# Mergers and Market Power: Evidence from Rivals' Responses in European Markets

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**Abstract** This paper analyzes the effects of mergers and acquisitions on the markups of non-merging rival firms across a broad set of industries. We exploit expert market definitions from the European Commission's merger decisions to identify relevant competitors in narrowly defined product markets. Applying recent methodological advances in the estimation of production functions, we estimate markups as a measure of market power. Our results indicate that rivals significantly increase their markups after mergers relative to a matched control group. Consistent with increases in market power, the effects are particularly pronounced in markets with few players, high initial markups and concentration. We also provide evidence that merger rivals reduce their employment, sales and investment, while their profits increase around the time of a merger.

**Keywords:** Merger, Markups, Productivity, Market Power, Innovation, Investment

**JEL Codes:** D22, L40, L13, O31

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# 1 Introduction

Recent research has raised concerns about the consequences of the rise in industry concentration and market power that has been documented across several sectors and countries (Autor et al., 2017; Basu, 2019; De Loecker et al., 2019; De Loecker and Eeckhout, 2018; Grullon et al., 2017; Syverson, 2019). It is likely that the increase in market concentration is at least partly a result of merger and acquisition (M&A) activity (Gutiérrez and Philippon, 2018) which has also increased substantially over the past decades with a combined value of worldwide deals exceeding $3 trillion per year.[1]

While higher market concentration through M&A can increase markups at the expense of consumers, M&A may also induce productivity gains through complementary assets, economies of scale and scope or an efficient reallocation of resources (e.g., Braguinsky et al., 2015; Farrell and Shapiro, 1990). To which extent the potential increase in market power through M&A is outweighed by efficiency gains is a central question in industrial organization and corporate finance that ultimately boils down to an empirical matter.

Due to ambiguous theoretical predictions and the difficulty of predicting observed price patterns with counterfactual merger simulations, it has been argued that more evidence from ex-post merger analysis is needed (e.g., Angrist and Pischke, 2010). Disentangling market power from other post-merger adjustments is, however, a challenging task which requires accurate measures of prices relative to marginal costs and a precise definition of the relevant market. For this reason, a growing literature which estimates the effects of M&A on prices and efficiency is mostly limited to specific industries or single merger cases where such precise measurement is possible. These studies have, however, produced mixed results that cannot easily be generalized.[2] A few recent studies have estimated the effects of acquisitions on markups of target firms for a broader set of industries. [3] However, these studies remain silent about the reactions of non-merging competitors and therefore the effects on the relevant market as a whole.

The focus on merging firms and the absence of evidence on non-merging competitors in the existing literature is unfortunate for several reasons. First, since acquirer and target might benefit from merger synergies, their markups could increase because of either rising prices or declining marginal costs. Unless one observes either prices or marginal costs (or assumes that one of the two variables remains constant), which is unlikely in a representative sample of industries, it is difficult to identify effects on market power. Since non-merging rival firms are, in contrast, unlikely to benefit from merger-specific cost reductions,

[1] See, for instance, `https://www.ft.com/content/9f0270aa-eabf-11e7-bd17-521324c81e23`, accessed June 7, 2019.

[2] For recent empirical studies see the overview of related literature in Ashenfelter et al. (2014).

[3] Most notably, Blonigen and Pierce (2016) find that target firms in the US significantly raise their markups upon acquisition which they attribute to increased market power. Stiebale and Vencappa (2018) provide evidence for increasing markups after domestic and foreign acquisitions in India as well but their results can be mainly explained by efficiency gains and quality upgrading rather than market power.

increases in markups in these firms are strongly suggestive of increases in market power. Second, a precise definition of rival firms is important to avoid comparing acquirers and targets to a control group that is affected by mergers. Finally, without taking the effects on rivals into account, it is not possible to get a complete picture about the effects of mergers on the relevant market as a whole.

The main reason for the lack of empirical evidence on rivals' markup responses to mergers is the absence of precise market definitions in commonly used firm-level datasets. To overcome this limitation, we construct a rich data set of M&A which exploits a unique feature of European merger control. In contrast to the US Federal Trade Commission, and other competition authorities we are aware of, the European commission (EC) publishes precise market definitions and the identities of rival firms in all merger cases undergoing a competitive assessment. These market definitions take product categories and geography into account and are based on the assessment of industry experts and confidential data that the EC is able to request from merging parties and their competitors. The difference to commonly used industry classifications is striking. While there are often several hundreds or even thousands of firms with the same industry code—even within a single country—the median number of non-merging rival firms across merger cases is 10 in our sample. We argue that such a precise market definition is essential to identify the effects of mergers on market power across a representative set of industries.

We combine information on M&A and the identity of rival firms with balance sheet data of European firms from Bureau van Dijk's Orbis database. Orbis contains standard variables that are used for the estimation of production functions including sales, material expenditures, the number of employees, capital stock and wage bill next to information on patents and a rich set of control variables. We use this data set to apply recent advances in the estimation of production functions which account for the endogeneity of inputs (Ackerberg et al., 2015; De Loecker and Warzynski, 2012). Estimates of production function parameters make it possible to estimate markups as a measure of market power at the firm-level. These estimated markups are used along with other outcome variables to study the pre- and post-merger performance of rival firms. Since mergers might not occur randomly, we apply a propensity score matching procedure to construct an adequate control group of firms with similar characteristics that have not been affected by mergers. We then compare changes in outcome variables around the time of merger cases between rival firms and the control group using a difference-in-differences (DiD) estimator.

To preview the findings, our estimates indicate that markups of non-merging rival firms increase on average by 2% to 4% in post-merger periods relative to the comparison group. This result is robust towards various alternative specifications of production functions to recover markups and holds with or without controlling for different combinations of industry and country-specific trends. We also provide evidence that changes in markups are unlikely to be explained by reductions in marginal costs upon mergers which could, for instance, stem from productivity enhancing investments. Our results indicate that if anything, investment, approximated by changes in tangible assets, and innovation, measured by citation-weighted patent applications, decline in post-merger periods. Further, consistent with increased market power, we find that merger rivals reduce their sales, employment and value added in post-merger periods, while profits increase. Markups seem to adjust to mergers with a time lag. While our results are small and statistically insignificant for the first two post-merger years, we estimate statistically significant effects for later years which increase over time and reach around 5% after 5 years on average.

We also provide evidence of heterogeneous effects which indicate that increases in rivals' markups are particularly pronounced when market power is likely to play an important role. Our estimated effects are concentrated among rivals with initially high market shares and markups, in market where the number of competitors is low and in domestic rather than cross-border merger cases.

Our paper is related to several strands of literature. There are a number of empirical studies that focus on changes in market prices after firm consolidation in specific industries including airlines (Kim and Singal, 1993; Kwoka and Shumilkina, 2010), banking (Prager and Hannan, 1998; Focarelli and Panetta, 2003), cotton spinning (Braguinsky et al., 2015), health care (Dafny et al., 2012; Lewis and Pflum, 2017), gasoline (Hastings, 2004; Houde, 2012), pharmaceuticals (Björnerstedt and Verboven, 2016) and retail (Allain et al., 2017; Hosken et al., 2018). The results of this literature have been mixed.[4]

A few studies provide evidence on the effects of M&A on markups in cross-industry studies. Blonigen and Pierce (2016) analyze effects of M&A on plants of target firms in the US and find increasing markups in post-acquisition periods while there is no significant effect on revenue-based measures of productivity. Stiebale and Vencappa (2018) estimate effects of acquisitions on target firms in India and provide evidence for cost reductions and quality improvements especially when targets are acquired by firms from high-income countries. However, the samples of M&A studied by these papers are not necessarily the most relevant from an economic policy point of view since they potentially include many acquisitions of firms which do not directly compete with acquirers as well as smaller target firms which do not generate concerns by antitrust authorities. In contrast to this literature, our paper studies effects on non-merging rival firms and focuses on horizontal mergers which have been under scrutiny by antitrust authorities. Gugler and Szücs (2016) make use of a similar dataset of market definitions as our paper. Employing synthetic control methods to analyze accounting measures of profits at the market level, they find that profitability increases in markets with mergers. Our paper instead analyzes markups at the firm level (instead of profits at the market level) as a more direct measure of market power.

This paper is also related to the broader literature on market power and market concentration. For instance, De Loecker et al. (2019) and De Loecker and Eeckhout (2018) find that markups, defined as prices relative to marginal costs, have increased from below 1.1 in 1980 to more than 1.6 in recent years.

[4] See also the survey of related empirical studies in Ashenfelter et al. (2014).

At the same time, a growing macroeconomic literature finds that market concentration has substantially increased[5] which could be a potential explanation for rising markups and declining investment (e.g., Gutiérrez and Philippon, 2016, 2017). However, the relationship between market power and measured market concentration is far from conclusive. First, market concentration is not necessarily associated with low competition and high market power. For instance, an increase in competition can lead to a reallocation of resources towards larger firms implying higher market concentration but lower markups (Melitz and Ottaviano, 2008; Syverson, 2019). Second, to measure market concentration accurately, one needs precise market definitions which are difficult to construct in cross-industry studies. Industry codes used in the literature are usually much broader than product markets[6] and macroeconomic patterns of market concentration seem to be very sensitive to the definition of local versus national product markets (Rossi-Hansberg et al., 2018). We contribute to this literature by analysing how changes in market concentration–induced by M&A–affect market power and other outcomes in narrowly defined product markets.

The rest of this paper is organized as follows. Section 2 describes our data set, while our empirical strategy is detailed in section 3. Results of the empirical analysis are discussed in section 4 and section 5 concludes.

## 2 Data, matching and estimation

### 2.1 Construction of the dataset

We analyze 194 merger cases that were notified to the EC between 1999 and 2007. From the EC's decision documents, we identify all firms, that were found to be direct competitors of the merging firms.[7]

In our selection of mergers, we have deliberately oversampled cases that went to a phase 2 investigation and/or were eventually remedied.[8] We focus on these cases, because the EC found them potentially problematic and therefore conducted extensive competitive assessments. Thus, we observe precise market definitions as well as rival firms and their market shares.[9] In cases that do not raise competitive concerns, the EC does not initiate a comprehensive market investigation.[10] Of the 194 mergers in the data, 95

[5] See the overview in Basu (2019) and Syverson (2019).

[6] See, for instance, Pittman and Werden (1990) for the inappropriateness of standard industry classifications for the definition of antitrust markets in merger cases.

[7] Data available at `http://ec.europa.eu/competition/mergers/overview_en.html`.

[8] When a merger is notified, the Commission has an initial timeframe of 25 working days for a first assessment (phase 1). Should additional time be required, the Commission can initiate phase 2 proceedings, lasting for up to 90 additional working days. We do not include mergers that were prohibited, as they did not entail a change in market structure.

[9] For confidentiality reasons, the EC decisions usually report market shares in 10 percentage point brackets; in our data, we use the midpoints of the intervals provided.

[10] In 2004, the EC introduced a fast track for cases unlikely to raise concerns. Since then, around 2/3 of cases have been handled under this simplified procedure.

(49%) were unconditionally cleared in phase 1, while 50 (26%) were cleared subject to remedies. The remaining 49 cases were evaluated in a phase 2 investigation, after which 14 (7%) were cleared, 35 (18%) were remedied.

The decision documents for these merger cases make reference to a total of 2,589 rival firms (corresponding to 2,107 unique firms, as some firms are rivals in more than one case). The rival firms are active in 132 different 4-digit NACE industries and originate from all member states. In 2,143 cases (83%, corresponding to 1,631 unique firms), we succeed in linking rival firms to Bureau van Dijk's Orbis database. We also link the rival firms to PATSTAT to account for their innovation activities.

To estimate productivity and markups (see below), we require information on a firms' capital, labour and material use, as well as lagged values thereof. These high data demands lead to substantial sample attrition. We further eliminate some outliers (we winsorize the top and bottom percentiles of estimated productivities and markups as well as all input variables and drop a few negative markup estimates) and impose some reasonable restrictions on the data (we drop rivals for which no industry deflators or suitable control observations could be found). Finally, we lose some data due to the inclusion of lagged values in the matching procedure (see also below). The final estimation sample includes 588 merger rivals (460 unique firms).[11]

The 588 merger rivals in our data are observed for an average of 15.3 years in an unbalanced panel, ranging from 1998 to 2015. They, on average, hold a market share of 12.9% in the markets affected by mergers. As a further measure of competition, we have recorded the total number of competitors in a a merger identified by the EC, which averages at 13 firms and 94 firm-market combinations (including double-counts of firms across product markets). 66% of cases are classified as cross-border deals, while in the remaining cases acquirer and target have their headquarters in the same country.

Most of the merger rivals in the final dataset are headquartered in France (32%), Germany (14%) and the UK (13%). A total of 18 European nations are included in the data. In terms of industry composition, the largest sectors included in the data are wholesale trade (30%), chemicals (10%) and pharmaceuticals (5%), while a total of 59 2-digit NACE sectors are included.

We complement the set of merger rivals with a large group of potential control firms from the Orbis database, which will serve as a donor pool in the construction of the control group.

[11] Most of the data is lost due to variables needed for productivity estimation not being reported for some countries. For example, while our original set of competitors includes firms from Bosnia, Bulgaria, Croatia, Denmark, Ireland, Romania and Russia, these countries are not included in the estimation sample. The industry composition of the sample, on the other hand, is not strongly affected by data availability.

## 2.2 Estimation of productivity and markups

Our starting point for the estimation of markups and productivity is a production function for firm $i$ producing in industry $j$ at time $t$:

$$Q_{it} = F_j(M_{it}, K_{it}, L_{it})\Omega_{it} \tag{1}$$

where $Q_{it}$ denotes output, $M_{it}$ is material input, $K_{it}$ and $L_{it}$ are capital stock and labour input respectively and $\Omega_{it}$ denotes total factor productivity (TFP). A firm minimizes costs subject to the production function and input costs. As shown by De Loecker and Warzynski (2012), this cost minimization yields an expression for the firm-specific markup, defined as the ratio of price to marginal cost, as:

$$\mu_{it} = \left(\frac{P_{it}Q_{it}}{P^M_{it}M_{it}}\right)\frac{\partial Q_{it}(.)}{\partial M_{it}}\frac{M_{it}}{Q_{it}} = \frac{\theta^M_{it}}{\alpha^M_{it}} \tag{2}$$

where $P_{it}$ denotes the output price, $P^M_{it}$ is the input price of materials, $\alpha^M_{it}$ is the ratio of expenditures on materials to a firm's revenue and $\theta^M_{it}$ is the elasticity of output with respect to material input. Intuitively, the output elasticity equals the input's revenue share only in the case of perfect competition. Under imperfect competition, the output elasticity will exceed the revenue share. As we describe below, $\theta^M_{it}$ can be estimated from a production function and $\alpha^M_{it}$ can easily be constructed from a firm's balance sheet.

In our baseline empirical implementation, we use a Translog production function. However, we also experiment with alternative functional forms such as a Cobb-Douglas production function. In logarithmic form, the production function can be written as:

$$q_{it} = f_j(m_{it}, k_{it}, l_{it}) + \omega_{it} + \varepsilon_{it} \tag{3}$$

where $\varepsilon_{it}$ denotes measurement error in output.[12]

To estimate the production function, we follow De Loecker and Warzynski (2012) and Ackerberg et al. (2015) and assume that a firms' material demand function can be inverted such that: $\omega_{it} = h(m_{it}, k_{it}, l_{it}, \boldsymbol{ma_{it}}, \boldsymbol{x_{it}})$ where $\boldsymbol{ma_{it}}$ is a vector of pre- and post-merger dummies and $\boldsymbol{x_{it}}$ contains additional control variables such as age, time and average wages.[13] Estimation relies on a two-step approach where the first stage does not identify any parameters of the production function but is used to eliminate measurement error:

$$q_{it} = \phi(m_{it}, k_{it}, l_{it}, \boldsymbol{ma_{it}}, \boldsymbol{x_{it}}) + \varepsilon_{it} \tag{4}$$

[12] For the Cobb Douglas case, $f(m_{it}, k_{it}, l_{it}) = \beta_m m_{it} + \beta_k k_{it} + \beta_l l_{it}$, for the translog production function: $f(m_{it}, k_{it}, l_{it}) = \beta_m m_{it} + \beta_k k_{it} + \beta_l l_{it} + \beta_{mm} m^2_{it} + \beta_{ll} l^2_{it} + \beta_{kk} k^2_{it} + \beta_{ml} m_{it} l_{it} + \beta_{mk} m_{it} k_{it} + \beta_{kl} k_{it} l_{it} + \beta_{mkl} m_{it} k_{it} l_{it}$

[13] The inclusion of additional variables such as average wages in the first stage addresses identification problems of gross output production functions found by Gandhi et al. (2018). See also De Loecker and Scott (2016) for a discussion.

Further, the following law of motion for unobserved productivity is assumed:

$$\omega_{it} = g(\omega_{i,t-1}, \boldsymbol{ma_{i,t-1}}) + \zeta_{it} \tag{5}$$

where we explicitly allow mergers to affect the productivity process and factor demand. The endogenous productivity process yields moment conditions:

$$E[\zeta_{it}(\beta) \times \boldsymbol{z_{it}}] = 0 \tag{6}$$

where $\boldsymbol{z_{it}}$ contains current capital and labour and lagged material input.[14] An estimate of productivity is obtained as $\hat{\phi}_{it} - f(\hat{\boldsymbol{\beta}}, m_{it}, k_{it}, l_{it})$ where $\hat{\phi}_{it}$ is a prediction from a first stage regressions in which we regress output on a polynomial in all production factors, average wages as well as time and merger dummies.

The production function is estimated separately for each 2-digit industry to allow for sector-specific production technologies. In our baseline specification, rivals of merging firms are therefore assumed to produce with the same production function as other firms in the same two-digit industry. The assumption of a common production function means that parameters related to inputs are assumed to be constant within an industry. This does, of course, not rule out differences in TFP across firms. Further, our Translog production function allows elasticities and returns to scale to vary with input use and therefore to differ across firms and time. Note that the assumption of sector-specific production technologies does not imply any assumptions about the definition of product markets. For instance, within the textile sector, t-shirts and dresses are arguably sold in different product markets but might still be produced with similar combinations of materials, labour and capital. However, we also experiment with several alternative specifications of production functions including a specification where we allow parameters to differ between rival firms and control observations within each industry. We discuss these alternative specifications in detail in section 3.4.

We estimate markups using estimated parameters from the production function and revenue shares of materials. For the Translog case, $\theta_{it}^{M} = \beta_m + 2\beta_{mm}m_{it} + \beta_{mk}k_{it} + \beta_{ml}l_{it} + \beta_{mkl}k_{it}l_{it}$ while in the Cobb-Douglas specification, $\theta_{it}^{M} = \beta_m$. We follow Ackerberg et al. (2015) and De Loecker and Warzynski (2012) and correct the revenue share $\frac{W_{it}^{M} M_{it}}{P_{it} Q_{it}}$ for measurement error in output using $\hat{\varepsilon}_{it}$, the residual estimated from the first stage.

As most other firm-level data sets, our database does not contain information about firm-specific input and output prices. Therefore, we approximate outputs by deflated revenues and materials and the capital stock by deflated monetary values of material expenditures and fixed assets, respectively. Hence, instead

[14] It is standard in the productivity literature to assume that materials can be flexible adjusted and are thus endogenous to current productivity shocks, while capital depends on past investment and is thus exogenous to $\zeta_{it}$. Since European labour markets are rather rigid, it is unlikely that labour input can be flexibly adjusted to current productivity shocks either.

of a measure of physical TFP, our estimates identify a measure of revenue TFP. Therefore, as common in the literature, our estimate of productivity is not a clean measure of efficiency as it is a combined measure of physical productivity and prices (De Loecker et al., 2016; Brandt et al., 2017).

To the extent that deviations of output prices from industry-specific means are reflected in higher input prices, the bias from using revenues instead of output quantities is reduced. However, if changes in relative firm-specific output prices, which are not associated with changes in input prices, are correlated with mergers, we cannot identify changes in physical TFP separately from changes in markups.

A further concern is that approximating quantities with sales expenditures yields biased production function coefficients. For the more general Translog production function, biased production function coefficients can in principle affect estimated markups across firms within industries. However, the price bias is unlikely to affect the estimates of the impact of mergers on markups in the Cobb Douglas case (De Loecker and Warzynski, 2012). Even if the use of monetary values in the production function biases estimated production function coefficients, this bias will be constant across firms and time since all variation in markups across firms and time within industries is due to variation in the revenue share of materials while production function parameters are constant within industries. A drawback of the Cobb-Douglas specification is that its functional form is more restrictive. However, the fact that we obtain qualitatively similar results for the effects of mergers on markups estimated from Translog and Cobb Douglas production function makes us confident that our results are unlikely to be driven by price bias of the production function. Descriptive statistics on estimated markups and output elasticities by sector are documented in the Appendix.

## 2.3 Matching and sample balance

We potentially face selection issues when evaluating the performance of merger rivals in the data: first, firms non-randomly select into merging with other firms (e.g., Dafny, 2009; Houde, 2012; Ornaghi, 2009; Szücs, 2014) and second, some of the drivers of merger activity are to be found on the market level rather than the firm level (Rhodes-Kropf and Viswanathan, 2004; Gugler and Szücs, 2016). Therefore, it seems natural that rivals of merging firms might not be randomly selected either.

Looking at firm characteristics, we find that merger rival firms are indeed quite different from the other firms in the data. The first three columns of Table 1 report the mean values of different variables for rivals and non-rival firms, as well as the $p$-value of a $t$-test for equal means. The table shows that rival firms are both larger (based on their total assets) and more innovative (based on both patent measures) than other firms. This is also reflected in the innovation dummy, which is equal to one for firms holding at least one patent. The average productivity and markup levels of rivals are 5-7% lower, but they have a higher probability of being on the fringes of both distributions (i.e. in the $1^{st}$ or $4^{th}$ quartile). Finally, also their lagged average productivity and markups (calculated from lags 2 to 5 relative to the merger year) are moderately lower.

Table 1: Covariate means before and after matching

| | Before Matching | | | After Matching | | |
|---|---|---|---|---|---|---|
| Variable | Treated | Control | p-value | Treated | Control | p-value |
| Propensity Score | 0.005 | 0.001 | 0.00 | 0.008 | 0.007 | 0.19 |
| log(Total Assets) | 10.769 | 9.678 | 0.00 | 10.605 | 10.474 | 0.26 |
| log(Cumulated Patents) | 1.038 | 0.140 | 0.00 | 0.963 | 0.968 | 0.97 |
| log(Current Patents) | 0.210 | 0.025 | 0.00 | 0.233 | 0.223 | 0.85 |
| Innovation Dummy | 0.276 | 0.148 | 0.00 | 0.269 | 0.276 | 0.79 |
| TFP | -0.072 | 0.001 | 0.00 | -0.078 | -0.104 | 0.19 |
| Markup | 0.356 | 0.409 | 0.00 | 0.359 | 0.380 | 0.28 |
| TFP: $1^st$ Quartile | 0.336 | 0.145 | 0.00 | 0.372 | 0.408 | 0.21 |
| TFP: $4^th$ Quartile | 0.204 | 0.146 | 0.00 | 0.204 | 0.206 | 0.94 |
| Markup: $1^st$ Quartile | 0.168 | 0.141 | 0.00 | 0.168 | 0.153 | 0.48 |
| Markup: $4^th$ Quartile | 0.155 | 0.141 | 0.00 | 0.168 | 0.182 | 0.54 |
| Average Markup | 0.348 | 0.404 | 0.00 | 0.350 | 0.371 | 0.26 |
| Average TFP | -0.077 | 0.003 | 0.00 | -0.084 | -0.108 | 0.22 |

*Notes:* Propensity scores are the predicted values from the model in Table 2. The innovation dummy is one for firms with at least one patent. The 'quartile'-variables are dummy variables, indicating the respective quartiles of the TFP and markup distributions. The averages are calculated from up to five lagged values.

A widespread approach to account for self-selection while generating a plausible counterfactual is the construction of a control group through a matching procedure and the application of DiD estimation.[15] The matching procedure is a two-stage process: first, we estimate the likelihood of treatment (the propensity score) for both treated and non-treated firms. The propensity score is the ex-ante probability of being a merger rival, calculated based on observable characteristics. This measure is then used by a matching algorithm to select the control group. By matching treated observations to control observations based on their propensity scores, we obtain two groups that do not differ systematically with respect to the characteristics that the propensity score was calculated upon (Rosenbaum and Rubin, 1983). The procedure thus controls for the observable heterogeneity between treated and non-treated firms.

To obtain the propensity score, we estimate a Probit model relating treatment status to firm character-

[15] Recent applications of matching in combination with DiD to evaluate the effects of M&A include Blonigen and Pierce (2016), Guadalupe et al. (2012) and Javorcik and Poelhekke (2017).

istics, as well as country, industry and year fixed-effects. The treatment indicator is set to one if a firm was named a relevant rival in an EC merger decision issued that year. Since the merger can only be implemented after being cleared by the EC and since it seems unlikely that the mergers' second order effect on rivals should manifest in rivals' balance sheet data instantly, it seems safe to assume that selection into treatment is calculated based on pre-treatment characteristics. This ensures that the treatment effect does not affect the matching. Propensity score estimation results are reported in Table 2.

Table 2: Selection into treatment

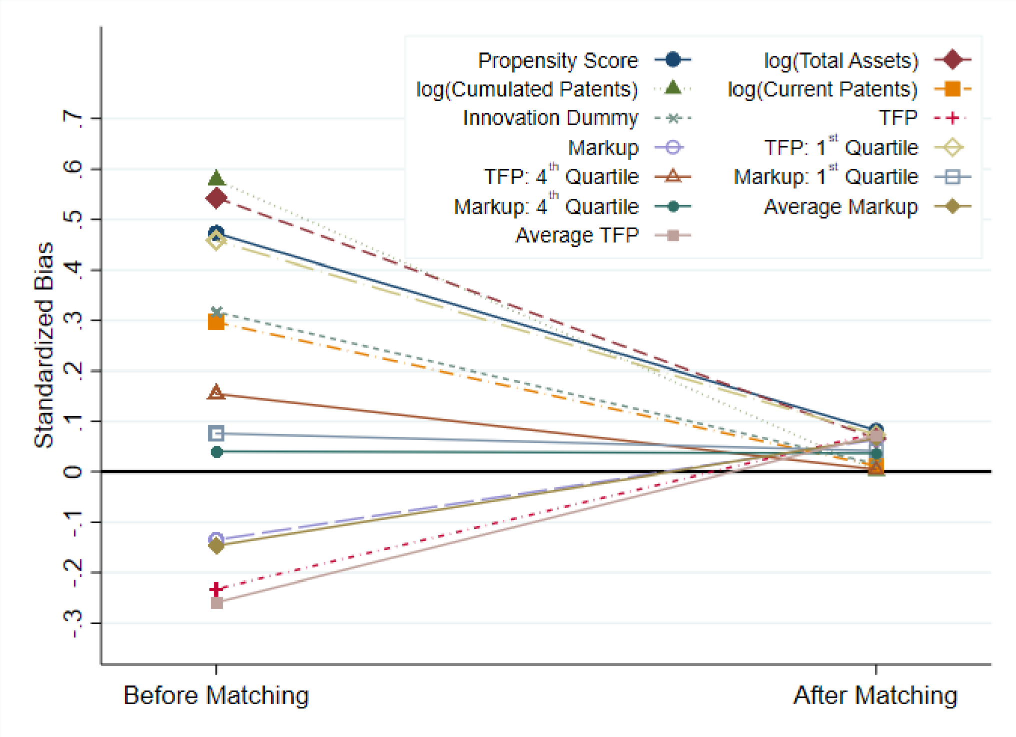
| | | |
|---|---|---|
| log(Total Assets) | 0.112*** | (0.011) |
| log(Cumulated Patents) | 0.054*** | (0.016) |
| log(Current Patents) | −0.014 | (0.023) |
| Innovation Dummy | 0.220*** | (0.057) |
| TFP | 0.281 | (0.274) |
| Markup | 0.427** | (0.180) |
| TFP: $1^st$ Quartile | 0.129*** | (0.048) |
| TFP: $4^th$ Quartile | 0.177*** | (0.046) |
| Markup: $1^st$ Quartile | −0.229*** | (0.045) |
| Markup: $4^th$ Quartile | −0.042 | (0.055) |
| Average Markup | −0.537*** | (0.202) |
| Average TFP | −0.121 | (0.291) |
| Observations | 1015050 | |
| Pseudo$R^2$ | 0.200 | |
| Year fixed-effects | ✓ | |
| Industry fixed-effects | ✓ | |
| Country fixed-effects | ✓ | |

*Notes:* Standard errors in parentheses, ** p<0.05, *** p<0.01. The dependent variable indicates if a firm was a merger rival in a specific year. Fixed-effects for 8 years, 34 sectors and and 21 countries included.

The estimation results of the Probit model confirm that the probability of being a merger rival increases in total assets and innovation activity. While their current markups are associated with a higher likelihood of merger exposure, average markups in pre-merger periods and being in the first quartile of the markup distribution decreases the probability of being a rival of merging firms. While both current and average TFP remain insignificant, being in both the first and fourth quartile of the productivity distribution increases the probability of being a rival.

The matching algorithm then proceeds to link merger rivals to control observations: for every firm in a market with a merger we identify a firm with a similar ex-ante likelihood of being a rival that was not in a market with a merger. If the algorithm successfully balances the samples of treated and non-treated firms, it solves the selection problems described above under the assumption of selection on observables: since both types of firms have the same ex-ante probability of receiving treatment, the assignment to treatment

Figure 1: Standardized biases before and after matching




is essentially random. Further, since the expected performance of nontreated firms differs from treated firms only by treatment, they serve as the counterfactual observations required for treatment analysis.

We implement 1-to-1 nearest-neighbour matching (Caliendo and Kopeinig, 2008; Blundell and Costa-Dias, 2000) without replacement (i.e. each control can only be assigned once). Each treated firm is matched to that non-treated firm which has the most similar probability of treatment. The pool of potential matches is restricted to the same year, sector and country as the treated firm. We thus ensure that each control observation refers to the same year (to control for time-specific effects) and originates from the same sector (to control for industry-specific shocks) and country (to control for macroeconomic and regional effects) as the treated observation it is matched to. We discard matches where no suitable match could be found and obtain a sample of 588 treated-control firm-pairs.

In the sample thus constructed, treated and non-treated firms are no longer significantly different in observed characteristics at the time that matching occurs. Columns 4 - 6 of Table 1 show that the mean values of all covariates do not significantly differ between treated and control group; the reduction in standardized biases achieved through matching is illustrated in figure 1.

### 2.4 Model and estimation equation

Based on the matched treatment and control groups, we estimate the impact of mergers on the markups of rival firms in the same market. The average treatment effect on the treated (ATT) is estimated within a DiD framework. The estimation equation is given by:

$$\mu_{i,s,c,t} = \delta \text{post}_t + \gamma \left(\text{treated}_i \times \text{post}_t\right) + \mathbb{X}_{i,s,c,t}\Pi + \epsilon_{i,s,c,t} \tag{7}$$

where $\mu_{i,s,c,t}$ designates the log markup of firm $i$, active in sector $s$ and country $c$ at time $t$ and $\text{post}_t$ indicates post-merger periods and captures any trend in outcomes that is common to treated and non-treated markets. The variable $\text{treated}_i$ indicates the treatment group (firms that are merger rivals) and $\gamma$, the coefficient of the interaction $\text{treated}_i \times \text{post}_t$, measures the ATT. The treatment group indicator, $\text{treated}_i$, does not enter as a separate variable due to the inclusion of firm fixed-effects. In all estimations, we exclude the merger period $t = 0$ since it is not obvious whether these observations such should be classified as pre- or post-merger observations. This is, however, not crucial for our results.

We subsume fixed effects in the matrix $\mathbb{X}_{i,s,c,t}$. It includes dummies for 1176 firms (588 treated and 588 controls) and 16 years. Sector- and country-specific effects are captured through either trends or year fixed-effects at these levels (see table notes for details). Finally, $\epsilon_{i,s,c,t}$ is an error term. To account for repeated observations, we cluster our standard errors at the firm level.

Note that observable differences between treatment and control group are captured by the propensity score while unobservable, time-invariant differences are controlled for by using the DiD estimator. Due to the absence of quasi-experimental variation, we cannot control for time-varying unobservables that are not captured by $\mathbb{X}_{i,s,c,t}$, a common problem in the M&A literature (e.g., Braguinsky et al., 2015; Guadalupe et al., 2012). Therefore, one should be cautious in interpreting the relationship between M&A and markups as causal. However, our identification strategy controls for many potential sources of bias based on persistent differences (via firm fixed effects), industry-country specific trends, and firm-specific trends that are common to firms with similar characteristics in treatment and control group. As argued by Braguinsky et al. (2015), identification of causal effects of M&A partly relies on the assumption that M&A create a discrete change around the event while other performance trends should be gradual enough to be distinguished from the more discrete direct effect. In the next section, we also provide evidence that there are no systematically different pre-merger trends between treatment and control group.

We also extend equation 7 to analyze heterogeneous effects in the following specifications:

$$\mu_{i,s,c,t} = \delta \text{post}_t + \gamma_1 \left(\text{treated}_i \times \text{post}_t\right) + \gamma_2 \left(\text{treated}_i \times \text{post}_t\right) \times H_{i,t} + \mathbb{X}_{i,s,c,t}\Pi + \epsilon_{i,s,c,t} \tag{8}$$

where $H_{i,t}$ captures heterogeneity at the market or firm level such as a firm's pre-merger market share, pre-merger market concentration, or variables indicating cross-border mergers and time passed after the merger.

We observe the firms in our data over time periods of different length: while for some firms ten years of post-treatment data might be available, others are observed for only three years after a merger. To ensure that our findings are not affected by different time windows in treatment and control firms, we limit estimation to the time-overlap of the treated-control pairs in our data. Thus, the observations used from each treated-control pair are restricted to lie within the first and the last year in which both firms are observed. Pooling across post-merger periods, instead of estimating time-specific coefficients, maximizes our sample size. However, our conclusions remain unchanged if we limit our estimation to a balanced sample of firms which we observe over 5 years after merger events and estimate time-heterogeneous treatment effects.

# 3 Results

## 3.1 Average Treatment Effects on the Treated

Table 3 reports how log markups of merger rivals, estimated from a Translog production function, evolve relative to their control group after a change in market structure (i.e. a merger) occurs. *M&A* refers to $\gamma$ in equation (8), the coefficient of the variable $(\text{treated}_i \times \text{post}_t)$, and thus measures the ATT.

Table 3: Markups of merger rivals

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.023*** | 0.024*** | 0.025*** | 0.023*** | 0.025*** | 0.024*** | 0.024*** | 0.023*** |
| | (0.008) | (0.007) | (0.007) | (0.008) | (0.008) | (0.007) | (0.007) | (0.007) |
| Observations | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 |
| $R^2$ | 0.860 | 0.866 | 0.875 | 0.863 | 0.867 | 0.869 | 0.878 | 0.907 |

*Notes:* Robust standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is log markup. The different columns of the table report estimation results for various sets of fixed effects: all eight columns include firm fixed-effects (1,176 regressors). Column (1) additionally includes year fixed effects (16 regressors). Columns (2) adds sector-specific time trends (32 regressors) while column (3) substitutes them for sector-specific year fixed-effects (383 regressors). Columns (4) and (5) add country-specific trends (18 regressors) and year fixed-effects (208 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (168 regressors). Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (1,513 regressors).

Table 3 shows a positive and significant treatment effect on rival firms' markups. The size of the effect is stable across specifications and ranges from 2.3% to 2.5%. Thus, there is empirical evidence for an increase in rival's market power after mergers.

An alternative way to control for firm-specific heterogeneity (instead of the firm fixed-effects approach implemented above) is to include pre-treatment values of firm markups as control variables. This approach is also more consistent with the autoregressive process employed to estimate the evolution of productivity (see section 2.2). The results are reported in Table 4. The coefficients of the pre-treatment markups are significant and large, indicating a high level of inertia in the evolution of markups. The treatment effects remain significant and increase by about one third in size relative to the initial specification. The results indicate that markups of merger rivals rise by 3 to 3.2% relative to the control group.

Table 4: Controlling for pre-treatment markups

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Pre-markup | 0.854*** | 0.801*** | 0.799*** | 0.839*** | 0.839*** | 0.786*** | 0.777*** | 0.781*** |
| | (0.009) | (0.010) | (0.010) | (0.010) | (0.010) | (0.011) | (0.012) | (0.012) |
| M&A | 0.030*** | 0.031*** | 0.032*** | 0.030*** | 0.031*** | 0.030*** | 0.031*** | 0.031*** |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) |
| Observations | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 |
| $R^2$ | 0.714 | 0.729 | 0.749 | 0.721 | 0.728 | 0.734 | 0.759 | 0.812 |

*Notes:* Robust standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is log markup. Instead of including firm fixed-effects, we control for pre-treatment markups. The different columns of the table report estimation results for various sets of fixed effects: in column (1), we include year (16 regressors) fixed effects. Columns (2) adds sector-specific time trends (32 regressors) while column (3) substitutes them for sector-specific year fixed-effects (383 regressors). Columns (4) and (5) add country-specific trends (18 regressors) and year fixed-effects (208 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (168 regressors). Column (8) includes a full set of country/sector/year fixed effects (1,513 regressors).

Next, we disentangle the average effect on markups into period-specific effects to investigate time-dynamics. We follow Braguinsky et al. (2015) and define three distinct time periods: one year before the merger (to test for pre-treatment effects), one to three years after the merger and four and more years after the mergers. The results of this specification are documented in Table 5.

Only one specification (column (8)) finds evidence for pre-treatment differences between treatment and control group, at a marginal level of significance. Thus, there is little evidence for anticipation effects. Significant markup increases in the first three years after a merger are only estimated in five out of eight specifications and ranges from 1.5% to 2.1%. However, all eight specifications find significant markup increases in later post-merger periods in the range between 3.7% and 4.5%. This is consistent with the idea that the effect on rivals is a second order effect, as they adapt to a changed competitive environment induced by a merger of their competitors. Note that the firms in our sample are rather large entities which might need time to adjust pricing behavior and product characteristics.

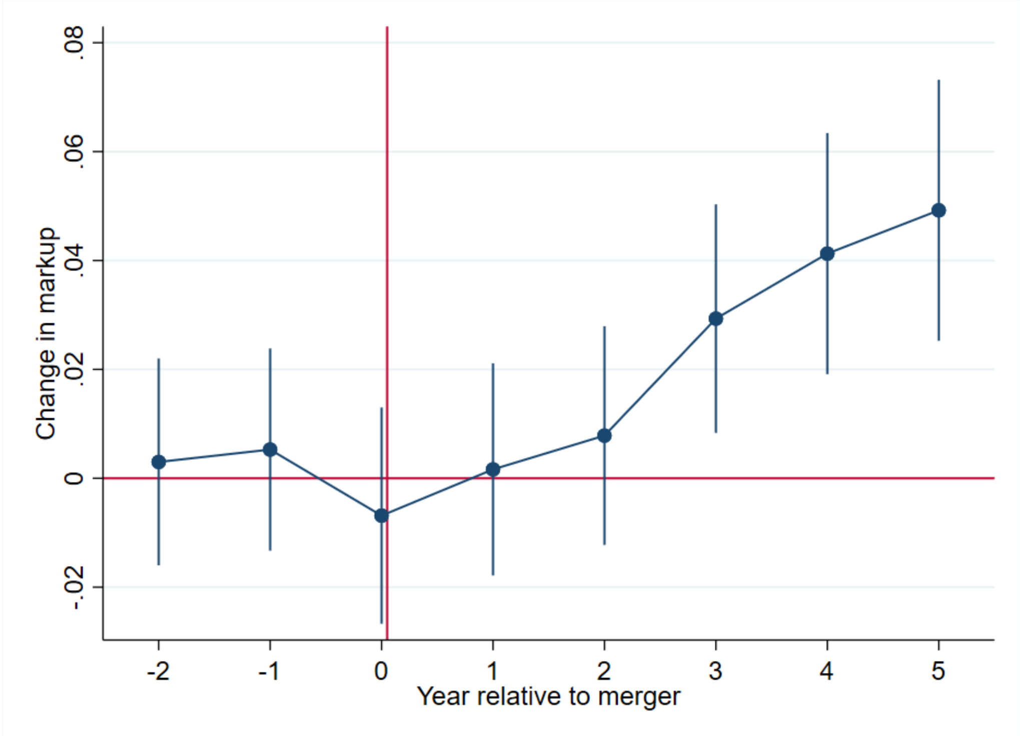
We also estimate separate coefficients for each pre- and post merger period from two years before till five years after mergers. Results of this exercise are depicted in figure 2. The figure shows that differences between treatment and control group are small and statistically insignificant in the two years before and shortly after the merger. The estimated coefficients for post-merger periods, however, increase over time and become statistically significant from the third post-merger year on and reach about 5% after 5 years.

Table 5: Markups of merger rivals over time

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A: [-1] | -0.003 | 0.005 | 0.011 | -0.000 | 0.002 | 0.008 | 0.009 | 0.015* |
| | (0.009) | (0.009) | (0.009) | (0.009) | (0.009) | (0.009) | (0.008) | (0.009) |
| M&A: [1, 3] | 0.010 | 0.015* | 0.020** | 0.012 | 0.014 | 0.017** | 0.018** | 0.021** |
| | (0.009) | (0.009) | (0.009) | (0.009) | (0.009) | (0.009) | (0.008) | (0.008) |
| M&A: [4+] | 0.037*** | 0.042*** | 0.045*** | 0.039*** | 0.043*** | 0.044*** | 0.045*** | 0.044*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.009) | (0.010) |
| Observations | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 |
| $R^2$ | 0.861 | 0.867 | 0.875 | 0.863 | 0.868 | 0.869 | 0.879 | 0.907 |

*Notes:* Robust standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is log markup. The different columns of the table report estimation results for various sets of fixed effects: all eight columns include firm fixed-effects (1,176 regressors). Column (1) additionally includes year fixed effects (16 regressors). Columns (2) adds sector-specific time trends (32 regressors) while column (3) substitutes them for sector-specific year fixed-effects (383 regressors). Columns (4) and (5) add country-specific trends (18 regressors) and year fixed-effects (208 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (168 regressors). Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (1,513 regressors).

Figure 2: Changes in rival markups around M&A



### 3.2 Heterogeneous Effects

In this section, we analyze heterogeneity in the effects on markups in post-merger periods using interaction terms between post-treatment observations and pre-merger characteristics. Specifically, we analyze heterogeneous effects with respect to the market share of the focal firm in affected market, the number of competitors in the relevant markets, an indicator for high pre-merger markups and by evaluating whether the effects of cross-border M&A differ from those of domestic deals. Market shares and the number of competitors are defined as deviations from sample means. Therefore, the M&A indicator measures the expected change in rivals' markups for a firm with an average market share or in a market with an average number of firms, respectively. All results are collected in Table 6, with the individual panels corresponding to the outcomes discussed above.

The results reported in Table 6, panel (A), allow for heterogeneous effects based on rivals' pre-merger market share. The ATT, i.e. the effect on a rival with a pre-merger market share of about 13%, is positive and similar to the baseline results (Table 3). The coefficients of market share are statistically significant in seven specifications and show that large initial market shares are associated with more substantial markup increases: an increase in a rivals' pre-merger market share of 10% leads to an increase of post-merger markups by 1% to 1.5%.

In Table 6, panel (B), we include the log of the number of competitors identified by the EC during its competitive assessment of the merger. An increase in the number of competitors by one log point is associated with a reduction in the expected markup change between 0.7 and 1%, significant in five out of eight specifications.

Table 6, panel (C), splits the treatment group at the median of firms' initial markups and calculates heterogeneous effects for firms with high initial markups. While the estimates for the M&A coefficients—which capture the effects on rival firms with initially low markups—are close to zero and statistically insignificant, firms with high initial markups are characterized by a further increase in markups between 3.6% - 4.7%. Thus, the results indicate that mergers in low markup industries have not led to substantial increases in markups while mergers in industries with high initial markups have led to further increases of approximately 4%.

Finally, we distinguish between domestic mergers, where the headquarters of acquirer and target are located in the same country, and cross-border mergers. It is likely that, on average, firms with headquarters in the same country compete more closely with each other because of trade barriers. Further, it can be argued that cross-border mergers are more likely to be pro-competitive because of efficiency gains associated with technology transfer and market access (e.g., Guadalupe et al., 2012; Javorcik and Poelhekke, 2017). Results in Table 6, panel (D), show that the association between cross-border mergers and markups is indeed less strong compared to domestic mergers, although the difference is not statistically significant in all specifications.

All in all, the results indicate that increases in markups are more likely to occur when we expect changes in market power to be most important, i.e. when pre-merger market concentration (indicated by pre-merger market shares or markups) is high, when there are few firms competing or when acquirer and target are located in the same country.[16]

### 3.3 Other Outcome Variables

In principle, increases in markups reported in previous tables could be caused by either decreasing marginal costs (and constant prices), increasing prices (and constant marginal costs) or a combination of changes in prices and costs. For instance, if rival firms increase productivity enhancing investment after mergers, they might reduce marginal costs and adjust markups accordingly. To distinguish this scenario from an increase in market power, we estimate the following regression equation (adapted from De Loecker and Warzynski, 2012):

$$\mu_{i,s,c,t} = \kappa + \psi \text{post}_t + f(TFP_{it}) + \tau\,(\text{treated}_i \times \text{post}_t) + \mathbb{X}_{i,s,c,t}\lambda + \epsilon_{i,s,c,t}$$

Estimating effects of mergers on markups conditional on contemporaneous changes in TFP enables us to interpret the results as an estimate of effects on firms' prices. If increasing markups are due to changes in market power—rather than changes in efficiency combined with incomplete pass-through—we should see positive and significant effects of mergers conditional on TFP. To control for productivity in a flexible way, we include TFP in a linear, quadratic and cubic form in the regression. We present the regression results of the above equation in Table 7.

As expected, a firm's markups are strongly associated with its productivity. However, most importantly, we find evidence for significant price increases in all eight specifications in post-merger periods. The size of the coefficients indicating price changes suggest that most of the estimated markup increases are indeed due to increases in prices rather than reductions in marginal costs. In fact, the estimated coefficients are only slightly lower than in our baseline specification (see Table 3). Note that the estimated price increases should be regarded as a lower bound since TFP partially captures pricing heterogeneity within industries and might thus eliminate some of the variation in markups that stems from market power.

As a further indicator for the importance of market power versus efficiency gains, we estimate effects of mergers on other outcome variables. If rival firms increase their productivity upon mergers, we would expect them to increase production relative to the control group while they are likely to cut production in the case of enhanced market power. Since our data set does not contain physical output, we use

[16] Table A14 in the Appendix documents results of a horse race between alternative dimensions of treatment heterogeneity.

Table 6: Heterogeneous effects on markups of merger rivals

| Panel (A) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.021** | 0.024*** | 0.024*** | 0.022*** | 0.024*** | 0.024*** | 0.022*** | 0.020** |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) |
| Market Share | 0.140** | 0.120** | 0.103* | 0.124** | 0.140** | 0.102* | 0.149*** | 0.095 |
| | (0.060) | (0.059) | (0.057) | (0.061) | (0.058) | (0.059) | (0.057) | (0.064) |
| Observations | 8526 | 8526 | 8526 | 8526 | 8526 | 8526 | 8526 | 8526 |
| $R^2$ | 0.860 | 0.866 | 0.875 | 0.863 | 0.868 | 0.868 | 0.878 | 0.908 |

| Panel (B) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.023*** | 0.024*** | 0.025*** | 0.024*** | 0.025*** | 0.025*** | 0.025*** | 0.023*** |
| | (0.008) | (0.008) | (0.007) | (0.008) | (0.008) | (0.007) | (0.007) | (0.007) |
| Competitors | -0.010** | -0.007* | -0.006 | -0.007* | -0.008* | -0.005 | -0.008* | -0.005 |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Observations | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 |
| $R^2$ | 0.861 | 0.866 | 0.875 | 0.863 | 0.868 | 0.869 | 0.878 | 0.907 |

| Panel (C) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.002 | 0.001 | 0.003 | 0.004 | 0.007 | 0.001 | 0.005 | 0.004 |
| | (0.008) | (0.008) | (0.007) | (0.008) | (0.007) | (0.008) | (0.007) | (0.008) |
| High Markup | 0.041*** | 0.045*** | 0.043*** | 0.039*** | 0.036*** | 0.047*** | 0.038*** | 0.037*** |
| | (0.011) | (0.011) | (0.011) | (0.012) | (0.011) | (0.011) | (0.011) | (0.012) |
| Observations | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 |
| $R^2$ | 0.861 | 0.867 | 0.875 | 0.863 | 0.868 | 0.869 | 0.878 | 0.907 |

| Panel (D) | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.041*** | 0.035*** | 0.036*** | 0.038*** | 0.040*** | 0.033*** | 0.034*** | 0.036*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) |
| Cross-border | -0.028** | -0.017 | -0.017 | -0.023** | -0.024** | -0.014 | -0.015 | -0.020* |
| | (0.012) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.012) |
| Observations | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 |
| $R^2$ | 0.861 | 0.866 | 0.875 | 0.863 | 0.868 | 0.869 | 0.878 | 0.907 |

*Notes:* Robust standard errors in parentheses, * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The dependent variable is log markup. The different columns of the table report estimation results for various sets of fixed effects: all eight columns include firm fixed-effects (1,176 regressors). Column (1) additionally includes year fixed effects (16 regressors). Columns (2) adds sector-specific time trends (32 regressors) while column (3) substitutes them for sector-specific year fixed-effects (383 regressors). Columns (4) and (5) add country-specific trends (18 regressors) and year fixed-effects (208 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (168 regressors). Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (1,513 regressors).

Table 7: Changes in firm prices

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| TFP | 0.516*** | 0.560*** | 0.553*** | 0.533*** | 0.536*** | 0.589*** | 0.579*** | 0.607*** |
| | (0.072) | (0.073) | (0.076) | (0.073) | (0.072) | (0.073) | (0.075) | (0.082) |
| TFP$^2$ | 0.365 | 0.227 | 0.229 | 0.299 | 0.306 | 0.123 | 0.182 | 0.029 |
| | (0.247) | (0.248) | (0.252) | (0.246) | (0.244) | (0.247) | (0.250) | (0.277) |
| TFP$^3$ | -0.347* | -0.259 | -0.273 | -0.296 | -0.328 | -0.177 | -0.254 | -0.163 |
| | (0.202) | (0.204) | (0.208) | (0.201) | (0.201) | (0.204) | (0.205) | (0.230) |
| M&A | 0.020*** | 0.021*** | 0.021*** | 0.020*** | 0.021*** | 0.021*** | 0.020*** | 0.019*** |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.006) | (0.006) |
| Observations | 9067 | 9067 | 9067 | 9067 | 9067 | 9067 | 9067 | 9067 |
| $R^2$ | 0.889 | 0.893 | 0.899 | 0.891 | 0.895 | 0.895 | 0.902 | 0.924 |

*Notes:* Robust standard errors in parentheses, * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The dependent variable is log markup. The different columns of the table report estimation results for various sets of fixed effects: all eight columns include firm fixed-effects (1,176 regressors). Column (1) additionally includes year fixed effects (16 regressors). Columns (2) adds sector-specific time trends (32 regressors) while column (3) substitutes them for sector-specific year fixed-effects (383 regressors). Columns (4) and (5) add country-specific trends (18 regressors) and year fixed-effects (208 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (168 regressors). Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (1,513 regressors).

deflated sales and the number of employees (as a physical measure of input into production) as outcome variables. As further checks, we include the estimated changes tangible capital, the number of citation-weighted patents[17] and value added (all in logs).

Results, documented in Table 8, indicate reductions of employment and sales of approximately 6% and 5% respectively. While the effects on sales is only weakly statistically significant, it is likely that it underestimates the effect on physical output since our results indicate that prices increase after mergers. There is no evidence that increases in markups stem from cost-reducing or quality enhancing investments since changes in tangible assets and citation-weighted patenting are negatively associated with mergers. Consistent with increases in market power, we also find that value added declines while two measures of profits increase. The first measure of profits is defined as sales less material costs, wage costs and financial costs.[18] The second measure is EBIT from firms' financial accounts.

[17] We only use patents that have been ultimately granted but date them back to the application year.

[18] A related measure of profits is used by De Loecker et al. (2019). To approximate financial costs, we follow Aghion et al. (2005) and assume a constant cost of capital which we set to 0.085 for all firms and years and multiply this number by a firm's capital stock.

Table 8: Effects on other covariates

| | Employment | Sales | Tangible | Patents | Value added | Profits | EBIT |
|---|---|---|---|---|---|---|---|
| M&A | -0.064** | -0.054* | -0.091** | -0.064* | -0.113*** | 5.809*** | 1.753** |
| | (0.029) | (0.029) | (0.046) | (0.033) | (0.032) | (1.569) | (0.701) |
| Observations | 6929 | 6929 | 6868 | 6929 | 6748 | 6058 | 6828 |
| $R^2$ | 0.957 | 0.965 | 0.957 | 0.839 | 0.963 | 0.933 | 0.804 |

*Notes:* Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. All regressions include firm fixed-effects and country/sector/year fixed-effects. Employment, sales, tangible assets, patents and value added are in logs, profits and EBIT are in million €.

### 3.4 Extensions and Further Robustness Checks

As discussed in section 2.2, a potential problem with markups estimated from a Translog production function is that any bias in elasticities—for instance due to using revenues and material expenditures instead of quantities—could potentially affect firms in treatment and control group to a different extent. As a robustness check, we therefore report effects of mergers on markups estimated from a Cobb Douglas production function where any bias in production function coefficients would affect all firms in an industry and therefore treatment and control group to the same extent. Thus, any such bias would be captured by industry-year fixed effects.

We also experimented with other specifications that have been used in the literature. To address potential concerns about identification of gross output production functions with several flexible production factors (Gandhi et al., 2018), we re-estimate our production function using a combined measure of variable costs instead of material and labour inputs separately. We label this specification cost of goods sold (COGS).

Further, instead of estimating a gross output production function, we estimate a structural value added production functions which relate value added (sales less material costs) to labour and capital. For this specification, we assume that labour is a production factor without adjustments costs that can be flexibly adjusted and is endogenous to innovations in the productivity process. In another extension, we estimate a dynamic specification of production functions which allows for time-varying coefficients.

Two further specifications attempt to address the possible bias from using revenues instead of quantities as the dependent variable in production functions. First, we follow De Loecker et al. (2019) and directly control for market shares as a main determinant of markups in the production function. Second, we apply a method suggested by Forlani et al. (2016) who derive a revenue-generating function using additional assumptions about demand and the evolution of productivity and quality.[19] Under these assumptions,

[19]Particularly, one has to assume that consumers' utility function depends on the product of consumed quantity and a quality parameter and that this quality parameter follows the same Markov process as physical TFP. See Forlani et al. (2016) for details.

one can consistently estimate markups up to scale (and precisely under constant returns to scale) even in the presence of pricing heterogeneity which biases the coefficients of standard production functions.

Finally, we relax the assumption of common production function parameters within each broadly defined industry. Specifically, we allow for separate coefficients for merger rivals in each 2-digit sector by including interaction terms between treatment status and inputs into the production function.

Results of these alternative specifications are documented in Table 9. The estimated coefficients indicate that merger rivals increase their markups after M&A in all specifications. Thus, our conclusions are not sensitive to the specification of the production function. In fact, in six out of seven cases, the estimated ATT is higher than in our baseline Translog specification.

Table 9: Markups of merger rivals estimated from other production function specifications

| | (1) Cobb-Douglas | (2) COGS | (3) Val. Add. | (4) Dynamic | (5) M.share | (6) FMMM | (7) Heterog. Param. |
|---|---|---|---|---|---|---|---|
| M&A | $0.040^{***}$ | $0.022^{***}$ | $0.074^{***}$ | $0.063^{***}$ | $0.036^{**}$ | $0.041^{***}$ | $0.041^{***}$ |
| | (0.015) | (0.005) | (0.022) | (0.016) | (0.016) | (0.013) | (0.014) |
| Observations | 9076 | 7333 | 7195 | 6934 | 7104 | 6069 | 6298 |
| $R^2$ | 0.903 | 0.976 | 0.923 | 0.936 | 0.909 | 0.909 | 0.902 |

*Notes:* Standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. All regressions include firm- and country/sector/year fixed-effects. The dependent variable is the log of firm-level markups. M&A takes a value of one in all post-merger periods for rivals of merging firms. In each column, markups are derived from different specifications of the production function. Column (1) uses a Cobb-Douglas production function, the production function in column (2) relates sales to a variable input cost bundle of material and wage costs next to capital. Column (3) uses a structural value added specifications where output is related to capital and labour (number of employees). Column (4) allows for year-specific production function parameters. Column (5) controls for firms' market share at the 3-digit industry level next to inputs. Column (6) is based on a sales-generating function derived by Forlani et al. (2016). Column (7) allows for separate production function parameters for treated and control firms within each 2-digit industry.

In the Appendix, we document the results of various additional robustness checks. One potential concern is that merger rivals might increase markups not because of strategic reactions but since they benefit from divestitures imposed on the merging firms, due to which they gain market shares. As Table A12 in the Appendix shows, our results are not driven by mergers with remedies.

We also check the robustness of our results to the construction of the control group. Tables A13 in the Appendix shows that our results are qualitatively similar when we use all potential control firms as a comparison group.

All in all, our results of increasing markups as a response to rivals' M&A seems to be very robust. It should be noted that our estimates are likely to reflect lower bounds of markup adjustments in the relevant product markets. While we base our evidence on expert definitions of product markets and relevant competitors, our estimates of market power are markups at the firm-level and are thus likely to partly depend on product markets that have not been affected by mergers.

In the Appendix, we discuss estimates of the effects of M&As on target firms using a broader sample of mergers from Bureau van Dijk's Zephyr data base. Results documented in Table A11 indicate that the effects of mergers on target firms are qualitatively similar to those on non-merging rivals and quantitatively slightly above our baseline specification. These results are consistent with those found by Blonigen and Pierce (2016) for the US.

## 4 Conclusion

This paper analyzes the effects of horizontal mergers on the markups of non-merging rival firms. We make use of the official decisions on mergers that have been investigated by the European Commission. These documents contain expert market definitions which enable us to identify the relevant markets and rival firms. We link these rival firms to production data and estimate their markups using recent advances in the estimation of production functions. To address potential issues of selection, we use propensity score matching to construct a comparison group which approximates counterfactual outcomes.

We employ a DiD estimator in order to quantify the impact of mergers on the markups of rival firms and find post-merger markup increases between 2% to 4% on average. Estimating heterogeneous treatment effects, we find that markup increases are most pronounced when pre-merger market shares and pre-merger markups are high, when the number of competitors is low and in domestic rather than cross-border M&A.

These findings suggest that market power increased after mergers and that markups are likely to have risen because of price increases rather than marginal cost reductions. Consistent with this interpretation, we find that markups increase even conditional on measures of firm-level productivity. Further, merger rivals also seem to decrease sales, employment, value added and investments in post-merger period, whereas their profits increase.

Our results indicate that consummated horizontal mergers in the EU have, on average, led to markup and price increases in affected markets. These increases are particularly pronounced in markets where pre-existing competition is weak, suggesting that some of the mergers that have been cleared by the EU were anti-competitive and have decreased consumer surplus to some extent. The results of this paper also indicate that increasing market concentration through mergers is a potential concern for consumers and may have contributed to the rise of markups that has been documented for many sectors and countries.

While we have examined mergers that were notified to and cleared by a competition authority, a substantial part of M&A related consolidation occurs below notification thresholds and thus without any review (Wollmann, 2019). Even with merger review, antitrust enforcement has become more lenient in recent decades, particularly in the US (Kwoka, 2017) and coinciding with an especially eminent rise in markups there. Berry et al. (2019) discuss specific concerns related to the underenforcement of antitrust, but emphasize that much more empirical evidence is needed.

In this spirit, an interesting extension to this area of research would be to analyze the extent to which mergers have contributed to rising markups at the aggregate level. Given the increased availability of product-level production data which might be linked to mergers in the future, it would be interesting to investigate the effects on markups in affected product markets—rather than firm-level markups—more precisely. Finally, another important area of future research are the effects of mergers on non-price strategies.

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## Appendix A: Additional Material

Descriptive statistics on estimated markups and output elasticities by sector are documented in Table A10.

Table A10: Estimation of markups and production functions by sector

| | Markup mean | Markup median | *θ_k* | *θ_l* | *θ_m* | RTS |
|---|---|---|---|---|---|---|
| Agriculture | 3.77 | 1.87 | 0.07 | 0.20 | 0.79 | 1.06 |
| Mining | 5.28 | 2.13 | 0.36 | 0.35 | 0.44 | 1.16 |
| Food, Beverages, Tobacco | 1.66 | 1.07 | 0.19 | 0.18 | 0.65 | 1.02 |
| Wearing Apparel | 1.76 | 1.13 | 0.10 | 0.06 | 0.54 | 0.70 |
| Paper Products | 2.90 | 1.64 | 0.19 | 0.20 | 0.74 | 1.12 |
| Wood and Wood Products | 1.29 | 1.14 | 0.10 | 0.18 | 0.60 | 0.89 |
| Printing | 3.39 | 2.19 | 0.15 | 0.10 | 0.83 | 1.07 |
| Chemicals | 2.08 | 1.11 | 0.23 | 0.20 | 0.59 | 1.02 |
| Pharmaceutical | 2.81 | 2.01 | 0.13 | 0.36 | 0.73 | 1.22 |
| Rubber and Plastic | 1.58 | 1.38 | 0.09 | 0.24 | 0.70 | 1.04 |
| Non-metallic Mineral Products | 1.93 | 1.45 | 0.14 | 0.28 | 0.66 | 1.08 |
| Metal Products | 1.73 | 1.20 | 0.09 | 0.27 | 0.55 | 0.91 |
| Electrical Machinery | 1.77 | 0.98 | 0.12 | 0.25 | 0.46 | 0.84 |
| Machinery and Equipment | 1.56 | 1.17 | 0.07 | 0.28 | 0.54 | 0.89 |
| Motor Vehicles | 1.46 | 0.96 | 0.10 | 0.24 | 0.53 | 0.87 |
| Transport Equipment | 2.46 | 0.96 | 0.08 | 0.45 | 0.39 | 0.92 |
| Furniture | 2.24 | 1.21 | 0.11 | 0.28 | 0.50 | 0.89 |
| Electricity | 5.87 | 1.27 | 0.18 | 0.26 | 0.63 | 1.07 |
| Water Supply | 3.78 | 1.20 | 0.16 | 0.33 | 0.23 | 0.73 |
| Construction | 4.47 | 1.66 | 0.12 | 0.23 | 0.53 | 0.87 |
| Wholesale and Retail Trade | 2.45 | 0.83 | 0.13 | 0.17 | 0.60 | 0.90 |
| Accomodation | 9.22 | 5.96 | 0.05 | 0.21 | 0.77 | 1.03 |
| Food/Beverage services | 2.83 | 1.95 | 0.14 | 0.20 | 0.54 | 0.89 |
| Publishing Activities | 9.59 | 2.03 | 0.23 | 0.40 | 0.31 | 0.95 |
| IT services | 16.60 | 3.03 | 0.23 | 0.44 | 0.32 | 0.99 |
| Financial Services | 11.24 | 1.04 | 0.15 | 0.25 | 0.45 | 0.84 |
| Real Estate | 17.28 | 2.34 | 0.29 | 0.34 | 0.44 | 1.07 |
| Business Services | 9.80 | 0.98 | 0.10 | 0.42 | 0.30 | 0.82 |
| Research and Development | 3.80 | 1.29 | 0.18 | 0.46 | 0.21 | 0.85 |
| Advertising, Design | 16.43 | 3.67 | 0.28 | 0.35 | 0.37 | 1.00 |
| Administration and Support | 10.91 | 4.29 | 0.31 | 0.56 | 0.24 | 1.10 |
| Human Health | 3.01 | 2.01 | 0.14 | 0.33 | 0.33 | 0.81 |
| Residential Care | 6.46 | 3.63 | 0.11 | 0.51 | 0.29 | 0.91 |
| Other Services and Arts | 12.21 | 2.88 | 0.32 | 0.39 | 0.38 | 1.09 |

*Notes:* Mean and median markups by 2-digit sector; $\theta_k$, $\theta_l$ and $\theta_m$ refer to the output elasticities of capital, labour and materials respectively. RTS are the industry-specific returns to scale.

While the focus of this paper are the effects that M&A have had on competing firms or the relevant market, we also provide evidence on merger targets. Unfortunately, linking merging firms of the transactions analyzed above to firm-level data yields a sample that is too small for robust econometric analysis. Instead, we use sample of merging firms obtained from Bureau Van Dijk's Zephyr merger database which includes a much braoder sample of mergers.

We proceed as in our main analysis: after estimating the merging firms' markups, we merge them with a donor pool of control observations and run the same matching procedure as described above, resulting in more than 5,000 merging-control pairs, that we observe before and after the merger. Table A11 reports the average change in the markups of merging firms in the post-period, relative to the control group. The coefficient estimate of M&A is positive and significant in all eight specifications, with a size ranging between 2.5 and 3.6%. Thus, the merger targets in these deals have, on average, increased their markups. The sizes of the effects are up to 50% larger than those reported for rivals in Table 3.

Table A11: Effect on merger targets

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.033*** | 0.036*** | 0.035*** | 0.030*** | 0.026*** | 0.034*** | 0.031*** | 0.025*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Observations | 62753 | 62753 | 62753 | 62753 | 62753 | 62753 | 62753 | 62753 |
| $R^2$ | 0.872 | 0.873 | 0.877 | 0.873 | 0.877 | 0.874 | 0.881 | 0.904 |

*Notes:* Robust standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is log markup. The different columns of the table report estimation results for various sets of fixed effects: all eight columns include firm fixed-effects (10,109 regressors). Column (1) additionally includes year fixed effects (15 regressors). Columns (2) adds sector-specific time trends (30 regressors) while column (3) substitutes them for sector-specific year fixed-effects (407 regressors). Columns (4) and (5) add country-specific trends (25 regressors) and year fixed-effects (347 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (392 regressors). Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (4,174 regressors).

Table A12 analyzes, whether remedies imposed by the EC had an effect of the markups of firms in the markets involved. First, the inclusion of remedies does not strongly affect the baseline effect of mergers: the estimated markup increases are significant and range between 2.8 and 3.1%. However, if a merger was remedied the markup increases by 0.7% - 1.7% less, but the effect is not significant. Thus, the mergers in our data have led to rival markup increases irrespectively of remedies imposed.

Table A12: Effect of remedies on markups

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.029*** | 0.029*** | 0.028*** | 0.030*** | 0.031*** | 0.030*** | 0.031*** | 0.031*** |
| | (0.010) | (0.010) | (0.009) | (0.010) | (0.009) | (0.010) | (0.009) | (0.009) |
| Remedies | -0.014 | -0.010 | -0.007 | -0.015 | -0.014 | -0.011 | -0.014 | -0.017 |
| | (0.012) | (0.011) | (0.011) | (0.012) | (0.011) | (0.011) | (0.011) | (0.011) |
| Observations | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 | 9091 |
| $R^2$ | 0.861 | 0.866 | 0.875 | 0.863 | 0.868 | 0.869 | 0.878 | 0.907 |

*Notes:* Robust standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is log markup. The different columns of the table report estimation results for various sets of fixed effects: all eight columns include firm fixed-effects (1,176 regressors). Column (1) additionally includes year fixed effects (16 regressors). Columns (2) adds sector-specific time trends (32 regressors) while column (3) substitutes them for sector-specific year fixed-effects (383 regressors). Columns (4) and (5) add country-specific trends (18 regressors) and year fixed-effects (208 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (168 regressors). Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (1,513 regressors).

While be believe that our approach of accounting for non-random selection into treatment is the appropriate way to address our research question, it would be reassuring to find qualitatively similar evidence in the raw dataset. For this reason, we repeat the above evaluation exercise in an unmatched sample, containing data on all firms in the Orbis database, for which markups could be estimated. This increases the number of observations to almost 1.4 million. The 'post' indicator now denotes the after-treatment periods for rival firms only, while the treatment group indicator is - as above - absorbed into the firm fixed-effects. Table A13 reports the findings. Across all eight specifications, merger rivals significantly increase their markups after mergers. The size of the effect ranges from 0.5 to 1.7% and is thus significantly smaller than the effects we estimate accounting for selection. A likely explanation is that the unmatched sample includes many young and small firms with high growth potential which are not comparable to the sample of firms affected by mergers. However, the results in the unmatched sample corroborate the qualitative findings of the selection-based approach and support our choice of methods.

Table A13: Markups of merger rivals in unmatched sample

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.005* | 0.013*** | 0.014*** | 0.010*** | 0.008*** | 0.017*** | 0.016*** | 0.015*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 1387886 | 1387886 | 1387886 | 1387886 | 1387886 | 1387886 | 1387886 | 1387886 |
| $R^2$ | 0.919 | 0.920 | 0.921 | 0.919 | 0.920 | 0.920 | 0.921 | 0.924 |

*Notes:* Robust standard errors in parentheses, * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The dependent variable is log markup. The different columns of the table report estimation results for various sets of fixed effects: all eight columns include firm fixed-effects (188,210 regressors). Column (1) additionally includes year fixed effects (21 regressors). Columns (2) adds sector-specific time trends (34 regressors) while column (3) substitutes them for sector-specific year fixed-effects (689 regressors). Columns (4) and (5) add country-specific trends (22 regressors) and year fixed-effects (385 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (675 regressors). Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (8,911 regressors).

In Table A14, we include all dimensions of heterogeneity discussed in section 3.2 in one regressions to analyze which dimension of treatment effect heterogeneity is the most important. The baseline effect on markups—which refers to the effect on a firm with average market share which is affected by a domestic merger in a low-markup industry with an average number of competitor—is small and statistically insignificant. Similarly, the effect of competitors, while remaining negative, is not significant. The remaining covariates achieve statistical significance in six (cross-border deals), seven (market share) and all (high initial markups) specifications, with coefficient sizes similar to those reported above. Thus, the most robust predictors of post-merger markup increases for rivals are indicators for the pre-existing competitive environment (initial market shares and initial markups), as well as the indicator for cross-border versus domestic transactions.

Table A14: Heterogeneous effects horse race

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| M&A | 0.014 | 0.007 | 0.009 | 0.012 | 0.016 | 0.006 | 0.007 | 0.005 |
| | (0.011) | (0.010) | (0.010) | (0.011) | (0.011) | (0.010) | (0.010) | (0.011) |
| Competitors | -0.007 | -0.005 | -0.004 | -0.003 | -0.003 | -0.002 | -0.005 | -0.001 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.005) |
| Market Share | 0.132** | 0.118** | 0.101* | 0.122** | 0.139** | 0.104* | 0.150*** | 0.101 |
| | (0.060) | (0.059) | (0.057) | (0.061) | (0.058) | (0.059) | (0.057) | (0.064) |
| Cross-border | -0.035*** | -0.024* | -0.024** | -0.029** | -0.029** | -0.021* | -0.017 | -0.016 |
| | (0.013) | (0.012) | (0.012) | (0.013) | (0.013) | (0.012) | (0.012) | (0.013) |
| High Markup | 0.063*** | 0.066*** | 0.064*** | 0.058*** | 0.054*** | 0.064*** | 0.054*** | 0.050*** |
| | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.014) |
| Observations | 8526 | 8526 | 8526 | 8526 | 8526 | 8526 | 8526 | 8526 |
| $R^2$ | 0.861 | 0.867 | 0.875 | 0.863 | 0.868 | 0.869 | 0.879 | 0.908 |

*Notes:* Robust standard errors in parentheses, * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The dependent variable is log markup. The different columns of the table report estimation results for various sets of fixed effects: all eight columns include firm fixed-effects (1,176 regressors). Column (1) additionally includes year fixed effects (16 regressors). Columns (2) adds sector-specific time trends (32 regressors) while column (3) substitutes them for sector-specific year fixed-effects (383 regressors). Columns (4) and (5) add country-specific trends (18 regressors) and year fixed-effects (208 regressors) respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country (168 regressors). Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (1,513 regressors).