M&A and R&D: Asymmetric Effects on Acquirers and Targets?*

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April 2011

We evaluate the impact of M&A activity on the growth of R&D spending and R&D intensity of 265 acquiring firms and 133 merger targets in the time period ranging from 1990 to 2009. We use a range of matching techniques to construct separate control groups for acquirers and targets and use appropriate difference-in-difference estimation methods to single out the causal effect of mergers on R&D growth and intensity. We find a significant reduction of R&D efforts by both acquirers and targets in the periods after the merger, pointing to a decrease of the incentive to innovate. Thus the mergers in this sample seem to have been undesirable from the point of view of innovation.

1 Introduction

The present paper continues to investigate the nexus between corporate mergers and the incentive of firms to allocate resources to innovation activities and hopes to overcome some of the shortcomings of previous efforts on the same issue. This paper's main contribution is probably the explicit differentiation of effects on acquirers and targets. Previous studies have included both acquiring firms and merger targets in their analysis (Cassiman et al. (2005), Ornaghi (2009)), but effects were measured in a pooled setting, due to either small sample sizes or the inability to differentiate the correct roles.

A common point of critique in studies investigating treatment effects in a non-experimental setting is whether a sufficiently similar control group can be obtained. This

*I would like to thank Klaus Gugler for valuable comments on an earlier draft of this paper. The author gratefully acknowledges financial support from Österreichische Nationalbank through Jubiläumsfonds project 14075.

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is important due to concerns about endogeneity and self-selection into the treatment group. We address this issue by using three different matching techniques (nearest-neighbor matching, Mahalanobis metric matching, caliper matching) and a very rich pool of potential control observations. In each case, separate control groups are constructed for acquirers and targets to account for firm heterogeneity due to their roles in the transaction. Estimation results are reported in all three samples thus obtained.

Furthermore, earlier studies on the subject matter where usually either of limited geographical scope (Bertrand (2009), Stiebale & Reize (2011)) or restricted to certain industries (Bertrand & Zitouna (2008), Ornaghi (2009)), the database utilized in this study contains firms from most major industrialized nations, active in numerous different industries. Thus we hope to overcome any industry or country-specific effects and provide a surveying picture of the phenomena in question.

Restructuring R&D activities is a protracted affair that can take a number of years to complete. Therefore the explanatory power of short-term studies on the topic is limited. To account for the relevant time horizon, we use balance sheet data from up to 6 periods after the acquisition year. A time window of up to [t+1, t+6] years after the acquisition year t allows us to check for drawn-out restructuring efforts after it. While we use pre-merger data (period t-1) in the estimation of the ex-ante probability to merge, data from the merger period t is excluded from the analysis to avoid measuring consolidation effects of the combination.

The goal of this paper is to contribute to the empirical discussion on the relationship between mergers and the *incentive* to conduct innovative efforts. We therefore analyze the effect of mergers on two measures of R&D inputs: the growth of R&D expenditures and R&D intensity, defined as the ratio of R&D expenditures over sales. By making R&D inputs instead of R&D outputs (patents, new products) the focus of the analysis, we examine the firms' willingness to invest in innovation instead of their success in attaining it. Thus, questions about synergies and changes in the efficiency of research are not addressed by this paper. However, Hagedoorn & Cloodt (2003) show that measures of R&D inputs and outputs are highly correlated and conclude that there is no major systemic disparity between them.

A much-discussed issue in the evaluation of non-experimental data concerns the issues of missing data and self-selection. The basic problem is that, in a non-experimental setting, self-selection into the 'treated' group cannot be ruled out and thus receiving the treatment might be non-random, confounding the measurement of the causal effect of treatment. Therefore great care has to be taken in the construction of an appropriate control group as well as in the specification of the empirical strategy to derive reliable results. In this respect, we follow the suggestion of Blundell & Costa Dias (2000) and combine matching techniques with difference-in-difference estimation.

When estimating the ex-ante probability to be involved in a merger, we find similar determinants for acquirers and targets: high values of R&D intensity, total assets and employees increase both the probability of being an acquirer or a target. The firms' profitability, on the other hand, raises the probability of being an acquirer and decreases that of being a target: acquirers are significantly more and targets are significantly less profitable than the average firm in the sample.

In the early periods after the merger, acquirers do not differ significantly from the control group in terms of R&D growth. We find some negative growth effects from $t = 3$ to $t = 5$, though only the effect in $t = 5$ is significant in all specifications. The R&D growth of merger targets, conversely, drops sharply relative to the control group in all periods after the acquisition $t = 1$ to $t = 5$ and all specifications.

The effects on R&D intensity are negative as well: while both groups start out at very high levels of R&D intensity (the average pre-merger R&D intensity of acquirers is almost 8%, that of targets is close to 13%) this changes significantly after the acquisition. We measure highly significant negative effects on acquirers in all periods ($t = 2$ to $t = 6$) and all specifications suggesting a monotonous reduction of R&D intensity amounting to almost 5% six periods after the acquisition. The effects on merger targets manifest later than those on acquirers: significant negative effects are found in periods $t = 4$ through $t = 6$. The coefficients suggest an average reduction in the R&D intensity of merger targets by 6-7%.

These observations are consistent with the interpretation that merging firms are very innovative prior to the merger, but that in the post-merger period the incentive to invest in innovation is substantially decreased. This points to a reduction in competitive pressure achieved by the merger, either through the elimination of an innovative competitor or through the advance of the acquirer's technological portfolio granting an advantage over competitors. In either way, the M&A activity in this sample has, on average, entailed a significant reduction of the innovative efforts of the parties involved.

2 Literature

The literature on the effects of mergers on innovation is a large and fast-growing field, since it receives a lot of attention from both economics and management scholars. To keep this section concise, we will focus on rather recent contributions in the economics tradition, thereby neglecting earlier studies and corporate governance considerations.

An article closely related to this one is the study by Ornaghi (2009), which analyzes the effect of 27 mergers in the pharmaceutical industry on various measures of R&D inputs and outputs. A combination of propensity score matching and difference-in-difference estimation and, alternatively, a measure of technological relatedness is used to address issues of endogeneity. When estimating the effects on acquirers and targets in a pooled setting, Ornaghi finds a decrease in innovative efforts after mergers. Stiebale & Reize (2011) report similar findings from a sample of 304 German merger targets and explicitly control for structural zeros in reported R&D values (see section 3.4 and Kleinknecht (1987)).

Desyllas & Hughes (2010) analyze a sample of 2624 acquirers in high-tech industries using a similar empirical strategy. They find that the R&D intensity of an acquiring firm decreases in the period after a merger ($t = 1$) but increases again in the $t = 3$-period. R&D productivity is not significantly affected. They also find evidence in favour of the view, that mergers between technologically-related firms perform better than mergers between firms that differ greatly with resepect to their knowledge bases. This argument is also advanced by Cassiman et al. (2005), who distinguish between technological and market-relatedness and use a detailed sample of 31 mergers. Contrariwise to Desyllas & Hughes (2010), they find that technologically complementary (substitutive) firms increase (decrease) their R&D level after the acquisition. Moreover, effects on R&D efficiency are more advantageous in complementary mergers.

Studies that find increases in R&D activity after mergers include Bertrand (2009) and Stiebale (2010). Using a sample of 123 French acquisition targets in crossborder mergers and a combination of propensity score and difference-in-difference methods, Bertrand (2009) finds that R&D budgets have significantly increased three years after the acquisition. Stiebale (2010) focuses on acquirers (324 firms) and finds that their R&D intensity significantly increases after the merger.

As can be seem from this brief overview, statistical studies on the effect of mergers on R&D efforts either find a positive, negative or ambiguous relationship. Other studies (e.g. Hall (1991), Bertrand & Zuniga (2006)) find mostly insignificant effects. Therefore, no clear-cut empirical conclusions have emerged so far. Still, most reviews of the literature (an excellent survey is provided by Veugelers (2006)) conclude in favour of a weak, negative relationship between R&D and M&A.

3 Data & Empirical Strategy

The dataset used in this study was created by joining datasets of mergers that were notified to either the European Commission (EC) or the Federal Trade Commission (FTC) between 1990 and 2009. These cases were reported to the respective regulatory authority by companies from 25 different nations[1] and many different product markets[2] and were either cleared or subjected to remedies by the authorities. The only common factor in all of these mergers is that they were significant enough to meet the notification thresholds of the EC or FTC.[3] Thus the sample does not include minor asset acquisitions, which entail no significant effect on companies, but major transactions resulting in significant corporate restructuring under the scrutiny of one of the two most important antitrust jurisdictions. Some of the firms in the sample merge more than once during the observation period; to ensure that the effects of multiple mergers do not confound the results, we drop observations where not at least 4 consecutive years lie between the acquisitions.

We combine this dataset of mergers with balance-sheet data containing the R&D expenditures of the merging parties and other relevant variables. After dropping all observations, for which R&D expenditures data was not available in a time window of $[t-1, t+1]$ around the merger, we are left with 398 firms (265 acquirers and 133 merger targets) for which we have full R&D data.[4] When checking for the completeness of R&D data, all observations reporting missing R&D values were dropped. We retained companies reporting zero R&D expenditures.

This sample of merging firms was then complemented with a very large sample of potential controls, from which the relevant control groups are constructed. Since the set of potential controls is more than 50 times larger than the set of merging firms, we are confident that a sufficiently close match can be found for each treated observation. For each of these firms we downloaded time series of balance sheet data on total assets, income, total sales, number of employees and R&D expenditures from the Thomson Reuters Worldscope database. After converting all values to USD and calculating the growth rate of R&D expenditures (defined as the percentage change in R&D expenditures between two consecutive periods) as well as R&D intensities (the ratio of R&D expenditures to total sales)[5] and profitability (the ration of net income and total assets) for all firms in all

[1] Most of the firms involved in a merger have their headquarters in the US, followed by Germany, France and the UK.

[2] 38 different 2-digit SIC codes are represented in the sample. The biggest single sector is SIC 28 ('Chemicals and allied products'), which includes a quarter of all observations.

[3] A merger has to be notified to the FTC if the deal-value exceeds 60 million USD (as of 2010). The EC uses a combined criterion of at least 5,000 million Euro worldwide turnover and at least 250 million Euro community-wide turnover, subject to further qualifications.

[4] Notice that acquirers are overweighted in the sample. This is due to the fact that post-merger data on targets is only available if the company continues to exist after the acquisition.

[5] In some cases, R&D intensities in excess of one were found, suggesting higher R&D expenditures than sales. Since these values are not implausible per se (most of them are found in high-tech sectors like pharmaceuticals or biotechnology) they were kept in the sample. To prevent any bias in the estimation coefficients due to outliers, R&D intensity values were capped at 1.5. All results are qualitatively robust to dropping these observations.

periods, we logarithmize all variables.[6]

A first look at the resulting dataset confirms that the mergers scrutinized by the FTC and the EC are indeed significant in terms of size: the average merging firm spends over 20 times more on R&D, has over 15 times more total assets and over 10 times more employees than the average firm in the dataset. Even when controlling for size effects by comparing R&D intensities, merging firms exhibit significantly higher values. It thus appears that the average firm involved in a merger, which is being scrutinized by an important competition authority, is quite different from the average firm listed on any stock market in the industrialized world. In consequence, when we want to infer the effect of merging activity on innovation efforts, not any kind of non-merging comparison group will do.

3.1 Propensity-score matching: missing data and self-selection

Studies estimating the causal effect of a treatment on a group of firms or persons receiving said treatment face the fundamental problem of not knowing, what would have happened in absence of the treatment. This is often called the problem of the missing counterfactual. If we denote (following Rosenbaum & Rubin (1983)) the outcome of observation unit i receiving treatment by r_i^1 and the outcome in absence of treatment by r_i^0, the individual treatment effect is given by

$$\Delta_i = r_i^1 - r_i^0. \tag{1}$$

Since in reality only one of the possible outcomes is observed, we are confronted with a missing data problem in estimating the individual treatment effect. Experimental studies overcome this hurdle by randomly assigning one group of observations to treatment - the treatment group -, while another group of observations does not receive treatment, the control group. The difference in outcome between the two groups can then be attributed to the effect of the treatment and is called the average treatment effect (ATE):

$$\text{ATE}_{\text{exp}} = E(r_i^1 - r_i^0). \tag{2}$$

Non-experimental studies face the additional difficulty that an appropriate control group is often hard to come by. Since the decision to receive treatment is not randomly determined by an experimenter, but - in the case of mergers - decided by the management of the firms, the assignment to treated or control group cannot plausibly be assumed to be random. Therefore, in addition to the missing data problem, one also faces a problem of endogeneity or self-selection, suggesting that the decision to receive treatment is caused

[6] We add one to all values of zero (e.g. the R&D expenditures of non-innovative firms) before taking the logarithm.

by certain firm-specific characteristics which in turn could also influence the effect of the treatment. Not recognizing this complication could cause a systematic bias in the estimated coefficients, since effects attributed to the treatment might actually be due to other factors.

For example, as mentioned above, merging firms in this sample are much larger than the average firm; not taking this fact into account might lead us to attribute certain effects to the merger, while they actually could be a consequence of the size of the firm. It is therefore necessary to construct a control group, that has the same pre-treatment characteristics and thus the same ex-ante probability of receiving treatment (i.e. being involved in a merger as acquirer or target) as the group of merging firms. In non-experimental studies, the ATE needs to be calculated conditionally on the treated and control observations not being systematically different with respect to a vector of characteristics, c_i:

$$\text{ATE}_{\text{nonexp}} = E(r_i^1 - r_i^0 | c_i) = E(r_i^1 | c_i) - E(r_i^0 | c_i). \tag{3}$$

We thus need to artificially construct a sample, in which the decision to engage in a merger is not driven by certain firm characteristics and hence, to the largest extent possible, random. If successful, this both yields an appropriate control group for the estimation of the average treatment effect and eliminates the problem of self-selection.

3.2 Propensity-score matching: matching algorithms

The usual approach in the literature to account for the missing data and self-selection problems is to construct a control group using propensity score matching (PSM).[7] The propensity score (Rosenbaum & Rubin (1983, 1985)) predicts the probability of receiving treatment based on observable characteristics using maximum likelihood estimation. By matching treated observations to control observations based on their propensity scores one obtains two groups that do not differ systematically with respect to the observable characteristics the propensity score was calculated upon (see Rosenbaum & Rubin (1983) for the proof). PSM thus controls for the observable heterogeneity between treated and control observations.

We follow this approach by creating separate control groups for acquirers and targets using three different matching algorithms: nearest-neighbor matching within the same year, Mahalanobis metric matching within same year and 2-digit industry code as well as (global) caliper matching. The propensity scores are calculated using pre-merger (t-1)

[7]Other options would be to follow an instrumental variable approach or to formulate an equation describing selection into the treatment group and estimating it jointly with the average treatment effect by using maximum likelihood methods.

data to ensure that the merger effect does not influence the matching. Each matching method faces a trade-off between variance of the estimates (depending on the size of the control group) and bias (depending on the similariness of the control group to the treated group, i.e. the quality of the matches).[8] The following paragraphs briefly describe the advantages and disadvantage of the three methods employed with regard to this trade-off.

3.2.1 Nearest-neighbor matching

Nearest-neighbor matching is probably the most intuitive matching algorithm we use and balances the trade-off between bias and variance: each merging firm is matched to exactly one non-merging firm within the same year. The match is thus the firm which is most similar to the merging firm based on the matching covariates in the year before the merger. Since every control is selected only once (matching without replacement), this yields a control group of the same size as the treated group. Matching within the same year ensures that both the treatment and the corresponding control observation refer to the same time window.

Thus the nearest-neighbor matching algorithm compromises with respect to the trade-off described above: having exactly one control for every treated observation ensures that the control group is not too small (variance), restricting matching to subsamples with corresponding time entries ensures that controls are sufficiently comparable to treated observations (bias).

3.2.2 Mahalanobis metric matching

The Mahalanobis metric approach places more emphasis on the bias aspect than on the variance aspect: we require the control observation to be an exact match with respect to time and industry classification, dramatically reducing the number of available matches on the one hand, while increasing the appropriateness of the remaining matches on the other.[9] Since this makes the number of available matches scarce, we allow matching with replacement in this specification, i.e. the same control can be assigned to multiple treated observations.

This yields a control group that is smaller than the treated group (since matches can be recycled) and has a lower matching quality than the nearest-neighbor matching approach (since we require exact matching in two dimensions). On the other hand we know that all matches refer to the same timeframe and are within the same industry as the corresponding treatment observation.

[8] Caliendo & Kopeinig (2008) and Dehejia & Wahba (2002) discuss this trade-off and the merits of different matching approaches.

[9] We use 2-digit SIC codes to group industries. Since we observe mergers in 38 different industries over a timespan of 20 years, this divides the sample in 760 subsamples to match in.

3.2.3 Caliper matching

Matching to multiple controls within a caliper provides a larger control group than the two approaches described above, thus alleviating concerns about the variance of the estimates. Caliper matching is implemented by matching each treated observation to the three most similar control observations, given that none of them differ by more than 0.1% from the treated observation's propensity score. Matching is performed without regard to temporal or industry subsamples; thus matches potentially are selected from different industries and/or time periods. Picking multiple matches per treatment observation provides a larger sample size for estimation.

Caliper matching results in a larger control group (since there are up to three controls per treated observation) with good matching quality (since the matches are selected from the largest posssible pool). Conversely, matches are not pre-selected from appropriate categories as in the other two approaches.

3.3 Propensity-score matching: results

The covariates employed in the PSM algorithm are magnitudes that could potentially influence both the decision to merge and future R&D efforts, namely pre-merger R&D intensity and growth, as well as measures of pre-merger size and earnings (total assets, number of employees, profitability). The dependent variable in both regressions is a dummy, indicating if a firm was an acquirer / a target in the following period. Table 1 reports the estimated propensity scores[10] and shows that acquiring firms are, on average, significantly more R&D-intensive, have more assets and employees and higher profitability than their non-merging peers. R&D growth is not a significant determinant for being an acquirer. The target coefficients for R&D intensity, total assets and employees are similar to those of acquirers in terms of size and significance. While the coefficient of R&D growth is insigificant as well, there is a negative relationship between profitability and the probability of being a merger target: merger targets are, on average, significantly less profitable than other firms.

After matching the respective control groups using the methods described above, we check whether a balanced sample was obtained by testing for systematic differences with respect to the covariates among treated and control observations in all six control groups. Table 2 contains the results of the ttests with the null hypothesis of equal variable means of treated and untreated observations.

As can be seen from table 2, both the nearest-neighbor matching and the caliper

[10]The regression for merger targets is run in a slightly smaller sample, because we drop firms that were previously matched as a control for an acquirer. This ensures that there is no overlap between the two control groups.

Table 1: Propensity score

	Acquirers		Targets	
R&D Intensity	1.200***	(0.126)	0.818***	(0.141)
R&D Growth	−0.022	(0.054)	−0.124	(0.081)
Total Assets	0.215***	(0.027)	0.251***	(0.034)
Employees	0.164***	(0.029)	0.145***	(0.037)
Profitability	2.060***	(0.236)	−0.623***	(0.239)
Observations	75677		72683	
Mergers	265		133	

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

Table 2: Variable means after matching

	Acquirers			Targets		
Nearest-neighbor	Control	Treated	Difference	Control	Treated	Difference
R&D Intensity	0.08	0.08	-0.003	0.10	0.12	-0.014
R&D Growth	0.13	0.13	-0.002	0.13	0.10	0.025
Total Assets	15.93	15.83	0.099	15.69	15.69	-0.000
Employees	10.03	10.04	-0.017	9.70	9.77	-0.075
Profitability	0.06	0.07	-0.009	-0.02	-0.00	-0.017
Mahalanobis						
R&D Intensity	0.14	0.07	0.063**	0.08	0.12	-0.040
R&D Growth	0.13	0.13	-0.006	0.23	0.11	0.120
Total Assets	12.52	15.84	-3.320***	13.27	15.63	-2.357***
Employees	7.08	10.07	-2.982***	7.55	9.73	-2.173***
Profitability	-0.05	0.06	-0.110***	-0.01	-0.00	-0.010
Caliper						
R&D Intensity	0.07	0.07	-0.002	0.08	0.12	-0.044
R&D Growth	0.15	0.14	0.010	0.12	0.11	0.013
Total Assets	15.75	15.81	-0.062	15.14	15.31	-0.173
Employees	9.94	10.03	-0.094	9.26	9.51	-0.249
Profitability	0.06	0.06	-0.007	-0.01	-0.01	-0.004

* p < 0.1, ** p < 0.05, *** p < 0.01

matching algorithm succeed in purging all observable heterogenity between treatment and control group: the two groups do not differ significantly with respect to the five covariates employed in estimation of the propensity score.

The Mahalanobis metric approach on the other hand, constrained by the large number of subsamples matching occurs in, does not succeed in balancing the sample; significant differences remain with respect to most matching covariates. Keeping this in mind, we still believe the the Mahalanobis control group has some merits over the other control groups and retain it for further analysis. While it is always preferable to compare observations that are as similar as possible, a control group made up entirely from firms within the same industry classification (i.e. horizontal competitors) and referring to the same timeframe certainly is a valuable counterfactual even in absence of balanced means.

We therefore conclude that the algorithms were successful in purging the observable heterogeneity between merging firms and non-merging firms in two out of three cases and retain the third control group for different considerations.

3.4 Structural zeros

Another possible bias arises due to the issue of structural zeros in accounting data on R&D spending (this is addressed in Stiebale & Reize (2011)). Many firms report zero R&D expenditures because they pursue very little or no innovative efforts and are therefore usually excluded from analysis. Yet, by excluding them one incurs a possible bias due to the selection into the group of innovative firms: it cannot be ruled out, that the effect one analyzes works systematically different on innovative firms (R&D>0) than on non-innovative firms (R&D=0). To avoid any such bias, this sample includes both innovative and non-innovative firms: Almost 7% of merging firms in this sample report zero R&D expenditures in the merger period.

3.5 Difference-in-difference strategy

After having created the relevant control groups, we proceed to estimate the effects of mergers on the variables of interest in a difference-in-difference setting.

We construct time windows around the respective merger events and use observations of the merging firms and the relevant controls from $[t-3, t+6]$, where t designates the period in which the combination took place. By using a set of dummies indicating whether a firm was involved in a merger one year ago, two years ago and so on, we create a merger timeline, allowing us to track the effects on innovative efforts over the time window. In the R&D intensity regression, we include further dummies for all treated observations (separately for acquirers and targets, equal to one in all periods) to control for unobservable differences between the treated and control groups. We estimate the following model

$$\begin{aligned} \text{rdint}_{ij} &= \alpha + \beta \sum_{t=1}^{6} \text{acquirer}_{i,j-t} + \gamma \sum_{t=1}^{6} \text{target}_{i,j-t} + \delta \, \text{treat\_acq} \\ &+ \zeta \, \text{treat\_tar} + \eta \, \text{controls} + \varepsilon_{ik} \end{aligned} \tag{4}$$

The R&D intensity of firm i in year j is regressed on a set of merger dummies ranging from the year after the merger ($t = 1$) up to six years after the merger ($t = 6$) and indicating the role of the firm (acquirer or target), dummies for being an acquirer / a target and controls for industry and time effects.

In the R&D growth regression, the dependent variable is a growth rate and thus purges individual fixed effects. We therefore exclude the acquirer/target dummies from the regression.

$$\text{rdgrowth}_{ij} = \alpha + \beta \sum_{k=1}^{6} \text{acquirer}_{i,j-t} + \gamma \sum_{t=1}^{6} \text{target}_{i,j-t} + \eta \, \text{controls} + \varepsilon_{ij} \tag{5}$$

In both regressions we do not include a dummy in the merger period ($t = 0$) to avoid the measurement of consolidation effects. Indeed, the R&D growth of acquiring firms skyrockets in the year of the merger, indicating significant asset transfers from the target. R&D intensity of both acquirers and targets does not significantly differ from that of the control group in the periods up to and including the merger period.

Even though we construct separate control groups for acquirers and targets, we estimate results jointly in a pooled setting including both targets and acquirers as well as their respective control groups. The results when effects on acquirers and targets are estimated separately in the respective subsamples are very similar to those found in the pooled setting and are reported in the appendix.

4 Results

Figure 1 charts the mean growth of R&D spending by acquirers and targets around the merger. Prior to the merger (periods -2 and -1) both acquirers and targets exhibit strong R&D growth rates of between 10 and 14 percent. In the year of the merger, the R&D growth of acquirers jumps to almost 25% and then strongly declines in the periods after the acquisition, with a minimum of 2.5% growth 5 years after the merger. It thus appears that in the period of acquisition, substantial asset transfers from the target occur. Since this spike in R&D growth is a pure bookkeeping phenomenon (consolidation of R&D efforts) and not a causal effect of the merger, we exclude period $t = 0$ from estimation.

After this one-period spike, the incentive of acquirers to increase innovative assets seems to diminish.

The R&D growth of merger targets is high in the periods prior to the acquisition, but starts dropping immediately in the period of the merger. From $t = -1$ to $t = 2$, R&D growth declines monotonically from more than 10% to about 1%. After $t = 2$, R&D growth starts to increase again, without reaching its former level in the observation period. It thus seems, that the acquisition creates a slump in the target's R&D growth profile and that a substantial recovery period is needed to return to the former growth path.

Figure 1: R&D growth of acquirers and targets around the merger

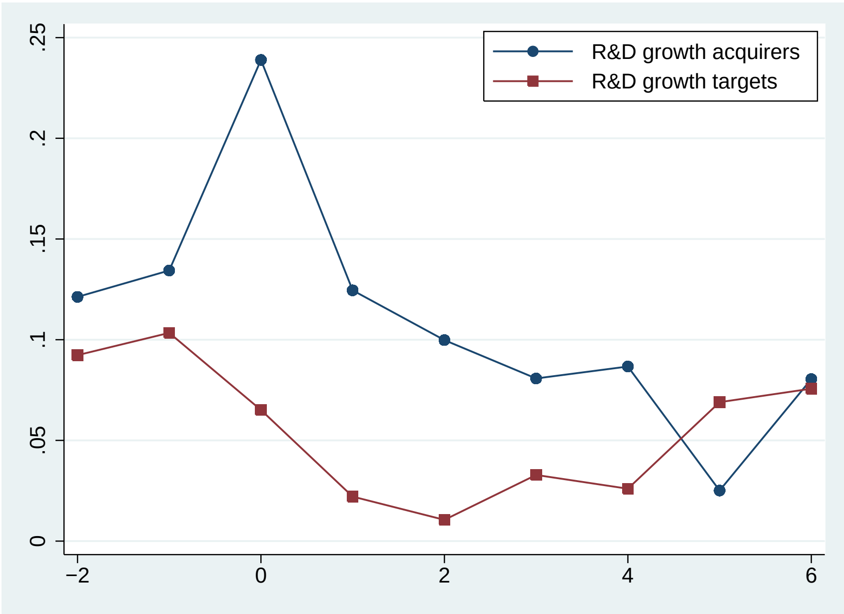
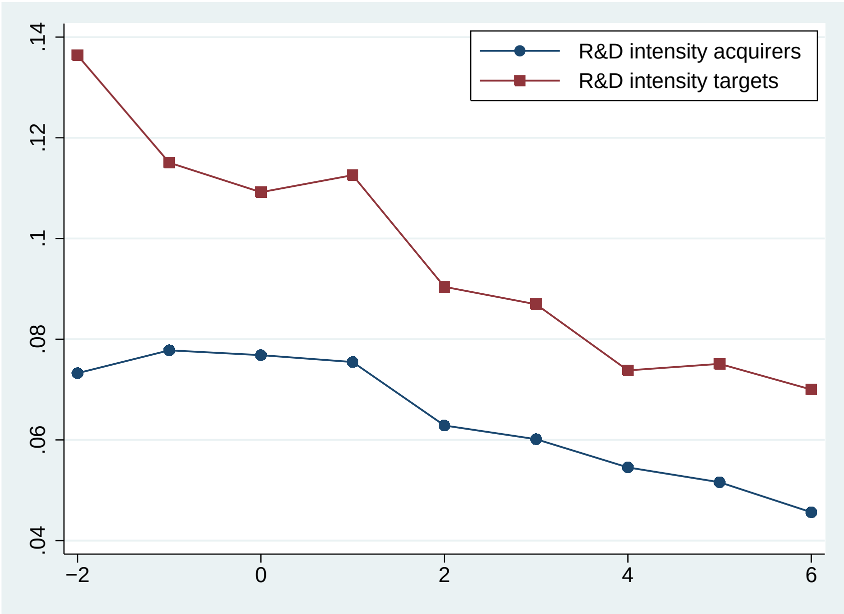


Figure 2 reports the R&D intensity (equal to total R&D spending divided by total sales) of acquirers and targets from two years before until six years after the merger. Prior to the merger, the R&D intensity of acquirers is relatively constant around a high level of 7 - 8%. Acquirers are, therefore, on average quite R&D-intensive firms. This remains unchanged in the period of the merger and the one after it. From $t = 1$ to $t = 6$ we observe a monotonous decline in the R&D intensity of acquirers, which drops from 7.5% to 4.6%. Thus, R&D intensity is reduced by more than a third on average in the five years after an acquisition is made. A similar, but even stronger pattern can be observed in the R&D intensity of merger targets: while starting out at an extremely high level of about 13%, the graph gradually decreases to 7% in the post-merger periods, suggesting a reduction in R&D intensity by almost 50%.

From figures 1 and 2 it appears that merger targets are chosen on the basis of being very innovative firms - they exhibit high R&D growth and extremely high R&D intensity -, but that their innovative efforts decrease substantially after the acquisition. A similar, but slightly weaker claim could be made for the buying firms. It thus appears that the incentive to invest in innovation is substantially reduced in post-merger periods.

Figure 2: R&D intensity of acquirers and targets around the merger



While these two figures suggest that certain changes in innovative behaviour occur around a merger, they contain only mean R&D growth and intensity of acquirers and targets, which do not permit inferences as to the significance or causality of the observed phenomena. To achieve this, we run regressions in a difference-in-difference setting (see section 3.5) within the relevant control group (see section 3.3). The dependent variables are R&D growth and intensity respectively. All regressions are reported in a (acquirers and targets) pooled setting[11] in the three different samples obtained by nearest-neighbor, Mahalanobis and caliper matching. All specifications include controls for industry and time effects (not reported). The R&D intensity regression includes two further dummies, which control for unobserved differences of acquirers / targets and the control group.[12]

[11] As mentioned before, all results qualitatively hold when estimating effects on targets and acquirers separately; see appendix.

[12] These dummies are not included in the R&D growth regression, since the growth rate specification purges time-constant unobserved fixed effects.

The results are reported in tables 3 and 4.

In table 3, the regression results for acquirers in the nearest-neighbor sample show no significant deviation from the control group in all periods except for $t = 5$, when acquirers experience significantly lower R&D growth. The same is found in the other two samples, which also show significantly negative effects in $t = 3$ and $t = 4$.

The growth effects on targets are much more clear-cut: in all periods from $t = 1$ to $t = 4$ and all three samples, merger targets experience lower R&D growth than their peers, significant at the 1% level. The significance of this result drops slightly in $t = 5$ and disappears in $t = 6$. Thus the R&D growth of merger targets is significantly lower than that of the control group for the five year period after the acquisition has occured.

The p-values reported at the bottom of the table test the null hypothesis that the sum of all acquirer (or target) timeline-dummy coefficients is not significantly smaller than zero. Since all of these hypotheses can be rejected at the 1% level (both for acquirers and targets), we conclude that the aggregate effect on R&D growth over the six periods following a combination is significantly negative for both acquirers and targets, but more so for targets.

Turning to the regression addressing R&D intensity, we find that the R&D intensity of acquirers is significantly affected by a merger: while the difference to the control group is insignificant in period $t = 1$ (and the periods prior to it), all coefficients are significantly negative from periods $t = 2$ until $t = 6$ in all three samples. The coefficients indicate a cumulative reduction of R&D intensity amounting to almost -5 percentage points in comparison to the control group in all three samples.

The effects on merger targets are qualitatively similar; although the reduction in R&D intensity seems to be even more pronounced, the standard errors of the coefficients are higher than those of the acquirers, pointing to a wider range of possible outcomes. In all three samples the timeline dummies are negative throughout and indicate significant deviations from the control group in $t = 2$ and $t = 4$ through $t = 6$.

In the pooled settings reported here, the dummies for being a target firm are significantly positive in all three samples to offset the generally lower level of R&D intensity among acquirers and their control group. In the Mahalanobis sample, this is achieved by a combination of a positive target and a negative acquirer dummy.

Similarly to the R&D growth regressions, we report the p-values of the hypothesis that the sum of all period effects is not significantly smaller than zero. Again, all null hypotheses are rejected at the 1% level suggesting that the R&D intensities of acquirers and targets are significantly negative affected in the six periods after a merger.

Table 3: R&D growth

	Nearest-neighbor matching		Mahalanobis matching		Caliper matching	
Acq t+1	0.013	(0.022)	−0.007	(0.021)	−0.019	(0.024)
Acq t+2	−0.014	(0.024)	−0.032	(0.026)	−0.045	(0.030)
Acq t+3	−0.032	(0.024)	−0.051*	(0.029)	−0.066**	(0.028)
Acq t+4	−0.027	(0.019)	−0.044**	(0.018)	−0.056***	(0.016)
Acq t+5	−0.089***	(0.018)	−0.105***	(0.020)	−0.117***	(0.021)
Acq t+6	−0.036	(0.054)	−0.051	(0.050)	−0.064	(0.053)
Tar t+1	−0.085***	(0.015)	−0.106***	(0.016)	−0.124***	(0.017)
Tar t+2	−0.100***	(0.023)	−0.117***	(0.023)	−0.128***	(0.020)
Tar t+3	−0.076***	(0.027)	−0.094***	(0.026)	−0.084***	(0.029)
Tar t+4	−0.083***	(0.027)	−0.104***	(0.027)	−0.108***	(0.030)
Tar t+5	−0.042**	(0.019)	−0.057***	(0.019)	−0.050**	(0.022)
Tar t+6	−0.037	(0.067)	−0.051	(0.069)	−0.048	(0.069)
Observations	5526		4686		9015	
Acquirers	265		264		261	
Targets	133		132		123	
p-Value Acquirers†	0.003		0.000		0.000	
p-Value Targets†	0.000		0.000		0.000	

All regressions include controls for industry and time effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors (in parentheses) are robust and allow for clustering on the year-level.

† p-Values of the Wald-test with the null-hypothesis, that the sum of merger-dummy coefficients of acquirers (or targets) is not significantly smaller than zero.

Table 4: R&D intensity

	Nearest-neighbor matching		Mahalanobis matching		Caliper matching	
Acq t+1	−0.011	(0.014)	−0.014	(0.010)	−0.015	(0.010)
Acq t+2	−0.026***	(0.007)	−0.025***	(0.006)	−0.027***	(0.008)
Acq t+3	−0.028***	(0.007)	−0.027***	(0.005)	−0.029***	(0.006)
Acq t+4	−0.034***	(0.006)	−0.034***	(0.006)	−0.034***	(0.007)
Acq t+5	−0.038***	(0.007)	−0.040***	(0.008)	−0.039***	(0.007)
Acq t+6	−0.048***	(0.011)	−0.049***	(0.011)	−0.048***	(0.009)
Tar t+1	−0.019	(0.026)	−0.022	(0.028)	−0.023	(0.028)
Tar t+2	−0.044*	(0.022)	−0.048**	(0.023)	−0.049*	(0.025)
Tar t+3	−0.045	(0.031)	−0.048	(0.032)	−0.049	(0.034)
Tar t+4	−0.060**	(0.024)	−0.063**	(0.025)	−0.067**	(0.028)
Tar t+5	−0.062***	(0.018)	−0.066***	(0.019)	−0.068***	(0.021)
Tar t+6	−0.070**	(0.029)	−0.074**	(0.031)	−0.073**	(0.032)
Acquirer	0.009	(0.007)	−0.018**	(0.007)	−0.003	(0.008)
Target	0.059***	(0.020)	0.035**	(0.017)	0.058**	(0.025)
Observations	5798		4964		9414	
Acquirers	265		264		261	
Targets	133		132		123	
p-Value Acquirers†	0.000		0.000		0.000	
p-Value Targets†	0.004		0.003		0.004	

All regressions include controls for industry and time effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors (in parentheses) are robust and allow for clustering on the year-level.

† p-Values of the Wald-test with the null-hypothesis, that the sum of merger-dummy coefficients of acquirers (or targets) is not significantly smaller than zero.

5 Conclusion

In this paper we have estimated the effect of M&A activity on the growth of R&D spending as well as R&D intensity of the parties involved, using a sample of merger cases that went under the scrutiny of either the EC or the FTC. In doing so, we have explicitly recognized the roles of the firms involved as either buying firms or merger targets and have evaluated the impact on both groups separately, using appropriately constructed control groups.

In terms of merger mechanics, the results suggest, that merger targets are chosen on the basis of being highly innovative firms, as indicated by an average pre-merger R&D intensity of 13%. The fact that the probability of being a target is negatively related to profitability (as indicated by the propensity score regression) supports the conjecture, that these firms have not yet been able to reap the profit of their innovative efforts. Acquirers thus seem to cherry-pick firms with attactive technological portfolios, that have not yet been commercially exploited. Acquirers themselves, on the other hand, are primarily characterised by being both large and profitable.

We find that the mergers in this sample entail a significant negative effect on the R&D efforts of firms in the subsequent periods. Specifically, looking at mean R&D intensitites over time, we find that the R&D intensity of acquirers six years after the acquisition has dropped by over a third compared to its pre-merger level. Similarly, the R&D intensity of merger targets decreases by almost one half. We corroborate these findings in a difference-in-difference setting, where the evolution of merging firms' R&D intensity is contrasted with that of appropriate control groups. While the effects on both groups (acquirers and targets) are unambiguously negative, the effects on acquirers are much more significant.

The mergers in this sample also entail detrimental R&D growth effects on both acquirers and targets: while the R&D stock of merger targets accumulates significantly more slowly than that of the control group in all periods until five years after the merger, this is only true in some periods for acquiring firms. Thus, while both groups of firms experience negative effects in terms of R&D growth as well as R&D intensity, the growth effects are more pronounced on targets, while the intensity effects primarily affect buying firms.

From the point of view of a policy-maker that aims to maximize the well-being of consumers it seems distressing that the average effect of a business combination in a sample consisting of acquisitions that are very diverse in nature, but major in size is unambiguously negative to such an extent. Competition authorities are traditionally reluctant to consider the effects of a merger on innovative activity, since such effects are hard to quantify, particularly from an ex-ante perspective. However, given that the findings of the literature on the effects of M&A on R&D are predominantly negative, it would seem desirable to find a way to incorporate such considerations into the evaluation of notified mergers.

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Table 5: R&D growth regressions in acquirer/target subsamples

	Nearest-neighbor		Mahalanobis		Caliper	
	Acquirers	Targets	Acquirers	Targets	Acquirers	Targets
Acq t+1	0.010		−0.013		−0.042	
Acq t+2	−0.016		−0.037		−0.069**	
Acq t+3	−0.035		−0.056*		−0.089***	
Acq t+4	−0.030		−0.048**		−0.079***	
Acq t+5	−0.092***		−0.107***		−0.138***	
Acq t+6	−0.039		−0.053		−0.086	
Tar t+1		−0.073***		−0.096***		−0.091***
Tar t+2		−0.086**		−0.109***		−0.094***
Tar t+3		−0.061**		−0.085***		−0.058
Tar t+4		−0.069**		−0.094***		−0.083**
Tar t+5		−0.030		−0.052*		−0.034
Tar t+6		−0.026		−0.046		−0.031
Observations	3661	1794	3070	1616	9015	9015
Mergers	265	133	264	132	261	123
p-Value†	0.004	0.008	0.000	0.000	0.000	0.007

All regressions include controls for industry and time effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors (not reported) are robust and allow for clustering on the year-level.

† p-Values of the Wald-test with the null-hypothesis, that the sum of merger-dummy coefficients of acquirers (or targets) is not significantly smaller than zero.

Table 6: R&D intensity regressions in acquirer/target subsamples

	Nearest-neighbor		Mahalanobis		Caliper	
	Acquirers	Targets	Acquirers	Targets	Acquirers	Targets
Acq t+1	−0.008		−0.010		−0.010	
Acq t+2	−0.022***		−0.021***		−0.021***	
Acq t+3	−0.024***		−0.023***		−0.023***	
Acq t+4	−0.030***		−0.030***		−0.028***	
Acq t+5	−0.035***		−0.035***		−0.032***	
Acq t+6	−0.043***		−0.043***		−0.040***	
Acquirer	0.011*		−0.023***		−0.000	
Tar t+1		−0.024		−0.027		−0.035
Tar t+2		−0.048**		−0.054**		−0.065**
Tar t+3		−0.049		−0.054		−0.061*
Tar t+4		−0.063**		−0.069**		−0.085***
Tar t+5		−0.067***		−0.074***		−0.091***
Tar t+6		−0.076***		−0.083**		−0.103***
Target		0.046*		0.046**		0.052*
Observations	3858	1869	3277	1687	9414	9414
Mergers	265	133	264	132	261	123
p-Value†	0.000	0.002	0.000	0.001	0.000	0.001

All regressions include controls for industry and time effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors (not reported) are robust and allow for clustering on the year-level.

† p-Values of the Wald-test with the null-hypothesis, that the sum of merger-dummy coefficients of acquirers (or targets) is not significantly smaller than zero.

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