

Table of Contents

List of Tables

List of Figures

Chapter 1: Ready-to-Mix: Horizontal Mergers, Prices, and Productivity

Section 1: Introduction

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 output and 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, output, 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 rate at which productivity reduces price 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 aggregate-data multinomial logit demand 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 the primary variables that ultimately drive the net welfare implications of mergers, output and efficiencies, 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 quantity sold and input data necessary to calculate productivity.[1] The US Census Bureau's plant-level data allows me to observe quantities of concrete sold and construct a measure of productivity for each observation in my sample so that I can simultaneously evaluate prices, output, 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, 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, however, 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 support for a causal interpretation of the results. Yet, because mergers are not natural experiments, my

[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.

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 because products often change their physical quality, package size or how they are sold. In contrast, I look at ready-mix concrete where the product 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 2 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 3 introduces my methodology and presents the primary regression results. Section 4 introduces a demand model to evaluate the welfare impact of the mergers in my sample, and Section 5 offers concluding remarks.

Section 2: Data and Measurement

2.1: 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 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

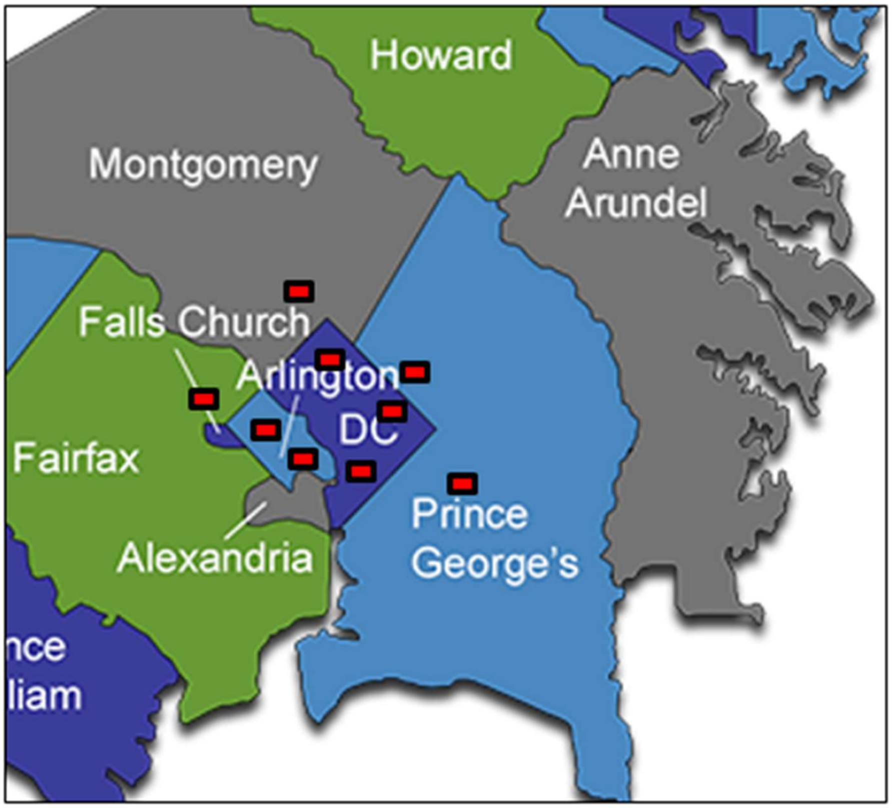
[2] The foundation of this dataset was originally developed in Foster et al. (2008). Although this study attempted to identify all observations with imputed product specific revenue and quantity data using a variety of methods, the original impute flags in the raw Census data had been lost. White et 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.

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 1.1, I provide a map that depicts the ACB associated with the Washington, D.C. county equivalent.

The map in Figure 1.1 depicts Washington, D.C. and its adjacent counties Montgomery, Prince George's, Arlington, Fairfax, and Alexandria and also indicates the locations of some 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

Figure 1.1: The Washington, D.C. Adjacent County Block



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. Furthermore, because ACBs are drawn as circles of counties around plants a merging plant is always centrally located 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.

2.2: Productivity

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

$$\mathrm{TFP}_i = y_i - \alpha_l l_i - \alpha_k k_i - \alpha_m m_i - \alpha_e e_i \qquad (1)$$

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 aggregated over the sample period.[3] 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

[3] I have also tried allowing the cost shares to vary by time and plant and the overall results remain very similar.

first order conditions linking observed output shares to out- put elasticities hold on average eliminating the effects of idiosyncratic adjustment cost-induced misalignments in input levels,[4] 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). Applying the methodology suggested in (Wooldridge, 2009) produces very similar productivity estimates.

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

[4] 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.

revenue from product sales deflated by the revenue weighted geometric mean price across the ready-mix concrete plants in the sample for a given year.[5]

2.3: 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 1.1 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.[6] For now, these distinctions are defined without any geographic

---

[5] 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 I document are the result of enhanced market power. The inflation of revenue is primarily revenue derived from the sale of ready-mix concrete as opposed to revenue related to other income sources.

[6] 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.

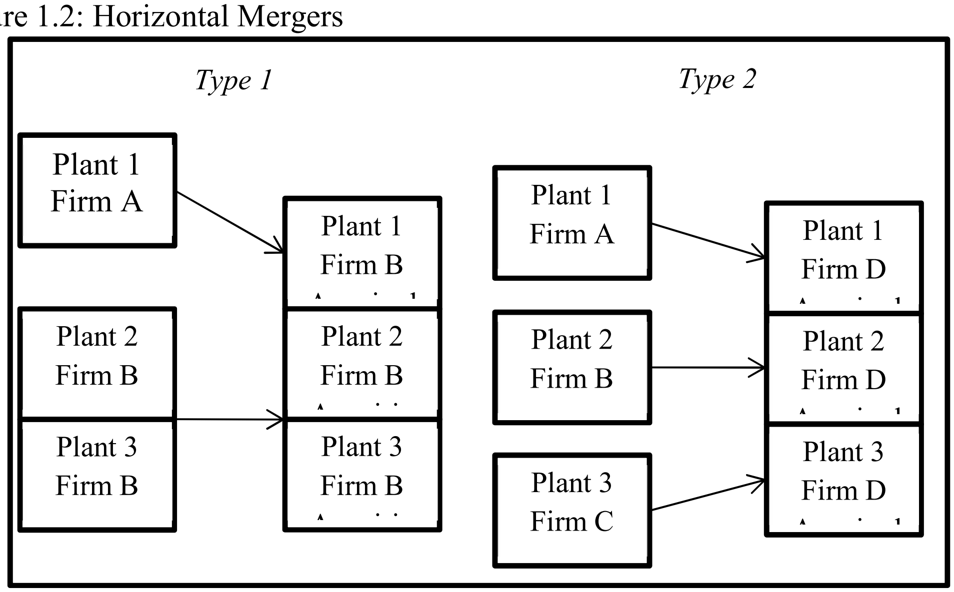
limitations. Later in this section, I explicitly distinguish local mergers from non-local mergers.

Table 1.1: 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 the Firm ID variable between year t and t'. Horizontal mergers in the data take two forms which are depicted schematically in Figure 1.2.

Figure 1.2: 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 1.2, I begin this process examining the extent to which there are important differences between *ACQUIRED HORIZONTAL*, *ACQUIRING*, and non-merging plants pre-merger.

Table 1.2: 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.

In Table 1.2, 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 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 same acquiring firm. The acquiring plants that are associated with within ACB mergers according to the above definition are denoted as *ACQUIRING ACB*. Table 1.3 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.

Table 1.3: Geographic Pattern of Horizontal Merger Activity

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

A number of patterns are evident in Table 1.3. 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 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, while being highly selective about which acquiring plants to base merger activity around.

Section 3: Methodology and Results

3.1: Descriptive Results

This section begins 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.[7] Because evaluating the effects of mergers on consumers 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 A1.2, I present unweighted results as a robustness check. The pattern 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 1.4 presents the results from estimating the descriptive model with changes in prices, quantity, and TFPQ as the dependent variables.

[7] 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.

Table 1.4: Descriptive Results

Dep. Var.	[4.1] *QUANTITY*	[4.2] *PRICE*	[4.3] *TFPQ*
ACQUIRED HORIZONTAL ACB	0.068***	−0.106	0.087***
	(0.019)	(0.069)	(0.032)
ACQUIRING ACB	0.039	−0.057	0.097
	(0.066)	(0.184)	(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.

3.2: 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

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

where p_{it} is price, X_{it} is a 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.[8] In using variables endogenous to the plant's profit maximization problem to identify the price effect of merger activity one would not

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

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-} . But, if, for instance, changes in service quality are what drive selection rather than the level of service quality, controlling for the lagged 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.[9] 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.[10] 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.

3.3: 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 that while the controls I apply are often powerful predictors of the dependent variables, not only do all of the effects reported

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

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

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 1.5 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 1.4. 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.

Lagged TFPR is a strong predictor of each dependent variable and is significant at the 1% level in all regressions in Table 1.5. 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 A1.1.

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

Table 1.5: Results Controlling for Lagged Endogenous Variables

Dep. Var.	[5.1] *ΔPRICE*	[5.2] *ΔPRICE*	[5.3] *ΔQUANTITY*	[5.4] *ΔQUANTITY*	[5.5] *ΔTFPQ*	[5.6] *ΔTFPQ*
ACQUIRED HORIZONTAL ACB	0.061***	0.062***	−0.117*	−0.118*	0.061***	0.058**
	(0.019)	(0.019)	(0.069)	(0.068)	(0.028)	(0.028)
ACQUIRING ACB	0.036	0.041	−0.063	−0.052	0.081	0.090
	(0.064)	(0.066)	(0.182)	(0.160)	(0.054)	(0.055)
TFPR	−0.140***	−0.156***	−0.264***	−0.270***	−0.631***	−0.652***
	(0.040)	(0.042)	(0.097)	(0.091)	(0.060)	(0.062)
EQUIPMENT		−0.002		−0.031		0.006
		(0.007)		(0.034)		(0.013)
STRUCTURE		−0.012***		0.029		−0.008
		(0.004)		(0.020)		(0.008)
LABOR		−0.021*		0.012		−0.025
		(0.012)		(0.039)		(0.017)
MATERIALS		0.023*		−0.195***		0.011
		(0.012)		(0.035)		(0.016)
ENERGY		0.006		0.012		−0.002
		(0.006)		(0.016)		(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*).

problem, they are set with respect to precisely the sort of unobserved factors that may induce problematic selection.[1] 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.[2] 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

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

[2] 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.

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 results, one would expect to see substantial movement in the coefficient estimates.[3] 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 1.6 continues the process of adding control variables likely to be associated with unobserved plant heterogeneity.

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, 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.

[3] 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.

Table 1.6: Benchmark Results

Dep. Var.	[6.1] *ΔPRICE*	[6.2] *ΔPRICE*	[6.3] *ΔQUANTITY*	[6.4] *ΔQUANTITY*	[6.5] *ΔTFPQ*	[6.6] *ΔTFPQ*
ACQUIRED HORIZONTAL ACB	0.075***	0.079***	−0.119*	−0.113*	0.064***	0.058**
	(0.018)	(0.019)	(0.067)	(0.069)	(0.023)	(0.023)
ACQUIRING ACB	0.064	0.065	−0.081	−0.125	0.033	0.022
	(0.057)	(0.058)	(0.157)	(0.148)	(0.041)	(0.040)
TFPQ	0.309***	0.307***	−0.403***	−0.408***	−0.842***	−0.838***
	(0.045)	(0.045)	(0.114)	(0.112)	(0.074)	(0.074)
REVENUE	−0.240***	−0.237***	−0.066	−0.099	0.034	0.019
	(0.039)	(0.038)	(0.072)	(0.075)	(0.034)	(0.035)
MU		−0.020		−0.029		0.014
		(0.038)		(0.037)		(0.016)
AGE		0.001		−0.005		−0.004
		(0.002)		(0.008)		(0.003)
CONSTRUCTION		0.057		0.470***		−0.028
		(0.053)		(0.144)		(0.050)
DENSITY		0.002		0.065***		0.014*
		(0.005)		(0.019)		(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.

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.[1] In fact, the estimated price effects are slightly larger.[2]

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

---

[1] 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.

[2] 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.

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.

3.4: Market Power

Table 1.7 assesses changes in price and quantity for within ACB mergers versus horizontal mergers lacking a local component using the full set of controls from Table 1.6. Acquired and acquiring plants associated with non-local horizontal merger activity are denoted as *ACQUIRED HORIZONTAL OUT* and *ACQUIRING OUT* respectively.

Table 1.7: Local Versus Non-Local Horizontal Merger Results

	[7.1]	[7.2]	[7.3]	[7.4]	[7.5]	[7.6]
Dep. Var.	*ΔPRICE*	*ΔPRICE*	*ΔPRICE*	*ΔPRICE*	*ΔQUANTITY*	*ΔQUANTITY*
ACQUIRED HORIZONTAL ACB	0.082***	0.100***	0.107***	0.125***	−0.126*	−0.170**
	(0.021)	(0.022)	(0.025)	(0.025)	(0.076)	(0.072)
ACQUIRED HORIZONTAL OUT	0.008	0.009	0.000	0.000	−0.037	−0.049
	(0.034)	(0.034)	(0.034)	(0.035)	(0.180)	(0.189)
ACQUIRING ACB	0.068***	0.073	0.089	0.093	−0.135	−0.163
	(0.059)	(0.060)	(0.061)	(0.062)	(0.153)	(0.146)
ACQUIRING OUT	0.011	0.028	0.012	0.030	0.011	−0.027
	(0.020)	(0.020)	(0.020)	(0.020)	(0.075)	(0.075)
NON-MERGING ACB			0.030*	0.030*	−0.018	−0.015
			(0.018)	(0.016)	(0.067)	(0.065)
ΔTFPQ		−0.265***		−0.265***		0.592**
		(0.042)		(0.043)		(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 (Δ*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 1.7, 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.[1] 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 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 rate at which productivity affects price 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

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

at *NON-MERGING ACB* plants. The addition of this control 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 market power. Acquired plants associated with local mergers experience 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 suggests strategic complementarity in rival pricing. At this point, however, 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 acquiring plants actually attains significance. Table 1.8 thus provides additional analysis to help better explain the pattern of pricing behavior at acquiring plants.

Table 1.8 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 1.8: Results Controlling for Lagged Price

	[8.1]	[8.2]	[8.3]	[8.4]
Dep. Var.	*ΔPRICE*	*ΔPRICE*	*ΔPRICE*	*ΔPRICE*
ACQUIRED HORIZONTAL ACB	0.067***	0.080***	0.068***	0.083***
	(0.023)	(0.023)	(0.025)	(0.025)
ACQUIRED HORIZONTAL OUT			0.004	0.006
			(0.029)	(0.033)
ACQUIRING ACB	0.062*	0.076**	0.063*	0.078**
	(0.033)	(0.038)	(0.034)	(0.039)
ACQUIRING OUT			0.004	0.009
			(0.021)	(0.019)
ΔTFPQ		−0.157***		−0.158***
		(0.028)		(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 (*ΔTFPQ*), 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.

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 1.7 or Table 1.8 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 1.9, I consider an analysis of initial pricing and output that is helpful for interpreting the pattern of the results and framing them in terms of the consumer welfare implications.

Table 1.9: Initial Price Results

Dep. Var.	[9.1] *PRICE*	[9.2] *QUANTITY*
ACQUIRED HORIZONTAL ACB	−0.055**	0.409**
	(0.027)	(0.173)
ACQUIRING ACB	−0.031	0.573*
	(0.027)	(0.307)
R-Squared	0.544	0.571
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, a dummy variable for non-merging rivals within an ACB, 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 variables are initial price and output. Controls are limited to concurrent TFPQ, multi-unit status, a dummy variable for non-merging rivals within an ACB experiencing horizontal merger activity, age and EA-year effects. These results are instructive for understanding how the results from the previous tables change when a control for initial price is included. Including initial price increases the precision of the estimates, but as a consequence of the below average initial pricing levels for merging plants, the coefficient estimates also fall. This effect is particularly pronounced for the acquired plants, which have statistically significant below average prices.

To the extent that we are primarily interested in the direction of the results, Table 1.8 provides compelling evidence that prices increase at both acquired and acquiring plants involved in local mergers. However, as these estimates will ultimately be used as inputs in a welfare calculation, it is important to consider whether the price effects from Table 1.7 or Table 1.8 are more informative about the market power effects of mergers at acquired plants.[2] Ultimately, the decision of which estimates to apply comes down to what one thinks to be the appropriate counterfactual. If one believes that prices would have risen to the average level in the absence of merger activity, then it is reasonable to only credit the price increases controlling for initial price as representative of a market power effect. On the other hand, 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.

---

[2] As only the Table 1.8 estimates for acquiring plants are statistically significant, I use these estimates in my welfare calculation to be conservative.

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. Table 1.9 also presents results on initial quantity to shed additional light on the question of whether these results constitute evidence of the targeting of mavericks. Regression [9.2] indicates that the statistically significant below average prices at *ACQUIRED HORIZONTAL ACB* are accompanied by significantly above average output. Thus, rather than being firms temporarily experiencing a negative demand shock or providing a low quality product, the evidence indicates that the acquired plants were charging low prices to gain market share–exactly the behavior we would expect from maverick firms. In terms of the welfare calculations in the next section, I will do the analysis both ways, using the 6.9% price increase from Table 1.7 as a conservative figure and the 11.3% price increase from Table 1.8 as a more aggressive estimate leaving it to the reader to decide which is more appropriate. However, I believe the evidence is consistent with the targeting of maverick firms and that the full price increase from Table 1.7 should be credited as a market power effect.

3.5: Temporal Variation

Table 1.10 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.[3] However, for disclosure reasons, I am not able to subdivide the pooled estimates for within ACB mergers to compare the period from 1977 to 1982 to the period from 1982 to 1992. Thus, for the purposes of this analysis, I extend consideration to all horizontal mergers which allows enough observations to examine the temporal variation. 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.

[3] 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 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.

Table 1.10: Pre- and Post-1982 Results

	[10.1]	[10.2]	[10.3]	[10.4]	[10.5]	[10.6]	[10.7]
Dep. Var.	*ΔPRICE*	*ΔPRICE*	*ΔPRICE*	*ΔTFPQ*	*ΔTFPQ*	*ΔTFPQ*	*ΔTFPQ*
ACQUIRED ALL	0.021			0.074***			
	(0.022)			(0.022)			
*ACQUIRED ALL*77–82*	−0.012			−0.042			
	(0.036)			(0.041)			
ACQUIRED HORIZONTAL		0.082***	−0.072***		0.064***	−0.074***	0.074***
		(0.019)	(0.020)		(0.023)	(0.023)	(0.023)
*ACQUIRED HORIZONTAL*77–82*		−0.134***	−0.121***		−0.122***	−0.124***	−0.123***
		(0.045)	(0.047)		(0.041)	(0.042)	(0.040)
ACQUIRED NON-HORIZONTAL			−0.079**			0.073	0.071**
			(0.036)			(0.049)	(0.036)
*ACQUIRED NON-HORIZONTAL*77–82*			0.110**			−0.007	
			(0.042)			(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 1.10, 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 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-HORIZONTAL* 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 1.7. 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 1.11, I examine how the results from Table 1.10 for mergers occurring between 1982 and 1992 vary with the timing of merger activity.

Table 1.11: Post-1982 Merger Activity by Merger Vintage

	[11.1]	[11.2]	[11.3]
Dep. Var.	$\Delta PRICE$	$\Delta PRICE$	$\Delta TFPQ$
ACQUIRED HORIZONTAL YR1	0.128***	0.147***	0.082**
	(0.035)	(0.039)	(0.037)
ACQUIRING 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.

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 1.11 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.

Section 4: Demand Estimation and Welfare Analysis

If we accept the argument that the price effects observed above were caused by market power, then we can conclude that consumer welfare fell as a result of price increases associated with horizontal merger activity. Furthermore, the evidence of reductions in output indicates that the consequences were not only a transfer of surplus from consumers to producers but a reduction in total surplus as a result of deadweight loss. In general, evidence that mergers will lead to price increases and decreases in output is sufficient for the regulatory authorities to block a merger as the consumer welfare impact is usually the focus of regulatory concern. However, as illustrated by Williamson (1968) when mergers create efficiencies that reduce marginal cost, net total welfare may increase even when mergers engender deadweight loss.

Thus, in the section I consider whether there is any compelling evidence that total welfare increased, despite the price and output effects associated with the mergers in my sample. To do so, I proceed in three steps. First I estimate a simple aggregate data multinomial logit model with unobserved product characteristics following Berry (1994) to model demand. Second, I estimate plant's marginal costs using the firm's first order conditions. Third, I use my estimates from the previous section to simulate counterfactual levels of price and marginal cost in the absence of the market power and efficiency effects created by mergers.

As is standard, it is assumed that there are $j = 0,1, \dots, J$ products in $t = 1, \dots, T$ markets each with $I = 1, \dots, I_t$ consumers. The key step in implementing this analysis is to account for the importance of spatial differentiation in the ready-mix concrete industry by defining each plant as a separate product. Thus, 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. To further account for the fact that some plants are located in superior locations, 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:

$$u_{ijt} = x_j^{\text{fe}} - \alpha p_{jt} + \xi_{jt} + \varepsilon_{ijt} = \delta_{jt} + \varepsilon_{ijt} \quad (5)$$

where ξ_{jt} represents unobserved differences in product quality, and ε_{ijt} is a stochastic error term. As from today/'s standard policy perspective, the evidence from the previous section would generally be sufficient to label the observed mergers as anticompetitive, in this section I proceed by making assumptions that are designed to give the efficiencies the benefit of the doubt in reversing the welfare losses associated with market power effects. Thus, first and foremost, I will assume that all increases in productivity are fully dual to marginal cost, i.e., that none of the efficiencies measured in the previous section represent fixed costs. Second, as will be discussed in more detail below, I will use merger simulation and my estimates from the previous suggestion to suggest a procedure for constraining the market size M_t.

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 following well-known 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:

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

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 necessary to estimate α is to set the market size M_t so that I can calculate shares. However, there is no direct way to calculate the market size in the concrete industry taking into account the potential role of substitution to materials like steel and asphalt which anecdotal evidence suggests can be substantial depending on concrete prices.[1] One methodology would be to simply take the quantity of concrete sold in each market and assume a fixed percentage of the outside good. This approach however has the disadvantage of being completely arbitrary in defining the share of the outside good and in not allowing any variation in substitution patterns by market. Another approach that creates more variation is taking the maximum value of

[1] For instance, a 1988 article in the New York Times real estate section entitled “Concrete or Steel?” discusses the factors that drive substitution between concrete and steel in large-scale building projects.

concrete sold in each market across time and then specifying a fixed percentage of the outside share for the maximal market-year observation. However, this still involves an undesirable degree of arbitrariness.

The good news is that employing these strategies over a broad range of specified shares leads to quite similar elasticity estimates. However, as the market size gets larger, the level of the estimated consumer surplus loss increases substantially. Since the main point of this section is to give efficiencies the benefit of the doubt, this is potentially problematic. Thus, my preferred approach involves modifying the second methodology so that the market size for the maximal market-year across each market is set by matching the reduced-form estimates from the previous section to the predicted price outcomes from simulating the mergers that occur in my sample based on their pre-merger characteristics.[2] Specifically, I begin by setting the share of the outside good in each of the maximal market-year observations to 50%.[3] Specifying the share of the outside good at this level in maximal market-year observations leads to predicted merger price effects that are far below the levels estimated in the previous section. Thus, I proceed by reducing the share of the outside good uniformly, until the average price increase at acquired plants matches the 11.3% price increase from Table 1.7. Here, I choose the larger predicted value between Tables 1.7 and 1.8 so that the market size is smaller and the estimated consumer surplus levels are conservative.

---

[2] Simulation is necessarily restricted to mergers that involve a within CEA change in market structure.

[3] Thus, for a given market in the non-maximal year, the share of the outside good will necessarily be greater than 50% at this initial step.

With the size of the market fixed, demand estimation follows as described above. Table 1.12 presents the results.

Table 1.12: Demand Estimation Results

N	Average Share Outside Good	α	Average Elasticity
11,600	0.268	−0.113*** (0.014)	−4.755*** (0.824)

Table 1.12 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 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 constant elasticity model from 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.

With these estimates, I now proceed to calculating the welfare affects for a given set of counterfactual prices and marginal costs. With this structure, following Small and Rosen (1981), the change in consumer surplus is given by applying the "logsum" formula:

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

where δ'_{jt} represents the counterfactual product-level component of utility. The key step here is to use my estimates from the previous section to set the level of prices that would have prevailed in the absence of the market power created by merger activity. Specifically, for each plant engaged in a within ACB merger, I reduce prices by the percentage indicated by my regression results.

I then calculate the change in marginal cost using my TFPQ estimates an exploiting the duality of this relationship with marginal cost. This change is multiplied by the model predicted change in quantity to arrive at an estimate of the gain in producer surplus. The change in welfare is then given by:

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

The welfare simulation results are summarized in Table 1.13.

Table 1.13: Welfare Simulation Results (1987 Dollars, Millions)

Price Effect	PS Gain	CS Loss	ACB
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 1.13 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.

Section 5: 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 under- stand 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.

Chapter 2: An Offer You Can't Refuse: Naked Exclusion, Refusal to Deal, and Exclusive Contracts

Section 1: Introduction

In this article, we introduce a model of anticompetitive exclusive dealing that provides a unified treatment of two of the major categories of potentially anticompetitive single-firm conduct recognized by the Federal Trade Commission (FTC): refusal to deal and exclusive purchase agreements. The FTC defines exclusive purchase agreements as contracts "requiring a dealer to sell [the] products of only one manufacturer." Contracts of this sort have long been the focus of the Naked Exclusion literature (Rasmusen et al., 1991; Segal and Whinston, 2000; Fumagalli and Motta, 2006; Simpson and Wickelgren, 2007; Abito and Wright, 2008; DeGraba, 2013), a family of models which serve as the primary applied theoretical framework for the economic evaluation of exclusive dealing cases. The basic structure of the model as introduced by Rasmusen et al. (1991) and Segal and Whinston (2000) [hereafter RRW-SW] revolves around the behavior of three sets of agents, an incumbent, a potential rival, and N downstream buyers. Exclusive contracts are defined in terms of a commitment by downstream buyers to purchase only from the incumbent and are enforced through an external mechanism (*i.e.*, the legal system). The success of this literature lies in its simple description of equilibria where the incumbent can use exclusive contracts with lump-sum compensation to profitably monopolize the upstream market by thwarting rival entry.

However, many important exclusive dealing cases involve fact patterns that do not fit neatly into existing Naked Exclusion models. For instance, many exclusive dealing cases involve accusations that competitive pressures created by the exclusive contracts force buyers to agree to exclusive contracts to their own detriment; many cases also do not involve any compensation of buyers. Yet, as we discuss in Section 2 of this article, the exclusionary equilibria predicted by the Naked Exclusion literature provide scant economic foundation for exclusive dealing cases characterized by these fact patterns.

Furthermore, many exclusive dealing cases involve contractual arrangements that are not consistent with the externally enforced buyer commitment contracts assumed in the literature. Rather, many of the most salient antitrust cases involve exclusive contracts that commit the seller to dealing only with buyers who purchase from the seller. One very prominent case that manifests all three of these characteristics is *United States v. Dentsply International, Inc.* (2005). Dentsply, a producer of artificial teeth, imposed a contractual term on distributors of its product known as “Dealer Criterion 6” which stipulated that in order to sell Dentsply products, dealers had to agree not to offer the products of competing manufacturers. The Third Circuit Court of Appeals decision in *Dentsply* suggests that the distributors agreed to the contracts despite being dissatisfied with the terms, were driven to acquiescing by competitive pressures, and were not provided compensation despite being made worse off by the contracts. Consequently, despite its legal importance, *Dentsply* lies outside the ambit of the economic literature on exclusive contracts.

Our approach to providing a firm economic foundation for cases like *Dentsply* is inspired by a theory of anticompetitive single-firm conduct known as a "refusal to deal." The FTC explains that in the context of exclusive dealing, the essence of a refusal to deal is a situation where the predatory firm imposes the condition, "I refuse to deal with you if you deal with my competitor." In Section 3 of this paper, we introduce a simple "Refusal to Deal Game" inspired by the seminal Supreme Court case *Lorain Journal v. United States* (1951). In *Lorain Journal*, the Supreme Court found that Lorain Journal, a local newspaper, prevented businesses who wanted to advertise on a new radio station from doing so by demanding exclusivity. Lorain Journal enforced its policy of exclusivity by refusing to let any business advertising on the radio station advertise in the newspaper.

Like the Naked Exclusion literature, we show that our simple model exhibits equilibria where the predatory firm (the incumbent) successfully prevents the rival from entering, allowing the incumbent to monopolize the upstream market and equilibria where the rival enters and competition prevails. Unlike the Naked Exclusion literature, however, we show that the exclusionary outcome, is a robust outcome of the game even in the absence of any compensation of the buyers. Furthermore, in applying a weak-dominance equilibrium refinement to isolate exclusion as the unique outcome, we identify a mechanism that demonstrates how the refusal to deal creates an environment where competitive pressures push the downstream buyers to capitulate. Specifically, when buyers are pivotal their incentive is to agree to the incumbent's scheme so that buyers attempting to deal with rival are excluded from the downstream market. Yet, because all buyers go along with the scheme, no buyer is pivotal and all buyers are strictly worse off.

In Section 4, we embed the simple Refusal to Deal Game into a Naked Exclusion model following the structure of Simpson and Wickelgren (2007). The key step in adapting our model to this setting involves defining exclusive contracts in terms of a seller commitment by the incumbent only to deal with downstream buyers who do not enter into exclusive purchase agreements with the rival. This is an alternative to the buyer commitment assumption that is employed in the Naked Exclusion literature (Elhauge and Wickelgren, 2012).

The Naked Exclusion structure also places more restrictions on the model relative to the simple Refusal to Deal Game and in many cases the additional structure may be more realistic. With this structure, the exclusionary outcome can still be isolated as the unique outcome by requiring that equilibria be perfectly coalition-proof. Although this equilibrium refinement is weaker, we show that the mechanism by which the exclusionary scheme prevents the downstream buyers from coordinating on their preferred equilibrium is fundamentally similar to the mechanism from Section 3 with pivotal coalitions of buyers taking the place of pivotal buyers. Finally, in Section 5 we compare the results of our model to results from the Naked Exclusion literature and consider the implications of our results for a number of issues in antitrust economics and jurisprudence.

In addition to providing a framework for understanding prominent cases like *Dentsply* and *Lorain Journal*, our approach provides an economic rationale for a number of recent enforcement actions by the FTC including *In the Matter of*

Transitions Optical, Inc. (2010) and *In the Matter of IDEXX Laboratories, Inc.* (2013).[1] Although the full details have not yet become public, the preliminary allegations suggest that our model may also be applicable to U.S. Justice Department's recently announced investigation of potential exclusion of craft brewers by AB InBev.[2]

Section 2: The Naked Exclusion Literature

Naked Exclusion models revolve around the behavior of three sets of agents, an incumbent, a potential rival, and N downstream buyers which purchase a necessary input from the upstream suppliers. The game proceeds over three periods. In period 1, the incumbent offers buyers exclusive contracts. The exclusive contracts are defined as a commitment by the buyer to purchase from the incumbent in return for a specified level of lump-sum compensation. Typically, the literature assumes that once a buyer signs such an agreement it must purchase only from the incumbent. However, as we will discuss below, Simpson and Wickelgreen (2007) consider contracts enforced through breach damages rather than an absolute commitment to buy from the incumbent enforced by an institution with the power to compel

[1] The need to clarify the relationship between cases involving an exclusionary strategy predicated on seller commitment and the exclusive dealing models currently considered as part of the Naked Exclusion literature is longstanding. Indeed, RRW cite *Lorain Journal* as one of the cases motivating the very first Naked Exclusion model.

[2] On October 12, 2015, the Washington Post reported, "Antitrust regulators are also reviewing craft brewers' claims that AB InBev pushes some independent distributors to carry only the company's products and end their ties with the craft industry."

purchases.[3] In period 2, the rival decides whether to enter. In period 3, prices are set and purchases are realized.

In the RRW-SW model, the rival has a cost function $c(\cdot)$ where $c(Q) = \overline{c}$ for $Q \geq Q^*$ and $c(Q) > \overline{c}$ at all $Q < Q^*$.[4] The incumbent has a constant marginal cost $c(Q) = \overline{c}$ so that if the rival reaches the minimum efficient scale it is an equally efficient competitor.[5] The exclusionary equilibria arising from the RRW-SW model and the related models of Fumagalli and Motta (2006), Simpson and Wickelgren (2007)[6] and Asker and Bar-Isaac (2014)[7] can be organized into four categories based on the compensation strategy employed by the incumbent, the specification of the buyers as independent purchasers or firms competing in a downstream market, and the payoffs realized by the buyers:

(i) The buyers are either independent purchasers or firms, and a pivotal segment of the downstream market is fully compensated for agreeing to the exclusive

[3] Simpson and Wickelgren's model is predicated on the observation that courts in the United States do not enforce contracts by forcing or compelling specific behavior, but rather, as will be discussed more below, through damages.

[4] While other papers in the literature implement alternative cost assumptions, we introduce the original RRW-SW cost structure as it will play an important role in Sections 3 and 4. Note, however, we have modified it slightly so that all that is required is that the rival's marginal cost is strictly greater than $\overline{c}$ below the minimum efficient scale.

[5] The results for the model in Section 4 can accommodate a more efficient rival, however, the results for the model in Section 3 will only hold up to an equally-efficient competitor.

[6] Fumagalli and Motta make assumptions that end up ruling out the exclusionary equilibria of their model, but Simpson and Wickelgren show that their model accommodates exclusionary outcomes as well. Because of their article's important role in considering downstream buyers as firms competing in terms of perfect substitutes, we include the article here despite their conclusion ultimately ruling out exclusion.

[7] Asker and Bar-Isaac considers the potential for exclusionary resale price maintenance rather than anticompetitive exclusive contracts. However, in the perfect competition setting they show that resale price maintenance can drive monopolization through a mechanism similar to the exclusive contracts from the Naked Exclusion literature. Their results help to clarify the potential for compensated exclusion in a downstream market characterized by perfect competition.

contracts. The remainder of the participants in the downstream market receive no compensation and are strictly worse off as they receive the input at the monopoly price.

(ii) The buyers are firms who compete with each other in the downstream market. Downstream competition is sufficiently strong so that the incumbent can afford to compensate all of the downstream firms for agreeing to the exclusive contracts without spending more than the total monopoly surplus. The downstream firms suffer no harm and all of the loss of surplus is suffered by the end users who purchase from the downstream firms. Included in this case is the specification where the downstream firms compete in terms of perfect substitutes and the incumbent can monopolize the market providing no compensation, as the downstream firms are indifferent between monopoly and competition in the upstream market.

(iii) The buyers are either independent purchasers or firms who compete in terms of imperfect substitutes. All buyers receive zero compensation for agreeing to the exclusive contracts but fail to coordinate on their preferred equilibrium. All buyers are worse off, but this anticompetitive equilibrium is weakly dominated. There is also always an equilibrium where the rival successfully enters the market and competition prevails.

(iv) The buyers are either independent purchasers or firms. The buyers receive positive compensation for agreeing to the exclusive contracts but still fail to coordinate on their preferred equilibrium as no buyer receives compensation sufficient to make up for the loss of surplus resulting from monopoly pricing. All buyers are worse off, but these equilibria do not survive application of the perfectly coalition-proof Nash equilibrium refinement. Again, there is also always an equilibrium where the rival successfully enters and this equilibrium survives the coalitional refinement.

This taxonomy of results gives rise to three observations. First, case (iii) indicates that the Naked Exclusion literature provides little theoretical support for the possibility of uncompensated exclusive dealing outside of a downstream market characterized by perfect competition.[8] Indeed, when uncompensated monopolization occurs in the context of perfect competition, the downstream buyers are not rendered strictly worse off as they are indifferent between either outcome. Second, in the most robust cases, (i) and (ii), successful monopolization essentially turns all or some portion of downstream buyers into accomplices rather than victims of the anticompetitive scheme. Third, for cases (iii) or (iv) there is no mechanism suggesting how or why the buyers fail to coordinate on their preferred equilibrium. Indeed, applying simple Nash equilibrium refinements in both cases illustrates specific rationales for why the competitive equilibrium is likely to succeed rather than the anticompetitive equilibrium. As discussed above, the absence of both a justification for exclusive dealing cases involving no compensation of buyers and, more generally, the absence of a mechanism indicating how exclusion succeeds in the absence of full compensation is problematic as it places a number of the most significant antitrust cases outside of the ambit of the Naked Exclusion literature.

[8] The case of perfect substitutes in the downstream market is likely to be of little relevance in real-world antitrust cases which tend to involve product markets with at least some differentiation.

Another aspect of the Naked Exclusion literature that is potentially problematic from the perspective of real-world antitrust analysis is the way in which the buyer-committing exclusive contracts are typically defined. Simpson and Wickelgren (2007) observe that although the Naked Exclusion literature assumes that once buyers sign an exclusive contract they have no choice but to purchase under the contract, this is not consistent with the legal treatment of contracts. Rather, they note that contracts are enforced through the imposition of breach damages by courts against parties that fail to perform their contractual obligations. Simpson and Wickelgren modify the basic structure of the RRW-SW model to allow for breach damages by splitting period 3 into three sub-periods. In period 3.1, prices are set. In period 3.2, the downstream buyers, which are specified as firms in competition with one another, decide whether to breach or maintain their contract with the incumbent. Finally, in period 3.3, sales are realized and breach damages are assessed.

The Simpson and Wickelgren model implicitly assumes that purchases from the rival require a forward purchase arrangement. Otherwise, breach could simply occur in period 3.2 with a downstream firm choosing to purchase from the rival. Thus, the effect of including breach damages in the model is to impose a cost on buyers transitioning from a purchasing arrangement with the incumbent to a purchasing arrangement with the rival. Using this structure, they show that when breach damages are set at or below the level of expectation damages,[9] the penalty under common law, the prediction of the model ceases to be monopolization through

[9] Expectation damages are damages paid by the breaching party to the injured party that place the injured party in the position it would have enjoyed had the contract been performed by the breaching party.

rival exclusion.[10] Instead, the incumbent firm maintains monopoly profits by allowing the rival to enter and collecting damages from downstream firms breaching the contracts.

While the Simpson and Wickelgren model addresses some potentially unrealistic aspects of the basic Naked Exclusion structure, it too has important limitations as a model of real world antitrust cases. Exclusion or impairment of rivals is at the heart of most major antitrust cases involving exclusive dealing and the absence of a compelling and general explanation for this phenomenon would be problematic for a literature predicated on understanding anticompetitive exclusive dealing. Furthermore, antitrust cases where contracts are enforced through breach damages are certainly far less common than strategies involving punishment or discounts. Thus, in developing our model of exclusive dealing we will focus on strategic or contractual arrangements that are observed in real-world antitrust cases while still taking advantage of simple structure and appealing characteristics of Naked Exclusion models.

[10] If breach damages are specified above the level of expectation damages, then the model will provide the same exclusionary outcome as in RRW-SW. However, Simpson and Wickelgren argue that this will not generally apply. Furthermore, when breach damages are set exactly equal to expectation damages, both the exclusionary RRW-SW result and the breach result are possible equilibria. They rule out the exclusionary equilibrium assuming that a downstream firm will choose to breach when indifferent. However, another way to arrive at this conclusion is to assume that legal action on the part of the incumbent has a small non-recoverable cost. This assumption is quite plausible given the costs and uncertainty associated with litigation. Either way, their model suggests important limitations on the power of buyer-committing exclusive contracts to induce exclusionary outcomes.

Section 3: A Simple Refusal to Deal Game

In this section, we introduce a simple "Refusal to Deal Game" which models anticompetitive exclusive dealing using *Lorain Journal* as inspiration. Specifically, we build the model around three features that are motivated as stylized representations of the fact pattern associated with the case.[11] First, as *Lorain Journal* did not involve explicit exclusive purchase contracts, the refusal to deal is imposed on the downstream market without a bargaining process in period 1. Second, as the only lever used by Lorain Journal to enforce compliance was access to its advertising platform, we assume that downstream firms are free to purchase from the rival at any time if it is active in the upstream market. Third, we specify that the refusal to deal is activated by any agreement a downstream enters to purchase from the rival.[12] In the next section, we adapt this simple model into the more structured setting of the Naked Exclusion model. Consequently, we maintain their convention of labeling period 3 in terms of three sub-periods.

As in the Naked Exclusion literature, the model involves an incumbent, a potential rival, and N buyers who we specify as firms competing in a downstream market. The incumbent and the rival produce a homogenous product that is essential for production of the downstream good. The downstream firms compete in a market

[11] Our assessment of the circumstances of the case are based on the description provided by the Supreme Court.

[12] The court records in cases involving exclusionary conduct of this nature frequently indicate that simply contacting or entering into an initial agreement with a rival is often sufficient to cause an incumbent to activate a refusal to deal. For instance, in *Lorain Journal*, the Supreme Court noted that mere suspicion of an agreement to advertise on radio was sufficient to trigger the refusal to deal. In *Dentsply*, when Trinity Dental entered into an agreement to sell another competitor's teeth, Dentsply responded by refusing to supply Trinity.

characterized by competition in Bertrand differentiated products. One of the primary factors that drives our model is the production technology, which is adapted from RRW-SW. As described above, the rival has a cost function $c(\cdot)$ where $c(Q) = \overline{c}$ for $Q \geq Q^*$ and $c(Q) > \overline{c}$ at all $Q < Q^*$ and the incumbent has a constant marginal cost $c(Q) = \overline{c}$. We assume where useful the existence of a sufficiently fine discrete price space to address open set problems. We also assume that the rival never needs to sell to the entire market in order to reach the minimum efficient scale. The timing of the game is as follows:

In period 1, the incumbent commits to a refusal to deal.

In period 2, the rival decides whether to enter.

In period 3.1, the incumbent and the rival set prices p_I and p_A, respectively.

In period 3.2, the rival takes orders to determine if it will reach the minimum efficient scale. The results of this model turn on how we specify what happens when the rival does not reach the minimum scale. By placing an order at this stage with the rival, a downstream firm is prevented from purchasing from the incumbent at any remaining point in the game as a result of the refusal to deal. Consequently, whether the rival reaches the minimum efficient scale is determined by the number of firms who place orders with the rival.[13] If the rival does not reach the minimum efficient scale and is not able to profitably honor the price set in period 3.1 the rival declares bankruptcy, exits the upstream market, and incurs a small exit cost. Let R represent

[13] There are however no similar limitations in this section on the purchasing behavior of firms that do not place orders with the rival. Thus. for simplicity, we assume that those firms which do not place orders with the rival will purchase from the incumbent unless the price offered by the rival is superior to that offered by the incumbent. As a result, the number of firms placing orders with the rival is sufficient for the rival to determine whether it will reach the minimum efficient scale.

the number of downstream firms that place orders with the rival and are thus subject to the refusal to deal and let I represent the number of downstream firms who remain eligible to purchase from the incumbent. Let $\mathcal{R}$ and $\mathcal{I}$ represent the respective sets associated with R and I.

In period 3.3, if the rival reaches scale, competition proceeds at the prices declared in period 3.1. If not and the rival exits, the downstream firms that placed orders with the rival are now excluded from the downstream market as they cannot gain access to the necessary input.[14] For the remainder of this section we also assume that if the rival does reach the minimum scale, the downstream firms in $\mathcal{I}$ remain free to purchase from the rival.

Lemma 1: If $p_I = p_R = \bar{c}$, there exists a number N^* such that the rival reaches the minimum efficient scale if and only if $R > N - N^*$.

Proof:

Let Q_m represent the size of the market when all of the downstream firms purchase the input at a price of $\overline{c}$ so that each downstream firm purchases $q_i = \frac{Q_m}{N} = q$ units and the rival's quantity supplied can be written as $Q_R = (N - I)q$. The rival reaches the minimum efficient scale if and only if $Q_R \geq Q^*$. Thus, the expression $Q^* = \left(N - \widehat{N}\right)q$ implicitly defines a real number $\widehat{N}$ such that the rival reaches the minimum efficient scale if and only if $I \leq \widehat{N}$. For a real number x, let $[x]$

[14] As will become clear from the results below, we need not specify what happens if the rival does not meet the minimum scale but remains in the market as the rival must set a price equal to marginal cost in period 3.1.

represent the closest integer greater than x. By rearranging the expression above, we have $\widehat{N} = N - \frac{Q^*}{q}$. Letting $N^* = \left\lceil N - \frac{Q^*}{q} \right\rceil$ we have that the rival reaches the minimum efficient if and only if $I < N^*$.[15] Note that $I + R = N$ so we can rewrite the condition as $N - R < N^*$. Rearranging we have that the rival reaches the minimum efficient scale if and only if $R > N - N^*$. ■

Lemma 2: The rival must set $p_R = \bar{c}$ to make any sales in an equilibrium of the subgame beginning in period 3.1.

Proof:

Suppose that the rival sets $p_R > \bar{c}$. If $p_I < p_R$ the rival makes no sales. If the incumbent sets $p_I \geq p_R$, the incumbent will deviate so that $p_I' = p_R - \varepsilon$ and sell to the entire market unless at $p_I = p_R$ the incumbent already sells to the entire market. ■

For the sake of simplicity, we will remove from consideration any pricing equilibrium in the subgame beginning in period 3.1 where either the incumbent or the rival cannot make any sales at the set prices no matter what transpires in the remainder of the game. Consequently, we assume without any loss of generality that the rival sets $p_R = \bar{c}$ throughout the remainder of the analysis.

For the next lemma, we define $\bar{p}$ as the price such that if $p_R = \bar{c}$, for any $p_I \geq \bar{p}$ a single downstream firm placing an order from the rival is sufficient for the

[15] A similar condition is applied, but not formally derived in RRW-SW. An implicit assumption in both papers is that $\widehat{N}$ is not integer valued so that the strict inequality holds after applying the function $f(x) = \lceil x \rceil$. For convenience, we maintain this assumption.

rival to reach the minimum efficient scale.[16] At this point it is also useful to introduce the following notation. Let π represent the profits of a downstream firm and let $\mathbf{Z}_S$ represent the vector with S elements where each element has the identical value z. Then $\pi(p, \mathbf{P}_{N-1})$ represents the profits of a buyer who receives the input at price p while the other $N-1$ firms also receive the input at price p. Furthermore, $\pi(p, \mathbf{P}_{N^*-1})$ represents the profits of a buyer who receives the input at p while only N^*-1 firms receive the input at p and the remaining firms are excluded from the market.

Lemma 3: Equilibrium in the subgame beginning in period 3.1 following rival entry can take any form where $p_R = \bar{c}$, $\bar{c} \leq p_I < \bar{p}$. Furthermore, given any equilibrium pricing pair $(\overline{c}, p_I)$, there is an equilibrium where the rival achieves the minimum efficient scale and an equilibrium where the rival does not achieve the minimum scale and is forced to exit.

Proof:

If $p_I \geq \bar{p}$, then the incumbent must achieve $I = N$ in period 3.2 to prevent the rival from reaching minimum scale and deviation by a single firm to purchasing from the rival is sufficient to render the rival viable. Since $\pi\left(\overline{c}, \overline{\mathbf{C}}_{N-1}\right) > \pi(p_I, \mathbf{P}^I_{N-1})$, such a deviation is optimal for a downstream firm, demonstrating that we can rule out $p_I \geq \bar{p}$ as the incumbent will never be able to make sales at that price.[17]

---

[16] Without loss of generality we assume throughout that $\bar{p} < p^m$.

[17] The deviating firm is equally well off in the model in this section. It can still purchase from the rival in period 3.3 as the refusal to deal only restricts the behavior of firms dealing with the rival. One of the

Now, using our assumption of a discrete price space we consider $p^* < \bar{p}$ such that p^* is one increment below $\bar{p}$. We also suppose for ease of exposition and without loss of generality that p^* is such that the rival need only secure purchase orders from two buyers in period 3.2 to reach the minimum efficient scale. Suppose in the subgame beginning in period 3.2, $I = N$. If a single downstream firm deviates, the rival does not achieve the minimum scale. However, the incumbent still enforces the refusal to deal so that the buyer is excluded from the downstream market and earns zero profits as opposed to $\pi_b(p^*, \mathbf{P}_{N-1}^*)$. Thus, $I = N$ represents an equilibrium of the subgame beginning in period 3.2.

If we alternatively suppose that in the subgame beginning in period 3.2 $R = N$, then following a deviation by a downstream firm the rival remains viable.[18] Thus, $R = N$ also represents an equilibrium of the subgame beginning in period 3.2.

Specifying $I = N$ as the continuation equilibrium following $p_I = p^*$ in the subgame beginning in period 3.2 implies that $p_I = p^*$ is the only equilibrium of the subgame beginning in period 3.1. Any higher price would result in deviation by a downstream firm, successful competition from the rival, and zero profits for the incumbent. Thus $p_I = p^*$ and the rival exiting the upstream market is an equilibrium of the subgame beginning in period 3.1.

However, specifying $R = N$ as the continuation equilibrium following $p_I = p^*$ in the subgame beginning in period 3.2 is also consistent with $p_I = p^*$

---

crucial differences introduced into the model in Section 4 is that the deviating firm would now be strictly worse off as it would have to purchase from the incumbent at p^*.

[18] This follows from our assumption above that we can rule out pricing equilibria where the upstream firms have no chance of making sales in period 3.3.

representing an equilibrium in the subgame beginning in period 3.1. If, for instance, we specify off of the equilibrium path that all downstream firms purchase from the rival unless $p_I = \bar{c}$, then any pricing deviation by the incumbent in period 3.1 will result in zero profit, which is the same profit the incumbent earns from setting $p_I = p^*$. Indeed, this same argument establishes any p_I such that $\bar{p} \geq p_I \geq \bar{c}$ can describe an equilibrium of the subgame beginning in period 3.1 where the rival achieves minimum scale and successfully competes.

Finally, it follows that by specifying off of the equilibrium path the latter type of equilibrium where in period 3.2 the rival achieves the minimum efficient scale for any price $p > p_I$, any p_I such that $p^* > p_I \geq \bar{c}$ can represent an equilibrium of the game beginning in period 3.1 where the rival is forced to exit the market. ■

Given that following the decision to enter in period 2, there are continuation equilibria in period 3 where the rival succeeds and continuation equilibria where the rival fails, the game as a whole has equilibria where the rival enters and equilibria where the rival does not enter. If the rival does not enter the incumbent is able to set $p_I = p^m$. The situation is summarized in Proposition One.

Proposition 1: Play along the equilibrium path for the simple Refusal to Deal Game can take two forms:

(i) the rival does not enter, $p_I = p^m$, and all downstream firms purchase from the incumbent.

(ii) the rival enters and all downstream firms purchase the input at $\bar{c}$.

Just as in the original RRW-SW Naked Exclusion model there are equilibria of the game as a whole where the downstream firms coordinate on their preferred outcome and equilibria where the downstream firms fail to coordinate on their preferred outcome. However, as discussed above, in the RRW-SW model there is no mechanism to explain why in the absence of compensation the downstream firms may fail to coordinate. Proposition 2 suggests a specific mechanism for how exclusion succeeds in the absence of any compensation of buyers by the incumbent and also indicates that in this simple model the exclusionary outcome is the more robust outcome.

Proposition 2: Suppose that the downstream firms always coordinate on their preferred equilibrium unless the inferior equilibrium is an equilibrium in dominant strategies (including weakly dominant strategies). Then the simple refusal to deal game has a unique equilibrium where the rival does not enter and all downstream firms purchase from the incumbent at $p_I = p^m$.[19]

Proof:

Suppose that in period 2 the rival enters, in period 3.1 $p_R = p_I = \bar{c}$, and consider the decision of an arbitrary downstream firm in period 3.2 which we label as firm one. If $R_{-i} > N - N^*$ then no matter what strategy firm one chooses, the rival

[19] Expressing the proposition in this way emphasizes the power of the mechanism preventing the downstream firms from coordinating on their preferred equilibrium. There are also two technical benefits of expressing the conditions for equilibrium in this manner. First, since neither equilibrium of the subgame beginning in period 3.1 is preferred when $p_R = p_I = \bar{c}$, the equilibrium refinement allows for the rival to succeed off the equilibrium path. Second, this equilibrium selection mechanism isolates a unique equilibrium for the game as a whole as opposed to only fixing behavior along the equilibrium path.

will reach the minimum efficient scale and firm one will be able to purchase at $\bar{c}$. If $R_{-i} = N - N^*$ then firm one is pivotal. Since $\pi(\bar{c}, \bar{\mathbf{C}}_{N^*-1}) > \pi(\bar{c}, \bar{\mathbf{C}}_{N-1})$, firm one benefits from preventing the rival from reaching the minimum efficient scale as $N - N^*$ downstream rival are now excluded from the downstream market. As a result, firm one will choose not to place an order from the rival. Finally, if $R_{-i} < N - N^*$, then no matter what strategy firm one chooses, the rival will not achieve the minimum efficient scale. Since firm one will make positive profits by choosing to purchase from the incumbent and zero profits by placing an order with the rival who will not achieve minimum scale, firm one will not choose to purchase from the rival. From our assumption of a sufficiently fine discrete price space there exists $p > \bar{c}$ such that $\pi(p, \mathbf{P}_{N^*-1}) > \pi(\bar{c}, \bar{\mathbf{C}}_{N-1})$ and such that the rival still much achieve $R > N - N^*$ to be viable. Thus, we are guaranteed the existence of a strictly profitable p_I such that it is weakly dominant strategy to acquiesce to the incumbent's scheme. It follows, in an equilibrium of the subgame beginning in period 3.1 the incumbent sets p_I to be the highest price such that when a firm is pivotal, it does not choose the rival, as the incumbent knows that for any higher price the downstream firms will coordinate on their preferred equilibrium. Thus, the rival does not enter and the incumbent sets $p_I = p^m$. ■

The power of the refusal to deal in this model is that it allows the incumbent to turn the downstream firms against each other when they attempt to coordinate on their preferred equilibrium. In the simple case explored here, to prevent coordination on the downstream firms' preferred equilibrium, the incumbent sets prices so that it is never worth it for a downstream firm to risk dealing with the rival. Although all of the downstream firms would be better off under the competitive outcome, when a downstream firm is pivotal, it is better off complying with scheme so that competition in the downstream market is reduced.

The model thus far omits two major elements from the Naked Exclusion literature that potentially limit both theoretical comparison of this model to Naked Exclusion models and practical application of this model to real antitrust cases. First, we assumed that the incumbent is able to make a very strong commitment not to deal with downstream firms who attempted to purchase from the incumbent without requiring explicit contracts. The omission of explicit contracts is also problematic in terms of real world antitrust cases as many, including *Dentsply* involve such contracts. Second, we have assumed that a downstream firm can simply switch to the rival in the last period of the game without any advanced preparation or forward agreement. However, the records in many major antitrust cases involving dealer or distributor markets indicate that forward purchase agreements are necessary for a nascent rival trying to gain traction in a market.

Section 4: A Naked Exclusion Model with Seller Committing Exclusive Contracts

In this section, we consider how the simple Refusal to Deal Game can be adapted into a model with the Naked Exclusion structure. Specifically, we consider Simpson and Wickelgren's structure as it has a number of appealing features. First, the Simpson and Wickelgren structure allows for downstream firms to switch from the incumbent to the rival in period 3.2. By setting the penalty for breach by downstream buyers to zero we are able to emphasize the crucial contribution of seller commit contracts, while still indicating that the model can easily be adjusted to accommodate bilateral exclusive contracts (contracts involving both seller and buyer commitment).[20] Second, the Simpson and Wickelgren model implicitly requires forward purchasing from the rival in period 3.2. This assumption is likely to be more realistic for many cases than the purchasing behavior in the previous section and forces us to consider how the model operates in a more rigid environment where the downstream firms who do not choose to purchase from the rival in period 3.2 now face some risk of paying a higher price for the input even if the rival achieves viability. Third, this structure allows us to consider the potential role of breach damages for seller commitment contracts and compare those to the results for buyer commitment contracts.

The key step in adapting the simple Refusal to Deal Game from the previous section to this setting is to define exclusive contracts in terms of a seller commitment to do business only with a downstream firm who has not contracted to purchase from the rival at any stage of the game. We will now use I to denote the number of downstream firms who agree to the exclusive contract in period 1 and R to represent all those who contract with the rival in period 1.

As a result of the refusal to deal, the downstream firms in $\mathcal{R}$ cannot purchase from the incumbent in period 3. However, the I firms choosing to sign the exclusive contracts in period 1 are able to switch to the rival in period 3.2 for free during the

[20] We thank Einer Elhauge for this observation.

switching period as we set the penalty for buyer breach to zero.[21] Any level of damages could be specified, creating exclusive contracts with bilateral commitment, but as we will see below, there is no need for bilateral commitment contracts in this model, as the seller commitment contracts are sufficient to allow the incumbent to enjoy the entire monopoly surplus.[22] The number of downstream firms choosing to switch to contracting with the rival at this stage of the game is denoted C_R. Thus, the total number of firms purchasing from the rival in period 3.3 is now given by $R + C_R$.

As discussed above, following Simpson and Wickelgren model we require that the downstream firms contract with the rival in period 3.2 if they want to buy from the rival in period 3.3. As a result, unlike the simple Refusal to Deal Game from Section 3, if a downstream firm chooses to stay with the incumbent in period 3.2 and the incumbent is charging a higher price, the downstream firm does not get to purchase from the lower priced rival in period 3.3. Lemma 4 presents an important consequence of this modification of the game.

Lemma 4: If the rival enters and $\overline{p} > p_I > \overline{c}$*, then equilibrium in the subgame beginning in period 3.2 can take only two forms. Either* $R + C_R = N$ *and all of the downstream firms purchase from the rival at* $p_R = \overline{c}$ *or* $C_R = 0$*, the rival exits, and* $N - R$ *firms purchase from the incumbent at* p_I*.*

[21] Setting the level of breach damages to zero in the Simpson and Wickelgren setting is equivalent to exclusive contracts entailing no buyer commitment.

[22] Even if exclusive contracts are technically bilateral, enforcing buyer commitment may be very costly. This may help to explain why we see many more examples of cases involving seller commitments to withhold sales from non-compliant buyers than cases involving lawsuits against buyers breaching contracts.

Proof:

First, note that Lemma 2 continues to hold so we have that in equilibrium $p_R = \overline{c}$. Since $p_I > \overline{c}$, all of the firms in $\mathcal{J}$ will either purchase entirely from the rival or will purchase entirely from the incumbent. Furthermore, the latter case can only occur if there are not sufficient firms in $\mathcal{R}$ after period 1 to render the rival viable.

Next, we show that $R + C_R = N$ is an equilibrium. By assumption, the rival never has to sell to the entire market to gain viability, so if $R + C_R = N$, after a single deviation by a downstream firm to the incumbent, the rival remains viable and the downstream firm is strictly worse off as it must now purchase the input at $p_I > \overline{c}$. Establishing the equilibrium where $C_R = 0$ and the rival exists depends on the price set by the incumbent and the number of firms in $\mathcal{R}$. For instance, if the incumbent sets $p_I = p^*$ which we defined above as the price one increment below $\overline{p}$ and if there are no firms in $\mathcal{R}$, $C_R = 0$ represents an equilibrium as a single deviation will not render the rival viable.[23] For any price less than p^* an equilibrium where $C_R = 0$ and the rival exists can be specified in a similar manner as we can always fix R so that two or more firms have a move in period 3.2. ■

From Lemma 4 it is clear that just as in the simple Refusal to Deal Game from the previous section, the game as a whole has equilibria where the rival successfully enters and equilibria where the incumbent is successful in excluding the rival. Thus,

[23] As in Section 3, we assume without loss of generality that rival only needs two downstream firms at p^*.

in the remainder of this section we focus on the mechanism that drives the exclusionary outcome and consider the robustness of this outcome.

Because a firm that purchases from the incumbent is now unable to switch to the rival in the final period of the game, the weak dominance result from Proposition 2 will no longer hold. However, we show in Lemma 5 that a weaker refinement requiring that equilibria be coalition-proof has a similar effect on the behavior of the downstream firms. The coalition-proof Nash equilibrium refinement requires that equilibria be immune to self-enforcing coalitional deviations (Segal and Whinston, 2000). Although this refinement is weaker, it has a very similar effect as to the weak dominance refinement applied in Section 3, with the role of a pivotal firm being replaced with pivotal coalitions of firms.

Lemma 5: If $R \leq N - N^*$ and the rival enters then the unique coalition-proof equilibrium of the subgame in period 3.1 is characterized by $C_R = 0$, the rival exits, and $N - R$ firms purchase from the incumbent at $p_I > \overline{c}$.

Proof:

Define $\underline{p}$ to be the smallest price increment such that $\underline{p} > \overline{c}$. From our assumption of a sufficiently fine discrete price space we have:

(i) $(N - N^*)q\left(\overline{\mathbf{C}}_{N-N^*}, \underline{\mathbf{P}}_{N^*}\right) < Q^*$

(ii) $\pi\left(\underline{p}, \underline{\mathbf{P}}_{N^*-1}\right) > \pi\left(\overline{c}, \underline{\mathbf{P}}_{N^*-1}, \overline{\mathbf{C}}_{N-N^*}\right) > \pi\left(\overline{c}, \overline{\mathbf{C}}_{N-1}\right)$

Condition (i) implies that even at a small price increment above $\overline{c}$ the rival must still receive purchases from $N - N^* + 1$ downstream firms to reach the minimum efficient scale. Condition (ii) indicates that the profit to a downstream firm of buying the input at $\underline{p}$ and facing only $N^* - 1$ competitors is greater than the profit from purchasing the input at $\overline{c}$, but competing against the entire downstream market when up to $N^* - 1$ purchase at $\underline{p}$.

Suppose that in period 3.1 the upstream firms set $p_I = \underline{p}$ and $p_R = \overline{c}$. Note that all downstream firms who elect for contracts with the rival in period 1 must purchase from the rival or make no purchases as a result of the refusal to deal so only $N - R$ firms have a move in period 3.2.

We now apply the coalitional refinement to the equilibria of the subgame beginning in period 3.2 noting that as a result of Lemma 4 we need only consider two possible equilibria.

In the first case where $R + C_R = N$, suppose now that a coalition of N^* firms deviates to the incumbent. Since $\pi\left(\underline{p}, \underline{\mathbf{P}}_{N^*-1}\right) > \pi(\overline{c}, \overline{\mathbf{C}}_{N-1})$ the initial deviation is optimal. Furthermore, as the optimal sub-coalitional deviation is for one firm to leave the coalition and contract with the rival the condition that $\pi\left(\underline{p}, \underline{\mathbf{P}}_{N^*-1}\right) > \pi\left(\overline{c}, \underline{\mathbf{P}}_{N^*-1}, \overline{\mathbf{C}}_{N-N^*}\right)$ ensures that no sub-coalition will deviate and the initial deviation is self-enforcing. Consequently, this equilibrium does not survive the coalition refinement.

For the latter equilibrium, where $C_R = 0$, the rival exits, and $N - R$ downstream firms have a move, consider a deviation by some subset of the $N - R$ downstream firms so that the rival has sufficient purchases to achieve the minimum efficient scale. From condition (i) above, following a price of $\underline{p}$, the rival must achieve $R + C_R > N - N^*$ to be viable.

The most profitable initial coalitional deviation is one such that $\pi(\overline{c}, \underline{\mathbf{P}}_{N^*-1}, \overline{\mathbf{C}}_{N-N^*}) > \pi\left(\underline{p}, \underline{\mathbf{P}}_{N-R-1}\right)$. While this initial deviation is not always optimal, for the history where $R = 0$ the initial is necessarily optimal. However, even when the initial deviation is optimal, a sub-coalitional deviation always exists as $\pi\left(\underline{p}, \underline{\mathbf{P}}_{N^*-1}\right) > \pi(\overline{c}, \underline{\mathbf{P}}_{N^*-1}, \overline{\mathbf{C}}_{N-N^*})$. Thus, no coalitional deviation from this equilibrium is self-enforcing and the equilibrium survives. Thus, following $p_I = \underline{p}$ the coalitional refinement selects the equilibrium where the rival fails.

However, as the incumbent's price rises the benefit of excluding rivals will fall both because of the direct price effect and because fewer firms will be excluded. At some p such that $\overline{p} \geq p > \underline{p}$, the critical inequalities will reverse and the coalitional refinement will select the equilibrium where the rival succeeds in reaching the minimum efficient scale. As the incumbent makes no profits following a price at or above this level, the incumbent will simply select the maximum price such that the coalitional refinement selects the outcome where the rival fails which we have shown is guaranteed to be such that $p_I > \overline{c}$. ■

With this Lemma we can now prove our final proposition.

Proposition 3: If $\pi(p^m, \boldsymbol{P}^{\boldsymbol{m}}_{N^*-1}) > \pi(\overline{c}, \overline{\boldsymbol{C}}_{N-1})$, *then the unique coalition-proof equilibrium of the game as a whole is* $I = N$, *the rival does not enter, and the incumbent sets* $p_I = p^m$.

Proof:

Lemma 5 demonstrates that when when $R \leq N - N^*$ the rival will fail. So equilibrium in period 1 must either be characterized by $R > N - N^*$ the rival enters and all downstream firms purchase at $\overline{c}$ or $R = 0$, the rival does not enter, and $p_I = p^m$. In the former case, consider a coalitional deviation away from $R > N - N^*$ so that $R = N - N^*$. Since $\pi(p^m, \mathbf{P}^{\mathbf{m}}_{N^*-1}) > \pi(\overline{c}, \overline{\mathbf{C}}_{N-1})$ the initial coalitional deviation is optimal and any deviation by a sub-coalition that restores the rival to viability will simply result in all of the downstream firms receiving payoffs of $\pi(\overline{c}, \overline{\mathbf{C}}_{N-1})$. Thus the initial deviation is self-enforcing. In the latter case, while the initial deviation is optimal as it is better for all of the downstream firms to purchase the input at $\overline{c}$ than at p^m, by the same logic as in Lemma 5, a pivotal sub-coalition will deviate to back to the incumbent so that the downstream firms agreeing to purchase from the rival are exposed to the refusal to deal and excluded from the downstream market. Thus, the initial deviation is not self-enforcing and the exclusionary equilibrium survives the coalitional refinement. ■

As with the simple model from Section 3, the exclusionary strategy turns the downstream firms against each other when they attempt to coordinate on their preferred equilibrium. Any pivotal coalition of firms will have the incentive to undermine the equilibrium where competition prevails to gain an advantage in the downstream market. However, because no firms will risk being shut out of the downstream market to enter into a purchase agreement with the rival, no coalition of downstream firms actually gets a competitive edge and all are strictly worse off.

An advantage of focusing on seller commitment as opposed to buyer commitment is that it is easy to see how commitment to the refusal to deal strategy could arise out of repeated interaction without institutional enforcement. However, it is still interesting to consider the potential for the seller to uphold the refusal to deal in a one-shot game where breach damages are the only force pushing the incumbent to honor the exclusive contract. Let p^{ce} represent the price set by the incumbent in a coalition proof equilibrium in the subgame beginning in period 3 and suppose incumbent can breach its exclusive contracts by paying expectation damages. The case where it will be most tempting for the incumbent to breach is when only N^* firms purchase from the incumbent so $N - N^*$ are left out of the market. Under this scenario, the payoffs to the incumbent at the end of period 3.3 are given by:

$$N^* \pi(p^{ce}, \mathbf{P}^{\mathbf{ce}}_{N^*-1}) \,. \tag{1}$$

However, if the incumbent chooses to breach and pay expectation damages its payoffs are:

$$N\pi(p^{ce}, \mathbf{P}^{\mathbf{ce}}_{N-1}) - N^*[\pi(p^{ce}, \mathbf{P}^{\mathbf{ce}}_{N^*-1}) - N^*\pi(p^{ce}, \mathbf{P}^{\mathbf{ce}}_{N^*-1})] \,. \tag{2}$$

Manipulating these equations indicates that the incumbent will not breach based on a one-shot interaction if and only if:

$$\frac{\pi(p^{ce}, \mathbf{P}^{\mathbf{ce}}_{N^*-1})}{\pi(p^{ce}, \mathbf{P}^{\mathbf{ce}}_{N-1})} > \frac{N + N^*}{2N^*} \,. \tag{3}$$

While this inequality can go either way, the important point is that unlike the Simpson and Wickelgren model, it is plausible that expectation damages alone can hold the exclusionary strategy in place. Thus, when factors like reputation or repeated interaction are considered as well, the seller commitment assumption employed in this section rests on a firm economic foundation.

Section 5: Discussion and Conclusion

Although the Naked Exclusion literature was the first to highlight the possibility that as a result of exclusive contracts downstream firms might fail to coordinate on their preferred equilibrium, the results of this literature fail to provide a strong foundation for why such behavior occurs. Rather, in the most robust exclusionary cases, Naked Exclusion models rely on full compensation either of a pivotal segment of the downstream market or, when downstream competition is sufficiently strong, full compensation of all buyers. The refusal to deal models introduced above predict exclusion as the robust outcome while maintaining the existence of an alternative equilibrium that is clearly preferred by all downstream firms. Furthermore, the model does so in a way that is consistent with legal and institutional context of real-world antitrust cases while still maintaining the traditional focus on monopolization through rival foreclosure.

The model in Section 3 assumed immense flexibility of the rival to supply the market with little notice and the incumbent's ability to commit to a refusal to deal without explicit contracts. Although these assumptions are admittedly quite strong, the result is one that turns the weak dominance result associated with the Naked Exclusion equilibria labeled as case (iii) in Section I on its head. In our simple model, when the firms calculate whether they should attempt to coordinate on their preferred equilibrium, they realize that if they are individually pivotal, it is better to let the rival fail and exclude other downstream firms from the market.

Once we assume the structure of the Simpson and Wickelgren's Naked Exclusion model, we require a weaker equilibrium refinement to identify a unique equilibrium. Yet, the underlying mechanism maintains its essential strategic purpose. Applying the coalitional refinement, pivotal coalitions play the same role as a pivotal firm in the model from Section 3. When the downstream firms attempt to coordinate on their preferred equilibrium, a coalition of firms will have the incentive to undermine this equilibrium just as a single firm does in the former case. What both refinements emphasize is that the strategic value of this scheme is that it turns the downstream firms against each other when they attempt to coordinate on their preferred equilibrium.

In providing an economic theory that is consistent with cases like *Dentsply* and *Lorain Journal*, we believe that our approach provides an economic interpretation of the idea of coercion in cases involving vertical restraints. Before the rise to prominence of the Chicago School of Economics, courts in the United States frequently expressed strong hostility towards vertical restraints like exclusive dealing. Courts evinced particular concern about situations where buyers were "coerced" or "forced" into vertical restraints.[24] In the first written statement of the Chicago argument, Director and Levi (1956) took direct aim at the coercion doctrine, arguing that firms attempting coercion through vertical restraints would "lose revenue because they cannot both obtain the advantage of the original [monopoly] power and impose

[24] A detailed history of the role of coercion in antitrust law can be found in Burns (1992).

additional coercive restrictions so as to increase their monopoly power." Over time, the scholars associated with the Chicago School expanded the argument to the formulation made famous by Bork (1978). Bork's argument consisted of two prongs: first, if exclusive dealing cannot be imposed through coercion, then "exclusivity is not an imposition, it is a purchase." Second, "a supplier cannot purchase its way to monopoly though exclusive dealing contracts." These arguments became highly influential in law, dramatically affecting courts' assessments of cases involving allegations of anticompetitive exclusive dealing. In addition, the Chicago School arguments effectively banished the notion of coercion from the economic discourse on exclusive dealing.

While the Naked Exclusion equilibria labeled (i) and (ii) in Section 2 belie the second prong of Bork's argument, to date the Naked Exclusion literature does not provide any clear mechanism through which buyers can be said to have been forced or coerced into failing to coordinate on their preferred equilibrium. On the other hand, here the failure to coordinate on a preferred equilibrium is clearly driven by the incumbent's ability to turn the downstream firms against each while providing no compensation for exclusivity. In other words, *exclusivity is an imposition, not a purchase*.

Our model also provides some interesting intuition on the role of contracts in cases involving instances of coercive exclusive dealing. The model in Section 3 assumed that the incumbent could commit to the refusal to deal without explicit contracts. However, as a result of the explicit contracts in Section 4, a much more restrictive condition applies to when the strategy will be feasible.

The $\pi(p^m, \mathbf{P}^{\boldsymbol{m}}_{N^*-1}) > \pi(\overline{c}, \overline{\mathbf{C}}_{N-1})$ from Proposition 3 arises because with explicit contracts the downstream firms gain an early mover advantage and now consider the tradeoff between the full exclusionary outcome and competition. The model thus suggests an interesting tradeoff: while explicit contracts may provide greater ability to commit, there is a cost in terms of the ability to implement the refusal to deal strategy.

In addition to these points of economic interest, we conclude by considering the potential of our models to clarify certain specific issues antitrust jurisprudence. For instance, some courts have argued that only long-term exclusive contracts have anticompetitive potential, and short-term contracts are generally permissible. In *Barry Wright Corp. v. ITT Grinnell Corp.* (1983), the First Circuit Court of Appeals cited the fact that the contracts at issue covered a "fairly short time period" in concluding that a series of exclusive arrangements did not represent anticompetitive exclusive dealing. In the Seventh Circuit the following year, Richard Posner one of the legal scholars most associated with the Chicago School of Economics, wrote in *Roland Machinery Co. v. Dresser Industries* that, "[e]xclusive-dealing contracts terminable in less than a year are presumptively lawful." Cases (iii) and (iv) from the Naked Exclusion literature discussed above could be seen as providing an economic basis for this presumption. Under this logic, short-term contracts with many opportunities for renegotiation would help to promote buyer coordination on a preferred equilibrium, undermining the potential for anticompetitive exclusion. However, in the refusal to deal model developed in this article, even when exclusive contracts are specified so that there are no breach damages and buyers are free to break the contracts, exclusion still succeeds. Thus, the presumption that short-term contracts are inherently procompetitive may not be warranted.

Another interesting application of this model is in the evaluation of class action cases involving direct purchasers. Antitrust class action cases seeking damages are often certified under Federal Rules of Civil Procedure Rule 23(b)3 which requires that courts find that "questions of law or fact common to class members predominate over any questions affecting only individual members." Legal commentators have asserted that the inherent conflicts among direct purchaser class members in exclusive dealing cases may render direct purchaser class actions non-viable, and the Naked Exclusion literature may currently be seen as supporting this view of exclusive dealing cases.[25] In the robust equilibria from the Naked Exclusion literature some subset of downstream firms act as participants rather than victims of the exclusionary scheme. On the other hand, our results suggest that is is possible for all direct purchaser class members to be harmed by an anticompetitive exclusive dealing scheme.

[25] Weick (2014) makes this argument in the context of an article that explicitly cites the Naked Exclusion literature.

Chapter 3: Mutual Fund Excessive Fee Litigation: A Case Study in Regulatory Enforcement

Section 1: Introduction

Mutual funds are governed under the Investment Company Act of 1940. In 1970, Congress added Section 36(b) of the Investment Company Act, which authorized both the Securities and Exchange Commission ("SEC") and private plaintiffs to sue mutual fund managers for charging "excessive fees." While the law and its economic consequences have been subject to extensive legal and policy debate, including the high profile case *Jones v. Harris Associates* which pitted Judge Frank Easterbrook and Judge Richard Posner against each other in the 7th Circuit before going the U.S. Supreme Court, the law has been subject to scant rigorous empirical analysis. In this paper, we use program evaluation techniques and the Center for Research in Security Price's (CRSP) extensive data on mutual funds to analyze the consequences of the onset of 36(b) enforcement on mutual fund fees, fund flows, fund returns, and exit rates before and after *SEC v. Fundpack* (1979), which was the first legal action to invoke the law. We find that high-fee mutual funds reduced their fees substantially in response to the onset of excessive fee litigation but we find no evidence of reduced mutual fund quality or consumer choice as indicated by fund flows, returns, or exit rates.

These findings are particularly salient when developed in the context of the prominent legal and policy analyses that have placed excessive fee litigation at the center of the debate over the proper scope and administration of financial regulation. In the 2008 case *Jones v Harris Associates*, Judge Frank Easterbrook wrote an opinion that attacked the economic logic of the law articulating a standard for excessive fee cases that would have, for all intents and purposes, vitiated its regulatory impact. Easterbrook argued that since the mutual fund industry is competitive, the notion of an excessive fee is meaningless in the absence of outright fraud. Furthermore, he averred that despite the fact that mutual funds rarely fire their investment advisors, "investors can and do 'fire' advisers cheaply and easily by moving their money elsewhere." Easterbrook dismissed the argument that most mutual fund investors are unsophisticated and fail to understand the pricing structure of the industry and countered that a limited number of sophisticated investors create sufficient competitive pressure to protect all investors. The implication of Easterbrook's argument is simple: rather than limiting rents, regulation through excessive fee enforcement only serves to render the mutual fund industry less competitive and less efficient by reducing mutual fund quality or consumer choice.

Although Judge Richard Posner has long been one the figures most associated with the "Chicago School of Economics", Posner issued a dissenting opinion expressing skepticism towards the validity of Easterbrook's economic analysis. Indeed, Posner wrote that Easterbrook's opinion was predicated on "an economic analysis that is ripe for reexamination on the basis of growing indications that executive compensation in large publicly traded firms is excessive because of the feeble incentives of boards of directors to police compensation." Posner's economic logic follows on the heels of two eminent bodies of economic literature: the behavioral economic literature suggesting that investors are subject to irrational behavior that can undermine the efficient functioning of markets (Thaler, 1985; Shefrin and Statman, 1985; Shleifer and Vishny, 1997; Odean, 1998; Barberis et al., 2001; Barberis and Thaler, 2003) and the empirical literature suggesting that actively managed mutual funds do not outperform passive, index-based strategies, (Jensen, 1968; Malkiel, 1995; Carhart, 1997; Fama and French, 2010). Posner's argument thus implies that mutual fund managers may earn inefficient rents by taking advantage of market imperfections or frictions.

In line with Easterbrook's position, a very prominent analysis of the mutual fund industry, Wallison and Litan (2007) asserts that far from reducing mutual fund fees, excessive fee litigation is both the reason that many funds continue to charge high fees and that the mutual fund industry is still subject to high price dispersion despite the existence of low-cost index funds. The premise of their argument is that section 36(b) creates a de facto "rate regulation" structure in the mutual fund industry. Wallison and Litan summarize the meaning of this analogy to rate regulation as follows, "[s]imply put, price competition and price convergence have not occurred in the mutual fund industry because there is little incentive for investment advisors to reduce costs."

Thus, our contribution is to bring rigorous program evaluation techniques to bear on the questions raised by these conflicting views of the mutual fund industry and the nature of the fees charged to investors. We begin by considering the consequences of the onset of excessive fee litigation for the fees charged by high-fee mutual funds. We find evidence of substantial declines in the fees charged by high-fee mutual before and after 1978, suggesting that the SEC's enforcement action against Fundpack and subsequent suits had a strong effect on prices, which in the absence of quality reduction or restriction of consumer choice would increase consumer welfare.

However, to the extent that the mutual fund industry is operating efficiently Easterbrook's and Wallison and Litan's economic logic would suggest just such a countervailing effect. We employ two measures of mutual fund quality: the responses of mutual fund investors as measured by fund flows and the returns offered by mutual funds. We find no evidence of fund outflows or reductions in returns for high-fee funds that would be indicative of a reduction in quality. We also find no evidence of an increase in the probability of exit by high-fee funds, suggesting that consumer choice was not impaired as a result of enforcement.

Section 2: Methodology

The primary methodology we employ to assess the consequences of the onset of 36(b) litigation on mutual fund fees and operations is difference-in-difference analysis. For the first step in our analysis, our main parameter of interest is the change in fees charged by mutual funds at risk of being prosecuted for charging excessive fees following *SEC v. Fundpack*, which was publicly announced in the SEC News Digest on March 23, 1979. While it is impossible to identify which funds were at risk for prosecution after this announcement, since the onset of 36(b) litigation, the fees charged by funds relative to their peers have always been one of the primary benchmarks for defining what is excessive under the law. Our primary approach in defining the at-risk group is essentially a "revealed preference" approach where we use the actions of the SEC to the assist in the definition of the treatment group. Thus, we define our treatment group based on whether a mutual fund had fees higher than Fundpack in 1978 at any point prior to *SEC v. Fundpack*. We classify all other mutual funds within the control group.

We evaluate the changes in fees charged by for high-fee funds relative to low-fee funds by evaluating the difference in fees charged before and after 1979. Formally, we estimate the following:

$$Y_{it} = \gamma_i + \gamma_t + \pi\, TNA_{it} + \beta\, Post_t * HF_i + \varepsilon_{it} \tag{1}$$

where Y_{it} is the expense ratio for mutual fund i and year t, γ_i are mutual fund fixed effects, γ_t are year dummies, and TNA_{it} represents the total net assets of mutual fund i at time t. We cluster standard errors by fund manager since there is likely to be serial correlation within management across years.

For our difference-in-difference analysis, we require a balanced panel. In balancing our panel we are forced to confront a trade-off between inclusion of as many funds as possible and the number of years our panel spans before and after *SEC v. Fundpack*. In setting our event window we also face a tradeoff between continued surveillance of the effects of excessive fee enforcement and inclusion of sufficient data before the event of interest. Our primary approach is to present results for a panel of funds spanning from 1975 to 1984. In selecting this 10-year span as our primary window we are motivated by a number of factors. First, 36(b) litigation continued after 1979 with notable cases such as *SEC v. American Birthright Trust* (1980) and *Gartenberg v. Merrill* (1982). Thus, it is of interest to track the continued path of high-fee mutual fund fees over the years following the onset of 36(b) litigation. Second, although a longer event window excludes funds that do not last the entire time period, the funds that continue to exist over a longer time span are the most economically significant and visible mutual funds. However, it is still relevant to consider how the results differ when different cohorts and event windows are chosen. Thus, in the appendices to this paper we provide results for a 10-year window from 1974 to 1983 and a 12-year window from 1973–1974. These results are qualitatively quite similar to our primary results indicating that our conclusions are robust to concerns about panel selection and time period.

Our identifying assumption is that there are no other time varying unobservable factors that systematically affect high-fee funds relative to low-fee funds. In terms of potentially overestimating the effect of excessive fee enforcement, our primary concern is that the lower fees are for a given fund, the less appropriate they are as a control for the group of high-fee funds. We use three approaches to mitigate this concern. First, in addition to our primary difference-in-difference estimates we include event study estimates, which allow us to test the pattern of the treatment and control groups before *SEC v. Fundpack*. Second, as a robustness test we consider variations on the control group where we remove funds below the 10^{th}, 25^{th}, and 50^{th} percentiles of the low-fee control group from the analysis. Third, we conduct placebo testing of our primary difference-in-difference specification using data for years before the onset of excessive fee litigation in 1979.

To the extent that excessive fee litigation drives high-fee mutual fund fees lower, this is not necessarily indicative that the regulation was successful in its aims.

If competition was operating effectively in the mutual fund industry and the high fees charged by high-fee funds reflected high operation costs rather economic rents, then reducing the fees charged by these funds could lead to the reduction of mutual fund quality or restricted consumer choice through widespread exit from the market. As we find evidence that high-fee mutual funds reduced their fees as a result of the onset of excessive fee litigation, we examine the quality of services offered by funds along two dimensions. First, to the extent that the quality of high-fee mutual funds was reduced as a result of the regulation, consumers perceiving this loss of quality would reduce their ownership in the affected funds. A useful metric for assessing the net investor activity for a given mutual fund is the fund flow. We define the fund flow following Sirri and Tufano (1998) as:

$$\text{Flow} = \frac{TNA_t - (1 + RR) * TNA_{t-1}}{TNA_{t-1}} \tag{2}$$

where TNA represent total net assets held by the fund and RR represents the rate of return the mutual fund earned on its investments over the year. Rate of return is defined as

$$RR \equiv \frac{NAV_t - NAV_{t-1} + DIV_t}{NAV_{t-1}} \tag{3}$$

where NAV is the net asset value or value per share of the mutual fund and DIV accounts for any dividends dispersed to investors. Second as returns are the primary metric that investors evaluate in determining the quality of a mutual fund we also consider a version of our difference-in-difference specification that compares the returns offered by high-fee funds relative to low-fee funds.

While flows and returns provide reliable quality measures for existing funds it is also possible that the onset of the regulation caused high-fee mutual funds to inefficiently exit the industry leaving consumers with fewer options. Thus, we also examine how the exit rate of funds evolved over time outside of the balanced panel that underlies our difference-in-difference results. Specifically, we assess whether high-fee mutual funds are more or likely to exit the market after the onset of excessive feel litigation by analyzing the exit rate probability for the set of mutual funds who were in operation from 1975–1979.

Section 3: Data

We use the Center for Research in Security Prices (CRSP) database for this analysis. The CRSP database contains detailed data for the universe of mutual funds from 1962 to the present. Critically for this study, CRSP maintains data on mutual fund fees as measured by the expense ratio which is the percentage of shareholders' total investment that is paid on an annual basis for participation in the fund. CRSP also maintains monthly data on a number of mutual fund characteristics, including total net assets (TNA), which measures fund size, and net asset value (NAV) which is the value per share of a fund. Because mutual fund fees are measured at the end of each year and monthly TNA and NAV are the values as of the last trading day of each month, we use the measured values for last day in December of each year as our primary unit of observation. Consequently, as *SEC v. Fundpack* was initiated and settled in the Spring of 1979, for the purposes of our analysis all results for 1979 and after follow the event of interest and all results for 1978 and before precede the event of interest.

As discussed above, in order to construct a balanced panel for difference-in-difference analysis we restrict our sample to mutual funds that operated continuously from 1975 to 1984. This restriction leads to the loss of some funds from the data. We now provide some summary statistics to characterize the nature of our 10-year cohort of funds.

Table 3.1

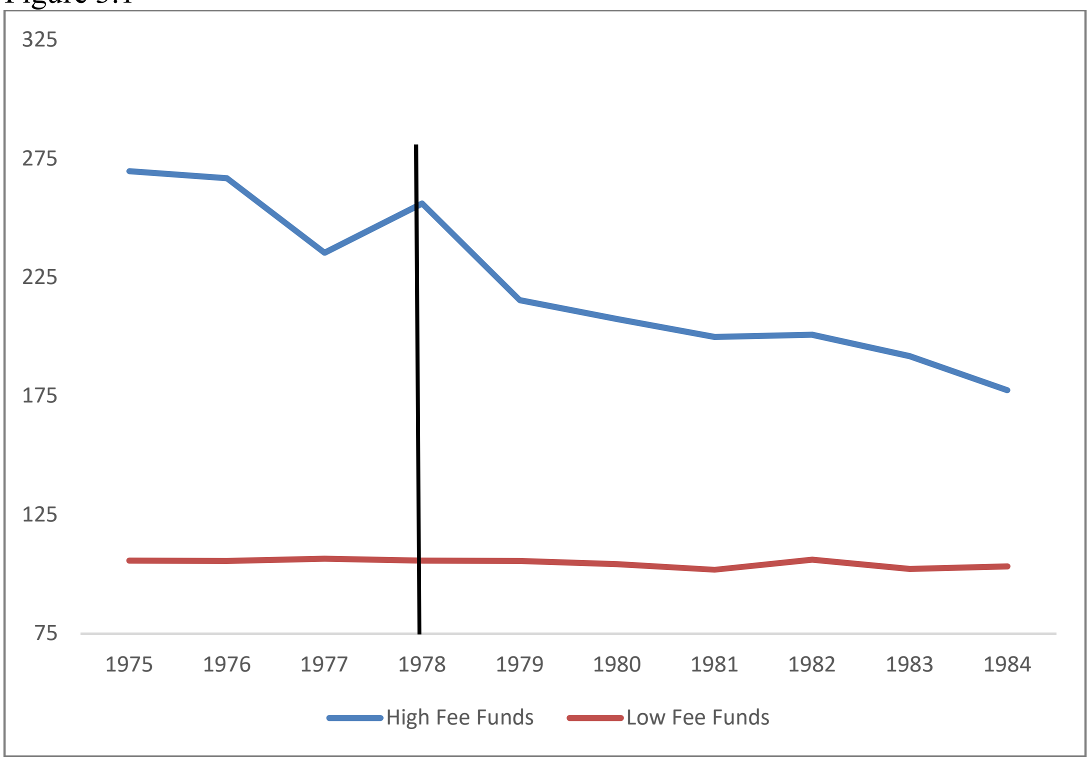
	High Fee Funds	Low-Fee Funds
# of Mutual Funds	53	250
# of Mutual Funds Kept	29	219
Economic Value	$302 million	$17,518 million
% Economic Value	86.1%	89.9%
	Cohort	
Average Fee (1975)	270 basis points	106 basis points
Average TNA (1975)	$9.29 million	$92.64 million
Average NAV (1975)	$10.49	$10.01

The first row of Table 3.1 indicates the entire universe of funds existing in 1975 and classifies them as either high fee or low fee based on our assignment strategy. As described above, in order to construct a balanced panel for difference-in-difference analysis we restrict our sample to mutual funds that operated continuously from 1975 to 1984. The top panel shows that although we lose some funds as a result of our restriction to a 10-year cohort, by far the majority of the economic value of the funds as measured by TNA is preserved. The bottom panel then provides the average fee, TNA, and NAV as of 1975 across both high fee and low fee groups.

Section 4: Results

We begin our investigation of the effects of excessive fee enforcement by plotting a simple time series of the mean fees for the high-fee and low-fee groups. In Figure 3.1, we plot the mean fee by year measured as of December in each year. As the SEC's enforcement action against Fundpack was initiated and completed in the Spring of 1979, to the extent that the suit is responsible for reducing mutual fund fees, the effect should manifest after 1978.

Figure 3.1

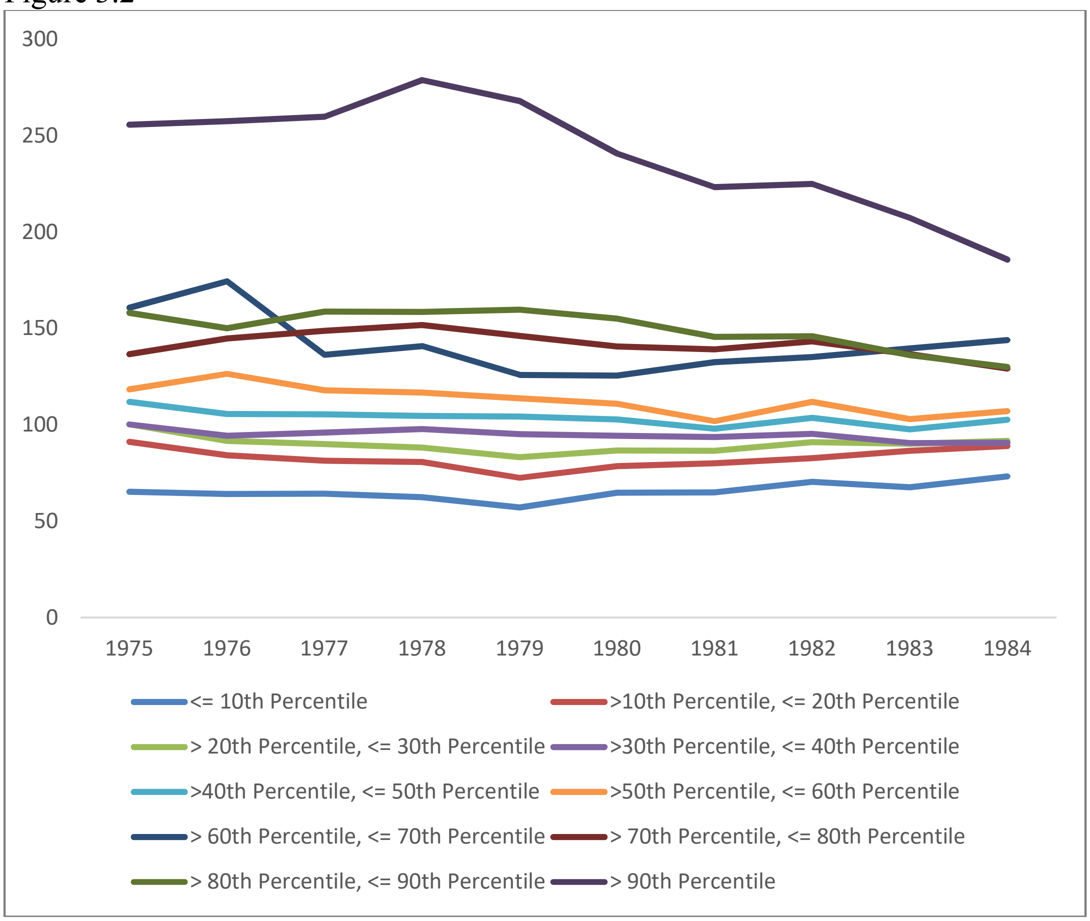


The cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1975 to 1984, inclusive. High fee funds are defined as mutual funds who had a fee higher than 193 basis points during any year from 1975 to 1978, inclusive, and low fee funds are defined as all other funds. The dependent variable used is the level of the fee, as defined by December fee fund of the calendar year.

Figure 3.1 indicates that the low-fee group of funds experiences very stable fees over the time period with no appreciable difference before and after 1978. On the other hand, the high-fee group of funds experiences a significant decline in fees starting after 1978.

In Figure 3.2, we consider the possibility of heterogeneity within groups by looking at the pattern of fund fees by decile based on fees in 1978.

Figure 3.2



In interpreting Figure 3.2, it is useful to keep in mind that Fundpack's 1978 fee of 193 basis points places it just below the 90^{th} percentile. Figure 3.2 suggests that there is little heterogeneity in the control group and provides strong suggestive evidence as to the appropriateness of our identification strategy.

Having considered these patterns in our underlying data, we now consider our primary difference-in-difference specification on fund fees. Results are in basis points.

Table 3.2

High Fee Fund * (Year ≥ 1979)	–58.325
Standard Error	23.982
P-Value	0.016
Unconditional Average	120.247

Results are in basis points. The cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1975 to 1984, inclusive. High fee funds are defined as mutual funds who had a fee higher than 193 basis points during any year from 1975 to 1978, inclusive, and low fee funds are defined as all other funds. The dependent variable used is the level of the fee, as defined by December fee fund of the calendar year. Standard errors are clustered by manager of the mutual fund.

Table 3.2 indicates that the basic patterns suggested by Figures 3.1 and 3.2 continue to hold after controlling for time, mutual fund, and fund size. The difference-in-difference results indicate that the decline in mutual fund fees for the high-fee group was approximately 60 basis points or almost one-third of the total fees charged by Fundpack in 1978 and almost half of the unconditional average fee.

To see how this effect evolved across time in more precise detail, in Table 3.3 we expand empirical equation (1) into an "event study" format, where we look at the yearly evolution while controlling for time-invariant mutual fund and aggregate year effects.[1]

---

[1] The empirical specification for this would be:

$$Y_{it} = \gamma_i + \gamma_t + \pi X_{it} + \sum_{t=1976}^{1985} \beta_t (\text{Year} = t) * HF_i + \varepsilon_{it}$$

where Y_{it}, γ_i, γ_t, X_{it}, HF_i are the same as they were in empirical equation (1). The coefficients of interest are $\beta_t, t = 1976 \ldots 1985$. β_{1980}, for example, represents the difference in Y from 1975 to 1980 for high fee funds relative to the change in low fee funds during that same time interval.

Table 3.3

	Point Estimate	Standard Error	P-Value
High Fee Fund*(Year ≥ 1976)	–3.129	30.213	0.918
High Fee Fund*(Year ≥ 1977)	–35.854	21.235	0.093
High Fee Fund*(Year ≥ 1978)	–15.392	29.262	0.600
High Fee Fund*(Year ≥ 1979)	–56.264	30.084	0.063
High Fee Fund*(Year ≥ 1980)	–63.534	30.110	0.036
High Fee Fund*(Year ≥ 1981)	–68.334	32.477	0.037
High Fee Fund*(Year ≥ 1982)	–71.742	34.296	0.038
High Fee Fund*(Year ≥ 1983)	–77.916	33.495	0.021
High Fee Fund*(Year ≥ 1984)	–93.347	32.027	0.004
P-Value from F test for equality: 1976=1977=1978=0		0.1788	

The cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1975 to 1984, inclusive. High fee funds are defined as mutual funds who had a fee higher than 193 basis points during any year from 1975 to 1978, inclusive, and low fee funds are defined as all other funds. The dependent variable used is the level of the fee, as defined by December fee fund of the calendar year.

Table 3.3 indicates that prior to 1979, the coefficients are not jointly significantly different from zero, suggesting that the cohort of high-fee funds are comparable with the low-fee funds. This provides further evidence as to the suitability of the low-fee fund group as a control group. Table 3.3 also indicates that after 1978 fees decline monotonically suggesting the continued and cumulative effect of mutual fund excessive fee enforcement in reducing the fees charged by high-fee mutual funds.

The above analysis provides compelling evidence that excessive fee enforcement was successful in reducing the prices charged by high-fee mutual funds. However, even if enforcement was successful in lowering fees, it is still possible, as suggested by Easterbrook's economic analysis, that lower fees could still be associated with worse consumer outcomes if lower fees reduce the quality of mutual fund management services or reduce consumers' set of mutual fund options. In the remainder of this section, we consider the effects of excessive fee enforcement on two measures of quality, fund flows and fund returns, and then conclude with an examination as to whether there is any evidence that SEC's enforcement action induced an increased likelihood of exit for high-fee funds.

In Table 3.4, we apply our difference-in-difference model where the dependent variable is now the fund flow as set forth in equation (2). The logic behind employing fund flow as a measure of quality is that it provides a "revealed-preference" measure of mutual fund quality. To the extent that the decreases in fees associated with high-fee funds cause mutual fund quality to fall, this should be associated with fund outflows as consumers react in response to the quality reductions.

Table 3.4

Point Estimate	0.276
Standard Error	0.31
P-Value	0.376
Unconditional Average	1.145

The cohort and high fee fund definitions are the same as those used within table 3.2. The dependent variable is flow, which is defined by empirical equations (2) and (3). Standard errors are clustered by manager of the mutual fund.

Table 3.4 indicates that there is no concomitant effect on fund flows for high-fee funds relative to low-fee funds associated with the decrease in fund fees induced by the SEC's enforcement action against Fundpack.

We also assess mutually fund quality directly by looking at the returns offered by high-fee mutual funds relative to low-fee funds following *SEC v. Fundpack*. As the main service offered by mutual funds is to provide superior returns to investors, to the extent that a fund's quality is degraded, this would likely be reflected in lower relative returns associated with the decreases in fees.

Table 3.5

High Fee Fund * (Year ≥ 1979)	0.042
Standard Error	0.036
P-Value	0.255
Unconditional Average	0.096

The cohort and high fee fund definitions are the same as those used within table 3.2. The dependent variable is flow, which is defined by empirical equations (2) and (3). Standard errors are clustered by manager of the mutual fund.

Like Table 3.4, Table 3.5 indicates that there is no statistically significant effect associated with this measure of mutual fund quality. Thus, although we find evidence of significant decreases in mutual fund fees as a result of the first enforcement action prosecuted under 36(b), we find little indication of countervailing reductions in quality.

Our final analysis in this section looks beyond the cohort of mutual funds examined above, which necessarily existed throughout the entire time period to consider the hypothesis that high-fee mutual funds were more likely to exit the industry as a result of the threat of excessive fee prosecution. We first subset our data to include only those mutual funds that existed during 1975 to 1979, inclusive. Then we analyze whether high-fee funds were more or less likely to exit the industry after the lawsuit by evaluating the additional probability of a fund being in operation based on being a high-fee fund relative to a low-fee fund. More formally, for each year from 1980-1984, in separate regressions, we estimate the following equations:

$$Y_i = \alpha + \beta \, HFfund_i + \pi \, TNA_i + \varepsilon_i \qquad (4)$$

The outcome, Y_i is a binary variable that equals 1 if mutual fund i had a record within a given year. TNA is once again the total net assets of the mutual fund i, and for these regressions we use the 1979 information of this variable. $HFfund_i$ is once again a dummy for whether or not the mutual fund is a high-fee fund. Our coefficient of interest is β, which is the difference in the probability of a high-fee fund staying within the market relative to a low-fee fund. We estimate the above equation separately for 1980, 1981, 1982, 1983, and 1984. The results are presented in Table 3.6:

Table 3.6

	1980	1981	1982	1983	1984
Point Estimate	0.029	0.014	–0.002	–0.032	–0.051
Std Error	0.028	0.047	0.051	0.052	0.056
P-Value	0.306	0.768	0.963	0.536	0.366

The cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1975 to 1979, inclusive. High fee funds are defined the same way as table 3.2. The dependent variable is a binary variable equaling 1 if the mutual fund existed within CRSP during that year, 0 otherwise.

As indicated above, there is no significant effect on the probability that a high-fee fund exits the market for any of the years listed above.

Section 5: Robustness

One of the biggest concerns is some of the low-fee funds may not be appropriate for inclusion in the control group, since mutual funds with very low fees may not be sufficiently comparable to the treatment group of high-fee funds. Our informal analysis in Figure 3.2 provides some evidence to suggest the overall comparability of the funds throughout the control group, but we now consider the issue more formally. Specifically, in Table 3.7 we consider the difference-in-difference estimates from equation (1) when we take out the bottom 10^{th} percentile, 25^{th} percentile, and even the median of the control group of low-fee funds.

Table 3.7

Removing the Bottom 10th Percentile	
High Fee Fund * (Year ≥ 1979)	–56.492
Standard Error	23.977
P-Value	0.020
Unconditional Average	127.983
Removing the Bottom 25th Percentile	
High Fee Fund * (Year ≥ 1979)	–55.265
Standard Error	24.026
P-Value	0.023
Unconditional Average	140.329
Removing the Bottom 50th Percentile	
High Fee Fund * (Year ≥ 1979)	–52.711
Standard Error	25.028
P-Value	0.039
Unconditional Average	166.192

Table 3.7 indicates that the results are very similar regardless of which specification of the control group is employed. Crucially, these results indicate that the primary results are not simply an artifact of comparing funds with high natural volatility relative to those with low natural volatility. Another test we implement to mitigate the effects of level differences is to run the same specification as (1) again, but this time using the natural log of fees as the dependent variable. These results are presented in Table 3.8 and indicate that our conclusions are robust to concerns about differences in levels.

Table 3.8

High Fee Fund * (Year ≥ 1979)	–0.2375
Standard Error	0.0643
P-Value	0
Unconditional Average	4.69

Finally, to conclude, in Table 3.9 we consider a series of placebo tests for ten-year cohorts beginning in 1965, 1966, 1967, 1968, and 1969 where the time period is completely unaffected by the SEC's litigation against Fundpack.

Table 3.9

1965–1974	
High Fee Fund * (Year ≥ 1970)	–9.152
Standard Error	10.836
P-Value	0.402
Unconditional Average	86.461
1966–1975	
High Fee Fund * (Year ≥ 1971)	–23.384
Standard Error	14.994
P-Value	0.124
Unconditional Average	90.347
1967–1976	
High Fee Fund * (Year ≥ 1972)	–14.313
Standard Error	16.588
P-Value	0.391
Unconditional Average	99.056
1968–1977	
High Fee Fund * (Year ≥ 1973)	18.954
Standard Error	19.958
P-Value	0.344
Unconditional Average	110.547
1969–1978	
High Fee Fund * (Year ≥ 1973)	15.072
Standard Error	18.569
P-Value	0.418
Unconditional Average	113.232

For the "1965–1974" years, the cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1965 to 1974, inclusive. The treatment year is 1970. For the "1966–1975" years, the cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1966 to 1975, inclusive. The treatment year is 1971. For the "1967–1976" years, the cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1967 to 1976, inclusive. The treatment year is 1972. For the "1968–1977" years, the cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1968 to 1977, inclusive. The treatment year is 1973. For the "1969–1978" years, the cohort used here are all domestic mutual funds within the CRSP data that appear every year from 1969 to 1978, inclusive. The treatment year is 1974. The dependent variable used is the level of the fee, as defined by December fee fund of the calendar year. Standard errors are clustered by manager of the mutual fund. Mutual funds are defined as "High-Fee" if the mutual fund had a fee higher than the 90th percentile of the year prior to the treatment year.

As none of these trials indicates a false positive, these results suggest that our primary estimates are credible.

Section 6: Conclusion

While excessive fee litigation has been the focus of vigorous debates among prominent figures in law and economics, the onset of excessive fee litigation has not previously been analyzed in the context of rigorous empirical methods. Our results suggest that the SEC's enforcement action against Fundpack in the Spring of 1979 ushered in a period of regulatory pressure that forced the highest-fee mutual funds to lower their fees quite substantially. However, this decrease in fees was not associated with any evidence of reduced quality as measured by either fund flows or the returns offered by high-fee mutual funds. Thus, in terms of the economic debates surrounding excessive fee enforcement, our analysis is quite consistent with Posner's suggestion that there is reason to believe that the mutual fund industry does not operate efficiently. Furthermore, our results are also inconsistent with the theory expounded by Wallison and Litan that excessive fee litigation is responsible for continued existence of high fees in the mutual fund industry.

Appendices

<u>Section A1: Appendix for Chapter 1</u>

A1.1: 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 A1.1 applies each propensity score specification to the benchmark price results from while Table A1.2 and Table A1.3 present the propensity score adjusted results for the benchmark quantity and TFPQ results. Table A1.4 presents the propensity score adjusted results for the local versus non-local horizontal merger analysis and Table A1.5 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 A1.1: Propensity Score Adjusted Benchmark Price Results

	[A1.1]	[A1.2]	[A1.3]	[A1.4]	[A1.5]
Dep. Var.	$\Delta PRICE$	$\Delta PRICE$	$\Delta PRICE$	$\Delta PRICE$	$\Delta PRICE$
ACQUIRED	0.074***	0.073***	0.073***	0.072***	0.073***
HORIZONTAL ACB	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
ACQUIRING ACB	0.060	0.061	0.063	0.056	0.061
	(0.059)	(0.058)	(0.060)	(0.057)	(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 A1.2: Propensity Score Adjusted Benchmark Quantity Results

	[A2.1]	[A2.2]	[A2.3]	[A2.4]	[A2.5]
Dep. Var.	$\Delta QUANTITY$	$\Delta QUANTITY$	$\Delta QUANTITY$	$\Delta QUANTITY$	$\Delta QUANTITY$
ACQUIRED	−0.146*	−0.147*	−0.147*	−0.148*	−0.148*
HORIZONTAL ACB	(0.078)	(0.082)	(0.078)	(0.078)	(0.079)
ACQUIRING ACB	−0.085	−0.085	−0.082	−0.062	−0.086
	(0.135)	(0.138)	(0.129)	(0.128)	(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 A1.3: Propensity Score Adjusted Benchmark TFPQ Results

	[A3.1]	[A3.2]	[A3.3]	[A3.4]	[A3.5]
Dep. Var.	$\Delta TFPQ$	$\Delta TFPQ$	$\Delta TFPQ$	$\Delta TFPQ$	$\Delta TFPQ$
ACQUIRED	0.044*	0.043*	0.046*	0.046*	0.044*
HORIZONTAL ACB	(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 A1.4: 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 A1.5: 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.

A1.2: Unweighted Results

In this appendix, I provide unweighted results to demonstrate the robustness of my conclusions to weighting used in the primary results. Table A1.6 considers the unweighted benchmark results for price, quantity, and TFPQ.

Table A1.6: Unweighted Benchmark Results

Dep. Var.	[A6.1] Δ*PRICE*	[A6.2] Δ*QUANTITY*	[A6.3] Δ*TFPQ*
ACQUIRED HORIZONTAL ACB	0.041***	−0.097	0.074***
	(0.019)	(0.067)	(0.024)
ACQUIRING ACB	0.040	−0.120	0.079
	(0.034)	(0.131)	(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 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 [A6.1] 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 A1.7, I consider the unweighted results for local versus non-local horizontal mergers. In regression [A7.1], I consider the effects on prices using the benchmark specification controlling for lagged revenue, and in regression [A7.2] I use the specification controlling for lagged price instead of revenue. Regression [A7.3] considers the effects on quantity.

Table A1.7: Unweighted Local Versus Non-Local Horizontal Merger Results

Dep. Var.	[A7.1] $\Delta PRICE$	[A7.2] $\Delta PRICE$	[A7.3] $\Delta QUANTITY$
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)	(0.121)	(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 A1.6 falling below the level of statistical significance, the decrease in change for *ACQUIRED HORIZONTAL ACB* plants in regression [A7.3] is statistically significant at the 5% level. Finally, in Table A1.8 I confirm the robustness of the pre- and post-1982 results in the absence of weighting.

Table A1.8: Unweighted Pre- and Post-1982 Horizontal Merger Results

	[8.1]	[8.2]	[8.3]	[8.4]
Dep. Var.	*ΔPRICE*	*ΔPRICE*	*ΔTFPQ*	*ΔTFPQ*
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.

Section A2: Appendix for Chapter 3

Table A2.1: 10-Year Cohort from 1974 to 1983

Fees	
High Fee Fund * (Year ≥ 1979)	–47.316
Standard Error	20.570
P-Value	0.023
Unconditional Average	120.972
Rate of Return	
High Fee Fund * (Year ≥ 1979)	0.036
Standard Error	0.033
P-Value	0.279
Unconditional Average	0.083
Flow	
High Fee Fund * (Year ≥ 1979)	0.330
Standard Error	0.351
P-Value	0.350
Unconditional Average	0.157

Table A2.2: 12-Year Cohort from 1973 to 1984

Fees	
High Fee Fund * (Year ≥ 1979)	–32.427
Standard Error	14.395
P-Value	0.026
Unconditional Average	118.057
Rate of Return	
High Fee Fund * (Year ≥ 1979)	0.025
Standard Error	0.026
P-Value	0.337
Unconditional Average	0.035
Flow	
High Fee Fund * (Year ≥ 1979)	0.302
Standard Error	0.281
P-Value	0.284
Unconditional Average	0.147

Bibliography

Abito, J. M. and Wright, J. (2008). Exclusive Dealing with Imperfect Downstream Competition. *International Journal of Industrial Organization*, 26(1):227.

Ackerberg, D. A., Caves, K., and Frazer, G. (2015). Identification Properties of Recent Production Function Estimators. *Econometrica*, 83:2411.

Aghion, P. and Bolton, P. (1987). Contracts as a Barrier to Entry. *American Economic Review*, 77(3):388.

Ashenfelter, O. C. and Hosken, D. (2010). The Effect of Mergers on Consumer Prices: Evidence from Five Mergers on the Enforcement Margin. *Journal of Law and Economics*, 53:417.

Ashenfelter, O. C., Hosken, D., and Weinberg, M. C. (2009). Generating Evidence to Guide Merger Enforcement. *Competition Policy International*, 5.

Ashenfelter, O. C., Hosken, D., and Weinberg, M. C. (2013). The Price Effects of a Large Merger of Manufacturers: A Case Study of Maytag/Whirlpool. *American Economic Journal: Economic Policy*, 6:308.

Ashenfelter, O. C., Hosken, D., and Weinberg, M. C. (2014). Did Robert Bork Understate the Competitive Impact of Mergers? Evidence from Consummated Mergers. *Journal of Law and Economics*, 57:S67.

Ashenfelter, O. C., Hosken, D., and Weinberg, M. C. (2015). Efficiencies Brewed: Pricing and Consolidation in the U.S. Brewing. *RAND Journal of Economics*, 46:328.

Asker, J. and Bar-Isaac, H. (2014). Raising Retailers' Profits: On Vertical Practices and the Exclusion of Rivals. *American Economic Review*, 104(2):672.

Backus, M. (2016). Why is Productivity Correlated with Competition?

Baily, M. N., Hulten, C., and Campbell, D. (1992). Productivity Dynamics in Manufacturing Establishments. *Brookings Papers on Economic Activity: Microeconomics*, page 187.

Baker, J. B. and Shapiro, C. (2008). Detecting and Reversing the Decline in Horizontal Merger Enforcement. *Antitrust*, 22:29.

Barberis, N., Huang, M., and Santos, T. (2001). Prospect Theory and Asset Prices. *Quarterly Journal of Economics*, 116:1.

Barberis, N. and Thaler, R. (2003). A Survey of Behavioral Finance. *Handbook of the Economics of Finance*, 1:1053.

Bernheim, B. D., Peleg, B., and Whinston, M. D. (1987). Coalition-Proof Nash Equilibria I. Concepts. *Journal of Economic Theory*, 42(1):1.

Bernheim, B. D. and Whinston, M. D. (1998). Exclusive Dealing. *The Journal of Political Economy*, 106(1):64.

Berry, S. T. (1994). Estimating Discrete-Choice Models of Product Differentiation. *RAND Journal of Economics*, 25:242.

Blonigen, B. A. and Pierce, J. R. (2016). The Effects of Mergers and Acquisitions on Market Power and Efficiency.

Bork, R. (1979). *The Antitrust Paradox*. Basic Books, New York.

Braguinsky, S., Ohyama, A., Okazaki, T., and Syverson, C. (2015). Acquisitions, Productivity, and Profitability: Evidence from the Japanese Cotton Spinning Industry. *American Economic Review*, 105:2086.

Burns, J. W. (1991). The New Role of Coercion in Antitrust. *Fordham Law Review*, 60(3):379.

Carhart, M. M. (1997). On Persistence in Mutual Fund Performance. *Journal of Finance*, 52:57.

Carlton, D. (2009). Why We Need to Measure the Effect of Merger Policy and How to Do It. *Competition Policy International*, 5.

Chen, Z. and Shaffer, G. (2014). Naked Exclusion with Minimum-Share Requirements. *RAND Journal of Economics*, 45(1):64.

Collard-Wexler, A. (2013). Demand Fluctuations in the Ready-Mix Concrete Industry. *Econometrica*, 81:1003.

Collard-Wexler, A. (2014). Mergers and Sunk Costs: An Application to the Ready-Mix Concrete Industry. *American Economic Journal: Microeconomics*, 6:407.

Davis, S. J., Haltiwanger, J., Handley, K., Jarmin, R., Lerner, J., and Miranda, J. (2014). Private Equity, Jobs, and Productivity. *American Economic Review*, 104:3956.

Davis, S. J., Haltiwanger, J., and Schuh, S. (1996). *Job Creation and Destruction*. MIT Press.

DeGraba, P. (2013). Naked Exclusion by an Input Supplier: Exclusive Contracting Loyalty Discounts. *International Journal of Industrial Organization*, 31(5):516.

Elhauge, E. (2003). Defining Better Monopolization Standards. *Stanford Law Review*, 56:253.

Elhauge, E. (2009). Tying, Bundled Discounts, and the Death of the Single Monopoly Profit Theory. *Harvard Law Review*, 123:397.

Elhauge, E. and Wickelgren, A. L. (2012). Anticompetitive Market Division through Loyalty Discounts without Buyer Commitment. *Harvard Discussion Paper No. 723*.

Fama, E. F. and French, K. R. (2010). Luck versus Skill in the Cross-Section of Mutual Fund Returns. *Journal of Finance*, 65:1915.

Farrell, J. and Shapiro, C. (1990). Horizontal Mergers: An Equilibrium Analysis. *American Economic Review*, 80:107.

Focarelli, D. and Panetta, F. (2003). Are Mergers Beneficial to Consumers? Evidence from the Market for Bank Deposits. *American Economic Review*, 93:1152.

Foster, L., Haltiwanger, J., and Syverson, C. (2008). Reallocation, Firm Turnover, and Efficiency: Selection on Productivity or Profitability? *American Economic Review*, 98:394.

Foster, L., Haltiwanger, J., and Syverson, C. (2016). The Slow Growth of New Plants: Learning about Demand? *Economica*, 83:91.

Friedman, R. D. (1986). Untangling the Failing Company Doctrine. *Texas Law Review*, 64:1375.

Fumagalli, C. and Motta, M. (2006). Exclusive Dealing and Entry, when Buyers Compete. *American Economic Review*, 96(3):785.

Haltiwanger, J., Jarmin, R., and Miranda, J. (2013). Who Creates Jobs? Small vs. Large vs. Young. *The Review of Economics and Statistics*, 95:347.

Hopenhayn, H. A. (1992). Entry, Exit, and Firm Dynamics in Long Run Equilibrium. *Econometrica*, 60:1127.

Hortaçsu, A. and Syverson, C. (2007). Cementing Relationships: Vertical Integration, Foreclosure, Productivity, and Prices. *Journal of Political Economy*, 115:250.

Innes, R. and Sexton, R. J. (1994). Strategic Buyers and Exclusionary Contracts. *American Economic Review*, 84(3):566.

Jensen, M. C. (1968). The Performance of Mutual Funds in the Period 1945–1964. *Journal of Finance*, 23:389.

Jovanovic, B. (1979). Job Matching and the Theory of Turnover. *Journal of Political Economy*, 87:972.

Jovanovic, B. (1982). Selection and the Evolution of Industry. *Econometrica*, 50:649.

Jovanovic, B. and Rousseau, P. (2002). The Q-Theory of Mergers. *American Economic Review*, 92:198.

Krattenmaker, T. G. and Pitofsky, R. (1988). Antirust Merger Policy and the Reagan Administration. *The Antitrust Bulletin*, 33:211.

Levinsohn, J. and Petrin, A. (2003). Estimating Production Functions Using Inputs to Control for Unobservables. *The Review of Economic Studies*, 70:317.

Lichtenberg, F. R. and Siegel, D. (1987). Productivity and Changes in Ownership of Manufacturing Plants. *Brooking Papers on Economic Activity: Special Issue on Microeconomics*.

Maksimovic, V. and Phillips, G. (2001). The Market for Corporate Assets: Who Engages in Mergers and Asset Sales and Are There Efficiency Gains? *Journal of Finance*, 56:2019.

Malkiel, B. G. (1995). Returns from Investing in Equity Mutual Funds 1971 to 1991. *Journal of Finance*, 50:549.

Mathewson, G. F. and Winter, R. A. (1987). The Competitive Effects of Vertical Agreements: Comment. *American Economic Review*, 77(5):1057.

McGuckin, R. H. and Nguyen, S. V. (1995). On Productivity and Plant Ownership Change: New Evidence from the Longitudinal Business Database. *RAND Journal of Economics*, 26:257.

Miller, N. H. and Weinberg, M. C. (2015). Mergers Facilitate Tacit Collusion: Empirical Evidence from the U.S. Brewing Industry.

Muris, T. J. (2008). Facts Trump Politics: The Complexities of Comparing Merger Enforcement Over Time and Between Agencies. *Antitrust*, 22:37.

Nevo, A. (2000). Mergers with Differentiated Products: The Case of the Ready-to-Eat Cereal Industry. *RAND Journal of Economics*, 31:395.

Odean, T. (1998). Are Investors Reluctant to Realize Their Losses? *Journal of Finance*, 53:1775.

Olley, G. S. and Pakes, A. (1996). The Dynamics of Productivity in the TelecommunicationS Equipment Industry. *Econometrica*, 64:1263.

Perry, M. K. and Porter, R. H. (1985). Oligopoly and the Incentive for Horizontal Merger. *American Economic Review*, 75:219.

Peters, C. (2006). Evaluating the Performance of Merger Simulation: Evidence from the U.S. Airline Industry. *Journal of Law and Economics*, 49:627.

Pierce, J. (2011). Plant-Level Responses to Antidumping Duties: Evidence from U.S. Manufacturers. *Journal of International Economics*, 85:222.

Posner, R. A. (2001). *Antitrust Law: An Economic Perspective*. University of Chicago Press, Chicago.

Rasmusen, E. B., Ramseyer, J. M., and Wiley, Jr., J. S. (1991). Naked Exclusion. *American Economic Review*, 81(5):1137.

Ravenscraft, D. J. and Scherer, F. M. (1987). *Mergers, Sell-offs, and Economic Efficiency*. Brookings Institution.

Salop, S. C. and Scheffman, D. T. (1983). Raising Rivals' Costs. *American Economic Review: Papers and Proceedings*, 73(2):267.

Segal, I. R. and Whinston, M. D. (2000). Naked Exclusion: Comment. *American Economic Review*, 90(1):296.

Shefrin, H. and Statman, M. (1985). The Disposition to Sell Winners Too Early and Ride Losers Too Long: Theory and Evidence. *Journal of Finance*, 40:777.

Shleifer, A. and Vishny, R. W. (1997). The Limits of Arbitrage. *Journal of Finance*, 42:35.

Simpson, J. and Wickelgren, A. L. (2007). Naked Exclusion, Efficient Breach, and Downstream Competition. *American Economic Review*, 97(4):1305.

Sirri, E. R. and Tufano, P. (1998). Costly Search and Mutual Fund Flows. *Journal of Finance*, 53:1589.

Small, K. A. and Rosen, H. S. (1981). Applied Welfare Economics with Discrete Choice Models. *Econometrica*, 49:105.

Stefanadis, C. (1998). Selective Contracts, Foreclosure, and the Chicago School View. *Journal of Law and Economics*, 41(2):429.

Syverson, C. (2004a). Market Structure and Productivity: A Concrete Example. *Journal of Political Economy*, 112:1181.

Syverson, C. (2004b). Product Substitutability and Productivity Dispersion. *Review of Economics and Statistics*, 86:534.

Syverson, C. (2008). Markets: Ready-Mixed Concrete. *Journal of Economic Perspectives*, 22:217.

Syverson, C. (2011). What Determines Productivity? *Journal of Economic Literature*, 49:326.

Thaler, R. H. (1985). Mental Accounting and Consumer Choice. *Marketing Science*, 4:199.

Van Biesebroeck, J. (2007). Robustness of Productivity Estimates. *Journal of Industrial Economics*, 55:529.

Wallison, P. J. and Litan, R. E. (2007). *Competitive Equity*. AEI Press.

Weick, D. P. (2014). The (Non-)Viability of Purchaser Class Actions in Exclusive Dealing and Loyalty Discount Cases Post-*Comcast*. *Perspectives in Antitrust*, 3(2).

Werden, G. J. (2015). Inconvenient Truths and Constructive Suggestions on Merger Retrospective Studies. *Journal of Antitrust Enforcement*, 3.

Whinston, M. D. (2006). *Lectures on Antitrust Economics*. MIT Press.

Whinston, M. D. (2007). *Antitrust Policy toward Horizontal Mergers*, volume 3 of *Handbook of Industrial Organization*, chapter 36, pages 2369–2440. Elsevier.

White, T. K., Reiter, J., and Petrin, A. (2015). Plant-level Productivity and Imputation of Missing Data in U.S. Census Manufacturing Data.

Williamson, O. E. (1968). Economies as an Antitrust Defense: The Welfare Tradeoffs. *American Economic Review*, 58:18.

Wooldridge, J. M. (2002). Inverse Probability Weighted M-Estimators for Sample Selection, Attrition, and Stratification. *Portuguese Economic Journal*, 1:117.

Wooldridge, J. M. (2004). Inverse Probability Weighted Estimation for General Missing Data Problems. CEMMAP Working Paper CWP05/04, Centre for Microdata Methods and Practice.

Wooldridge, J. M. (2009). On Estimating Firm-Level Production Functions Using Proxy Variables to Control for Unobservables. *Economics Letters*, 104:112.

Wooldridge, J. M. (2010). *Econometric Analysis of Cross Section and Panel Data*. MIT Press.

Wright, J. (2008). Naked Exclusion and the Anticompetitive Accommodation of Entry. *Economics Letters*, 98:107.

Wright, J. (2009). Exclusive Dealing and Entry, when Buyers Compete: Comment. *American Economic Review*, 99(3):1070.