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Event Studies in Merger Analysis: Review and an Application Using U.S. TNIC Data*

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Abstract

There is a growing concern that U.S. merger control may have been too lenient, but empirical evidence remains limited. Event studies have been used as one method to acquire empirical insights into the competitive effects of mergers. However, existing work suffers from strong identifying assumptions, unreliable competitor identification or small samples. After reviewing the use and challenges of event studies in merger analysis, I use a novel application of Hoberg-Phillips (2010, 2016) Text-Based Network Industry Classification (TNIC) data to readily proxy a ranking of competitors to 1,751 of the largest U.S. mergers between 1997 and 2017. I document that following a merger announcement, the most likely competitors experience on average an abnormal return of around one percent. These abnormal returns are also associated with concerns of market power, which suggests that results are at least in part driven by an anticipation of anti-competitive effects, and hence insufficient merger control.

JEL Codes: G14, G34, L13, L40
Keywords: Mergers, Antitrust, Event Studies, Text-Based Network Industry Classification

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1 Introduction

A broad discussion has emerged on the observation of increased industry concentration, markups and market power (Autor, Dorn, Katz, Patterson and Van Reenen, 2019; Basu, 2019; De Loecker and Eeckhout, 2018; De Loecker, Eeckhout and Unger, 2018; Grullon, Larkin and Michaely, 2019; Syverson, 2019). One concern is that concentration and market power may have increased as a consequence of an insufficient deterrence of anti-competitive mergers, especially in the U.S. (Baker, 2019, p. 15; Gutiérrez and Philippon, 2018; Kwoka, 2015; Philippon, 2019; Shapiro, 2018; 2019; Wollmann, 2019). Despite the prominence of this concern, empirical evidence remains limited. In particular, ex post reviews of merger decisions remain complex or costly (and often politically unfavorable), and hence scarce.[1]

Event studies have been proposed as a simple alternative to acquire empirical insights into the anticipated competitive effects of mergers. By estimating the abnormal stock returns around an event date, event studies aim to identify the anticipated effect of this event on future firm performance. In the context of mergers, existing event studies look at the abnormal returns of competitors around a merger announcement. This follows from seminal microeconomic theory that predicts that competitors to a merger benefit if anti-competitive effects from increased market concentration dominate, but lose out if pro-competitive effects from merger efficiencies dominate (Farrell and Shapiro, 1990). Existing studies then often use as identifying assumption that an abnormal increase (decrease) in competitor stock price following a merger announcement indicates that financial markets anticipate the merger to be anti-competitive (pro-competitive). However, this inverse relationship between competitor stock price and consumer welfare is not guaranteed, as it may be weakened by the presence of other mechanisms as well as stock market noise.

This paper starts with a discussion on the use of event studies in merger analysis.

[1]For a review of existing work, see in particular Kwoka (2015). The relative absence of empirical evidence within Industrial Organization on the competitive effects of mergers is prominently criticized by Angrist and Pischke (2010).

Existing studies generally suffer from the following limitations. First, the identifying assumption that abnormal competitor returns and consumer welfare are inversely related may fail to hold for various reasons and would have to be tested if used. Second, event studies in merger analysis require the reliable identification of competitors, which is not straightforward. And third, they involve the identification of small effects in very noisy data. This necessitates a sufficiently large dataset.

In a novel application of Hoberg-Phillips (2010, 2016) Text-Based Network Industry Classification (TNIC) data I am able to readily proxy a ranking of likely competitors to 1,751 U.S. mergers and acquisitions between 1997 and 2017 with a real transaction value above one billion dollar and a publicly-traded target. I document that likely competitors experience on average a positive and statistically significant abnormal return – with an estimated effect of around one percent. I also find that abnormal returns show a positive association with a TNIC-based Herfindahl-Hirschman Index (HHI). Because HHI serves as an indicator of market power concerns, this association suggests that results are at least in part driven by an anticipation of anti-competitive market power effects, and hence an insufficient deterrence of anti-competitive mergers.

The remainder of this paper is as follows. Section 2 provides an overview of possible mechanisms through which a merger announcement may affect stock prices, reviews event studies in merger analysis and identifies and discusses the main challenges in using event studies in merger analysis. Sections 3 discusses the data collection and cleaning. Section 4 outlines the methodology used and Sections 5 and 6 discuss the results and sensitivity checks. Section 7 concludes.

2 Review Event Studies in Merger Analysis

Event studies are a well-developed method within Finance and Economics that aims to identify the anticipated effect of an event on firm performance (Fama, Fisher, Jensen and Roll, 1969; MacKinlay, 1997). Event studies are based on the efficient market hypothesis, which states that stock prices reflect all publicly available information on future profits. By estimating the abnormal stock returns around an event date, event studies aim to identify the anticipated effect of this event on future firm performance.

The estimation of abnormal return for a particular stock is generally done in the following three steps (MacKinlay, 1997). First, a linear relationship between stock and market return (the 'market model') is estimated during some estimation window prior to the event date. Usually, the estimation window lasts for around 250 days and stops several days before the event, so as to exclude any event effects in the estimation. Second, the abnormal return for each day around the event date is derived as the difference between actual stock return and the return as predicted by the market model. Third, the cumulative abnormal return from the event is derived as the sum of all daily abnormal returns during some event window – which is the range of days in which the new information has likely become public. This cumulative abnormal return is supposed to capture the anticipated effect from the event on future firm performance. Different event window specifications are often used for robustness.

Event studies have been used to analyze the anticipated effects of mergers by looking at the abnormal returns that occur around a merger announcement date. Below I first discuss the mechanisms through which a merger announcement may affect stock prices and how they relate to the anticipated competitive effects of the merger. I then review existing literature that aims to identify these mechanisms and close off with a review of the main challenges in using event studies in merger analysis.

2.1 Mechanisms

When using event studies in merger analysis, the identifying assumption that is often used is that a positive (negative) abnormal competitor return at the time of the merger announcement indicates an anticipation of anti-competitive (pro-competitive) effects (Cichello and Lamdin, 2006). This inverse relationship follows from two opposing mechanisms within seminal microeconomic theory (Farrell and Shapiro, 1990): on the one hand, mergers generate cost efficiencies and other synergies that benefit the merging parties and consumers, but hurts competitors through reduced relative competitiveness; on the other hand, mergers increase unilateral market power of all market participants through a reduction in amount of firms active in the market. This benefits all firms, but hurts consumers. While merging firms benefit from both mechanisms, competitors lose out if the pro-competitive efficiency effect dominates and benefit if the anti-competitive market power effect dominates.

The inverse relationship between consumer welfare and competitor performance, as measured by its stock price, may however be weakened by the presence of other mechanisms. First, as also noted by Kwoka (2015, p. 42), a merger may enable the merged entity to foreclose its competitor through predatory behavior or other exclusionary practices. An increased expectation of such behavior would lead to a negative abnormal competitor return, but driven by an anticipation of anti-competitive effects.

Second, a merger may signal that similar firms are “in play” and hence have an increased probability of being acquired in the future (Servaes and Tamayo, 2014; Song and Walkling, 2000). Because being acquired often involves a stock price premium, any increase in the acquisition probability would already cause the current stock price to increase. In principle, this mechanism can be either anti- or pro-competitive, as any future merger may again generate both anti- and pro-competitive effects. Contrasting this “in-play” effect is also a possible “out-of-play” effect, in which a merger announcement signals that competitors have lost a race to acquire the target (Fridolfsson and Stennek, 2010), or perhaps be acquired themselves.

Finally, a merger announcement may reveal positive information on market fundamentals, industry prospects or a general scope for efficiencies that was previously private. For instance, Derrien, Frésard, Slabik and Valta (2019) show that positive abnormal returns for industry peers occur in merger announcements when the target is a public firm, but not when it is a private firm. They argue that an acquirer –

Table 1: How Merger Announcements May Affect Stock Prices

Mechanism	Merging Firms	Competitor Firms
Anticipated pro-competitive efficiencies	+	−
Anticipated anti-competitive market power effects	+	+
Anticipated anti-competitive exclusion effects	+	−
"In-play" effects	·	+
"Out-of-play" effects	·	−
Signalling on industry health and prospects	+	+

who is assumed to be better informed on current and future industry performance – will prefer a public over a private firm when public firms are undervalued, all else equal. The acquisition of a public firm may therefore involve a signal that similar public firms are undervalued. This may cause competitors to experience a positive abnormal return even in the absence of any anticipated anti-competitive effects or "in-play" effects.

Table 1 summarizes these mechanisms and the way in which they affect stock prices. It shows that the effect on the merging firms jointly is unambiguously positive. It also shows that, a priori, the effect on competitors is ambiguous: different mechanisms affect the competitor returns differently, and these mechanisms can be anti-competitive, pro-competitive or competitively neutral.

Note that each mechanism may already be present prior to an official merger announcement when the merger is to some degree anticipated. In as far as the different mechanisms are affected differently by any anticipation, estimates of the abnormal returns may be biased when looking at returns too close to the official announcement. This requires a careful consideration of the event window. Additionally, when financial markets believe that there is a probability that competition authorities will object to the merger, the effect of mechanisms that are conditional on the merging actually occurring may be discounted. Finally note that empirically, these mechanisms are generally obscured by the presence of stock market noise. This noise is amplified by the fact that firms are often large and diversified and the merger may only relate to one part of the business.

2.2 Existing Literature

Pioneering work on the use of event studies in merger analysis includes Stillman (1983), Eckbo (1983) and Eckbo and Wier (1985). Stillman (1983) looks at 11 U.S. horizontal mergers that occurred between 1964 and 1972 and were challenged by the competition authorities. Competitors are identified using opinions published in litigations or fact memoranda prepared by the competition authorities. Using a variation to the event study methodology mentioned above, he observes that in only one of these challenged mergers there is a positive abnormal competitor return around the time of the merger announcement, which he says suggests that U.S. competition authorities have challenged too many mergers.

Eckbo (1983) and Eckbo and Wier (1989) instead look at the average effect of a larger sample of up to 82 challenged U.S. horizontal mergers between 1963 and up to 1981. They use both SIC codes and public case summaries to identify competitors. Using event windows of up to 20 days prior and 10 days after the announcement, both studies find a positive and statistically significant average abnormal competitor return. However, they reject the hypothesis that this is driven by any anticipation of anti-competitive market power effects, because they do not observe the opposite result at the time of a merger challenge, which would have reduced the likelihood of merger approval. They suggest that the positive average abnormal competitor returns are instead driven by some signalling on the scope for competitors to improve performance. Fee and Thomas (2004) and Shahrur (2005) replicate these results for later periods, looking also at the effects on customers and suppliers.

These earlier studies have been criticized on several other grounds. First, their datasets are often limited to a few dozen mergers. Because using stock prices for merger analysis involves detecting small effects in noisy data, event studies have a very low precision in classifying individual mergers, or even small samples, as anti-competitive (Kwoka and Gu, 2015; McAfee and Williams, 1988; Werden and Williams, 1989a; 1989b). Second, they generally rely on industry codes such as SIC to identify competitors. This is problematic, because industry codes do a poor job at identifying antitrust markets (Werden, 1988; Hoberg and Phillips, 2016). Third, their event windows – often only a few days around the announcement – may be too restrictive, because an anticipation of a merger announcement may already occur much earlier (Duso, Gugler and Yurtoglu, 2010). And finally, these papers fail to test properly for the different possible mechanisms through which competitor stock prices are affected. For instance, they reject an anticipation of anti-competitive effects (despite the positive average abnormal competitor returns at the time of announcement) solely on the basis of an absence of statistically negative returns at the time of a merger challenge – which may simply be the consequence of low statistical power. Alternative explanations are suggested but not subjected to scrutiny.

Several more recent papers use event studies to analyze EU instead of U.S. merger control. These have the advantage over the earlier criticized work that they can reliably identify competitors to a large subset of EU mergers by using the published decisions by the European Commission – which is generally not available in the case of U.S. mergers. Instead of looking at the average abnormal competitor return (which these papers generally find is not statistically different from zero), they use the estimated abnormal returns to inform other policy questions.

More specifically, Duso, Neven and Röller (2007) look at 167 EU mergers between 1990 and 2002 and classify 46 as anti-competitive and 121 as pro-competitive – using the inverse relationship between competitor stock prices and consumer welfare as identifying assumption and an event window of up to five days prior and after the announcement. They go on to explain how the EU institutional and political environment can explain false positive and negative. Duso, Gugler and Yurtoglu (2010) additionally show that a positive and statistically significant pairwise correlation coefficient exists between the estimated abnormal returns and ex post accounting profit, provided a sufficiently long event window of up to 50 days prior to the event is used. Duso, Gugler and Yurtoglu (2011), using an event window of 50 days prior and five days after the announcement, go on to show that EU merger control has been partially effective at reversing positive abnormal returns when they are observed – correcting additionally for an estimated probability of antitrust interference. Finally, Duso, Gugler and Szücs (2013) extends these papers by looking at 368 EU mergers between 1990 and 2007. They also show how the 2004 merger reforms have partially improved EU merger control.[2]

For their approach, these papers require the classification of individual cases as either anti- or pro-competitive based on whether abnormal returns are positive of negative. Because of stock market noise, this approach may have a very low precision. Additionally, the existence of possible alternative mechanisms affecting the abnormal returns is acknowledged, but generally ignored in the analysis. For instance, Duso, Neven and Röller (2007) accept that merger announcements may signal a scope for competitor efficiencies as well as "in-play" or "out-of-play" effects. However, they argue that none of these mechanisms have a convincing empirical or theoretical basis and hence ignore them (pp. 462-464).

Finally, two recent papers use event studies to explicitly test for two of the other mechanisms. Firstly, Bernile and Lyandres (2019) show for 480 U.S. horizontal mergers between 1996 and 2005 a negative association between announced efficiencies and abnormal competitor returns – in line with an anticipation of pro-competitive efficiencies. Competitors are identified using the SIC3-granularity TNIC provided by Hoberg and Phillips (2010, 2016), providing on average 12.5 competitors per merger, and they use an event window of up to 20 days prior and after the event. While they do report positive average abnormal competitor returns overall, they do not explore

[2]While not using event studies, Stiebale and Szücs (2019) also look at EU competitor performance – using micro panel data instead. They show for 194 EU mergers between 1999 and 2007 that competitors experience a statistically significant increase in estimated markups relative to a control, with larger estimates when market power concerns are more likely.

the possible underlying mechanisms.

Secondly, Derrien, Frésard, Slabik and Valta (2019) observe that positive average abnormal competitor returns occur when the target firm is public, but not when it is private – looking at 984 horizontal U.S. mergers involving a public target and 7,010 involving a private target occurring between 1990 and 2015 and with a deal value above 10 million dollar. They identify competitors using four-digit SIC codes and an event window of at most five days prior and after the event. They argue that the difference in case of a public or private target is driven by a signal on industry health: an acquirer – who is assumed to be better informed on industry performance – would prefer a public firm when public firms are undervalued. The acquisition of a public firm therefore involves a signal that similar public firms are undervalued. They reject an anticipation of anti-competitive market power effects or "in-play" effects, based on an absence of statistical significance with SIC-based HHI and future competitor acquisition.

2.3 Challenges

The review above identifies the following main challenges to the application of event studies in merger analysis. First, any assumed mechanism would have to be tested explicitly. For instance, the identifying assumption that a positive (negative) abnormal competitor return implies an anticipation of anti-competitive (pro-competitive) effects may fail to hold. When testing any mechanism, good practice dictates that a lack of statistical significance on itself does not prove an absence of effect.

Second, the use of event studies in merger analysis requires the reliable identification of competitors. This is not straightforward. As mentioned, the often-used method of relying on industry codes such as SIC does a poor job at identifying antitrust markets (Werden and Williams, 1988; Hoberg and Phillips, 2016). This is because they classify firms based on the supply instead of demand side, are often too broad, hardly reclassify firms as markets evolve and their binary nature imposes transitivity (in which the set of competitors to any two firms has to be identical).

And third, event studies in merger analysis involves the identification of small effects in very noisy data (Werden and Williams, 1989a). This means that event studies have a very low precision when looking at individual mergers or even small samples. In other words, they require a sufficiently large dataset to achieve sufficient statistical power, which many of the existing studies do not have.

In the remainder of this paper I use event studies to look at a large subset of major U.S. mergers that occurred between 1997 and 2017. I deal with the second and third challenges by using a novel application of Hoberg-Phillips data, which can be used to proxy a ranking of competitors to many mergers. I deal with the first challenge by testing whether the cumulative abnormal returns are associated with indicators of market power concerns. I remain agnostic on whether results are additionally driven by any of the other mechanisms.

3 Data Collection and Cleaning

I use the Refinitiv SDC Platinum database to identify all major U.S. mergers between January 1997 and December 2017 with a real transaction value above one billion dollar and a publicly-traded target. Using the Hoberg-Philips TNIC database, I am able to proxy a ranking of competitors to 1,751 of the largest U.S. mergers with a public target. Daily stock market returns of all relevant firms and the S&P 500 as benchmark stock market index are collected using Compustat. Below the data collection and cleaning is discussed in more detail.

3.1 Subset of Mergers

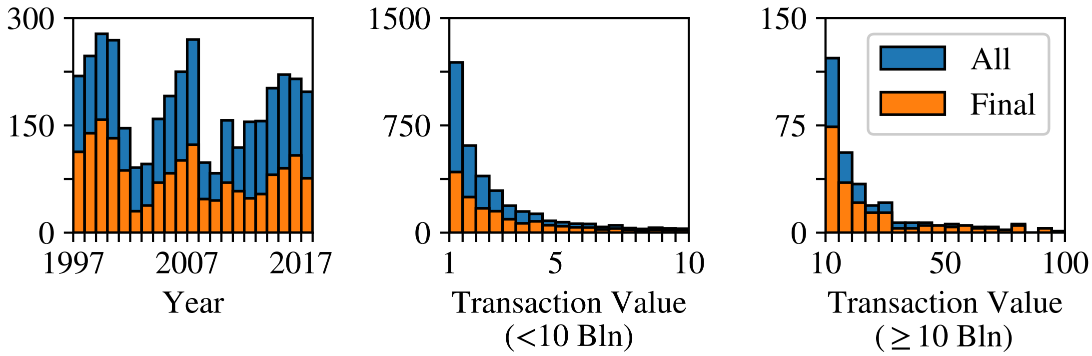
A dataset of all 98,123 transactions between January 1997 and December 2017 with a U.S. firm as target is extracted from the Refinitiv SDC Platinum database. The collected variables include target firm name, acquiring firm name, date of official announcement, transaction value, increase in shareholding, post-transaction shareholding and whether or not the transaction has been completed.

From this dataset, 3,794 transactions are identified that (i) had a transaction value of at least one billion dollar (in December 2017 value), (ii) have been completed, (iii) involved a share acquisition of at least 50 percent and (iv) did not have the acquirer listed as 'shareholders'. Of these, 1,661 transactions had to be dropped because the name of the target firm could not be identified in the Hoberg-Phillips database. In most cases, this is because the target firm is not publicly listed and hence not even included in the Hoberg-Phillips database (which only includes publicly-traded U.S. firms, as discussed below). In some cases however, an absence of identification may be because the names in the Refinitiv SDC Platinum database were not properly matched with those in the Hoberg-Phillips database. This has been checked manually, but with no guarantee that all matches have been correct.

Finally, 382 mergers were dropped because the merging parties had no TNIC score despite both being identified in the Hoberg-Phillips database. This could be because Hoberg and Phillips drop scores if firms are classified as vertically related, discussed below. Excluding these is justified because we are interested in horizontal mergers. Another reason could be an erroneous matching of firm names. Such observations are essentially random noise, which justifies excluding them.

Figure 1 shows the distribution of mergers over the years and real transaction values – separately for all 3,794 transactions and for the final dataset of 1,751 transactions. The left panel illustrates the well-known merger waves: before 2000, before 2008 and until recently. The middle and right panels show that the distribution over the real transaction value is skewed. In both the full and the final dataset, about 50% of mergers have a real transaction value between one and 2.5 billion dollar, 25% between 2.5 and five billion dollar, 20% between five and 20 billion dollar and only the remaining five percent above 20 billion dollar. While the distributions of the full and final dataset are relatively similar, the final distribution does have more observations with higher transaction value. This is most likely because the final dataset only includes transactions with a target firm that is publicly traded and higher-valuation firms are more often publicly traded.

Figure 1: Distribution of Mergers



Notes: Distribution of mergers over years and real transaction value, for all U.S. mergers with a real transaction value of at least one billion dollar (in December 2017 value) and for the final dataset used.

3.2 Hoberg-Phillips TNIC Scores

The Hoberg-Phillips (2010, 2016) Text-Based Network Industry Classification (TNIC) database consists of yearly matrices with pair-wise firm differentiation scores for all 13,808 publicly-traded U.S. firms from 1996 to 2017. The scores are derived from a text-based analysis of their 10-K business descriptions, which firms submit yearly to the Securities and Exchange Commission and which is legally required to represent a concise and accurate summary of their product offerings. Hoberg and Phillips claim that the TNIC scores capture the degree of competition between two firms, based on the premise that firms with more common vocabulary in their 10-K product descriptions are nearer competitors. As such, the TNIC scores can be interpreted as a Hotelling-like product differentiation score: firms with higher TNIC scores have more similar 10-K product descriptions and are therefore closer competitors.[3]

For the purpose of this paper, I identify the most likely competitors as those firms with the highest TNIC score with the target firm. I also vary the lowest admissible rank, to see for which ranks an average effect may be found. An alternative approach would select all firms with a TNIC score above a certain cut-off value. This approach is omitted for two reasons. First, it requires the assumption that TNIC scores are also cardinal instead of only ordinal – which may not hold across industries or years. Second, empirically this approach leads to many mergers with no competitors at all, while at the same time leading to many other mergers with unreasonably many competitors.

TNIC scores have several advantages over industry codes such as SIC. Most importantly, TNIC scores are a continuous measure between zero and one, instead of a binary in-out classification. This allows for a much finer selection of potential competitors than industry codes, which often include many hundreds of firms. Additionally, they are based on product descriptions (demand side) instead of the production process (supply side). Furthermore, because TNIC scores are pair-wise, they are not restricted to transitivity – in which two competitors have to have the same set of other competitors. Finally, TNIC scores are updated yearly, which accommodates changing industry relations following from innovation, dynamic product differentiation or mergers.

It may be that vertically related firms use similar vocabulary for the 10-K product descriptions as well, without competing with each other. Hoberg and Phillips purge the TNIC scores for possible vertical relations as follows. Using Benchmark Input-Output Accounts of the U.S. Economy, they calculate the fraction of inputs that flow between the four-digit SIC codes of each firm pair. If this fraction exceeds one percent of all inputs, it is assumed that the firms have a vertical relation and their TNIC scores are excluded. This occurs in four percent of all firm pairs. While this

[3] All data is available open-source on http://hobergphillips.tuck.dartmouth.edu. This website also includes further explanations, as well as instructions on how to use and interpret the data.

approach is not exhaustive in excluding all vertical relations, note that any remaining vertically related firms identified as competitors will bias results downwards, making any positive estimates more conservative.

The Hoberg-Phillips TNIC database provides a convenient differentiation proxy without requiring any market definition or detailed price and quantity data. The main limitation is that it remains a proxy. Because we use the TNIC scores for the ranking of most likely competitors, we risk including weak competitors while excluding strong competitors. Note however, that this cause our estimates to be biased towards zero, making any estimate more conservative. Additionally, the TNIC database only includes publicly-traded U.S. firms. Results are therefore not necessarily externally valid in case of privately-owned or foreign-traded target firms.

3.3 Stock Market Returns

The daily closing prices of all 13,808 firms in the Hoberg-Phillips TNIC database are collected from Compustat for 1 January 1995 to 31 December 2018. In case a company has multiple issue IDs trading at the same time, the oldest issue ID is maintained. The first observation of any new issue ID is also dropped, because it often involves a different stock price level and hence a discontinuous jump in stock return. The S&P 500 is used as the market index.

The daily stock market returns are dropped when they are larger than 30% or smaller than -30%. This occurs in 1.4% of all daily stock market returns. Dropping these observations is done to exclude outliers: the remaining dataset includes several inexplicably large stock price movements that have a large impact on the final results. One possible explanation for such major stock market movements are (reverse) stock splits. These cause sudden jumps in stock prices but are unfortunately not flagged in the Compustat stock market data. Sensitivity checks show that results are robust when dropping daily stock market returns beyond bounds of 20% and -20% or of 50% and -50% – which occurs in 2.4% and 0.7% of all cases, respectively. In the event study estimation, I also only include stocks that have at most 20 days without an observation during the estimation and event window.

4 Event Study Methodology

Using event study methodology, the cumulative abnormal return of each of the proxied competitors is calculated around the official merger announcement date. The event study methodology is used as follows, which is in line with existing literature (Cichello and Lamdin, 2006). For an estimation window of 290 to 51 days prior to the official announcement date of each merger, a linear relationship between stock return R_{it} of competitor i to this specific merger on day t and the market index R_{mt} is estimated:

$$\mathrm{R}_{it} = \alpha_i + \beta_i \mathrm{R}_{mt} + \varepsilon_{it}, \tag{1}$$

where we omit a subscript indicating the merger. For an event window of 50 days prior and 30 days after the official announcement date, abnormal return AR_{it} is calculated for each separate day as the actual return minus the return as predicted by the above market model:

$$\mathrm{AR}_{it} = \mathrm{R}_{it} - \hat{\alpha}_i - \hat{\beta}_i \mathrm{R}_{mt}, \tag{2}$$

where $\hat{\alpha}_i$ and $\hat{\beta}_i$ are the coefficient estimates from the market model. Finally, we derive cumulative abnormal return CAR_{it} for each competitor i up to day t as the sum of the average abnormal returns in window $[t_0, t]$:

$$\mathrm{CAR}_{it} = \sum_{s=t_0}^{t} \mathrm{AR}_{is}, \tag{3}$$

with starting date $t_0 \geq$ -50. CAR_{it} has a straightforward interpretation: it shows for each competitor i to a merger how much more cumulative return it has gotten since

t_0 than what would be expected based on its previous performance.

CAR_{it} is derived for those firms proxied as closest competitors to each of the 1,751 mergers, based on a TNIC-based ranking. The amount of observations per day is therefore 1,751 times the lowest admissible rank. This allows us to estimate the average and confidence interval of the cumulative abnormal returns of competitors for each day around the official merger announcement.

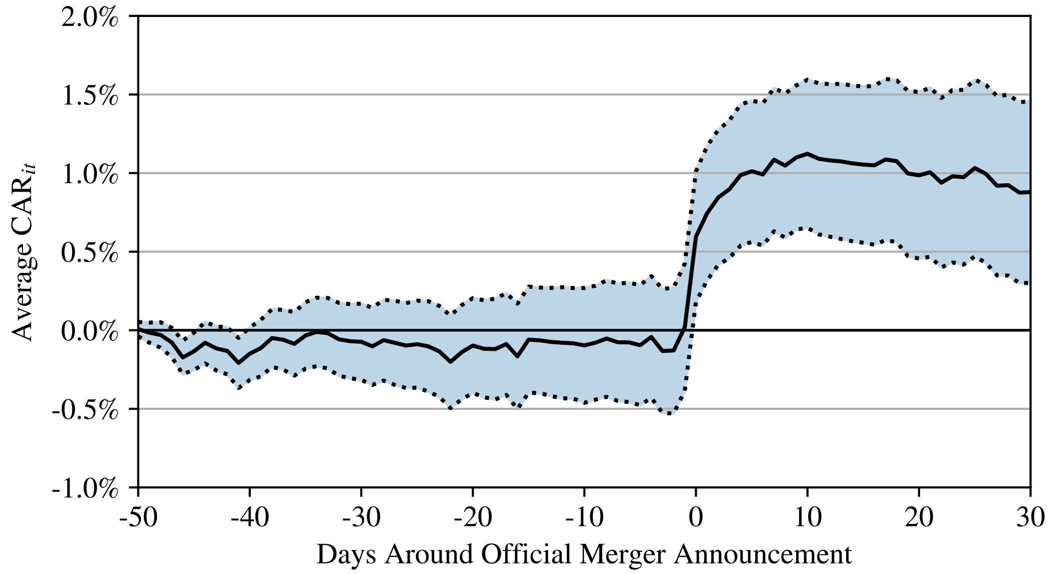
Note that market awareness of the upcoming merger may occur already in the run-up to the official merger announcement. News of the merger may leak out or markets may anticipate such a move. This is why the estimation window runs until 51 days prior to the official announcement and the event window starts at most 50 days prior to the announcement. This is in line with the literature discussed. The trade-off is that setting the cut-off earlier reduces the risk of excluding merger effects from the event window, but increases noise and vulnerability to structural breaks in the estimated market model – and vice versa when setting the cut-off later.

5 Results

Estimates are derived using robust regression (Berk, 1990; Hamilton, 1991), with standard errors clustered per merger. Robust regression has the benefit over ordinary least squares (OLS) in that it reduces the weight of observations that disproportionately affect the estimation, hence reducing the vulnerability to outliers. While not (yet) common within event studies, Sorokina, Booth and Thorton (2013) show how in event studies the accuracy cumulative abnormal returns generally improves under robust regression. Additionally, sensitivity checks in the next section suggest that results are conservative relative to OLS.

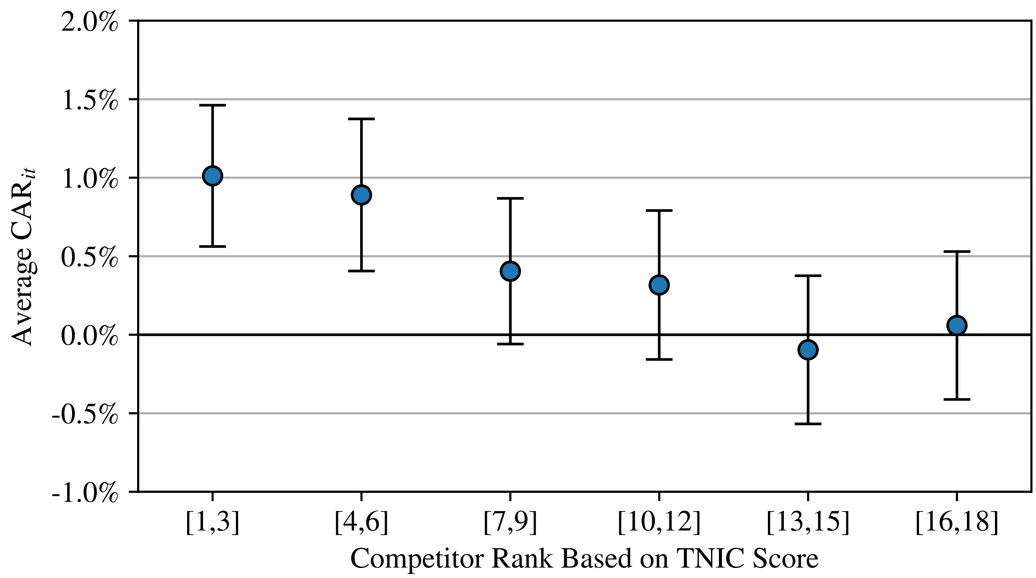
Figure 2 plots the average and 95% confidence interval for all CAR_{it} for the three closest competitors to all mergers, taking t_0 = -50 as the start of the event window. Results clearly show a positive average cumulative abnormal competitor return of

Figure 2: Estimated Average Cumulative Abnormal Returns Around Event



Notes: Average cumulative abnormal returns for three closest competitors, for event window starting at t_0 = -50. Estimate and 95% confidence interval are based on robust regression with merger-clustered standard errors.

Figure 3: Estimated Average Cumulative Abnormal Returns, Effect Ranks



Notes: Average cumulative abnormal returns for different ranks, for event window $t \in$ [-50, 5]. Estimate and 95% confidence interval are based on robust regression with merger-clustered standard errors.

around one percent. In other words, financial markets anticipate on average a higher discounted future cash flow for competitors following a merger announcement. Figure 3 shows that the average cumulative abnormal competitor returns are only statistically significantly different from zero for firms ranked more closely to the merger target.

Existing work often interprets such results as an anticipation of anti-competitive effects, based on the assumed inverse relationship between competitor returns prices and consumer welfare. To test this, I regress the cumulative abnormal returns on a TNIC-based Herfindahl-Hirschman Index (HHI) for market concentration for each competitor, which serves as an indication of market power concerns. This TNIC-based HHI is provided by Hoberg and Phillips and is derived as a conventional HHI by summing the squared market shares of all firms within a market.[4]

Table 2 shows that there is indeed an association between the TNIC-based SIC3-granularity HHI of each competitor, at least for a sufficiently wide event window. This suggests empirically that competitors in markets where there is potentially a bigger market power concern experience a larger abnormal return following a merger announcement. While this result does not exclude additional mechanisms, it does suggest that results are at least in part driven by an anticipation of anti-competitive market power effects.

It is unclear exactly why there is no significance for the shorter event window starting 10 days prior to the official announcement. One reason could be that mergers with a larger market power concern are generally anticipated earlier, such that the stock price may have already increased prior to this shorter window. Note finally that the difference in amount of observations is driven by the fact that robust regression drops observations from the total of 5,253 observations that have a particularly

[4]Hoberg and Phillips define the relevant market for each firm as all other firms with a TNIC score above a certain threshold. They set this threshold such that the granularity is equivalent to three-digit SIC codes. This means that if you pick two random firms, they have the same probability of being in the same market as under three-digit SIC codes. See http://hobergphillips.tuck.dartmouth.edu for this data and further use-instructions.

Table 2: Results Regressing CAR_{it} on Market Concentration Proxy

Event Window	[-50, 5]	[-40, 5]	[-30, 5]	[-20, 5]	[-10, 5]
TNIC3 HHI	0.0345**	0.0273**	0.0454***	0.0291***	0.0106
	(0.0152)	(0.0131)	(0.0116)	(0.0093)	(0.0069)
Constant	0.0054*	0.0058*	0.0031	0.0034*	0.0059***
	(0.0031)	(0.0028)	(0.0024)	(0.0019)	(0.0014)
Observations	5,160	5,155	5,169	5,163	5,141

Notes: Robust regression estimates for cumulative abnormal return for three closest competitors for different event windows, controlling for competitor TNIC-based SIC3-granularity HHI. Merger-clustered standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

disproportionate effect on the estimation.

An additional possible mechanism is the "in-play" effect, in which the announcement signals that similar firms are more likely to be acquired in the future. Tables 3 and 4 show the association between the cumulative abnormal returns and whether the competitor itself or any of the six closest competitors becomes a target in the subsequent 12 or 60 months (and in this sample). There is a general absence of statistically significant results, apart from the narrow event window of 10 days prior and five days after the announcement date. It therefore remains unclear whether this mechanism is very prominent. Note again that the difference in amount of observations is driven by the fact that robust regression drops apparent outliers, from the total of 5,025 in case of 12 months and 4,026 in case of 60 months (which are also less than the previous 5,253 observations, because of less available data on the future).

Note that the proxy for the anticipation of anti-competitive effects or "in-play" effects may still be insufficient to fully capture these mechanisms. Specifically, the TNIC-based HHI has the limitation that it only considers publicly-traded U.S. firms, while many markets also include privately-owned or foreign-traded firms. This is only a concern however when abnormal competitor returns are somehow lower in markets

Table 3: Results Regressing CAR_{it} on Future Competitor Merger

Event Window	[-50, 5]	[-30, 5]	[-10, 5]	[-50, 5]	[-30, 5]	[-10, 5]
Competitor Merges 12M	-0.0051	0.0059	0.0114**			
	(0.0091)	(0.0072)	(0.0047)			
Competitor Merges 60M				0.0045	0.0084	0.0068**
				(0.0066)	(0.0053)	(0.0031)
Constant	0.0118***	0.0096***	0.0075***	0.0112***	0.0088***	0.0072***
	(0.0024)	(0.0019)	(0.0011)	(0.0029)	(0.0023)	(0.0013)
Observations	4,934	4,947	4,920	3,952	3,961	3,943

Notes: Robust regression estimates for cumulative abnormal return for three closest competitors for different event windows, controlling for whether the competitor mergers within the next 12 or 60 months. Merger-clustered standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

Table 4: Results Regressing CAR_{it} on Any Future Competitor Mergers

Event Window	[-50, 5]	[-30, 5]	[-10, 5]	[-50, 5]	[-30, 5]	[-10, 5]
A Competitor Merges 12M	0.0012	0.0037	0.0039			
	(0.0059)	(0.0046)	(0.0027)			
A Competitor Merges 60M				-0.0028	-0.0027	0.0012
				(0.0055)	(0.0044)	(0.0025)
Constant	0.0112***	0.0091***	0.0072***	0.0134***	0.0115***	0.0076***
	(0.0026)	(0.0021)	(0.0012)	(0.0039)	(0.0031)	(0.0018)
Observations	4,936	4,947	4,921	3,952	3,961	3,945

Notes: Robust regression estimates for cumulative abnormal return for three closest competitors for different event windows, controlling for whether any of the six closest competitors mergers within the next 12 or 60 months. Merger-clustered standard errors are in parentheses. ***, ** and * indicate statistical significance at 1%, 5% and 10% respectively.

with more privately-owned or foreign firms (and hence an artificially lower TNIC-based HHI). Additionally, any abnormal competitor returns may still, at least in part, be driven by the signalling on industry health and prospects as well as countervailing mechanisms.

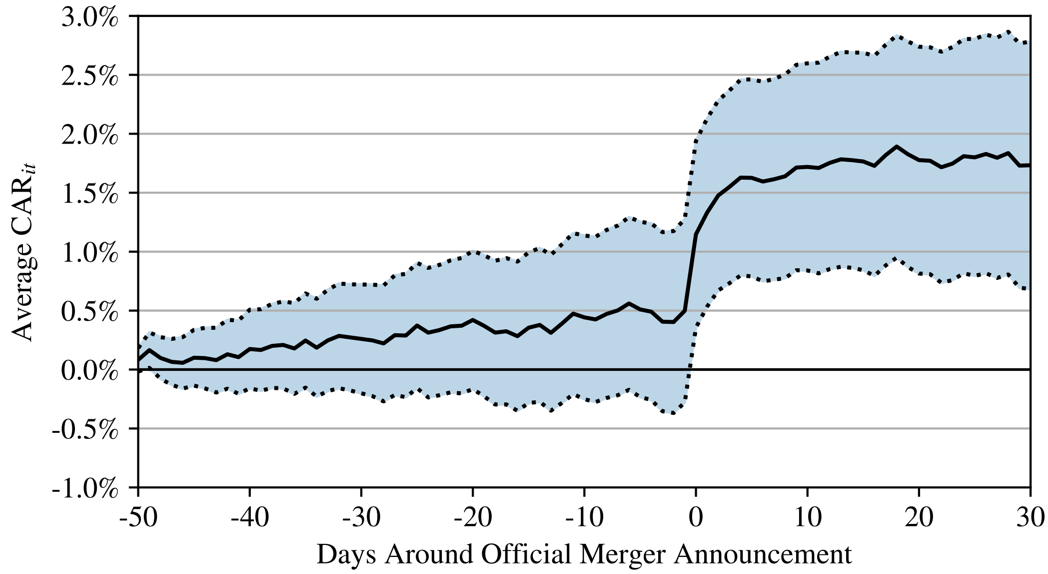
6 Sensitivity Checks

In the empirical strategy used, there are two choices in particular that may warrant attention. First, the choice for robust regression instead of OLS reduces the vulnerability of the estimates to outliers during the event window, but is at the same time much less conventional. Second, daily stock market returns in excess of -30% or 30% have been dropped to deal with outliers in the estimation window, but this cut-off has been chosen arbitrarily.

Figure 4 shows the estimates when using OLS instead of robust regression. Note that the estimated average cumulative abnormal return is now higher at around 1.5% instead of one percent. While not statistically significant, the estimate now also increases in the run-up to the official announcement date. Additionally, the standard errors under OLS are larger than under robust regression. The difference is driven by the fact that robust regression reduces the weight of observations that heavily affect the estimation – in the most extreme case dropping the observation. This makes OLS much more vulnerable to outliers than robust regression.

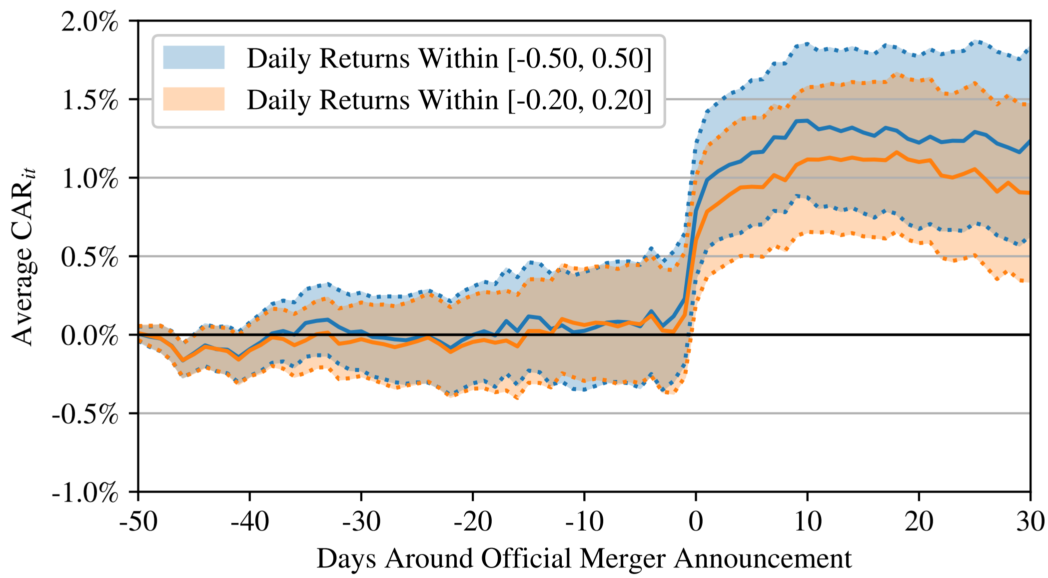
Figure 5 shows the estimated average cumulative abnormal return, with 95% confidence interval, separately for when daily returns are dropped if they are in excess of -50% and 50%, or in excess of -20% and 20%. Results are not materially affected relative to the benchmark of -30% and 30%. While not shown here, the qualitative conclusions from regressing CAR_{it} on the market concentration proxy and future competitor merger are also similar. While not shown here, results are robust to reasonable variations in the estimation and event window.

Figure 4: Estimated Average Cumulative Abnormal Returns, OLS



Notes: Average cumulative abnormal returns for three closest competitors, for event window starting at t_0 = -50. Estimate and 95% confidence interval are based on ordinary least squares regression with merger-clustered standard errors.

Figure 5: Estimated Average Cumulative Abnormal Returns Around Event



Notes: Average cumulative abnormal returns for three closest competitors, for event window starting at t_0 = -50. Estimate and 95% confidence interval are based on robust regression with merger-clustered standard errors.

7 Concluding Remarks

There is a growing concern that U.S. merger control may have been too lenient. Empirical evidence remains limited however, mostly because of the limited data availability necessary for proper ex post merger reviews. Event studies have been proposed as a simple alternative to acquire at least some empirical insights into the anticipated competitive effects of mergers.

This paper shows how event studies can be used as supplementary or circumstantial empirical evidence on the concern of insufficient merger control – at least in the aggregate. Existing studies do generally suffer from strong identifying assumptions, unreliable competitor identification or small samples. I am able to overcome these challenges by using a novel application of Hoberg-Phillips TNIC data, which allows me to readily proxy the most likely competitors to a large subset of the largest U.S. mergers between 1997 and 2017. I find that the most likely competitors benefit on average from a major merger announcement.

The association with a TNIC-based HHI further suggests that results are at least in part driven by an anticipation of anti-competitive effects. This provides the circumstantial evidence that U.S. merger control has been too lenient – even though event studies cannot say anything about individual mergers, for which case-specific ex ante or ex post merger reviews remain necessary.

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