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Mergers and the Markups of Rival Firms: Evidence from European Markets

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Mergers and the Markups of Rival Firms: Evidence from European Markets

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Joel Stiebale & Florian Szücs *

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Abstract This paper analyzes the effects of mergers on markups of non-merging rival firms in narrowly defined markets. Combining data from the European Commission's market assessments in merger decisions with production data, we use recent methodological advances in the estimation of production functions to estimate markups and total factor productivity. We investigate the performance of almost 600 rival firms in close to 200 merger cases before and after the change in market structure. Using a propensity matching approach combined with a difference-in-differences estimator, we find that rivals significantly increase their markups in the years after a merger. The effects are concentrated in markets with few competitors, among firms with initially high market shares and in domestic merger cases. Consistent with increasing market power, we also provide evidence that changes in markups are unlikely to be explained by cost reductions and that sales and employment decline after mergers.

Keywords: Merger, Markups, Productivity, Innovation

JEL Codes: D22, L40, L13, O31

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1. Introduction

Merger and acquisition (M&A) activity has been increasing over the past decades with a combined value of worldwide deals that exceeds \$3 trillion per year.¹ This development has important implications since M&As are a potential explanation for the substantial rise in industry concentration and market power that has been measured in many sectors in the US and Europe (Autor et al., 2017; De Loecker and Eeckhout, 2017; Grullon et al., 2017).

While higher market power implies rising prices at the expense of consumers, M&As may also induce productivity gains through complementary assets, economies of scale and scope or an efficient reallocation of resources which may benefit consumers in the form of lower prices (e.g., Farrell and Shapiro, 1990). To which extent the increase in market power through M&As is outweighed by efficiency gains is a fundamental question in industrial organization 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 recently been argued that more evidence from ex-post merger analysis is necessary (e.g., Angrist and Pischke, 2010). Disentangling market power and efficiency gains in ex-post merger analyses is, however, a challenging task. Commonly used proxies of productivity are based on revenues instead of quantities and could therefore vary across firms not only because of cost-based efficiency but also due to heterogeneity in markups, demand and product quality. In particular, increases in market power upon acquisition which lead to higher prices and markups would show up as higher values in common measures of productivity even in the absence of efficiency gains (Syverson, 2011).

While a growing literature estimates the effects of M&As on prices and efficiency for specific industries and merger cases, these studies have produced mixed results that cannot easily be generalized.² A few recent studies have estimated the effects of mergers on markups for a larger set of industries in the US (Blonigen and Pierce, 2016) and India (Stiebale and Vencappa, 2018). However, the samples of M&As studied by these papers are not necessarily the most relevant from a competition policy point of view, and the authors remain silent about the effects on non-merging competitors. This paper addresses this gap and provides evidence on the effects of mergers on non-merging rival firms using a rich data set of European firms.

A unique feature of our merger data set is that it contains an expert market definition from the European Commission's competition authority which enables us to identify competitors in the relevant product markets for each merger case. We combine information on mergers with balance sheet data of European firms from the Orbis database which contains standard variables that are used for the esti-

¹See, for instance, <https://www.ft.com/content/9f0270aa-eabf-11e7-bd17-521324c81e23>, accessed March 3, 2018.

²For recent empirical studies see Braguinsky et al. (2015) and the overview of related literature in Ashenfelter et al. (2014).

mation 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 (Akerberg et al., 2015; De Loecker and Warzynski, 2012). Estimates of production function parameters make it possible to estimate markups and to construct a measure of revenue productivity at the firm level. These estimated values 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 our results, our estimates indicate that markups of non-merging rival firms increase by 2% to 4% in post-merger periods relative to a control group of unaffected firms. These effects are concentrated in markets with few competitors, among firms with initially high market shares and in domestic rather than cross-border merger cases. 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. Consistent with increased market power, we find that mergers are accompanied by reductions in sales and employment in affected firms.

The rest of the paper is organized as follows. Section 2 discusses the related theoretical and empirical literature. Our data set and our empirical strategy are detailed in section 3. Results of our empirical analysis are discussed in section 4, section 5 concludes.

2. Literature

There is a number of empirical studies that focus on changes in market prices after firm consolidation, typically in a specific industry. Examples of examined industries include the airline industry (Kim and Singal, 1993; Kwoka and Shumilkina, 2010), the banking industry (Prager and Hannan, 1998; Focarelli and Panetta, 2003), the health care industry (Dafny et al., 2012; Lewis and Pflum, 2017), gasoline retail (Hastings, 2004; Houde, 2012), pharmaceuticals (Björnerstedt and Verboven, 2016), retail (Allain et al., 2017; Hosken et al., 2018) and many more. While these studies use a large variety of different datasets and evaluation approaches, all of them find evidence that the mergers in the markets under examination have led to price increases, often in a range of approximately 5 to 15%.

Ashenfelter and Hosken (2010) conduct case studies on five consummated mergers affecting the markets for various consumer products. They find that prices significantly increased by 3 to 7% in four out of five cases. A survey article on the price effects of horizontal mergers (Ashenfelter et al., 2014) reports

that of 49 surveyed studies, 36 find evidence for price increases. Thus, there is overwhelming evidence that mergers, on average and across time and industries, will lead to price increases.

Three closely related papers are those by Blonigen and Pierce (2016), Stiebale and Vencappa (2018) and Gugler and Szücs (2016). The first two papers also explicitly address the decomposition of markup changes and efficiency gains, while the third paper also uses expert market assessments to estimate indirect merger effects on rivals. Blonigen and Pierce (2016) examine mergers in the US manufacturing industry in the 1998 to 2006 period using plant-level data. Decomposing productivity and markup effects, they find that markups significantly increase after M&A, while the effect on productivity is either negative or insignificant.

Stiebale and Vencappa (2018) use data from India to estimate the effect of mergers on markups, but also on efficiency and product quality. They find that merger targets raise both their markups and their output significantly which is made possible through substantial reductions in marginal cost. Effects are stronger when the acquirers originate from technologically advanced countries, hinting at knowledge transfers from acquirers to merger targets. They also present evidence for increased product quality and conclude that in emerging markets, M&A may benefit both firms and consumers.

Finally, Gugler and Szücs (2016), like this paper, focus on rival firms in EU mergers. Employing synthetic control methods, they find that merger rivals become significantly more profitable in the periods after a merger. They also find that the size of the effect is predominantly driven by market characteristics such as market shares, market definition and the number of competitors and conclude, that merger rivals reap the benefits of decreased competition after mergers.

3. Data, matching and estimation

3.1. Construction of the dataset

We analyze merger cases that were notified to the EC between 1990 and 2007. From the EC's decision documents, we identify all firms, that were found to be direct competitors of the merging firms. Some firms are found to be competitors in more than one case. The rival firms are active in 132 different 4-digit NACE industries and originate from all member states.³

We proceed to match the rival firms to firm-level data obtained from Bureau van Dijk's Orbis database. Unfortunately, since many of the mergers in the data occurred in the 1990s and early 2000s and the firm-level data is very sparse before 2003, many observations are lost. We also link the rival firms to PATSTAT to account for their innovation activities.

³Note that while non-European merging firms may be investigated by the EC, if they conduct sufficient business in the European Union, the rivals are exclusively European firms.

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 more sample attrition. After the elimination of some outliers (we drop the top and bottom percentiles of estimated productivities and markups; we drop a few negative markup estimates), the imposition of some reasonable restrictions (we drop rivals in mergers that ended up being prohibited; we drop rivals for which no suitable control observations could be found) and due to the inclusion of lagged values in the matching procedure (see also below), the final estimation sample includes 460 firms appearing 588 times as rivals.

These firms appear in a total of 194 merger cases investigated by the EC, for which we collected extensive information. Of the cases, 95 (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.⁴

We summarize the information collected on mergers at the firm/case level in table 1.

Table 1: Case characteristics

Variable	Observations	Mean	Median	SD	Min	Max
mean_hhi	581	2432.46	2110.00	1428.19	25	6650
mean_deltahhi	561	516.81	372.12	480.95	0	2750
mshare	474	12.54	10.00	9.83	1	60
c1_max	584	61.99	60.00	22.72	10	100
c1_mean	584	33.24	32.07	11.47	8	69
complexity	588	36.83	21.00	40.66	1	177
fullmerger	588	0.79	1.00	0.41	0	1
mean_national	588	0.56	0.75	0.41	0	1
mean_euwide	588	0.31	0.17	0.34	0	1
mean_ww	588	0.11	0.00	0.23	0	1
mean_barriers	588	0.18	0.00	0.35	0	1

Notes:

[description of variables]

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

⁴When 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.

3.2. 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 :

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

where Q_{it} denotes output, M_{it} is material input, K_{it} and L_{it} are capital stock and labor input respectively and Ω_{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:

$$\mu_{it} = \left(\frac{P_{it}Q_{it}}{P_{it}^M M_{it}} \right) \frac{\partial Q_{it}(\cdot)}{\partial M_{it}} \frac{M_{it}}{Q_{it}} = \frac{\theta_{it}^M}{\alpha_{it}^M} \quad (2)$$

where P_{it} denotes the output price, P_{it}^M is the input price of materials, α_{it}^M is the ratio of expenditures on materials to a firm's revenue and θ_{it}^M 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, θ_{it}^M can be estimated from a production function and α_{it}^M can easily be constructed from a firm's balance sheet.

For the empirical implementation, we experiment with both a Translog and a Cobb-Douglas production function. In logarithmic form, the production function can be written as:

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

where ε_{it} denotes measurement error in output.⁵

To estimate the production function, we follow De Loecker and Warzynski (2012) and Akerberg et al. (2015) and assume that a firm's material demand function can be inverted such that: $\omega_{it} = h(m_{it}, k_{it}, l_{it}, \mathbf{ma}_{it}, \mathbf{x}_{it})$ where \mathbf{ma}_{it} is a vector of pre- and post-merger dummies and \mathbf{x}_{it} contains additional control variables such as age, time and average wages. 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:

$$q_{it} = \phi(m_{it}, k_{it}, l_{it}, \mathbf{ma}_{it}, \mathbf{x}_{it}) + \varepsilon_{it} \quad (4)$$

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

$$\omega_{it} = g(\omega_{i,t-1}, \mathbf{ma}_{i,t-1}) + \zeta_{it} \quad (5)$$

⁵ 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: $f(m_{it}, k_{it}, l_{it}) = \beta_m m_{it} + \beta_k k_{it} + \beta_l l_{it} + \beta_{mm} m_{it}^2 + \beta_{ll} l_{it}^2 + \beta_{kk} k_{it}^2 + \beta_{ml} m_{it} l_{it} + \beta_{mk} m_{it} k_{it} + \beta_{kl} k_{it} l_{it} + \beta_{mkl} m_{it} k_{it} l_{it}$

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

$$E[\zeta_{it}(\beta)\mathbf{z}_{it}] = 0 \quad (6)$$

where \mathbf{z}_{it} contains current capital and labour and lagged material input. 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, averages wages, time and merger dummies. The production function is estimated separately for each 2-digit industry to allow for sector-specific production technologies. The Cobb-Douglas specification yields a markup:

$$\mu_{it} = \beta_m \frac{P_{it} Q_{it}}{W_{it}^M M_{it}} \quad (7)$$

and we correct output for measurement error, ε_{it} , estimated from the first stage following Akerberg et al. (2015) and De Loecker and Warzynski (2012).⁶

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 of a measure of physical TFP, our estimates identify a measure of revenue TFP (De Loecker et al., 2016; Brandt et al., 2017). For the Cobb Douglas case:

$$RTFP_{it} = \omega_{it} + (p_{it} - \bar{p}_t) + (p_{it}^M - \bar{p}_t^M)\beta_m + (p_{it}^K - \bar{p}_t^K)\beta_k \quad (8)$$

where p_{it} , p_{it}^M and p_{it}^K are firm-specific prices of output, materials and capital and \bar{p}_t , \bar{p}_t^M and \bar{p}_t^K denote industry-specific price deflators. 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

⁶For the translog case, β_m is replaced with the output elasticity of materials: $\beta_m + 2\beta_{mmm}m_{it} + \beta_{mkk}k_{it} + \beta_{mll}l_{it} + \beta_{mkl}k_{it}l_{it}$

materials while production function parameters are constant within industries. The fact that we obtain qualitatively similar results for the effects of mergers on markups estimated from a Cobb Douglas production function makes us confident that our results are unlikely to be due to bias in estimated elasticities from the production functions.

Descriptive statistics on estimated markups and output elasticities by sector are documented in Table 14 to Table 16.

3.3. Matching and sample balance

We face two potential selection issues when evaluating the performance of merger rivals in the data: first, firms non-randomly select into merging with other firms (see Dafny (2009) and Houde (2012) for IV approaches or Ornaghi (2009) and Szücs (2014) for PSM approaches); as 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), it seems reasonable to assume that the rivals of merging firms could be affected as well. Second, the process of being designated a rival in the EC's competitive assessment could be non-random. It seems likely that the EC should focus its market evaluation on firms, which would be able to actively constrain an exercise of market power by the merging firms. Thus, rivals could be chosen on the basis of being large and / or productive.

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 2 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.

Table 2 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 productivity and markups of rivals are 5-7% lower, but they have a higher probability of being on the fringes of both distributions (i.e. in the 1st or 4th quartile). Finally, also their average productivities and markups (calculated from up to five lags) are moderately lower.

In addition, we are also confronted with a 'missing counterfactual' problem, since we cannot assess the performance of treated firms (i.e. merger rivals) in absence of the treatment (i.e. if they had not been active in a market, in which a merger occurs).

A widespread approach to account for self-selection and the missing counterfactual problem is the construction of a control group through a matching procedure and the application of difference-in-difference (DiD) estimation. 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

Table 2: Covariates means before and after matching

Variable	Before Matching			After Matching		
	Treated	Control	p-value	Treated	Control	p-value
Propensity Score	0.0053	0.0006	0.0000	0.0079	0.0069	0.1929
log(Total Assets)	10.7687	9.6780	0.0000	10.6049	10.4744	0.2642
log(Cumulated Patents)	1.0380	0.1402	0.0000	0.9633	0.9680	0.9669
log(Current Patents)	0.2105	0.0251	0.0000	0.2327	0.2228	0.8520
Innovation Dummy	0.2760	0.1481	0.0000	0.2687	0.2755	0.7935
TFP	-0.0724	0.0009	0.0000	-0.0781	-0.1038	0.1913
Markup	0.3559	0.4091	0.0000	0.3587	0.3804	0.2815
TFP: 1 st Quartile	0.3359	0.1451	0.0000	0.3724	0.4082	0.2097
TFP: 4 th Quartile	0.2044	0.1458	0.0000	0.2041	0.2058	0.9425
Markup: 1 st Quartile	0.1684	0.1410	0.0000	0.1684	0.1531	0.4753
Markup: 4 th Quartile	0.1551	0.1408	0.0001	0.1684	0.1820	0.5398
Average Markup	0.3478	0.4039	0.0000	0.3495	0.3713	0.2636
Average TFP	-0.0771	0.0028	0.0000	-0.0841	-0.1080	0.2220

Notes: Propensity scores are the predicted values from the model in table 3. 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.

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 characteristics, 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 3.

Table 3: 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	
PseudoR ²	0.200	
Year fixed-effects		✓
Industry fixed-effects		✓
Country fixed-effects		✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

The estimation results of the Probit model confirm that the probability of being affected by a merger 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 both problems described above under the assumption of selection on unobservables: since both types of firms have the same ex-ante probability of receiving treatment in a balanced sample, the assignment to treatment 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 1:1 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 firms-pairs.

In the sample thus constructed, treated and non-treated firms are no longer significantly different at the time that matching occurs. Columns 4 - 6 of table 2 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.

Figure 2 illustrates the evolution of markups in the treated and control groups relative to the merger period. The period-specific means evolve similarly in the pre-merger period, but appear to be on a higher level for rivals after the change in market structure. This is confirmed by a preliminary regression analysis, estimating the slope of markups over time in the four subsamples defined by the two groups and the pre/post periods: While both groups seem to be following a common trend in the periods leading up to the change in market structure, merger rivals have higher markups than their control group in the post-period.

3.4. Model and estimation equation

After having constructed the treatment and control groups, we proceed to estimate the impact of mergers on innovation rival firms in the same market in a DiD setting. The average treatment effect on the treated (ATT) is estimated as the difference between both the before and after periods and the treatment and control group. The estimation equation is given by

$$\mu_{i,s,c,t} = \alpha + \delta \text{post}_t + \gamma (\text{treated}_i \times \text{post}_t) + \mathbb{X}_{i,s,c,t} \Pi + \varepsilon_{i,s,c,t} \quad (9)$$

Figure 1: Standardized biases before and after matching

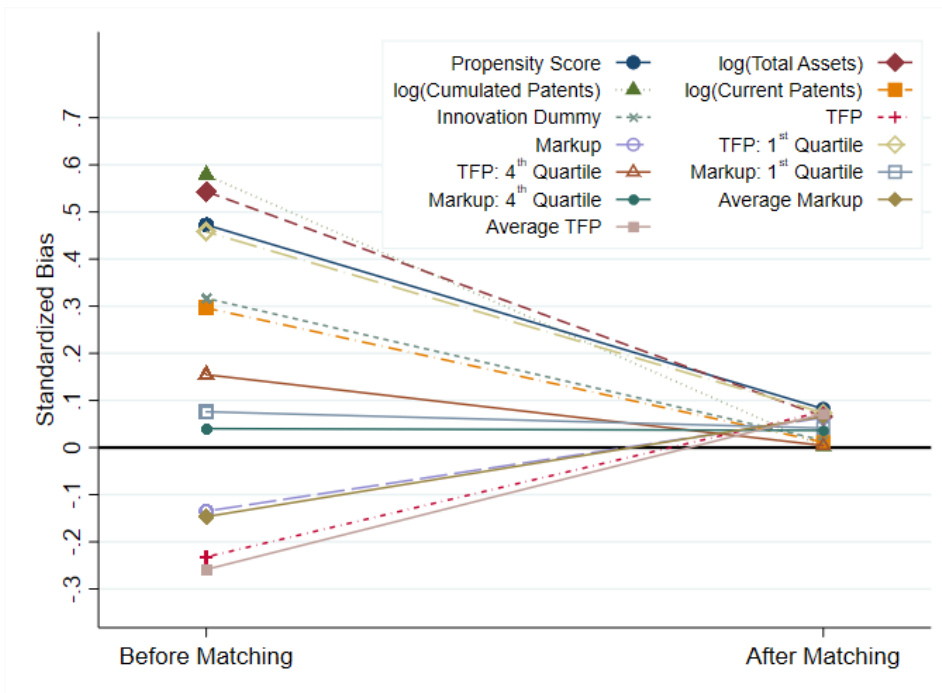
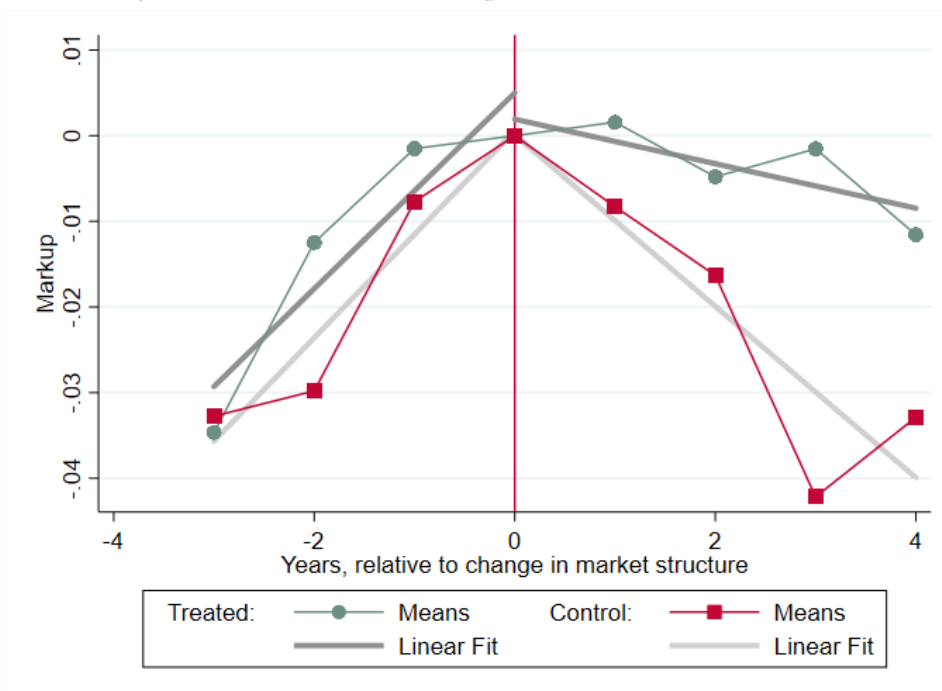


Figure 2: Evolution of markup before and after treatment



where $\mu_{i,s,c,t}$ designates the log markup of firm i , active in sector s and country c at time t and post_t indicates the post-merger evaluation period, $t > 0$. post_t measures any post-merger trend in outcomes, that is common to treated and non-treated markets. The variable treated_i indicates the treatment group (firms that are merger rivals) and the interaction $\text{treated}_i \times \text{post}_t$ measures the ATT. Note that observable differences between treatment and control group are captured by the propensity score while unobservable time-invariant differences are controlled for using the DiD estimator.⁷

We subsume fixed effects in the matrix $\mathbb{X}_{i,s,c,t}$. It includes dummies for 17 years, 32 sectors and 18 countries and, in many specifications, interactions of these effects among each other or with trend variables. Finally, $\varepsilon_{i,t}$ is an error term that is clustered at the firm-level.

We also extend the equation 9 to analyze heterogeneous effects in the following specifications:

$$\mu_{i,s,c,t} = \alpha + \delta \text{post}_t + \gamma_1 (\text{treated}_i \times \text{post}_t) + \gamma_2 (\text{treated}_i \times \text{post}_t) \times H_{i,t} + \mathbb{X}_{i,s,c,t} \Pi + \varepsilon_{i,s,c,t} \quad (10)$$

where $H_{i,t}$ captures heterogeneity at the market or firm level such as a firm's market share, the number of competitors it faces, a dummy variable for cross-border mergers or a dummy variable indicating time passed after the merger.

In all estimations, we exclude the merger period $t = 0$. As it is not clear whether a mergers' (second order) effect on rivals should already be manifest in the year of the merger decision or not, we exclude the year in which the merger was realized from the analysis.

Further, we observe the firms in the data over differing time windows: while for one firm ten years of post-treatment data might be available, others are observed for only three post-years. To ensure that our findings are not affected by this, 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 restrained to be within the first year in which both are observed and the last year in which both are observed. This prevents our results from being affected by 'asymmetric' attrition across treatment and control groups.

4. Results

4.1. Average Treatment Effects on the Treated

In table 4 we estimate the changes in the markups of merger rivals, estimated from a translog production function, relative to their control group, after the change in market structure. The columns of the table report estimation results for different sets of fixed effects: in column (1), we include firm and year fixed effects. Columns (2) adds sector-specific time trends while column (3) substitutes them for sector-specific

⁷In the regressions, treated_i should be small and insignificant, as this indicates that the matched control group approach succeeds in leveling differences between treated and non-treated units. In our regressions, the treatment group indicator is dropped due to the inclusion of firm fixed-effects, but we have verified that this conditions holds in our sample.

year fixed-effects. Columns (4) and (5) add country-specific trends and year fixed-effects respectively. Column (6) includes both country-specific and sector-specific trends separately, while column (7) allows for sector-specific trends by country. Finally and most comprehensively, column (8) includes a full set of country/sector/year fixed effects (more than 1,500 regressors).

Table 4: Markups of merger rivals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-0.035*** (0.007)	-0.030*** (0.007)	-0.033*** (0.008)	-0.033*** (0.008)	-0.039*** (0.008)	-0.029*** (0.007)	-0.029*** (0.007)	-0.026*** (0.008)
M&A	0.023*** (0.008)	0.024*** (0.007)	0.025*** (0.007)	0.023*** (0.008)	0.025*** (0.008)	0.024*** (0.007)	0.024*** (0.007)	0.023*** (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
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

The post-indicators pick up some general negative tendencies in the latter half of the sample (see also figure 2). The interaction term with the treatment group indicator shows a positive and significant treatment effect on rival firms' markups. The size of the effect ranges from 2.3% to 2.5%. Thus, there is empirical evidence for rival markups increasing 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 in line with the autoregressive process employed to estimate the evolution of productivity (see section 3.2). The results are reported in table 5. The coefficients of the pre-treatment markups are significant and close to one, indicating a high level of inertia in the evolution of markups. Controlling for pre-treatment markups also seems to pick up a part of the variation over time, as the post-period indicators are no longer significant. The treatment effects remain significant and increase by about 1/3 in size relative to the initial specification: markups of merger rivals rise by 3 - 3.2% relative to the control group.

Next, we disentangle the average effect on TFP into period-specific effects to investigate time-dynamics.

Table 5: Controlling for pre-treatment markups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-markup	0.854*** (0.009)	0.801*** (0.010)	0.799*** (0.010)	0.839*** (0.010)	0.839*** (0.010)	0.786*** (0.011)	0.777*** (0.012)	0.781*** (0.012)
Post	0.005 (0.007)	0.004 (0.007)	0.007 (0.007)	-0.002 (0.007)	0.000 (0.007)	-0.002 (0.007)	-0.005 (0.007)	0.007 (0.006)
M&A	0.030*** (0.007)	0.031*** (0.007)	0.032*** (0.007)	0.030*** (0.007)	0.031*** (0.007)	0.030*** (0.007)	0.031*** (0.007)	0.031*** (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
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

We define three distinct time periods (one year before the merger, to check for pre-treatment effects; one to three years after the merger; four and more years after), and report the results in table 6.

First, 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 testimony for anticipation effects.

Second, only five out of eight specifications find significant markup increases in the first three years after a merger. This is consistent with the idea that the effect on rivals is a second order effect, as they adapt to a changed competitive environment. The size of the increase in markups ranges from 1.5 to 2.1%.

Third, all eight specifications find significant markup increases between 3.7 and 4.5% four years after the mergers.

4.2. Heterogeneous Effects

Next, we look for heterogeneity in the effects on markups in the post-merger period, by using the market share of the focal firm in the affected market as a measure of treatment intensity (table 7), by estimating the impact of the number of competitors on markups in the relevant markets (table 8) and by evaluating the effect of merger remedies by the EC on markups (table 9).

As reported in table 7, we find that with the inclusion of market shares, the baseline effect of mergers is

Table 6: Markups of merger rivals over time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-0.025*** (0.008)	-0.018** (0.008)	-0.021*** (0.008)	-0.022*** (0.008)	-0.027*** (0.008)	-0.017** (0.008)	-0.017** (0.008)	-0.014* (0.009)
M&A: [-1]	-0.003 (0.009)	0.005 (0.009)	0.011 (0.009)	-0.000 (0.009)	0.002 (0.009)	0.008 (0.009)	0.009 (0.008)	0.015* (0.009)
M&A: [1, 3]	0.010 (0.009)	0.015* (0.009)	0.020** (0.009)	0.012 (0.009)	0.014 (0.009)	0.017** (0.009)	0.018** (0.008)	0.021** (0.008)
M&A: [4+]	0.037*** (0.010)	0.042*** (0.010)	0.045*** (0.010)	0.039*** (0.010)	0.043*** (0.010)	0.044*** (0.010)	0.045*** (0.009)	0.044*** (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
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneous treatment: effect of market share on markups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-0.023*** (0.008)	-0.018** (0.008)	-0.023*** (0.008)	-0.022*** (0.008)	-0.027*** (0.008)	-0.018** (0.008)	-0.019** (0.008)	-0.020** (0.009)
M&A: [1, 3]	-0.010 (0.012)	-0.004 (0.012)	-0.000 (0.011)	-0.007 (0.012)	-0.007 (0.011)	-0.001 (0.012)	-0.008 (0.011)	0.001 (0.011)
M&A: [4+]	0.023 (0.014)	0.027** (0.014)	0.028** (0.013)	0.027* (0.014)	0.026* (0.013)	0.031** (0.014)	0.021 (0.013)	0.017 (0.013)
Market share: [1, 3]	0.160*** (0.062)	0.133** (0.061)	0.121** (0.060)	0.145** (0.062)	0.155** (0.061)	0.119* (0.061)	0.158*** (0.058)	0.086 (0.066)
Market share: [4+]	0.096 (0.069)	0.087 (0.069)	0.066 (0.067)	0.076 (0.071)	0.101 (0.067)	0.060 (0.070)	0.115* (0.069)	0.096 (0.076)
Observations	8526	8526	8526	8526	8526	8526	8526	8526
R ²	0.861	0.866	0.875	0.863	0.868	0.869	0.879	0.908
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

rendered insignificant indicating that there are no substantial changes in markups for firms with initially small market shares. The coefficients of market share show large and positive values and are statistically significant in three early post-periods and in all late post-periods. The size of the effect is economically significant and ranges from 14 to 20%. Thus, an increase in market share by 10% would lead to 1.4% to 2% higher post-merger markups.

Table 8: Heterogeneous treatment: effect of competitors on markups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-0.024*** (0.008)	-0.020*** (0.008)	-0.023*** (0.008)	-0.022*** (0.008)	-0.027*** (0.008)	-0.019** (0.008)	-0.019** (0.008)	-0.017** (0.008)
M&A: [1, 3]	0.051** (0.020)	0.044** (0.019)	0.042** (0.019)	0.043** (0.019)	0.045** (0.019)	0.037* (0.019)	0.046** (0.018)	0.030 (0.020)
M&A: [4+]	0.071*** (0.023)	0.058*** (0.022)	0.049** (0.022)	0.060*** (0.023)	0.064*** (0.022)	0.048** (0.022)	0.059*** (0.022)	0.061*** (0.023)
Competitors: [1, 3]	-0.010** (0.004)	-0.008* (0.004)	-0.007 (0.004)	-0.008* (0.004)	-0.008* (0.004)	-0.006 (0.004)	-0.008** (0.004)	-0.004 (0.004)
Competitors: [4+]	-0.008 (0.005)	-0.005 (0.005)	-0.003 (0.005)	-0.005 (0.005)	-0.006 (0.005)	-0.002 (0.005)	-0.005 (0.005)	-0.006 (0.005)
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
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

In table 8 we include the log of the number of competitors identified by the EC during its competitive assessment of the merger. This uncovers some opposing effects in the data: while the baseline effects on markups increases (relative to the findings of the previous section) to 4 to 6%, doubling the number of competitors decreases post-merger markups by 0.7% - 1.2%. Interestingly, this effect only manifests in the three years after mergers and disappears in the longer run.

Table 9 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 2% - 2.3% less and this effect is significant in seven out of eight

Table 9: Heterogeneous treatment: effect of remedies on markups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-0.024*** (0.008)	-0.019** (0.008)	-0.023*** (0.008)	-0.022*** (0.008)	-0.027*** (0.008)	-0.019** (0.008)	-0.019** (0.008)	-0.017** (0.008)
M&A: [1, 3]	0.017* (0.010)	0.018* (0.010)	0.019* (0.010)	0.018* (0.010)	0.019* (0.010)	0.019* (0.010)	0.020** (0.009)	0.019** (0.010)
M&A: [4+]	0.049*** (0.011)	0.046*** (0.011)	0.043*** (0.011)	0.051*** (0.011)	0.052*** (0.011)	0.047*** (0.011)	0.049*** (0.011)	0.050*** (0.011)
Remedies: [1, 3]	-0.012 (0.012)	-0.011 (0.012)	-0.007 (0.011)	-0.012 (0.012)	-0.013 (0.012)	-0.012 (0.012)	-0.013 (0.011)	-0.013 (0.012)
Remedies: [4+]	-0.020 (0.014)	-0.012 (0.013)	-0.010 (0.013)	-0.022 (0.014)	-0.020 (0.013)	-0.015 (0.013)	-0.019 (0.013)	-0.027** (0.013)
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
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

specifications. Thus, in mergers that were allowed to proceed only subject to remedies, about 2/3 to 3/4 of the increase in markups after a merger was mitigated.

4.3. Effects on Prices and Other Outcome Variables

Since estimates of productivity are based on revenues and not on physical quantities, it is difficult to distinguish changes in productivity from changes in prices. Related to that, 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)):

$$\mu_{i,s,c,t} = \kappa + \psi \text{post}_t + f(\text{TFP}_{it}) + \tau_1 (\text{treated}_i \times \text{post}_t) + \tau_2 (\text{treated}_i \times \text{post}_t) \times H_{i,t} + \mathbb{X}_{i,s,c,t} \lambda + \varepsilon_{i,s,c,t}$$

Estimating the treatment effect on markups, while at the same time controlling for firms' changes in TFP, enables us to interpret the results as effects on firms' prices. If increasing markups are due to changes in market power—rather than changes in efficiency and 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 10.

As expected, a firm's markups strongly depend on its productivity. Further, we find evidence for price increases of about 1% in the first three years in four out of eight models. All eight models report price increases between 3 and 3.5% in later periods. The size of the coefficients indicating prices changes suggests that most of the observed markup increases (compare, for example, table 6) are indeed due to increases in prices rather than decreases in marginal cost. To be specific, the price effects after 4 years and longer after the merger estimated in Table 10 are 70-92% of the size of the respective markup coefficients reported in Table 6. Thus, only 8% - 30% of markup changes could be potentially explained by efficiency gains. Note that this is rather an upper bound since TFP partly 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 deflated sales and the number of employees (as a physical measure of input into production) as outcome variables. Results, documented in Table 11, indicate reduction in employment and sales of approximately

Table 10: Changes in firm prices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TFP	0.512*** (0.072)	0.555*** (0.073)	0.550*** (0.076)	0.529*** (0.073)	0.532*** (0.071)	0.585*** (0.073)	0.575*** (0.074)	0.604*** (0.082)
TFP ²	0.368 (0.246)	0.231 (0.248)	0.229 (0.251)	0.301 (0.246)	0.307 (0.244)	0.126 (0.247)	0.181 (0.249)	0.024 (0.276)
TFP ³	-0.343* (0.201)	-0.255 (0.204)	-0.265 (0.207)	-0.291 (0.201)	-0.322 (0.201)	-0.174 (0.204)	-0.246 (0.205)	-0.150 (0.230)
Post	-0.013* (0.007)	-0.008 (0.007)	-0.010 (0.007)	-0.013* (0.007)	-0.016** (0.007)	-0.009 (0.007)	-0.009 (0.007)	-0.009 (0.008)
M&A: [1, 3]	0.010 (0.007)	0.011* (0.007)	0.012* (0.007)	0.011 (0.007)	0.011 (0.007)	0.012* (0.007)	0.011* (0.007)	0.010 (0.007)
M&A: [4+]	0.034*** (0.008)	0.035*** (0.008)	0.034*** (0.008)	0.034*** (0.008)	0.035*** (0.008)	0.034*** (0.008)	0.034*** (0.007)	0.031*** (0.008)
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
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

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. All in all, it seems like markups increases after mergers stem from changes in market power rather than changes in efficiency. Columns (3) and (4) of Table 11 show that mergers are associated with negative but insignificant reductions in wages and little change and the labour share in revenue.

Table 11:

	Employment		Sales		Wages		Labour Share	
Post	0.106***	(0.036)	0.103***	(0.038)	0.082**	(0.032)	-0.005	(0.005)
M&A	-0.064**	(0.029)	-0.054*	(0.029)	-0.041	(0.026)	0.002	(0.004)
Observations	6929		6929		6929		6929	
R ²	0.957		0.965		0.973		0.907	

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.

4.4. Extensions and further robustness checks

As discussed in section 3, a potential problem with markups estimated from a translog production function is that biased 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. Results documented in Table 12 show that results for Cobb Douglas markups are qualitatively similar and even larger than for translog markups.

As a further dimension of treatment effect heterogeneity, we distinguish between domestic mergers, where acquirer's and target's headquarter 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, cross-border mergers are possibly more likely to be pro-competitive because of technology transfer and market access (e.g., Guadalupe et al., 2012; Javorcik and Poelhekke, 2017). Results in Table 13 show that the association between cross-border mergers and markups is indeed less strong than for domestic mergers.

5. Conclusion

This paper analyzes the effects of horizontal mergers on the markups of non-merging rival firms which are identified by market definitions of the European commission. We use a propensity score matching

Table 12: Markups of merger rivals estimated from Cobb-Douglas production function

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-0.067*** (0.016)	-0.060*** (0.016)	-0.062*** (0.016)	-0.068*** (0.016)	-0.067*** (0.017)	-0.062*** (0.016)	-0.062*** (0.015)	-0.057*** (0.018)
M&A	0.043*** (0.016)	0.044*** (0.015)	0.047*** (0.015)	0.042*** (0.016)	0.043*** (0.015)	0.043*** (0.015)	0.041*** (0.015)	0.040*** (0.015)
Observations	9076	9076	9076	9076	9076	9076	9076	9076
R ²	0.863	0.867	0.873	0.864	0.868	0.868	0.878	0.903
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

approach combined with a DiD estimator and find that rival firms substantially increase their markups in post-merger periods. Consistent with the interpretation that changes in markups stem from enhanced market concentration, we find that markups even increase after controlling for TFP and that mergers are associated with reductions in sales and employment. We further provide evidence that changes in markup are concentrated in markets with few competitors, among firms with initially high market shares and in domestic mergers. Our results indicate that consummated horizontal mergers in the EU are often anti-competitive.

Table 13: Markups of merger rivals and cross-border mergers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-0.024*** (0.008)	-0.019** (0.008)	-0.023*** (0.008)	-0.022*** (0.008)	-0.027*** (0.008)	-0.018** (0.008)	-0.019** (0.008)	-0.017** (0.008)
M&A: [1, 3]	0.023** (0.011)	0.019* (0.010)	0.021** (0.010)	0.022** (0.011)	0.024** (0.011)	0.019* (0.010)	0.021** (0.010)	0.027** (0.011)
M&A: [4+]	0.067*** (0.012)	0.059*** (0.011)	0.057*** (0.011)	0.062*** (0.012)	0.062*** (0.012)	0.056*** (0.011)	0.055*** (0.011)	0.048*** (0.012)
Cross-border: [1, 3]	-0.018 (0.012)	-0.009 (0.012)	-0.009 (0.012)	-0.015 (0.012)	-0.018 (0.012)	-0.008 (0.011)	-0.010 (0.011)	-0.021* (0.012)
Cross-border: [4+]	-0.043*** (0.014)	-0.030** (0.013)	-0.028** (0.013)	-0.035*** (0.013)	-0.032** (0.013)	-0.025** (0.012)	-0.024* (0.012)	-0.019 (0.013)
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
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓		✓	✓	
Sector trends		✓				✓		
Sector/Year FE			✓					
Country trends				✓	✓	✓		
Country/Year FE					✓			
Cou/Sec trends							✓	
Cou/Sec/Year FE								✓

Notes: Standard errors in parentheses, * p<0.1, ** p<0.05, *** p<0.01.

A. Productivity summary statistics

Table 14: Markups, by sector

	mean	p50
1	3.77	1.87
5	5.28	2.13
10	1.66	1.07
13	1.76	1.13
14	2.90	1.64
16	1.29	1.14
18	3.39	2.19
20	2.08	1.11
21	2.81	2.01
22	1.58	1.38
23	1.93	1.45
24	1.73	1.20
26	1.77	0.98
28	1.56	1.17
29	1.46	0.96
30	2.46	0.96
31	2.24	1.21
35	5.87	1.27
36	3.78	1.20
41	4.47	1.66
45	2.45	0.83
55	9.22	5.96
56	2.83	1.95
58	9.59	2.03
62	16.60	3.03
64	11.24	1.04
68	17.28	2.34
69	9.80	0.98
72	3.80	1.29
73	16.43	3.67
84	10.91	4.29
86	3.01	2.01
87	6.46	3.63
90	12.21	2.88
Total	4.21	1.06
<i>N</i>	1034003	

Table 15: Output elasticities, by sector

	beta_k	beta_l	beta_m	rts
1	0.07	0.20	0.79	1.06
5	0.36	0.35	0.44	1.16
10	0.19	0.18	0.65	1.02
13	0.10	0.06	0.54	0.70
14	0.19	0.20	0.74	1.12
16	0.10	0.18	0.60	0.89
18	0.15	0.10	0.83	1.07
20	0.23	0.20	0.59	1.02
21	0.13	0.36	0.73	1.22
22	0.09	0.24	0.70	1.04
23	0.14	0.28	0.66	1.08
24	0.09	0.27	0.55	0.91
26	0.12	0.25	0.46	0.84
28	0.07	0.28	0.54	0.89
29	0.10	0.24	0.53	0.87
30	0.08	0.45	0.39	0.92
31	0.11	0.28	0.50	0.89
35	0.18	0.26	0.63	1.07
36	0.16	0.33	0.23	0.73
41	0.12	0.23	0.53	0.87
45	0.13	0.17	0.60	0.90
55	0.05	0.21	0.77	1.03
56	0.14	0.20	0.54	0.89
58	0.23	0.40	0.31	0.95
62	0.23	0.44	0.32	0.99
64	0.15	0.25	0.45	0.84
68	0.29	0.34	0.44	1.07
69	0.10	0.42	0.30	0.82
72	0.18	0.46	0.21	0.85
73	0.28	0.35	0.37	1.00
84	0.31	0.56	0.24	1.10
86	0.14	0.33	0.33	0.81
87	0.11	0.51	0.29	0.91
90	0.32	0.39	0.38	1.09
Total	0.14	0.23	0.55	0.92
N	1034003			

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