

Gugler, Klaus; Szücs, Florian

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Spillover effects in oligopolistic markets

Klaus Gugler* and Florian Szücs†

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Abstract

We estimate the spillovers on firm profitability and market shares in oligopolistic markets through the transition from an n to an $n - 1$ player oligopoly after a merger in the industry. Competitors are identified via the European Commission's market investigations and our methodology allows us to disentangle the spillover due to the change in market structure from the merger effect. We obtain results consistent with the predictions of standard oligopoly models: non-merging rivals expand their output and increase their profits, while merging firms barely break even. The size of the effect is larger in industries with fewer oligopolists and higher initial profits.

1 Introduction

In markets characterized by oligopolistic competition, the strategic interaction between the firms makes the profitability of an individual firm depend on the actions of the other firms in the sector. Since there is a maximum amount of total profits that can be achieved in an industry (monopoly profits), a larger number of firms implies lower profits per firm. Also, more players push the industry towards a more competitive equilibrium, eroding firm profits. A reduction of the number of players, on the other hand, increases firm profits by making each firm's share of total profits larger and by decreasing the competitive pressure in the industry. The number of oligopolists can be reduced by either firms exiting the market (voluntarily or due to bankruptcy) or by firms ceasing to be independent players after being acquired in a merger. The focus of this study lies on the latter case.

Standard oligopoly models predict two countervailing effects of a merger: the efficiency effect is a consequence of the reallocation of productive assets within the merging firms. This effect comprises possible rationalizations, technology transfers or the exploitation of economies of scale. If a merger generates a sizeable efficiency effect the merged entity produces more efficiently than before and increases the competitive pressure on its rivals. The merger thus exerts a pro-competitive externality on the relevant market. The second effect, the market power effect, captures the impact of the reduction of firms active in the relevant market. In a market characterized by quantity competition or price competition with differentiated goods, the merged entity finds it optimal to reduce its production. In the new equilibrium, the remaining firms have increased their output, but by less, such that aggregate output is reduced and market prices and industry profits increase. Therefore a merger induces an anti-competitive externality as well. The net effect of these two externalities on the market is often called the spillover of the merger.

*WU (Vienna University of Economics and Business), Augasse 2-6, 1090 Vienna, Austria. E-mail: klaus-gugler@wu.ac.at.

†WU (Vienna University of Economics and Business), Augasse 2-6, 1090 Vienna, Austria. E-mail: florian.szuecs@wu.ac.at.

While most models that incorporate elements of market structure would predict such a spillover effect, there are, as far as we know, no empirical studies that attempt to quantify the spillover on the profits of non-merging firms. This is due to a number of conceptual and practical difficulties that arise when attempting to measure spillover effects, notably the questions which firms are affected and how to measure the spillover. First, since industries in which a merger would cause a sizeable spillover effect would typically consist of only a few competing oligopolists,¹ the correct measurement of the effect crucially depends on the identification of the affected firms, that is, the assessment of the relevant market. Gross approximations, for example by using industry classifications, would be insufficient, as the inclusion of unaffected firms would bias the results towards zero. Only by conducting a market investigation in the specific product market concerned by each merger can the rivals be reliably identified.

Secondly, even if the affected firms are found, the problem of how to correctly measure the spillover effect remains. A simple before and after comparison of the performance of the firms neglects other shocks the firms are exposed to and, more importantly, the issue of endogeneity: since mergers are a means of asset reallocation, they often occur in response to an industry-wide shock. Therefore a naive before and after approach to measuring the spillover would confound the spillover effect with that of the shock that initially triggered the merger. This can be accounted for by constructing an appropriate control group and using difference-in-difference (DiD) estimation. However, this generates another problem in turn: since the merger spillover cannot reliably be confined to a specific set of firms, we cannot know with certainty which firms were not exposed to it. Thus, symmetrically to the problem of finding the firms which *were* affected, we face the problem of finding firms which *were not* affected by the spillover. Failing this, we risk that the control group is contaminated by the effect we would like to measure. For these reasons the spillover of a merger on the relevant market has so far been an elusive magnitude.

In this paper we try to address the problems described in the previous paragraphs and to provide empirical estimates of spillover effects on the market. This is achieved through the use of a detailed and unique dataset, containing the market assessments of the European Commission (EC) in merger cases. The EC routinely publishes a competitive assessment and a delineation of the relevant markets in its decisions on notified mergers; we propose to employ this information to circumvent the above problems as follows: the Commission's competitive assessment identifies the most important competitors in the product markets concerned by the merger, thus providing us with a set of firms that are most likely to be affected by the merger's spillover.² Furthermore, the Commission publishes its assessment of the relevant geographic market concerned by the merger, which can be either national, EU-wide or worldwide in scope. This information can be used to find a valid counterfactual: by selecting control observations from another nation if the geographic market is found to be national, from a non-European nation if the market is defined as EU-wide and by dropping cases with global markets, we ensure that the control group is not contaminated by the spillover effect. Thus the information contained in the decisions of the EC can be utilized to identify the set of firms affected by the spillover and construct a valid control group.

This careful approach is important for a number of reasons: First, the use of correctly identified merger rivals and a carefully selected and non-contaminated control group permits us to clearly delineate the spillover effect of mergers and provide the first estimates of its size in a cross-industry study. Secondly, by using separate control groups for merging firms and competitors, we ensure that our measurements are not confounded by the relative sizes of the efficiency and the market

¹All standard models predict that for a large number of firms the oligopolistic equilibrium moves towards the competitive equilibrium, reducing the importance of the merger externalities.

²Previous studies on the effect of mergers on rivals' prices were able to delineate the relevant market by focussing on industries characterized by many different local markets - e.g. hospitals (Dafny, 2009) or banks (Prager and Hannan, 1998) - such that some non-merging firms were merger rivals and some were not. This approach is not possible in a cross-industry study.

power effect.³ Finally and related to this point, we are able to distinguish the mergers' spillover effect from the effect on merger insiders, allowing us to draw conclusions relevant to competition policy.

Our findings are compatible with the predictions of Cournot and differentiated Bertrand competition: competitors (i.e. the non-merging firms) grow faster than both the control group and the merging firms in the post-merger period, indicating a gain in market share at the expense of the merged entity. While the profitability of merging firms is lower after the acquisition, the competitors experience an increase of around one percentage point in their profit-to-assets ratio. This strongly suggests that in the average merger in our sample, the market power effect induced by the merger significantly outweighs the efficiency effect. Additionally, we distinguish between spillovers in industries with many and few oligopolists, as well as industries with high and low initial profits, and again find the theoretical predictions fulfilled: positive spillovers on rivals are higher if the number of competitors is low or the pre-merger profitabilities are high.

The remainder of the article is structured as follows: section 2 discusses related empirical literature and derives hypotheses from theory, section 3 is concerned with the creation of the dataset, a number of methodological issues in the estimation of causal effects and our empirical approach. Results are presented in section 4 and section 5 concludes.

2 Literature & hypotheses

2.1 Related empirical literature

While we are not aware of previous studies on the impact of mergers on the profitability and sales of rival firms, there is some work on the price effects of mergers.⁴

Dafny (2009) employs an instrumental variable approach to estimate the effect of US hospital mergers between 1989 and 1996 on the price of other nearby hospitals. In his sample, the non-merging hospitals close to a merger (i.e. the merger rivals) exhibit different pre-merger price dynamics than those not close to a merger, suggesting that merger rival status cannot be assumed to be exogenously given. Using topographic proximity as an instrument for the occurrence of mergers, he finds a sharp increase in the prices of merger rivals.

Another strategy to measure the price effect of mergers on rival firms is used by Prager and Hannan (1998): focussing on the US banking industry, they argue that different metropolitan areas constitute different markets and use firms in markets without horizontal bank mergers as a control group. They find that the occurrence of a horizontal merger in a market leads to an 18% drop in deposit interest rates, i.e. the price banks pay their customers, over a period of two years around the merger and conclude that the mergers in their sample have increased the market power of the firms. Focarelli and Panetta (2003) find that merging banks decrease their deposit rates in the short run, but increase them in the long run, which they interpret as efficiency gains in the long run being passed on to consumers. Rival banks, however, weakly decrease their deposit rates in both the short and the long run, pointing to a preponderance of market power effects over efficiency effects.

³If, for example, the profitability of merging firms were to increase by two percentage points relative to their competitors after the merger, but a positive spillover (market power effect > efficiency effect) increased rival profitability by one percentage point, the actual increase in the profitability of merging parties is three percentage points and our estimate would be biased downwards. Contrarily, in the presence of a negative spillover on rivals we would overestimate the profitability effect on the merging parties.

⁴There is a small literature evolving around the abnormal returns of rivals in an event study setting. While early studies (Eckbo (1983) and Stillman (1983)) find no evidence for anticompetitive effects, Clougherty and Duso (2009) find positive abnormal returns for rivals around the announcement of a merger. Duso, Gugler, and Yurtoglu (2011) employ the same approach to evaluate the effectiveness of European merger control.

Kim and Singal (1993) apply the same approach to the airline industry, where each route can be considered a separate market. Their results show, that fares increase by almost 10% after a merger due to increased market power. Interestingly, price increases also occur on routes where the merging parties do not overlap, i.e. where concentration does not increase; the authors interpret this finding as evidence that increased multimarket-contact between the airlines facilitates collusion.

Ashenfelter and Hosken (2010) conduct case studies on five mergers in various consumer product industries, finding some increases in prices in four cases. They use two control groups, one containing other branded products and one containing private label products. In three out of the four cases with post-merger price increases they find that the increase is more pronounced with respect to the private label products, suggesting that the prices of other brands (which are closer substitutes to the merging firms' products) have increased as well.

Most studies looking at the effects of mergers on the prices of rivals (see Weinberg (2008) for a survey) conclude that large, horizontal mergers entail significant market power effects. However, the conclusions of these studies are restricted to specific mergers or specific markets, or they use control observations from markets that are affected by the merger, thus failing to identify the spillover. Below we (i) assess the spillover effects for a large cross-section of industries and (ii) solve many of the ambiguities concerning proper, i.e. uncontaminated, control groups.

2.2 Theoretical predictions

In standard Cournot oligopoly models (Salant, Switzer, and Reynolds, 1983; Perry and Porter, 1985; Faulí-Oller, 1997) horizontal mergers are profitable for the merging firms only under rather specific circumstances, a fact that is often called the merger paradox. They are, however, always profitable for competitors, since - absent efficiency gains - the merged entity will decrease its output, which increases the market price. In reaction, the competitors expand their output, but by a lesser amount than the insiders decrease their output, such that in the new equilibrium they sell a higher quantity at a higher price, which clearly is profitable. Farrell and Shapiro (1990) consider the possibility that the merged entity experiences efficiency gains through economies of scale or learning. Using simple Cournot examples, they show that these efficiency gains would have to be quite large to make the merger unprofitable for outsiders. In Bertrand oligopolies with differentiated products (Deneckere and Davidson, 1985) mergers are profitable for both insiders and outsiders and their profitability increases with the number of merging firms, but again the free-riding outsiders benefit more than the merging firms. Thus from both kinds of standard IO oligopoly models (quantity competition and price competition with differentiated products) we would infer a positive spillover on the profits of the rivals in the relevant market concerned by the merger, absent substantial efficiency gains.

Based on these theoretical considerations, we conjecture that mergers are likely to exert a positive net externality on the profitability of other firms in the market. For example, assuming a symmetric n -player Cournot oligopoly with undifferentiated goods, constant marginal cost c and a linear demand function of the form $p = a - \sum_{i=1}^n q_i$, the profit of a single firm is given by $\Pi_i^{pre} = (\frac{a-c}{n+1})^2$. After a merger the number of players in the market decreases to $(n - 1)$ and we assume that the merged entity, through efficiency gains, now has a new marginal cost of $c_M < c$. This changes the profit of a non-merging firm to $\Pi_i^{post} = (\frac{a-2c+c_M}{n})^2$. Normalizing c to 1, we calculate the minimum values for c_M , such that $\Delta\Pi_i = \Pi_i^{post} - \Pi_i^{pre} \geq 0$ conditional on the parameters of market structure, a and n .

Table 1 here

Table 1 shows that the efficiency gains would have to be rather sizeable for the spillover to become negative: in a market with five oligopolists and a ratio of maximum reservation price to marginal cost of four ($n = 5$, $a = 4$), the merged entity would have to halve its marginal cost for the spillover to become negative. In markets where the ratio of maximum reservation price to cost of production is very high, the critical value of c_M is zero, such that no amount of efficiency gains will offset the positive spillover. Since there is no empirical, cross-industry evidence that mergers in general achieve even moderate efficiency gains⁵ and the size of the gains required to cancel the market-power externality is very high, we expect that:

Hypothesis 1 Spillover effects will on average increase the profitability of rival firms in the same relevant market due to the market power effect exceeding the efficiency effect. Moreover, the market shares of rivals will rise.

For a fixed level of efficiency gains, we can compute the size of the spillover, $\Delta\Pi_i$, conditional on a and n . Table 2 reports the spillovers if no efficiency gains are achieved by the merger, $c_M = c = 1$, such that $\Delta\Pi_i = \Pi_i^{post} - \Pi_i^{pre} = \frac{(2n+1)(a-1)^2}{n^2(n+1)^2}$.⁶

Table 2 here

Absent efficiency gains, all spillovers are positive and the size of the spillover decreases exponentially with the number of oligopolists, because of the associated increase in competitive pressure; thus

Hypothesis 2 Spillovers on rival profitability will be larger in markets with fewer oligopolists.

The parameter a , the vertical intercept of the demand curve, increases the size of the spillover on rivals. Since high values of a reduce the elasticity of the residual demand curve the individual firm faces, a higher a makes all firms in a given market - *ceteris paribus* - more profitable; we thus use the profitability of merging firms to identify 'High a ' and 'Low a ' markets.

Hypothesis 3 Spillovers on rival profitability will be larger in markets where firms have a high initial profitability.

3 Data & methodology

In this section we describe how the dataset on competitors and merging firms was created, discuss the assumptions necessary to identify a causal effect and try to establish their empirical validity in our approach to measuring the spillover effects of mergers.

3.1 Constructing the dataset

To construct the dataset we combine merger-level information on 241 acquisitions that underwent the scrutiny of European competition law between 1990 and 2007 with firm-level information on the firms involved in these mergers. Merger-level data was gathered from the official decisions of the European Commission, from which we collected the names of the merging firms and the most important rivals in the relevant product markets, as found in the market investigation, as well as the geographic size of the market affected by the merger. The firms thus identified were

⁵For an industry study see Gugler and Siebert (2007).

⁶Assuming other values for c_M changes the size of the spillover, but does not change the direction of effect of n and a .

linked to balance-sheet data from the Thomson Reuters Worldscope database, providing data on income, sales, total assets, employees, debt, depreciation and market capitalization. Profitability is calculated as net income divided by total assets, Tobin’s Q is the sum of market capitalization and total debt divided by total assets. All monetary variables are CPI-adjusted to 2005 USD and logarithmized.

After imposing all necessary restrictions on the data (firms have to be listed for firm-level data to be available; the relevant geographic market in the merger must not be worldwide;⁷ observations where multiple mergers occur within the relevant timeframe have to be dropped; full firm-level data on all variables mentioned above are required; top and bottom percentiles of the profitability variable are dropped) we obtain a sample of rivals and merging firms, which we complement with a large sample of almost 50,000 firms used for matching as described below.

Since after a merger, some (or all) of the target’s business activities are added to the books of the acquirer, there is a surge in the acquiring firm’s turnover between $t - 1$ and $t + 1$, where t designates the period in which a merger occurred. In our sample, we observe an average increase of more than a quarter in the sales of acquiring firms from $t - 1$ to $t + 1$. This consolidation effect is not a causal effect of the merger, but a bookkeeping phenomenon. To control for this, we construct a sample of consolidated entities by artificially consolidating the merging firms: we add up the sales, assets, income and other relevant variables of acquirer and target. To do this, we require full information on both the acquirer and the target in the same merger, resulting in a sample of 59 consolidated entities. For these 59 mergers, we have data on 174 competitors identified by the EU Commission.

This is the ideal approach to study the aggregate effect of the merger on the merging parties; however, due to the high data requirements, we are left with a rather small sample. To broaden our analysis, we construct a second sample including all acquirers (130 firms) as well as all rivals identified for these 130 mergers (357 firms).⁸ By focussing on acquirers we make sure to include all the firms that decided to engage in an acquisition and are subject to its most immediate effects and their direct competitors. To avoid estimating an effect on sales that is due to the consolidation of the targets, we estimate the spillover and merger effect on sales in periods $t + 2$ to $t + 5$ relative to $t + 1$.

3.2 Identifying the causal effect of treatment

The estimation of causal treatment effects using DiD, matching or a combination of the two requires a set of assumptions on the composition of the treatment and control groups as well as on the nature of the treatment effect. Lechner (2010) identifies five necessary assumptions: the stable unit treatment value assumption (SUTVA), the exogeneity assumption (EXOG), the no-effect pre-treatment assumption (NEPT), the common trends assumption (CT) and the common support assumption (COSU), which will be discussed with regard to estimating oligopolistic spillover effects in the following.

SUTVA states that exactly one outcome (treatment or no treatment) is observed for each member of the population and that there are no relevant interactions between the groups. Letting $\delta_i \in \{0, 1\}$ denote the treatment status of observation i , $j \in \{t - 1, t + 1\}$ denote the pre- and post-treatment periods, $Y_{i,j}$ denote i ’s observed outcome in j and Y_j^d denote the treatment and time specific, potential outcomes we can formally write SUTVA as:

⁷We compare firms in mergers with national or European-wide relevant geographic markets to firms outside of the market delineation. We drop worldwide mergers, because we cannot ensure that the control group would be unaffected by the merger. See appendix A for a robustness check including worldwide mergers.

⁸Actually, to estimate the effects of mergers on rival firms we do not need any information on the merging firms. Appendix B estimates the spillover effect using all available rival observations.

$$Y_{i,j} = \delta_i Y_j^1 + (1 - \delta_i) Y_j^0 \quad (1)$$

Thus, since δ_i is binary only one of the potential outcomes is relevant for i 's outcome; the outcomes do not interact. When applied to the context of spillover effects, SUTVA is violated by definition: the very effect we are looking for - the spillover - contradicts the assumption of no relevant interactions between groups. It is therefore not possible to infer either the impact of the merger on the merging firms or the spillover on competitors by looking at just these two groups of firms (this was illustrated with an example in footnote 3). Even the use of an industry-wide control group may not suffice to obtain unbiased estimates, as the control group could be affected by the spillover as well. Particularly, the better (i.e. closer to the merging firms in terms of distance in physical and product space) the controls, the higher the probability of being affected.

EXOG, the assumption of exogeneity, requires that the observed distribution of covariates, X , does not depend on treatment status, i.e.:

$$X = X^1 = X^0. \quad (2)$$

This is not necessarily true for the merger treatment: when comparing non-merging firms with merging firms, the latter could be larger, more profitable or differ in other ways. While one could argue that being the recipient of a spillover, i.e. a rival, is not a particularly distinguishing quality in itself, the EC's selection process for rivals might be. If the EU Commission identifies the most competitive rivals, rivals will be more efficient than the population of firms. Hence we cannot assume that EXOG holds ex-ante for either merging firms or competitors.

NEPT denotes the assumption, that there is no treatment effect in the pre-treatment period, thus excluding that anticipation of treatment influences the behaviour of individuals. This is very similar to the exogeneity assumption, with the focus shifted from covariates to the outcome variables. Letting $\theta_j(x_i)$ denote the treatment effect in j for $X^1 = x_i$, NEPT demands that

$$\theta_{t-1}(x_i) = 0 \quad \forall x_i. \quad (3)$$

Thus there should be no effect on profitability and sales in $t-1$ due to being a merging firm or a rival in the next period. While this seems plausible in the latter case (it seems unlikely that firms foresee being labeled a competitor by the EC and adjust their behaviour due to this), merging firms would typically know about their merger plans in advance and could act accordingly, or they could become merging firms in the first place because they earn high profits in previous periods, both of which would contradict NEPT.

The common trends assumption is central for the DiD approach and requires that except for the effect of treatment the control group faces the same time trends as the treatment group. This implies that in absence of treatment their expected evolution would have been the same:

$$\begin{aligned} E(Y_{t+1,i}|X = x_i, \delta_i = 1) - E(Y_{t-1,i}|X = x_i, \delta_i = 1) = \\ E(Y_{t+1,i}|X = x_i, \delta_i = 0) - E(Y_{t-1,i}|X = x_i, \delta_i = 0) \end{aligned} \quad (4)$$

This assumption would be violated if, for example, firms from sectors in which mergers occur are subjected to other trends than the control group. Since mergers are often triggered by sector-specific shocks (Fridolfsson and Stennek, 2005), this is not entirely implausible. Alternatively, changes in the political or legal framework could potentially favor or penalize merging firms vis-à-vis other firms.

Finally, since in this framework the identification of the causal effect relies on comparing treated and non-treated individuals in the pre- and post-treatment periods, COSU requires that observations are available in all four subsamples thus defined.

3.3 Quantifying the spillover

This section describes the econometric methods employed to estimate spillover effects and how they serve to address the concerns raised in the previous section. As discussed above, the assumption that there are no relevant interactions between groups, SUTVA, is clearly violated between merging firms and their rivals. Furthermore, even the use of a control group may be problematic, because a well-matched control group may be affected by the spillover. Thus we are looking for a valid control group, that is guaranteed not to be affected by the effects of the merger. This can be achieved by using control observations that are outside of the mergers' geographic scope as defined in the EC's market investigation.⁹ We therefore constrain the algorithm used in the construction of the control group to select control observations from a different nation if the geographic extent of the merger was found to be national and from a non-European nation if the market was found to be Europe-wide. The strategy of selecting matches beyond the relevant geographic market of each merger should make sure that SUTVA holds.

Propensity-score matching is used to construct the control groups for merging firms and competitors: this is done by first estimating the determinants of receiving treatment and then choosing control observations that have similar ex-ante probabilities of receiving treatment. We estimate a probit model of the probability of being a merging firm or a competitor in period t based on firm characteristics measured in period $t - 1$, where t ranges from 1990 to 2007. By using $t - 1$ data,¹⁰ we make sure not to confound the determinants of a merger with its effects.¹¹ The probability to be a merging firm or a competitor is modelled as a function of a set of firm-level covariates, meant to capture the relevant dimensions of firm performance.

Profitability and sales are included because they are measures of performance and size and because changes in these magnitudes are our primary research interest. We therefore want to make certain, that no significant ex-ante differences exist between treatment and control groups. The number of employees is intended to capture the firms' labour endowment. Depreciation is a proxy for the fixed costs of an industry. Finally, Tobin's Q is a measure of the stock market's assessment of the quality of a firm and its management. Additionally, we include a full set of 2-digit SIC dummies to make sure that the groups do not systematically differ with respect to their distribution across industries. Table 3 contains the results of the probit estimation.¹²

Table 3 here

The probability of being a relevant competitor increases with the number of employees and the amount of depreciation. While higher firm quality, as captured by Tobin's Q, is also a significant determinant, profitability and sales remain insignificant. The model also contains 23 2-digit SIC dummies (21 of them significant), which are not reported. In the model for merging firms, employees and depreciation turn out to be significant determinants. Out of the 17 unreported industry dummies included, 10 are significant.¹³ The pseudo- R^2 values of 26 and 38 percent indicate, given the size of the dataset and the complex nature of the decision being modelled, a reasonably good fit of the models to the data.

⁹Sometimes the EC provides different market sizes for different product markets. In these cases, the largest market definition was retained to ensure that controls are chosen from an unaffected area.

¹⁰The probit models were also estimated using $t - 2$ and $t - 3$ data, yielding similar results. However, the log-likelihood and goodness-of-fit measures favored the $t - 1$ specification.

¹¹For the same reason, the t period is excluded from all estimations.

¹²Table 3, all further balance and summary tables as well as all figures report results obtained from the main sample of consolidated firms. While the acquirer-only sample yields very similar results, it is only used for regression analysis.

¹³The number of industry dummies differs between the models, because their coefficients can only be estimated if there are enough observations of merging firms or competitors in a particular industry. In both models, the maximum number of estimable industry dummies were included.

As indicated by the coefficients of the probit model, competitors and merging firms differ from the average listed firm in various dimensions of firm performance. Column 1 of table 4 contains the standardized biases¹⁴ before matching and shows that while not all covariates are significant determinants of being either a competitor or a merging firm, all of them do substantially differ between treated and non-treated firms. This means that in the initial sample the EXOG assumption is violated: clearly, the covariates are not independent of treatment status.

Table 4 here

After the probit models are estimated, we use their coefficients to predict the propensity score, i.e. the ex-ante probability of receiving treatment. This is the basis for matching: balancing the dataset with respect to the propensity score controls for selection on observables and produces a sample that is balanced with respect to the covariates included in the probit model. In addition to selecting control observations from beyond the mergers' relevant geographic market, we also impose a time constraint: by matching categorically on the year variable, we ensure that each control observation contains the same time period as the merger to which it is compared.

Thus, for every acquirer and every competitor in the sample, we select the best match in terms of their propensity score from the same year and outside of the relevant geographic market (1:1 matching without replacement). Column 2 of table 4 contains the remaining standardized biases and demonstrates that after the matching procedure most biases in the matched sample are below 3% and none of them are statistically significant.¹⁵ Furthermore, matching levels all differences in industry distribution.¹⁶ Figure 1 graphically shows the reduction in standardized bias achieved through matching, with sales, employees and depreciation on a logarithmized scale.

Figure 1 here

Table 4 and figure 1 demonstrate that the matching procedure successfully levels the differences in firm-level covariates. Since the covariates are now independent of treatment status, EXOG should hold in the matched sample. Also, since all control observations are selected from regions that are - as per the EC's appraisal - unaffected by the merger in question, we can be fairly certain to conclude that there are no relevant interactions between the groups and that SUTVA holds as well.

The matching algorithm was constrained such that each control observation refers to the same time-window as the corresponding treated observation and included a comprehensive set of industry dummies in the matching covariates. The CT assumption would be violated if macro trends differ between treated and control groups. Controlling for time and industries rules out that either external, time-dependent shocks or industry shocks systematically influence the results, but particularly merging firms could still react differently to changes in the political or legal framework. While there is no way to prove that CT holds, we graph the average, yearly growth rates of profitability and sales in all four subsamples and over the whole 1990 - 2009 period in figure 2.

Figure 2 here

¹⁴The standardized bias $((\bar{X}^1 - \bar{X}^0)/\sigma^1)$, the difference in means of treated and control group divided by the standard deviation in the treatment group) is the bias one incurs by comparing treated to non-treated firms.

¹⁵The largest remaining bias (11.7% bias in Tobin's Q in the competitor sample) is clearly insignificant at $p = 0.383$.

¹⁶Initially, 9 out of 23 dummies in the rival model and 5 out of 17 dummies in the merging firms' model differ significantly between treatment and control group. After matching, no significant differences in industry distribution remain.

Figure 2 shows that changes in profitability and sales of merging firms, rivals and their respective control groups are highly correlated over the whole sample period. The correlations of the yearly average profitability growth of competitors and merging firms to their control groups are .76 and .44 respectively. The respective correlations for sales growth are .96 and .77. This is an indication that CT holds in our sample.

Temporary, individual-specific shocks are well known from the evaluation of training programs for the unemployed: program participants typically experience a drop in wage - called Ashenfelter's dip after Ashenfelter (1978) - prior to program participation. In the context of mergers one could envision a scenario in which merging firms temporarily earn high windfall profits, e.g. due to some positive demand shock before the merger, use the profits to acquire another firm and return to a normal level of profits after either demand returns to its normal level or has been accommodated by an increase in supply. In both scenarios the assumption that treatment has no effect on the treated in the pre-treatment period, NEPT, is violated. To corroborate the validity of NEPT in our data, we graph profitability and sales data prior to a merger in figure 3 to check for systematic shocks on either group of firms.

Figure 3 here

Since both profitability and sales do not strongly change in the years before a merger, we find no evidence for time-dependent firm-specific shocks prior to treatment (violating NEPT). If both CT and NEPT are valid, unobserved firm-specific and time dependent heterogeneity is controlled for. Finally, COSU, the requirement that pre- and post-treatment observations have to be available for both the treated and the control group is fulfilled by construction. Summary statistics on all four groups in all periods of interest are reported in table 5.

Table 5 here

Given that all necessary assumptions appear to be valid in the sample, we can proceed to estimate the causal effect of mergers on profitability and sales in a DiD framework. We implement the DiD setting by constructing a merger-timeline around the year in which a combination occurred: t designates the period in which a merger took place. Since matching occurs one year before the merger, treated and control firms do not significantly differ in the $t - 1$ period. Therefore, we calculate the growth rate of the profitability of firm i in periods $j = t + 1, \dots, t + 5$, $\Delta\Pi_{i,j}$, with respect to $t - 1$, create a set of dummy variables $\delta_{i,j}$ to indicate treatment status ($\delta_{i,j} = 1$ if i was treated in t , otherwise 0) and estimate the following equation

$$\Delta\Pi_{i,j} = \alpha + \sum_{j=1}^5 \delta_{i,j} + \varepsilon_{i,j}. \quad (5)$$

Thus the coefficients of the dummies $\delta_{i,j}$ measure the differential in the growth path of profitability, $\Delta\Pi_{i,j}$, in period j between the treatment and control groups, with reference to a point in time when the target variable did not significantly differ between groups, the $t - 1$ period. We estimate the above equation in three subsamples: in the first subsample, only the rivals and their control group are included, such that the $\delta_{i,j}$ measure the profitability growth of rivals in excess of their control group. The second subsample only includes the merging firms and their control group. In these two estimation settings there is, as argued above, no reason to believe that SUTVA does not hold. In the third subsample, we drop all control observations and directly compare the performance of rivals and merging firms by letting the $\delta_{i,j}$ denote the incremental profitability growth

of rivals over merging firms. Here, SUTVA is violated and both the spillover and the merger effect are intermingled, which we use as a check on our methodology. If the profitability growth of rivals exceeds that of their control groups while merging firms profits are negatively affected, we would expect an even higher profitability growth differential when directly comparing the two groups. Conversely, if rival profits grow slower and merging firm profits grow faster the direct comparison should reveal a more pronounced negative impact on rivals. If both groups are affected in a similar fashion, the direct comparison should partially or completely conceal the effect. The same logic applies to sales growth.

The effect on the growth of sales is estimated in almost the same setting with one difference: while the consolidation effect after a merger is accounted for by construction in the sample of consolidated firms, we have to correct for it in the sample including all acquirers, but where we lack the data on targets. Thus instead of calculating the growth of sales with respect to the $t - 1$ period, we calculate it with respect to the $t + 1$ period and report the δ_{ij} in periods two through five.¹⁷

4 Results

First we provide some preliminary findings by graphing the mean profitability and sales of merging firms and rivals over the post-merger period, normalizing the $t - 1$ values to one (figure 4). The average profitability of rivals initially decreases, but then increases to a significantly higher level: the rivals of merging firms are 25 - 45% more profitable in periods $t + 3$ to $t + 5$ than in the periods before. Contrarily, the profitability of the merging firms is lower after the merger than before. Consolidated entities are, on average, 14% less profitable in the post-merger period. They seem, however, to be converging back to their old level of profitability towards the end of the observation period. Nevertheless, by then their rivals are significantly more profitable than they are. The second panel of figure 4 shows that the sales of consolidated firms increase by an average of 4.5% by period $t + 5$, while those of their rivals increase by 27%, indicating an increase in rival market shares at the expense of the merging firms.

Figure 4 here

Table 6 contains the regression results when estimating the post-merger sales growth of merging firms and rivals using robust regression techniques. When comparing the sales growth of merger rivals to the control group, we find no statistically significant difference in the first two years after the merger. Starting in $t + 3$ in both samples, merger rivals significantly outgrow their control group: their cumulative sales growth exceeds that of the control group by 16 and 7 percentage points respectively in $t + 5$, indicating a sizeable expansion of output during the observation period.

Contrarily, the sales growth of merging firms is lower than that of the control group in the consolidated firms sample during most of the observation period. While the coefficients are statistically significant in periods $t + 1$ and $t + 2$ only, their size suggests economic significance of the effect until four years after the merger. In the $t + 5$ period the growth differential to the control group is closed. In the acquirer sample, all coefficients are insignificant.

Finally, when directly comparing the evolution of sales growth of merging firms and their rivals we find an unambiguous situation: rivals significantly outgrow the merging firms in the years after the acquisition. Given the positive growth dynamics of rivals and the growth slump of merging firms found in the previous regressions, this is hardly surprising. In both samples, the effect

¹⁷Neglecting this would lead us to believe that the merging firms strongly expanded their output in comparison to the control group and their rivals, while all we measure is just a consolidation effect.

monotonically increases over the observation period and culminates in a growth differential of 8 percentage points in the acquirer sample and of 19 percentage points when using consolidated firms.

Table 6 here

The robust regression results for profitability are contained in table 7. The profitabilities of rivals and their controls are matched in $t - 1$ and show no substantial discrepancy until $t + 3$. Starting in $t + 4$, the profitability of rivals exceeds that of the control group. At the end of the observation period the change in rival profitability points to an economically and statistically significant spillover effect of about one percentage point in both samples.

Merging firms appear to be on average less profitable than the control group in the post-merger period. In the consolidated firms sample, the merging firms are less profitable in $t + 1$ through $t + 4$ by 0.5 - 1.1 percentage points, but the effect is only statistically significant in $t + 1$ and $t + 2$. In the sample containing only acquirers the reduction of profitability is of similar size and significant in periods $t + 1$ through $t + 4$.

The direct comparison of rivals and merging firms reveals significant effects on profitability ranging between 1.3 and 1.6 percentage points in periods $t + 4$ and $t + 5$ in the sample of consolidated firms and between 0.7 and 1.9 percentage points in all post-merger periods in the acquirer sample. In this sample, SUTVA is violated and we perform a contaminated comparison. The fact that the negative effects on the merging firms reinforce the positive effects on rivals indicates that the results are not driven by the choice of control group.

Table 7 here

Thus the oligopolistic markets contained in the sample seem to support the first hypothesis derived from standard IO models: spillover effects due to large horizontal mergers on average exert a positive externality on the profits and market shares of rival firms, which become more profitable in both relative and absolute terms after the deal.

We now test hypotheses two and three by evaluating spillover effects in the four subsamples of markets with a low/high number of oligopolists and markets with low/high initial profitabilities. In the former case, the Cournot model predicts that spillovers on the profitability of rival firms are higher with fewer and lower with more competing firms, while in the latter case the opposite is true: if the initial profitabilities are low, spillovers should be low; if they are high, so should the spillovers be. The median number of competitors in a market in the sample is seven; we thus define 'Low n ' markets as those with less than seven rivals, while 'High n ' markets have seven or more rivals. Similarly, the median profitability of acquirers prior to the deal is 3.7%. We use this level to distinguish deals in 'Low Π ' industries from those in 'High Π ' industries. Table 8 reports the robust regression results in the four subsamples.¹⁸

Table 8 here

Columns 1 and 2 of table 8 distinguish markets with few and many competing firms. While in 'Low n ' industries the spillover on the profitability of rivals is significant in all five subsequent periods, the effect in 'High n ' industries is insignificant in four out of five periods and noticeably

¹⁸Table 8 reports results obtained in the (larger) sample of acquiring firms, because further subdividing the - already small - sample of consolidated firms leaves us with few observations. However, all results reported below can be qualitatively replicated in the consolidated firms sample, with some reduced significances.

smaller: 'Low n ' industries have higher and more significant spillovers in every period, lending empirical support to hypothesis 2, stating that the size of the spillover and the number of competitors are negatively related.

Columns 3 and 4 of table 8 differentiate between markets with low and high initial profitabilities. In the 'Low Π ' sectors, no significant effects on the profitability of rivals are found. In 'High Π ' sectors, spillovers are higher in all periods and significant in three periods. This is consistent with hypothesis 3, the prediction that oligopolies with higher initial profits allow for larger spillover effects.

5 Conclusion

This article has attempted to empirically quantify the changes in firm performance brought about by a change in the structure of an oligopolistic market. The changes in market structure in our sample were due to horizontal mergers in the sector, i.e. a transition from an n player oligopoly to an $n - 1$ player oligopoly. These mergers were reviewed by the European Commission, which identified the relevant competitors and the geographic extent of the relevant market. We exploit this information by confining the measurement of the spillover to the identified competitors, which are directly affected by the merger, thus avoiding the bias towards zero incurred by including irrelevant firms. Furthermore, we use the geographic market definition as a delineation criterion for the choice of the control group.

Standard oligopoly models (i.e. Cournot competition or Bertrand competition with differentiated goods) predict that mergers mostly benefit the non-merging parties: while the merging firms reduce their combined output to raise the market price, competitors gain market shares. In the new equilibrium, a higher market price prevails, resulting in higher profits for competitors. With quantity competition the merged entity will - except for very specific circumstances (Fauli-Oller, 1997) - earn less than before the merger. A profit-maximizing firm in such a market therefore has no incentive to merge, a fact that - when contrasted with the factual frequency of mergers - is often called the merger paradox.

Our results are consistent with these theoretical predictions: we find that rivals (non-merging firms that are active in the same product market) gain market shares and become significantly more profitable after an acquisition in the industry. The sales and profits of the merging parties, on the other hand, at best stagnate during the post-merger evaluation period. The finding that rivals significantly profit from the reduction of players in the market suggests that the anti-competitive externality due to reduced competition in general outweighs the pro-competitive externality due to efficiency gains. We also derive predictions on the relationship of the size of the spillover and the parameters of industry structure. Again we find theory corroborated by empirics: spillovers on rivals are larger in industries, in which the EC identified few competitors and larger if initial profitabilities are high.

From the point of view of economic theory our results can be regarded as a piece of evidence that the predictions of standard IO models appear to describe the mechanics of real-world oligopoly markets very well. Alternatively, from the point of view of effective competition the mergers in the dataset appear to have been to the detriment of the consumers in the respective markets.

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Appendixes

A Worldwide mergers

Table 9 contains the regression results when the acquirer sample is extended to include worldwide mergers.¹⁹ We now observe 159 acquiring firms and 466 competitors, increasing total sample size by about a quarter. We find weaker effects and significances in spite of the increased number of observations.

Table 9 here

B All rivals

Since in our main specification we incur a possible selection bias by only evaluating the performance of rivals in cases for which we have data on the merging firms, we run a robustness check in a sample including as many rivals as possible: we retain all rival observations from all merger cases including those for which we lack data on the merging firms. This boosts the number of rivals included in the sample to 506, increasing the sample size by almost fifty percent.

Table 10 shows that while we observe a weaker effect on profitability and a stronger effect on sales than in the acquirer sample, the qualitative findings are comparable.

Table 10 here

¹⁹Extending the sample of consolidated firms to include worldwide mergers only slightly increases the sample size (+10 consolidated entities, +19 rivals) and does not strongly affect the results.

Tables & figures

Table 1: c_M such that $\Delta\Pi_i = 0$

	$a = 2$	$a = 4$	$a = 10$
$n = 3$	0.75	0.25	0
$n = 5$	0.83	0.50	0
$n = 10$	0.91	0.73	0.18

Table 2: $\Delta\Pi_i$ if $c_M = c = 1$

	$a = 2$	$a = 4$	$a = 10$
$n = 3$	0.049	0.438	3.938
$n = 5$	0.012	0.110	0.990
$n = 10$	0.002	0.016	0.141

Table 3: Propensity score estimation

	Competitors		Merging firms	
Profitability	-0.055	(0.202)	1.270	(1.082)
Sales	0.031	(0.047)	0.113	(0.095)
Employees	0.170***	(0.037)	0.230***	(0.073)
Depreciation	0.107***	(0.039)	0.173**	(0.069)
Tobin's Q	0.043***	(0.016)	-0.066	(0.074)
Observations	354937		354937	
Pseudo R^2	0.260		0.383	
Industry dummies	yes		yes	
Treated	174		59	

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

Table 4: Standardized biases before and after matching

	Initial Bias (%)	Remaining Bias (%)
Competitors		
Profitability	35.64***	0.26
Sales	446.38***	0.60
Employees	594.00***	2.72
Depreciation	607.73***	0.61
Tobin's Q	60.05***	11.72
Merging firms		
Profitability	38.36***	2.21
Sales	905.31***	1.08
Employees	1630.70***	8.41
Depreciation	1336.38***	0.68
Tobin's Q	39.66***	1.15

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

Table 5: Summary statistics

Competitors	$t - 1$	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$
Profitability	0.040	0.035	0.029	0.048	0.057	0.054
ln(Sales)	15.787	15.923	15.912	15.898	15.933	16.073
ln(Total Assets)	15.963	16.122	16.129	16.090	16.146	16.307
ln(Employees)	10.138	10.207	10.193	10.167	10.167	10.317
Tobin's Q	1.343	1.197	1.219	1.181	1.163	1.142
N	174	174	174	172	148	134
Control group						
Profitability	0.039	0.038	0.048	0.047	0.039	0.053
ln(Sales)	15.770	15.919	15.912	15.958	15.920	16.045
ln(Total Assets)	15.908	16.028	16.060	16.141	16.140	16.216
ln(Employees)	10.191	10.259	10.293	10.353	10.188	10.226
Tobin's Q	1.640	1.286	1.183	1.101	1.005	1.034
N	174	174	174	172	143	127
Consolidated firms						
Profitability	0.047	0.039	0.029	0.045	0.045	0.051
ln(Sales)	17.037	17.115	17.118	17.183	17.002	17.051
ln(Total Assets)	17.365	17.534	17.557	17.709	17.465	17.552
ln(Employees)	11.274	11.235	11.222	11.327	11.225	11.246
Tobin's Q	1.025	0.876	0.854	0.704	0.727	1.183
N	59	59	59	59	52	48
Control group						
Profitability	0.040	0.051	0.043	0.051	0.050	0.059
ln(Sales)	17.010	17.147	17.139	17.127	17.072	16.990
ln(Total Assets)	17.308	17.427	17.436	17.279	17.209	17.345
ln(Employees)	11.415	11.361	11.358	11.268	11.243	11.226
Tobin's Q	1.039	1.037	1.018	0.919	0.924	0.669
N	59	59	59	59	50	47

Table 6: Sales

Consolidated firms sample						
	Rivals and controls		Merging and controls		Rivals and merging	
t+1	-0.017	(0.029)	-0.109**	(0.045)	0.034	(0.034)
t+2	-0.003	(0.029)	-0.109**	(0.045)	0.055	(0.034)
t+3	0.062**	(0.031)	-0.046	(0.049)	0.103***	(0.035)
t+4	0.119***	(0.033)	-0.073	(0.051)	0.154***	(0.036)
t+5	0.163***	(0.034)	0.000	(0.054)	0.190***	(0.038)
Obs.	1388		461		913	
Acquirer-only sample						
	Rivals and controls		Merging and controls		Rivals and merging	
t+1	-0.015	(0.020)				
t+2	-0.001	(0.020)	-0.019	(0.022)	-0.017	(0.017)
t+3	0.043**	(0.021)	-0.007	(0.023)	0.025	(0.018)
t+4	0.057**	(0.022)	-0.013	(0.024)	0.045**	(0.019)
t+5	0.067***	(0.024)	0.011	(0.026)	0.076***	(0.020)
Obs.	2771		823		1403	

Table 7: Profitability

Consolidated firms sample						
	Rivals and controls		Merging and controls		Rivals and merging	
t+1	-0.004	(0.004)	-0.011**	(0.006)	0.001	(0.005)
t+2	-0.005	(0.004)	-0.011*	(0.006)	0.002	(0.005)
t+3	0.003	(0.004)	-0.005	(0.006)	0.008	(0.005)
t+4	0.010**	(0.005)	-0.008	(0.006)	0.016***	(0.005)
t+5	0.009*	(0.005)	0.003	(0.007)	0.013**	(0.005)
Obs.	1380		462		908	
Acquirer-only sample						
	Rivals and controls		Merging and controls		Rivals and merging	
t+1	-0.000	(0.003)	-0.009**	(0.004)	0.009***	(0.003)
t+2	-0.002	(0.003)	-0.009**	(0.004)	0.007*	(0.003)
t+3	0.003	(0.003)	-0.009**	(0.005)	0.013***	(0.004)
t+4	0.002	(0.003)	-0.010**	(0.005)	0.012***	(0.004)
t+5	0.008**	(0.003)	-0.007	(0.005)	0.019***	(0.004)
Obs.	2748		1068		1826	

Table 8: Spillovers on rivals and market structure

	Low n		High n		Low Π		High Π	
t+1	0.007*	(0.004)	0.002	(0.004)	-0.006	(0.004)	0.006*	(0.003)
t+2	0.008*	(0.004)	-0.003	(0.004)	-0.006	(0.004)	0.003	(0.004)
t+3	0.012***	(0.004)	0.004	(0.004)	-0.003	(0.004)	0.010***	(0.004)
t+4	0.012***	(0.004)	0.003	(0.004)	-0.000	(0.004)	0.005	(0.004)
t+5	0.016***	(0.004)	0.009*	(0.005)	0.006	(0.005)	0.013***	(0.005)
Obs.	1332		1631		1257		1743	

Table 9: Results including worldwide relevant markets

Profitability						
	Rivals and controls		Merging and controls		Rivals and merging	
t+1	-0.003	(0.002)	-0.007*	(0.004)	0.008**	(0.003)
t+2	-0.005**	(0.002)	-0.007*	(0.004)	0.005	(0.003)
t+3	-0.002	(0.003)	-0.008*	(0.004)	0.008***	(0.003)
t+4	0.000	(0.003)	-0.009**	(0.004)	0.011***	(0.003)
t+5	0.004	(0.003)	-0.006	(0.005)	0.015***	(0.004)
Obs.	3540		1292		2297	
Sales						
	Rivals and controls		Merging and controls		Rivals and merging	
t+1	-0.013	(0.019)				
t+2	-0.001	(0.019)	-0.019	(0.020)	-0.060***	(0.022)
t+3	0.038*	(0.020)	-0.010	(0.021)	-0.025	(0.023)
t+4	0.056***	(0.022)	0.010	(0.022)	0.001	(0.025)
t+5	0.105***	(0.024)	0.035	(0.024)	0.042	(0.027)
Obs.	3560		995		2310	

Table 10: All rival firms

	Rival profitability		Rival sales	
t+1	-0.001	(0.002)	0.001	(0.018)
t+2	-0.001	(0.002)	0.018	(0.018)
t+3	0.002	(0.002)	0.059***	(0.019)
t+4	0.002	(0.003)	0.072***	(0.020)
t+5	0.006**	(0.003)	0.111***	(0.022)
Obs.	4026		4059	

Figure 1: Standardized biases before and after matching

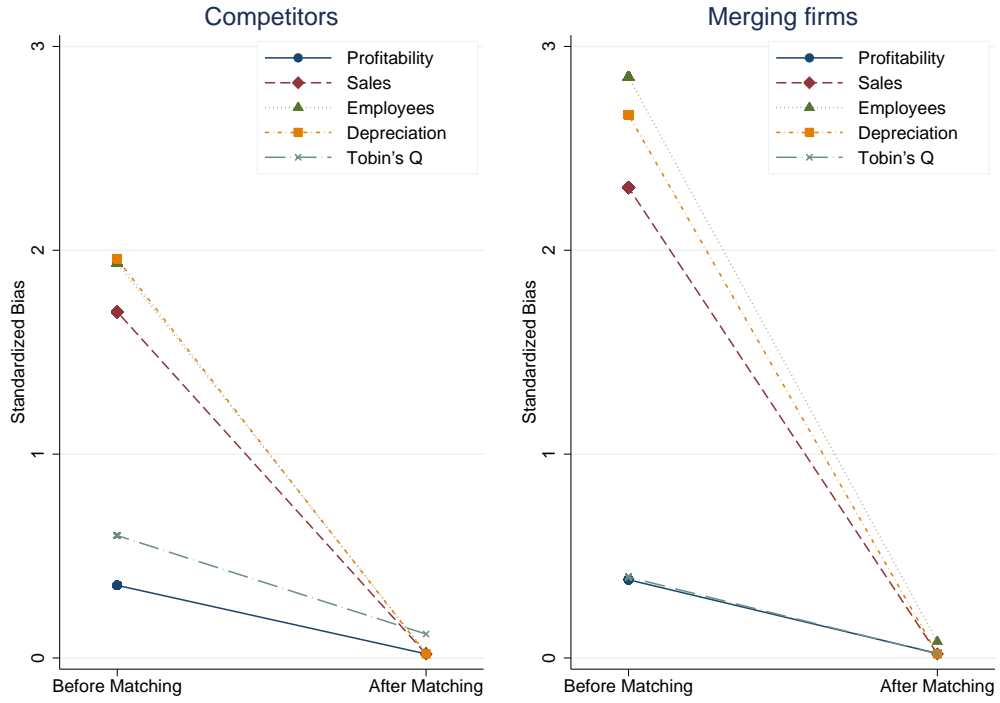


Figure 2: Yearly average change in profitability and sales

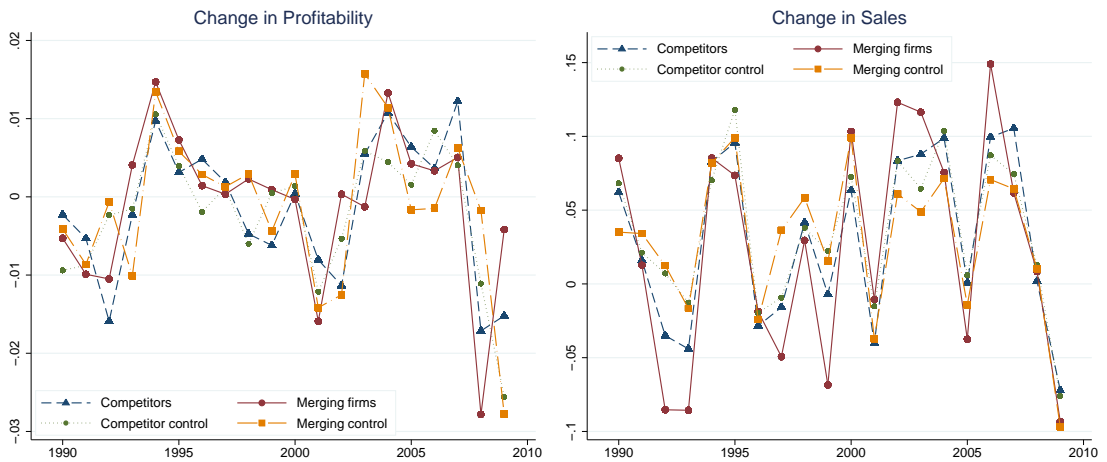


Figure 3: Evolution of profitability and sales

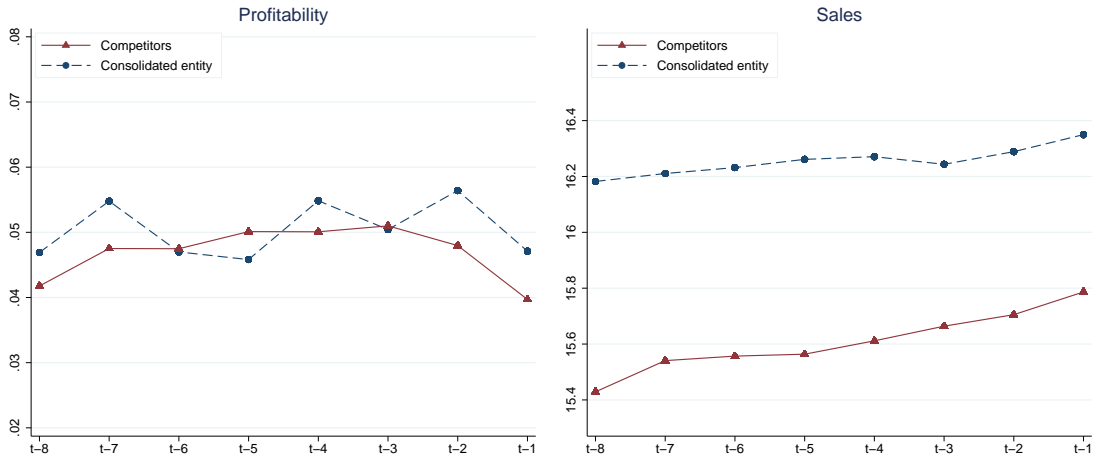


Figure 4: Evolution of profitability and sales in the post-merger period

