An Empirical Assessment of the 2004 EU Merger Policy Reform*

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Abstract Based on a sample of 368 merger cases scrutinized by the European Commission (EC) between 1990 and 2007, we evaluate the impact of the change in European merger legislation in 2004. We propose a general framework based on standard static oligopolistic theory and the use of event studies. To assess the effectiveness of merger policy we then focus on four different policy dimensions and compare them before and after the reform: 1) predictability, 2) decision errors, 3) reversion of anti-competitive rents, and 4) deterrence. We find that after the reform the ex-ante predictability of decisions has slightly improved and the frequency of type I discrepancies between the EC decision and the stock markets' assessment has decreased. Yet, the policy shift away from prohibitions, which are effective in terms of rent reversion and as a deterrence mechanism, does not seem to be well-grounded.

Keywords: merger control, regulatory reform, EU Commission, event study
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1. Introduction

The modernization of European merger control led to the adoption of Council Regulation 139/2004 in May 2004 (ECMR 04). Several observers interpreted this major institutional change as a shock reaction to events that had happened in the early 2000s, when three prohibition decisions of the Directorate General for Competition (DG Comp) were overruled by the Court of First Instance (CFI).[1] In all three successful appeals, the CFI identified the main problems as being related to the rigor of economic analysis conducted by DG Comp and the standard of proof the decision was based upon. While these reverses certainly were an indicator of the need for reform, they were not the cause: A Green Paper calling for a revision of European merger law had been published as early as December 2001.

Numerous important changes were made to achieve an approach in merger control closer to economic principles: an efficiency defense clause was introduced, the office of the chief economist and her team were created, the timetable for remedies was improved, guidelines for horizontal mergers were issued, and the old 'dominance test' (DT) was abandoned in favor of the 'significant impediment of effective competition test' (SIEC).[2] The reception of the new merger regulation was generally favorable, yet a first assessment of its effects is still missing.

In this paper, we propose a framework to empirically identify some robust tendencies in the effectiveness of EU merger policy along different dimensions. We analyze 368 mergers covering most major cases scrutinized by DG Comp from 1990 until December 2007 to assess the economic impact of the legal and institutional changes brought about by ECMR 04. We base our evaluation exercise on a number of maintained theoretical assumptions from standard merger theory in an oligopolistic setting, which imply that horizontal mergers benefitting competitors are detrimental to consumers (Farrell and Shapiro, 1990). To empirically operationalize this concept and measure the change in profits due to the merger, we use stock-market event studies (Eckbo, 1983; Stillman, 1983). From this starting point, we propose four dimensions of effectiveness of merger policy: predictability, decision errors or discrepancies,[3] rent-reversion, and deterrence. For each of these, we adopt a before-and-after approach to single out the effects of the reform.

[1] The cases in question are *Airtours/First Choice*, *Schneider/Legrand* and *Tetra Laval/Sidel*.

[2] Lyons (2004) discusses these reforms in greater detail. The problems with the DT and the advantages of the SIEC are summed up in Vickers (2004).

[3] We use the term 'decision errors' when referring to the theoretical identification of these concepts, but prefer the term 'discrepancy' when we talk about their empirical measurement.

First, we test the predictability of European merger policy. We emulate the firms' or markets' expectations around the notification of a transaction by estimating a probit model, where the decisions of DG Comp are a function of ex-ante observable merger characteristics. We find that the ex-ante predictability of the merger review process increases post-reform and identify several robust predictors for the decisions.

Second, we assess whether the introduction of the new merger regulation has influenced the frequency and determinants of systematic discrepancies between the EU Commission's (EC) decisions and the stock market expectations about the competitive nature of the merger (Duso, Neven, and Röller, 2007). We distinguish cases in which DG Comp remedied mergers that hurt the rivals and can therefore be assumed to be pro-competitive (weak type I discrepancies) from unconditionally cleared mergers that benefitted the rivals and can therefore be assumed to be anti-competitive (type II discrepancies). We further identify mergers where rivals' abnormal returns are not significantly affected and show that these 'welfare-neutral' cases are significantly more frequent after the reform. Moreover, we find that the frequency of type I discrepancies significantly decreases in the post-reform period. By means of probit regressions, we then identify systematic predictors of such discrepancies.

In a third step, we estimate the degree of rent-reversion induced by the different merger control instruments used by DG Comp. Under a set of maintained assumptions, the negative relation between the abnormal returns around the EC's decision and those around the merger's announcement can be interpreted to indicate the success of merger policy in eliminating anti-competitive rents created by a merger (Duso, Gugler, and Yurtoglu, 2011). We find that prohibitions significantly and substantially reverse anti-competitive rents pre-reform, whereas the effectiveness of remedies appears to be limited before as well as after the introduction of ECMR 04.

Finally, we look at how past policy decisions affect the competitive nature of the merger. An effective competition policy should induce firms to obey antitrust rules and deter firms from proposing anti-competitive mergers. Yet it should not discourage firms from proposing efficiency-increasing combinations. Thus, we estimate the probability of a merger to significantly hurt rivals (pro-competitive mergers), significantly benefit rivals (anti-competitive mergers), or not significantly affect rivals' profitability (welfare-neutral mergers) as a function of past EC decisions. We find that past prohibitions reduce the likelihood that mergers benefit rivals, while they do not affect the probability that mergers hurt rivals pre-reform. We interpret these results as a sign of the effective deterrence of anti-competitive mergers but not over-deterrence. After 2004, the deterrence properties of prohibitions are partially replaced by those of withdrawn mergers and phase I remedies.

The paper proceeds as follows. Section 2 discusses the assumptions which allow us to achieve an empirically implementable identification of anti-competitive mergers. Section 3 presents the sources of the data, some summary statistics, and the estimations of the merger announcement and merger decision effects by means of stock-market event studies. Section 4 presents the empirical tests for the four dimensions of effectiveness pre- and post-reform and the results. Section 5 concludes.

2. Identification

The starting point of our methodology is that an effective merger control aims to avoid the anti-competitive (i.e., consumer welfare-decreasing) effects of mergers by either blocking, remedying, or deterring them. One of the main challenges in the assessment of merger control is the ability to, first, theoretically define and, second, empirically measure the anti-competitive nature of a merger. Our theoretical setting is a standard static merger model in oligopolistic markets. The well-documented result of this literature is that mergers exert two externalities on rivals. The *market power effect* captures the impact of the reduction in competition brought about by a combination, absent any efficiency gains (Stigler, 1950). The *efficiency effect* (Williamson, 1968), relies on the assumption of merger-specific synergies: economies of scale, knowledge sharing, patent-pooling, etc., allow the merged entity to produce more efficiently than before, increasing the competitive pressure on its rivals and thus exerting a negative externality on them.

In most mergers both effects co-exist and what matters for welfare is the net effect of these antipodal forces. As Farrell and Shapiro (1990) show, there exists a critical level of merger-specific efficiency gains such that the market power effect is exactly compensated for and the new equilibrium price and aggregate production is the same pre- and post-merger. Thus, looking at this net effect allows us to infer the competitive nature of a merger. When the efficiency gains are not enough to compensate for the market power effect, rival profits increase and consumer surplus decreases, since prices are higher than before the merger. The *theoretical* identification assumption of our framework thus is that a post-merger increase in competitors' profits is an indication of the merger being anti-competitive.

This theoretical identification is quite general and robust and holds for the standard oligopoly models that investigate the unilateral effects of horizontal mergers in a static setting.[4] However, some caveats are in order: the theoretical identification might not be

[4] Duso, Neven, and Röller (2007) and Gugler and Siebert (2007) show that the same identification can be achieved in a model with Bertrand competition and differentiated goods.

achieved in models of vertical or conglomerate mergers. In these cases, a merger could be to the detriment of both rivals and consumers if it entails market foreclosure. We thus run a robustness check where we exclude all mergers that are not purely horizontal and show that our main findings are not affected (see online appendix B.2). Moreover, in a dynamic setting the theoretical identification depends on the sequence of notified mergers and is therefore achieved only under specific, more stringent, conditions, yet it is still valid in general (Nocke and Whinston, 2010). Finally, our identification would also work in models of endogenous mergers if the so-called in- and out-of-play effects are not particularly strong (Fridolfsson and Stennek, 2010). As we discuss next, since the latter issues are partially related to our empirical implementation, we try to account for some of them by carefully designing it.

The next step in our framework is an assumption on the empirical measurement of the profitability effects brought about by the merger. Following an extensive literature, we use stock market reactions to the merger announcements - i.e., a stock-market event study - to measure the change in profits of rivals firms. This methodology relies on the semi-strong version of the efficient capital market hypothesis, which asserts that stock prices fully reflect the *public* information available to the market on the given commodity at any point in time. Under this assumption, we can estimate the coefficients of a market model to predict the 'normal' returns of a firm. We then calculate the 'abnormal' returns that accrue due to the announcement of an event as the difference between the predicted and the actual returns. To account for information leakages, we sum up the abnormal returns over a specific time interval called the event window obtaining the cumulative abnormal returns (CARs), which are subsequently aggregated into cumulative average abnormal returns (CAARs): the market-value-weighted profitability measures for both merging firms and competitors (see online appendix A.1 to A.3 for details).[5]

Clearly, the measured CAARs around a merger's announcement might entail effects other than the merger's pure competitive effects - i.e., the theoretical change in profits due to the merger that we discussed above. In particular, the CAARs might incorrectly measure the rivals' change in profit and be biased toward zero if one uses the wrong set of rival firms, that is when the product market definition is not precise (McAfee, 1988). Moreover, the CAARs can entail the effects of other specific forces triggering the

[5]As we discuss in Duso, Gugler, and Yurtoglu (2010) one might use different empirical methodologies to measure the change in profits. In particular, we there build on Gugler, Mueller, Yurtoglu, and Zulehner (2003) and provide a different indicator of the merger's effect based on accounting data. We then show how and when these alternative measures correlate with those based on event studies. We further show that the correlation is higher and more significant the larger the event window. This is particularly true for the competitors.

merger (Jovanovic and Rousseau, 2002), information about the roles of merging and rival firms (Fridolfsson and Stennek, 2010), and the market expectations about the outcome of the merger control decision (Eckbo, 1992). The second important assumption of our methodology is therefore that we can effectively control for these other forces.

First and foremost, the definition of the rivals in our sample is very accurate. These are the real competitors in the defined product market as they have been identified by the EC in their in-depth antitrust investigation. This is a large advantage of our data in comparison to previous event studies in merger control and answers one of the main original critiques advanced against the pioneering work by Eckbo (1983) and Stillman (1983). Second, we claim that we might control for the merger's triggering events and the allocation of roles, by choosing the right announcement dates and event windows. We use the date of the first merger-specific rumors in the business press as the merger announcement (Banerjee and Eckard, 1998).[6] The surprise element to the stock market is likely to be largest around this date, since the likelihood that the merger is already anticipated is still low. Moreover, using the *merger-specific* rumors coupled with a large event window ranging from 50 trading days before to five trading days after the merger announcement should help us to control for the uncertainty in the allocation of the roles (acquirer, target, rival) (Fridolfsson and Stennek, 2010).[7] Third, we try to correct for the market expectations about the merger control procedure. We estimate the probability of an intervention by the EC as a function of observable merger characteristics and then use this prediction as a measure of the market expectations to calculate the 'corrected CAARs' (see Duso, Gugler, and Yurtoglu, 2011, and the online appendix A.4).

Given this empirical setup, we are then more confident that the corrected CAARs around merger j's announcement (Π_{fj}^{A*}) can be seen as a meaningful measure of the competitive effect of the merger on merging firms ($f = M$) or competitors ($f = C$), that is, as an empirical measurement of the change in profits derived from theory. To summarize, from a theoretical point of view we classify a merger to be anti-competitive if its impact on competitors' profits is positive. Empirically, we then assume that this is the case when the corrected CAARs are *sufficiently* large - i.e., Π_{Cj}^{A*} exceeds a certain

[6] As a robustness check, we collected data on the merger's official announcement date from the SDC database (Thomson Reuters) and were able to identify 240 of our mergers. Most of the official announcements are in an interval around five days before and two days after the first rumors.

[7] In Duso, Gugler, and Yurtoglu (2011), we discuss these issues in more depth and provide several additional robustness checks based on different types of mergers, industries, and time periods to control for the possible failure of our framework's crucial assumptions. The results suggest that running the model in sub-samples where our assumptions are less likely to hold does not seem to have a significant impact on our main findings. We therefore do not replicate all of them in this paper.

threshold $\bar{\pi}$. Symmetrically, a merger is classified as pro-competitive if the change in rivals' profits is negative - and this is assumed to be the case when the corrected rivals' CAARs (Π_{Cj}^{A*}) are smaller than $-\bar{\pi}$. This means that, for any positive value of $\bar{\pi}$, we define a symmetric interval around 0, where it is assumed that event studies do not measure any significant change in profit of the rival firms. We label these mergers as 'welfare-neutral.' Since the choice of the threshold level $\bar{\pi}$ is arbitrary, we consider different values for $\bar{\pi}$, namely $\bar{\pi} = 0$, $\bar{\pi} = 3\%$, $\bar{\pi} = 5\%$, and $\bar{\pi} = 10\%$. In the main regressions reported in the paper, we adopt an intermediate threshold of $\pm 3\%$.[8] In the online appendix B.4 we discuss the robustness of our results to the use of different threshold values.

A few examples might illustrate the empirical relevance of our identification strategy. On November 12, 2009, two large mergers were announced, one of which was viewed as clearly anti-competitive by the business press and the other one as clearly pro-competitive. British Airways (BA) and Iberia announced their decision to merge, creating the world's third largest airline after Air France-KLM and Lufthansa. The share prices of BA and Iberia rose by ca. 10% and 15% respectively around this announcement. Likewise, their main rivals, Lufthansa and Air France-KLM, outperformed the stock market by 6% and 5%, respectively. Many commentators viewed this merger as anti-competitive mainly on the grounds that the Oneworld alliance (i.e., BA's alliance) already had a 'tight grip' on the Heathrow airport, and the merger would make matters worse particularly concerning take-off and landing slots (e.g., AFX News, November 13, 2009). The observed announcement abnormal returns are consistent with this interpretation. The same day, Hewlett-Packard (HP) announced the takeover of 3Com, paying a 40% premium over the pre-announcement share price. Despite that, HP shares outperformed the Dow Jones by 2%. The deal was widely seen as being aimed at creating a competitor to Cisco Systems, the leader in computer networking, since the biggest companies that provide corporate computing infrastructure were trying to become 'one-stop technology shops.'[9] 3Com's assets being complementary to HP's, the merger would allow HP to offer more integrated solutions to corporate customers. Cisco lost 2% in value on the day of the announcement of the deal, in line with the idea that the stock market believed it to be a pro-competitive takeover.

[8] Note that an average CAAR of 3% for the competitors sums up to quite large effects in terms of value. At the mean value of our sample this average effect is more than $63 million.

[9] See e.g., Jordan Robertson, November 12, 2009, AP Technology, "HP's 3Com takeover marks a shot at Cisco".

3. Data

Figure 1 gives a representation of the evolution of notifications and actions in the population of over 3,800 mergers analyzed by the EC from the beginning of 1990 to the end of 2007. Notifications show an increasing trend with a single big drop around 2002. The proportion of remedies in phase 2 oscillates before 1999 and then takes a downward trend, while the proportion of remedies in phase 1 increases. The prohibitions ratio displays a downward trend, with only two prohibitions after the merger reform.

[insert Figure 1 here]

Our sample includes 368 of these merger cases. We include almost all phase 2 cases since these are the most interesting ones in terms of merger policy.[10] Moreover, since the entire population of phase 1 mergers during the sample period exceeds 3,600 cases and is therefore too large to be used, we chose a sample of this population, deliberately over-representing the amount of cases where the EC imposed some remedies. Again, this is because these are the most interesting merger policy cases.[11] The exact composition of this sample is designed to mimic the dynamics of the population of EC merger cases prior to and following the introduction of ECMR 04, as discussed below. By carefully reading the text of publicly available merger decisions handled by DG Comp, we identified the merging parties, their rivals, relevant markets, decision types, the dates of the notification, phase 1 and possibly phase 2 decision, and some other merger-specific characteristics. Among these 368 mergers, 250 were cleared in phase 1 and 216 were notified during the pre-reform period.

As we mentioned above, using the EC's merger assessment to identify the rivals represents a particular strength of this sample. It has the big advantage of being a much more realistic description of the relevant product markets than, say, SIC codes, which would yield a sample of firms active in the same branch, but possibly not competing in the specific product market concerned by the merger. Following Banerjee and Eckard (1998), the announcement date of a merger is defined as the date on which the first rumors about

[10] Since not all merging parties or main rivals are quoted firms, we had to drop some of the phase 2 mergers from our sample for which we were unable to identify this crucial information.

[11] In a robustness check (online appendix B.1), we randomly drop cases cleared with remedies in phase 1 so that our sample exactly matches the proportion of phase 1 remedies in the population. We then run all our regressions using this reduced sample where the full population of phase 2 cases is matched to a random sample of phase 1 cases. We repeat this process several times to make sure that the results do not strongly depend on which cases are dropped. Our qualitative results remain unchanged.

that particular merger leaked to the market. This is usually before the official notification to the EC as well as the official merger announcement. We used the financial press and the Dow Jones Interactive database to identify the dates when the first definitive indications of the combination between the merging parties became known. The total return index, market value, and branch index time series for the identified parties were downloaded from the Thomson Reuters Datastream database that provides daily data for the variables in question.

Table 1 summarizes the variables in our dataset and the dynamics of the sample and population for the periods before and after the merger policy reform.

[insert Table 1 here]

In our sample, the percentage of cases that were cleared with remedies decreases from 42.1% in the pre-reform period to 33.6% post-reform. This mimics the 20% decrease in remedies in the EC mergers' population from 6.9% to 5.5% during the respective periods. The same is true when looking at the phase in which the remedies were applied: Phase 1 remedies increase from 14.4% to 23.7% in the sample and from 4.1% to 4.4% in the population, while the use of phase 2 remedies is strongly reduced in both the sample (from 27.8% to 9.9%) and the population (from 2.8% to 1.1%). Prohibitions decrease from 5.2% to 1.3% of the cases pre- and post-reform in the sample, and from 0.8% to 0.01% in the population. The ratio of cases going to phase 2 drops from 42.1% to 17.8% in the sample and from 5.5% to 3.2% in the population. All other cases have been cleared without conditions and obligations. Thus, while we over-sample cases which resulted in an action by the EC – which are the most interesting in terms of merger policy – we made an effort to mimic the evolution of the population in the sample. For the same reason, we also sampled the same relative amount of cases pre- and post-reform: our sample accounts for 9% of the population in both periods. For the population data, we also have information on withdrawn cases.[12] These represent 3.3% and 2.4% of the notified cases pre- and post-reform, respectively.

For the mergers in our sample, we also report some additional information. The proportions of geographic market definitions (national, EU-wide, worldwide) do not change much between the two periods. Post-reform, the proportions of conglomerate and full mergers increase, the frequency of cross-border mergers slightly increases, while barriers to entry are found less often. Dominant firms (dummy equal to 1 if one market participant in a relevant market has a market share in excess of 50% prior to the merger), as

[12] Since no formal decision is published for withdrawn cases, we lack the information on rivals that would be necessary to include them in the sample.

well as firms from the US or a big EU country (Germany, France, Italy, Spain, or the UK) are observed with approximately the same frequency before and after ECMR 04. The average market values of both merging firms and rivals increase.

Table 2 reports the mean CAARs around the merger's announcement and the EC's decision for merging firms and rivals, in the pre- and post-reform periods respectively.

[insert Table 2 here]

On average, the mergers in the sample are profitable for merging firms pre-reform and yield an increase in their stock value of around 1.6%, which is significant at the 5% confidence level. After the reform, mergers are still significantly profitable for merging firms with an average CAAR of 1.4%. The impact of DG Comp's decisions on the valuation of merging firms is negative pre- and post- reform and entails an insignificant drop in the firms' stock value by 0.3% pre-reform, which increases to 0.8% post-reform and becomes significant at the 10% level.

The competitors' merger announcement effects are positive (0.8%) but not significant prior to the reform, and are of equal magnitude but become significant at the 10% level post-reform. Similar to the merging firms, rivals suffer an average negative reaction around the EC decision date: the insignificant pre-reform effect of -0.3% increases to -0.8% post-reform and it also becomes significant at the 10% level.

4. Methodology & Results

In this section we describe the four steps of our effectiveness measurement and present the related empirical results. The objective of this paper is to use this framework to measure the impact of the modernization package of European merger control by comparing the periods pre-reform (January 1990 to May 2004) and post-reform (June 2004 to the end of 2007).[13]

The four dimensions of policy effectiveness can be seen in a natural chronological order. First, before the announcement of a merger, the predictability of the merger control procedure is an important determinant of firms' choices of the kind of merger they plan to propose. Therefore, the first test analyzes the determinants of interventions by DG Comp to infer its predictability. The second event we look at is the EC decision. An effective policy should avoid mistakes. Thus, we analyze the frequency and determinants

[13]We chose the date in which the new merger regulation legally came into force to define the pre- and post-reform periods. However, in the online appendix B.3, we show that our results are robust to the use of different dates and discuss the appropriateness of our choice.

of the discrepancies between the EC decisions and the market predictions that we call (weak) type I and type II errors following Duso, Neven, and Röller (2007). Third, it is not only important whether the EC intervenes in the 'right' mergers, but also whether its intervention achieves the 'desired' results. Thus, we look at the degree of rent-reversion achieved by the different merger policy instruments. Finally, past decisions might have consequences on the future merger behavior of other firms. We therefore analyze the deterrence effects of EC merger policy by estimating how past interventions affect the rivals' stock market reactions in newly proposed mergers which, as we saw, we consider to be indicative of the merger's competitive nature.

4.1. Predictability

We estimate the degree of ex-ante predictability of EC merger decisions based on observable merger characteristics. Let P_j be the actual decision taken by the agency on merger j, which is equal to 1 when the merger is remedied or blocked (*action*) and zero otherwise (*clear*). Let X_j be a set of observable characteristics related to the specific merger. Note that for the estimation of this model none of the assumptions related to the identification of anti-competitive mergers are required. We measure the predictability of the decision on the basis of goodness-of-fit measures of the following regression:[14]

$$P_j = \alpha_0 + \alpha_1 X_j + \varepsilon_j \tag{1}$$

This model is supposed to provide a measurement of how well the parties notifying a merger and, more generally, the stock markets can anticipate the outcome of DG Comp's investigation. Thus, the explanatory variables in this model are limited to some merger specific-variables (full, cross-border and conglomerate merger dummies, market values), variables related to the firms' country of origin (US and big EU countries), measures of past merger policy enforcement of the EC (lagged notifications, antitrust actions, and merger withdrawals) as well as industry dummies and a time trend. Table 3 reports the marginal effects of these variables on the EC's decision.

[insert Table 3 here]

In the post-reform period, both the R^2 and the percentage of correct predictions increase by over 5%. Although the R^2 is quite low, the ability of the model to correctly predict the outcome based on these few external factors is quite high and it increases

[14]Since we assume that the error terms ε_j are potentially correlated over time, we cluster the standard errors at the year level.

from 71% to 76% in the post-reform period. In the pre-reform period, we observe four significant predictors: mergers involving firms from the US are 26% less likely to be challenged; full mergers (as opposed to share acquisitions or joint ventures), conglomerate mergers, and mergers where the parties have high market values are more likely to receive scrutiny. After the reform, the likelihood of regulatory intervention is lower for mergers involving US firms (34% lower probability), full and cross-border mergers (6% and 7% lower probability respectively), and is higher for conglomerate mergers (higher probability by 20%). Moreover, the number of lagged notifications becomes a significant predictor of the outcome suggesting that DG Competition might intervene less if the workload is high during the past quarter. Mergers among large firms in terms of market value are less likely to be challenged, but the size of the competitors has a positive yet not strongly significant effect on the likelihood of intervention.

4.2. Type I and Type II Discrepancies

The first assessment of a particular decision is whether it conforms to the objectives of merger control. A benevolent agency should intervene in a merger if and only if consumer surplus (CS) is reduced, hence the optimal decision rule for merger j is:

$$D_j = \begin{cases} 0 & \text{(clear) if } \Delta CS_j \geq 0 \\ 1 & \text{(action) if } \Delta CS_j < 0 \end{cases}$$

Let P_j again be the actual decision taken by the agency on merger j, which is equal to 1 if the merger is remedied or blocked, and zero otherwise. We say a type I error occurs if the agency intervenes in a merger that should have been cleared without commitments - i.e., $E1_j = 1$ if $P_j = 1$ and $D_j = 0$, else 0 - and a type II error when the agency clears a merger that should have been blocked or remedied - i.e., $E2_j = 1$ if $P_j = 0$ and $D_j = 1$, else 0.[15]

To measure $E1_j$ and $E2_j$, we need to measure D_j, which requires an estimate of the impact of the merger on consumer surplus. Under our maintained assumptions, consumer surplus decreases after the merger when the profits of the rivals to the merging firms increase. This change in profit can be measured by a 'sufficiently' large CAAR for the rivals. Hence, the consumer welfare-maximizing merger control decision is:

[15] The notion of type I errors we use here corresponds to the *weak* type I errors in Duso, Neven, and Röller (2007). Given that prohibitions are a very rare event in the entire sample and, especially, in the post-reform period, it would be impossible to perform any econometric analysis on the *strong* type I errors - i.e., pro-competitive mergers which are blocked.

$$D_j = \begin{cases} 0 & \text{if } \Pi^{A*}_{Cj} < -\bar{\pi} \\ 1 & \text{if } \Pi^{A*}_{Cj} > \bar{\pi} \end{cases}$$

where Π^{A*}_{Cj} represents the corrected merger announcement CAAR of the competitors (C) for merger j and $\bar{\pi}$ is either 0%, 3%, 5%, or 10%.

We defined the concept of decision errors in a theoretical fashion above. When referring to the same conecpt in an empirical context, we call them discrepancies (between the Commission's decision and the stock market's assessment). Under our assumptions, the definition of type II discrepancies is not problematic, especially when we use a demanding threshold: these are cases that the market considers anti-competitive where the EC did not intervene. The definition of type I discrepancies, instead, might be more cumbersome. Even if a merger is *on average* pro-competitive as captured by a large negative value for Π^{A*}_{Cj}, it might still be that it entails some anti-competitive concerns which could effectively be tackled by means of appropriate remedies. It would then be correct for the EC to intervene and we would wrongly identify this case as a type I discrepancy. Yet, also in this case the choice of a demanding threshold for the definition of pro-competitive mergers might help us to correctly identify true type I discrepancies. Mergers where rivals' losses are large are less likely to entail anti-competitive elements. Because of these considerations, we will base our econometric analysis on a threshold $\bar{\pi} = 3\%$.

Table 4 reports the composition of mergers according to the stock market reactions of rival firms and different thresholds. The larger the interval [-$\bar{\pi}$,$\bar{\pi}$], the more mergers are defined to be welfare-neutral.

[insert Table 4 here]

Post-reform, the percentage of mergers that do not strongly affect rivals significantly grows by 17%-20% independently of the threshold and this increase is compensated by an equal decrease in pro- and anti-competitive mergers. This has two implications for our further tests. First, it means that we should observe less type I and type II discrepancies after the reform, due to the change in the nature of proposed mergers. Second, we need to ask whether this composition change is due to the changes in merger policy enforcement or other determinants in section 4.4.

Based on these definitions of pro- and anti-competitive mergers, we look at the evolution of the discrepancies between the EC decisions and the stock market assessment in table 5.

[insert Table 5 here]

The propensity of type II discrepancies (unconditional clearance of an anti-competitive merger) significantly increases post-reform when we use the 0% threshold, it increases only weakly and not significantly with the 3% and 5% definitions, and it even decreases when employing a 10% threshold. The propensity of type I discrepancies (action in a pro-competitive merger) decreases by more than 10% with all four thresholds, and in most cases the difference is significant at the 10% level. Thus, the decrease in the frequency of type I discrepancies seems to be more robust and does not depend on the chosen threshold. From now on, we use the definition based on $\bar{\pi} = 3\%$. Results based on the other thresholds are discussed in the online appendix B.4.

Once we have defined type I and type II discrepancies, we can analyze their determinants to identify systematic tendencies or predictors. We therefore run the following probit regressions:

$$E1_j = \alpha_0 + \alpha_1 X_j + \varepsilon_j \text{ if } D_j = 1, \tag{2}$$

$$E2_j = \beta_0 + \beta_1 X_j + \varepsilon_j \text{ if } D_j = 0. \tag{3}$$

We consider a number of potential determinants of these discrepancies: as claimed by Aktas, de Bodt, and Roll (2007), the European Commission might be protectionist and favor European versus US firms, hence the country of origin of the merging parties might be a determinant of the EC's discrepancy between the EC decision and the stock market's assessment. The size of the country from which the merging firms originate could also play a role in the outcome of a merger investigation, presumably (but not exclusively) because of the political pressure that can be exerted by large countries (e.g., Neven, Nuttall, and Seabright, 1993, and Horn and Levinsohn, 2001). A merger involving conglomerate concerns or a full merger as compared to a partial merger or a joint venture might be seen as more problematic since the anti-competitive effects that it generates might be expected to be larger (e.g., Bresnahan and Salop, 1986, and Gugler and Siebert, 2007), whereas a cross-border merger might be treated more leniently since the market power aspects might be less problematic (Neary, 2007). Moreover, the EC was often alleged to have defined the relevant geographical markets too narrowly, which might imply a lower frequency of discrepancies when the market is either EU- or worldwide (Neven, Nuttall, and Seabright, 1993). Finally, procedural issues, such as the time available to undertake the merger analysis, may also be important. In particular, whether the case has been decided in phase 1 instead of being subject to a more substantial phase 2 investigation might influence the likelihood of discrepancies.

To assess how the new merger regulation affected the likelihood and determinants of such discrepancies, we run the basic regressions 2 and 3 separately on the pre- and post-reform sub-samples. The regression results of equation (2) are reported in table 6.

[insert Table 6 here]

If one of the merging parties is a US-based firm, the likelihood of eliciting an action in mergers which clearly penalize the competitors (pro-competitive) is, ceteris paribus, 20% lower in the pre-reform period and 25% lower after the reform. Similarly, weak type I discrepancies are almost 20% less likely in case of cross-border mergers both pre- and post-reform. Full mergers, and mergers involving large firms and large competitors in terms of market value, are more likely to result in a type I discrepancy pre-reform. Post-reform, conglomerate mergers that hurt rivals (pro-competitive) are 75% more likely to be remedied than non-conglomerate mergers. Since the identification of the competitive nature of these mergers is problematic, one should be careful in interpreting this result.

All variables related to the investigation (barriers to entry, phase 2, and national markets) are significantly related to an increase in the likelihood of a type I discrepancy pre-reform. Post-reform, most results remain the same, yet the phase 2 dummy does not correlate with the likelihood of a discrepancy anymore, possibly indicating a more pro-active merger policy in the first investigation phase. The model's predictive power is high both in terms of R^2 (decreasing from 0.74 pre-reform to 0.65 post-reform) and in terms of correct predications (90% and 87% pre- and post-reform respectively).

We then move to the estimation of equation (3), the determinants of type II discrepancies. The marginal effects of the probit estimations are reported in table 7.

[insert Table 7 here]

We estimate significantly more type II discrepancies in mergers involving US firms both pre- and post-reform (13% and 19% respectively). Conglomerate mergers are 30% less likely to result in a type II discrepancy both pre- and post-reform. Full and cross-border mergers significantly increase the likelihood of a type II discrepancy only post-reform by 23% and 14% respectively. Merging parties' market values negatively and significantly affect the probability of type II discrepancies pre-reform, while they increase it post-reform. On the contrary, the coefficient estimate for rivals' market value is negative and significant in both periods.

Except for barriers to entry, which significantly reduce the likelihood of type II discrepancies pre- and post-reform, the effects of the other variables derived from the EC files changed after the introduction of ECMR 04. A narrow geographic market definition no longer reduces the probability of a discrepancy. Moreover, conditional on a merger being anti-competitive, the opening of a phase 2 investigation significantly increases type II discrepancies post-reform while it significantly decreased them before. Again the predictions of the model are quite accurate with a pre-reform R^2 of over 60% (50% post-reform) and the percentage of correct predictions of 89% and 83% in the pre- and post-reform periods respectively.

4.3. Rent-Reversion

The next step is to assess the ability of different policy tools to effectively reduce the market power effects of a merger and, at the same time, to maintain the benefits to consumers generated by increased efficiency. Hence, an additional assumption that we need at this point is that the market power and efficiency effects of a merger can, at least partially, be separated by an effective antitrust action. Well-implemented remedies imposed by the EC should eliminate the market power effect while preserving the efficiency gains generated by the merger. We thus assume that the corrected CAARs around the EC's decision on merger j (Π^{D*}_{fj}) can be seen as a meaningful measure of the effect of the decision on firms' profitability.[16]

The logic behind the approach developed by Duso, Gugler, and Yurtoglu (2011) is that there should be a reversion of the (anti-competitive) rents measured around the merger announcement due to the decision, if the antitrust action is effective. This implies that decision CAARs should be systematically negatively related to announcement CAARs when a decision is effective. In particular, one should observe a perfect negative correlation of -1 in case of prohibitions since they reverse all rents. We assess the effectiveness of antitrust actions by running the following regression separately for merging firms and rivals:

$$\Pi^{D*}_{fj} = \sum_{d} \alpha_{fd} + \sum_{d} \beta_{fd} \Pi^{A*}_{fj} + \gamma_f X_j + \epsilon_{fj} \tag{4}$$

where Π^{D*}_{fj} is the probability-corrected decision CAAR of merging firms ($f = M$) and competitors ($f = C$), respectively, for merger j, while Π^{A*}_{fj} is the probability-corrected

[16]For the phase 1 decision, we use a short window of 11 days (-5, +5), since information leakages are likely to be modest before the phase 1 decision given the strict timing of the EU merger control procedure. For a phase 2 decision, however, we use the long window of 56 days (-50, +5) to account for information leakages due to the investigation and negotiation process during that phase (see also online appendix A.2). Also in this case, Duso, Gugler, and Yurtoglu (2011) discuss this assumption in more depth.

announcement CAAR. We estimate different intercepts (αs) and slopes (βs) for the different decisions (d=*clearance*, *phase*1 *remedies*, *phase*2 *remedies*, or *prohibition*).

Duso, Gugler, and Yurtoglu (2011) explain in depth the sizes and signs of the intercepts and slopes which are expected if merger control is perfectly effective and under our maintained assumptions. *Prohibitions* are the most extreme action taken by the EC and should dissipate market power as well as the efficiency rents. Hence, $\alpha_{fd} = 0$, $\beta_{fd} = -1$ if $d = prohibition$. If a merger is *cleared* without commitments, we do not expect decision effects that are systematically related to announcement returns. Hence, $\alpha_{fd} = 0$, $\beta_{fd} = 0$ if $d = clearence$. This does not need to be the case if the reaction around the decision date conveys good news to the market about the feasibility of future mergers, in which case rivals would profit. In the case of *remedies*, only market power rents should be dissipated by the antitrust decision if it is effective. Hence, each remedial action should entail a negative decision effect for merging firms and rivals. Hence, $\alpha_{fd} < 0$, $\beta_{fd} < 0$ if $d = remedies$.

We estimate equation (4) for the merging parties and their rivals separately, while pooling pre- and post-reform observations and interacting the independent variables with pre/post dummies. The regression results reported in table 8 for the pre-reform period are in line with those obtained by Duso, Gugler, and Yurtoglu (2011) for the years 1990 - 2002.

[insert Table 8 here]

The slope coefficient for prohibitions is significantly negative and large for both merging firms (-1.27) and rivals (-0.44), where rent reversion is reinforced by the significantly negative prohibition constant (-0.31). However, for both types of firms remedies do not seem to entail significant rent reversion. Furthermore we find that clearances have a positive coefficient for the rivals, suggesting that they were potentially a positive signal. Post-reform, we cannot estimate the degree of rent-reversion achieved by prohibitions, since only two mergers were blocked. Remedies applied after a phase 2 investigation even increase the returns of both merging parties and rivals. Unconditional clearances still have a positive impact on the returns of rivals.

4.4. Deterrence

As pointed out by Sørgard (2009), there is an optimal level of merger policy enforcement where some actions, which in isolation would be welfare-detrimental, can be optimal to achieve deterrence and thus increase overall welfare. The role of deterrence is especially important if the competition authority commits errors and if remedies are not completely effective. If this was not the case and the merger policy was perfectly effective, then firms would know ex-ante that every anti-competitive merger would be blocked or effectively remedied by the antitrust authority and, therefore, they would not even attempt to propose such combinations. Moreover, in the absence of type I errors, firms would always propose a pro-competitive merger knowing that it would always be cleared. Hence, the existence of decision errors is a key ingredient in a deterrence model. Another crucial aspect is that a good policy should deter firms from proposing socially detrimental mergers but it should not over-deter, that is, discourage firms from proposing efficiency-increasing combinations.

Our analysis tries to take an important step in this direction if compared to the limited existing literature which simply looks at frequency and composition effects (e.g., Seldeslachts, Clougherty, and Barros, 2009 and Clougherty and Seldeslachts, forthcoming). By using the theoretical identification discussed in section 2, we relate the merger's competitive nature to its profitability for rivals. Hence, we look at how past merger policy decisions affect rivals' CAARs for a newly notified merger. Instead of using a linear model and so as to use the variation in the data more efficiently, we take a discrete representation of the rivals' profitability. This has the additional advantage of making an interpretation of the results easier in terms of the competitive nature of the merger. Therefore, for each merger j notified in quarter t, we define a categorical variable ($\widetilde{D}_{jt}$) which takes on a value of 1 if the merger significantly hurts rivals ($\Pi_{Cj}^{A*} < -\bar{\pi}$) and hence is pro-competitive, 2 if the merger does not significantly affect rivals' profitability ($-\bar{\pi} \leq \Pi_{Cj}^{A*} \leq \bar{\pi}$) and hence is welfare-neutral, and 3 if the merger benefits rivals ($\Pi_{Cj}^{A*} > \bar{\pi}$) and hence is anti-competitive.[17] We can then analyze how the complete past history of the EC's merger policy enforcement affects the odds of a particular merger being pro- or anti-competitive if compared to the reference category of welfare-neutral mergers. We thus combine measures of DG Comp's merger policy from the entire population of over 3,800 mergers scrutinized in the sample period with our dataset to estimate a multinomial probit equation of the following type:

$$\widetilde{D}_{jt} = \alpha_0 + \alpha_1(n_{t-1} + n_{t-2}) + \sum_d \alpha_{2d} \frac{d_{t-1} + d_{t-2}}{n_{t-1} + n_{t-2}} + \alpha_3 X_{jt} + \epsilon_{jt} \tag{5}$$

The variable n_{t-i} is equal to the total number of notifications to the EC i quarters before merger j was notified in quarter t, and d_{t-i} is the total number of mergers with outcome d ($d = remedies, blockings,$ or $withdrawals$) i quarters before the notification. We thus regress our indicator of the competitive nature of the merger on the total number

[17] If $\bar{\pi} = 0$ our model collapses to a simple probit model. In such a model the effect of the explanatory variables on the likelihood of a merger of being pro- and anti-competitive is symmetric.

of notifications in the last two quarters and on the ratios of possible actions over the total notifications to identify whether past merger policy is a predictor for the merger profitability effect - i.e., its competitive nature. Again, we control for other merger-specific determinants X_j.

The lagged number of notifications control for merger wave effects (Gugler, Mueller, Weichselbaumer, and Yurtoglu, 2012). More importantly, different merger policy tools might send signals to firms about the toughness of the authority. From a theoretical view point, the kind of signal a particular decision sends to the firms and, hence the kind of merger the firms propose, crucially depends on the expectations the firms have about the merger policy (Seldeslachts, Clougherty, and Barros, 2009). Prohibitions should have a deterrence effect, as they represent the toughest action an antitrust authority can take. Similarly, one could argue that when the merger parties withdraw a notified merger, this might be interpreted as an 'almost-prohibition' (Bergman, Jakobsson, and Razo, 2005) and, therefore, this can be expected to have similar deterrence effects. The deterrence effects of remedies are not so clear cut and depend on whether they come at the expense of clearances or prohibitions: if the antitrust authority imposes remedies on mergers which were expected to be cleared unconditionally, this signals a tough antitrust stance while remedying mergers which were expected to be blocked signals that merger control has become more lenient. Hence, if merger policy effectively deters, one should expect the EC's actions to decrease the rivals' abnormal returns and therefore the likelihood of the merger being anti-competitive. Yet, the policy should not over-deter and thus none of the EC's actions should negatively affect the likelihood of pro-competitive mergers.

We estimate the model (5) on the full sample interacting the independent variables with pre- and post-reform dummies and adding a time trend and a post-reform dummy. The coefficient estimates of the multinomial probit estimation where welfare-neutral cases are the reference category are reported in table 9.

[insert Table 9 here]

We estimate a negative and significant coefficient for the prohibitions ratio in the pre-reform period for the anti-competitive outcome. When the EC increases the use of prohibitions in the two quarters prior to a newly notified merger, the likelihood of observing rivals' returns above $\bar{\pi}$ (anti-competitive merger) is significantly lower. This is not the case for clearly pro-competitive mergers. Thus, our interpretation is that prohibitions seem to deter but do not over-deter.

While remedies do not affect the odds of mergers hurting rivals post-reform (pro-competitive), phase 1 remedies deter mergers that benefit rivals (anti-competitive) and phase 2 remedies seem to encourage them. Both the phase 1 and phase 2 withdrawals ratios significantly reduce mergers that benefit rivals, with phase 2 withdrawals having the larger effect. Withdrawals in phase 1 also significantly discourage notifications of mergers that hurt rivals (pro-competitive). Once again, we cannot test for the effects of prohibitions post-reform, but withdrawals appear to at least partially take over their deterrent role. One possible interpretation of these findings is that firms were pushed by the EC to withdraw particularly problematic mergers by setting the anti-competitive concerns at such a high level that any kind of remedy would have become too costly. Hence, these withdrawals might effectively have been prohibitions.[18]

5. Conclusion

In our attempt to assess the economic impact of the change in legislation due to the 2004 merger policy reform in Europe, we brought forward four pieces of evidence: (1) estimation of the determinants of intervention, (2) estimations of the frequency and determinants of type I and type II discrepancies, (3) estimation of rent-reversion by merger decisions, and (4) estimation of the effect of past merger decisions on future mergers (deterrence). The identification of the reform's effects is achieved by comparing the performance of merger control along these four dimensions in the pre-reform and post-reform periods.

Our main findings can be summarized as follows. First, we find that the predictability of the antitrust procedure has improved. We observe an increase in the number of significant predictors of the probability of an action by the Commission, as well as increases of the R^2s and correct predictions between the two periods. This suggests that it has become easier for the market and the firms to form a prior about the outcome of the investigation.

Second, we observe that more mergers which do not significantly affect the profitability of rival firms have been proposed post-reform. Given our identification assumptions, we define these cases to be welfare-neutral and conclude that this result leaves less room for possible decision errors. Conditional on this finding, the percentage of type I discrepancies between the EC decision and the stock market assessment significantly decreased after the introduction of ECMR 04, independently of the threshold used to define pro-competitive mergers. The percentage of type II discrepancies, instead, slightly increased

[18]As noticed by Papanikolaou and Rosenthal (2011) 'if the parties and the Commission are unable to agree on remedies, a fairly common result is the withdrawal of the notification to avoid the publication of a negative decision.' However, since no ultimate decision is taken in the case of withdrawals, transparency and predictability may suffer.

or decreased depending on the adopted thresholds. We analyze the determinants of such discrepancies and find that merger characteristics as well as procedural issues systematically affect them. For instance, US firms seem to be treated more leniently than other firms.

Third, according to our rent-reversion regressions, remedies are not effective either before or after the reform. Only prohibitions achieve substantial rent-reversion, however, we can estimate their effect only pre-reform since only two mergers were blocked post-reform. Given the observed effectiveness of this merger policy tool compared to remedies, it appears that the EC blocks too few mergers.

Finally, we measure significant effects of past policy enforcement on the type of mergers proposed both pre- and post-reform. We interpret these results in terms of deterrence. Pre-reform, prohibitions achieve deterrence of anti-competitive mergers without deterring pro-competitive mergers, which confirms their role as most the effective merger control tool. Post-reform it appears that withdrawals and phase I remedies substitute for the role of prohibitions.

We propose several robustness checks to support our main identification assumptions and sample selection. Our main findings hold if we replicate our tests using different subsamples where i) we focus on purely horizontal mergers, ii) we use different thresholds to define pro- and anti-competitive mergers, iii) we select our sample to more closely mimic the population of EC mergers in terms of policy actions and, iv) we choose a different timing for the reform.

In conclusion, the introduction of the ECMR 04 seems to have changed European merger policy. Yet, in terms of effectiveness along our four dimensions we paint a mixed picture. On the one hand, we observe an increase in predictability and a decline in the frequency of type I discrepancies between the EC decision and the stock market assessment post-reform. On the other hand, we also find that the increased focus on remedies was only partially successful and cannot replace the policy tool of straight prohibitions. Clearly, this policy shift was not only the product of the reform. Foremost, it might be the persistent reaction to the substantial shock and political climate which originated from the CFI's reverses of three prominent cases in the early 2000s. Yet, an approach to merger control that is more firmly based on economic principles does not necessarily mean abandoning the use of prohibitions, as shown by US antitrust authorities which are far less hesitant to block mergers than their European counterpart. Thus, according to our analysis, the positive impact on the efficiency of European merger control is dampened by the fact that DG Comp deprives itself of its most powerful tool: prohibitions.

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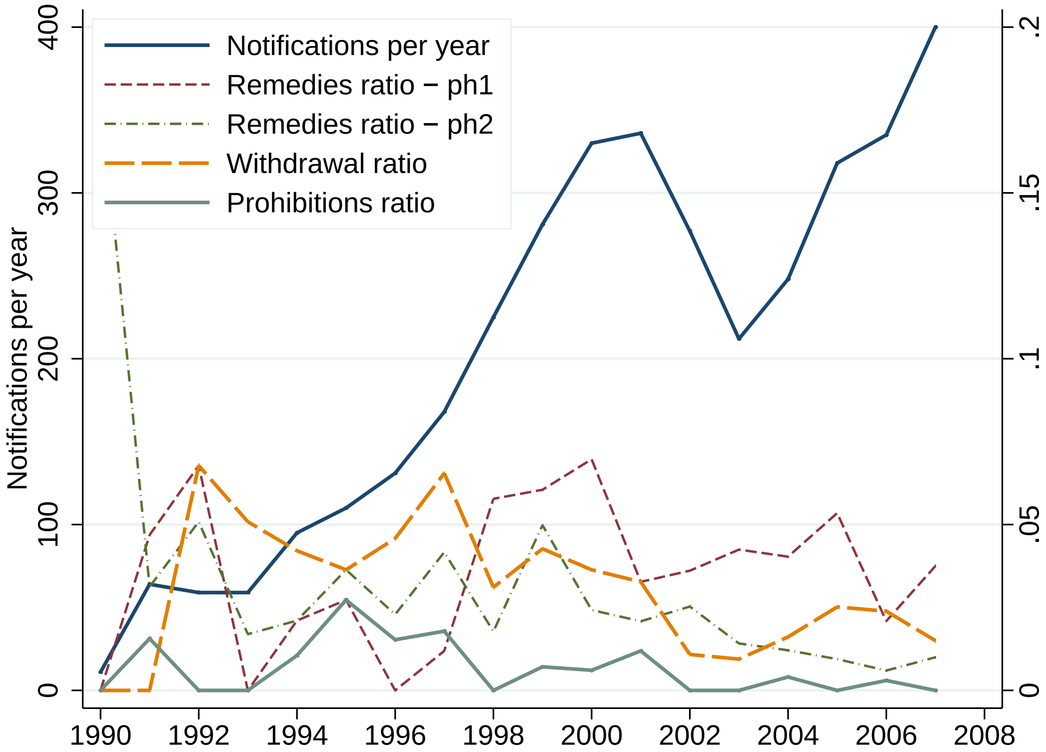
6. Figures and Tables

Figure 1: Evolution of cases and decisions in the population



We report notified cases per year (left axis) as well as the ratio of different decisions (remedies in phase 1 and phase 2, prohibitions) to the notified cases (right axis).

Table 1: Summary Statistics

	Sample				Population			
	Pre-		Post-reform		Pre-		Post-reform	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Remedies	0.421	(0.49)	0.336	(0.47)	0.069	(0.05)	0.055	(0.04)
in Phase 1	0.144	(0.35)	0.237	(0.43)	0.041	(0.04)	0.044	(0.03)
in Phase 2	0.278	(0.45)	0.099	(0.30)	0.028	(0.03)	0.011	(0.01)
Cleared	0.523	(0.50)	0.651	(0.48)	0.931	(0.05)	0.945	(0.04)
Prohibited	0.056	(0.23)	0.013	(0.11)	0.008	(0.01)	0.001	(0.00)
Phase2	0.421	(0.49)	0.178	(0.38)	0.055	(0.23)	0.032	(0.18)
Withdrawn					0.033	(0.03)	0.024	(0.01)
National markets	0.384	(0.49)	0.349	(0.48)				
EU-wide markets	0.407	(0.49)	0.414	(0.49)				
Worldwide markets	0.204	(0.40)	0.230	(0.42)				
Conglomerate merger	0.250	(0.43)	0.382	(0.49)				
Full merger	0.579	(0.49)	0.691	(0.46)				
Crossborder merger	0.671	(0.47)	0.717	(0.45)				
Barriers to entry	0.458	(0.50)	0.243	(0.43)				
Dominant firm	0.523	(0.50)	0.500	(0.50)				
US firms involved	0.315	(0.47)	0.316	(0.47)				
Big EU country	0.644	(0.48)	0.618	(0.49)				
MV merging	14.391	(5.02)	15.860	(6.22)				
MV rivals	16.628	(5.11)	17.923	(5.84)				
Observations	216		152		2403		1645	

Market values (MV merging, MV rivals) are reported as logs of 1000 USD.

Table 2: CAARs of merging parties and rivals by period and event

	Pre-Reform			Post-Reform		
	N	Mean	S.E.	N	Mean	S.E.
Merging Firms						
Announcement	200	0.016**	(0.01)	133	0.014**	(0.008)
Decision	197	−0.003	(0.009)	133	−0.008*	(0.005)
Rivals						
Announcement	208	0.008	(0.008)	147	0.008*	(0.006)
Decision	207	−0.003	(0.009)	147	−0.008*	(0.006)

The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

Table 3: Probit Model: Probability of Intervention

	Pre-Reform		Post-Reform	
US firms involved	−0.258***	(0.069)	−0.340***	(0.098)
Big EU country	0.005	(0.073)	−0.068	(0.096)
Conglomerate Merger	0.174*	(0.092)	0.200***	(0.050)
Full merger	0.212***	(0.043)	−0.055***	(0.016)
Crossborder merger	−0.101	(0.078)	−0.070***	(0.027)
Log(MV) merging firms	0.015***	(0.006)	−0.006**	(0.003)
Log(MV) rivals	0.012	(0.009)	0.019*	(0.011)
Lagged notifications	0.002	(0.002)	−0.001**	(0.001)
Lagged actions ratio	0.501	(0.622)	1.592	(1.010)
Lagged abortions ratio	−0.109	(1.137)	−1.106	(3.319)
Observations	211		152	
Pseudo R^2	0.19		0.25	
Correctly classified	70.6%		76.3%	

The dependent variable is action, equal to one when the merger is remedied or blocked, and zero otherwise. Marginal effects are reported. Standard errors in parentheses are robust and allow for correlation among observations from the same year. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively. All regressions include a set of industry dummies and a time trend.

Table 4: Breakdown by threshold

	Pre-Reform		**Post-Reform**		
	Cases	Share	Cases	Share	Difference
$\bar{\pi} = 0\%$					
Procompetitive	100	0.48	76	0.52	0.04
Neutral	0	0	0	0	0
Anticompetitive	108	0.52	71	0.48	-0.04
$\bar{\pi} = 3\%$					
Procompetitive	74	0.36	39	0.27	-0.09^{**}
Neutral	53	0.25	62	0.42	0.17^{***}
Anticompetitive	81	0.39	46	0.31	-0.08^{*}
$\bar{\pi} = 5\%$					
Procompetitive	56	0.27	24	0.16	-0.11^{***}
Neutral	85	0.41	90	0.61	0.2^{***}
Anticompetitive	67	0.32	33	0.22	-0.1^{**}
$\bar{\pi} = 10\%$					
Procompetitive	27	0.13	8	0.05	-0.08^{***}
Neutral	142	0.68	126	0.86	0.18^{***}
Anticompetitive	39	0.19	13	0.09	-0.1^{***}

Table 5: Type I/II discrepancies by period and definition of pro-/anti-competitivity

	Pre-Reform			Post-Reform			
	Cases	Mean	S.D.	Cases	Mean	S.D.	Difference
0% threshold							
Type I ($\Pi^A_{Cj} < 0$)	100	0.46	0.50	75	0.35	0.48	-0.11*
Type II ($\Pi^A_{Cj} > 0$)	108	0.52	0.50	71	0.65	0.48	0.13**
3% threshold							
Type I ($\Pi^A_{Cj} < -0.03$)	74	0.47	0.50	39	0.36	0.49	-0.11
Type II ($\Pi^A_{Cj} > 0.03$)	81	0.58	0.5	46	0.63	0.49	0.05
5% threshold							
Type I ($\Pi^A_{Cj} < -0.05$)	56	0.52	0.50	24	0.33	0.48	-0.19*
Type II ($\Pi^A_{Cj} > 0.05$)	67	0.57	0.50	33	0.58	0.50	0.01
10% threshold							
Type I ($\Pi^A_{Cj} < -0.10$)	27	0.56	0.51	8	0.25	0.46	-0.31*
Type II ($\Pi^A_{Cj} > 0.10$)	39	0.51	0.51	13	0.46	0.52	-0.05

Frequency of type I discrepancies (action in a pro-competitive merger) and type II discrepancies (unconditional clearance of an anti-competitive merger) in the sample.

Table 6: Probit Model: Probability of Type I discrepancies

	Pre-Reform		Post-Reform	
US firms involved	-0.196^{*}	(0.117)	-0.254^{**}	(0.105)
Big EU country	0.110	(0.070)	-0.040	(0.145)
Conglomerate Merger	-0.027	(0.049)	0.746^{***}	(0.194)
Full merger	0.258^{***}	(0.100)	0.174	(0.212)
Crossborder merger	-0.196^{*}	(0.118)	-0.182^{***}	(0.064)
Log(MV) merging firms	0.018^{*}	(0.010)	0.024^{**}	(0.010)
Log(MV) rivals	0.019^{*}	(0.010)	-0.017	(0.014)
Barriers to entry	0.289^{***}	(0.074)	0.366^{***}	(0.114)
Phase 2 Case	0.507^{***}	(0.175)	-0.028	(0.148)
National markets	0.339^{***}	(0.095)	0.724^{***}	(0.142)
Time trend	-0.005	(0.003)	0.008	(0.007)
Observations	73		39	
Pseudo R^2	0.74		0.65	
Correctly classified	90.4%		87.2%	

The dependent variable is one if $\Pi^{A*}_{Cj} < -0.03$ and merger j was remedied or blocked and zero otherwise (action in a pro-competitive merger). Marginal effects are reported. Standard errors in parentheses are robust and allow for correlation among observations from the same year. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

Table 7: Probit Model: Probability of Type II discrepancies

	Pre-Reform		Post-Reform	
US firms involved	0.134**	(0.059)	0.185***	(0.044)
Big EU country	0.057	(0.071)	0.141	(0.097)
Conglomerate Merger	−0.296***	(0.055)	−0.297***	(0.091)
Full merger	0.063	(0.075)	0.234***	(0.029)
Crossborder merger	0.041	(0.058)	0.143**	(0.063)
Log(MV) merging firms	−0.020***	(0.007)	0.010**	(0.005)
Log(MV) rivals	−0.013	(0.008)	−0.028***	(0.009)
Barriers to entry	−0.314***	(0.063)	−0.417***	(0.071)
Phase 2 Case	−0.306***	(0.058)	0.284***	(0.109)
National markets	−0.104**	(0.051)	−0.075	(0.202)
Time trend	−0.001	(0.003)	0.007	(0.010)
Observations	80		46	
Pseudo R^2	0.65		0.50	
Correctly classified	88.8%		82.6%	

The dependent variable is one if $\Pi_{Cj}^{A*} > 0.03$ and merger j was cleared and zero otherwise (unconditional clearance of an anti-competitive merger). Marginal effects are reported. Standard errors in parentheses are robust and allow for correlation among observations from the same year. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

Table 8: Effectiveness Regressions

	Merging Parties		Rivals	
Pre reform				
Clearance	0.037	(0.047)	−0.009	(0.054)
Phase 1 Remedy	0.114	(0.120)	−0.040	(0.135)
Phase 2 Remedy	−0.061	(0.094)	−0.028	(0.076)
Prohibitions	−0.085	(0.176)	−0.306**	(0.111)
Π_{ij}^{A*}*Clearance	−0.024	(0.067)	0.210***	(0.057)
Π_{ij}^{A*}*Phase 1 Remedy	0.012	(0.148)	0.563*	(0.281)
Π_{ij}^{A*}*Phase 2 Remedy	0.018	(0.111)	−0.369	(0.513)
Π_{ij}^{A*}*Prohibition	−1.265***	(0.423)	−0.442***	(0.101)
Post reform				
Clearance	0.031	(0.092)	−0.028	(0.091)
Phase 1 Remedy	0.103	(0.100)	−0.043	(0.084)
Phase 2 Remedy	−0.008	(0.126)	−0.081	(0.105)
Π_{ij}^{A*}*Clearance	0.039	(0.046)	0.278**	(0.112)
Π_{ij}^{A*}*Phase 1 Remedy	−0.109	(0.178)	0.009	(0.170)
Π_{ij}^{A*}*Phase 2 Remedy	0.998**	(0.466)	0.931***	(0.317)
Observations	325		349	
R^2	0.20		0.25	

The dependent variable is the decision corrected CAAR in merger j (Π_{fj}^{D*}) for the merging firms ($i = M$) and competitors ($i = C$) respectively. Standard errors in parentheses are robust and allow for correlation among observations from the same year. We control for merger-specific effects (full, crossborder and conglomerate mergers) and a time trend. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

Table 9: Deterrence Regressions

	Procompetitive		Anticompetitive	
Pre reform				
Lagged notifications	0.009	(0.012)	−0.003	(0.007)
Lagged remedies ph1	−7.639	(6.277)	2.207	(8.737)
Lagged remedies ph2	0.650	(10.116)	1.391	(10.535)
Lagged withdrawals ph1	27.063***	(8.495)	40.852***	(8.660)
Lagged withdrawals ph2	38.242**	(16.039)	55.054***	(20.016)
Lagged prohibitions	−38.672	(26.428)	−55.247**	(24.429)
Post reform				
Lagged notifications	0.023	(0.020)	−0.045***	(0.006)
Lagged remedies ph1	−6.721	(5.585)	−33.958**	(15.826)
Lagged remedies ph2	−48.243	(60.431)	32.446***	(5.809)
Lagged withdrawals ph1	−87.640***	(10.941)	−77.644***	(20.559)
Lagged withdrawals ph2	101.736	(123.704)	−106.109***	(29.293)
Post Reform 2004	0.542	(1.067)	5.670***	(1.149)
Time trend	−0.017	(0.026)	0.017	(0.017)
Observations	347		347	
Pseudo R^2	0.11		0.11	

The dependent variable is $D_j = 1$ if $\Pi_{Cj}^{A*} \leq 3\%$, $D_j = 2$ if $3\% \leq \Pi_{Cj}^{A*} \leq 3\%$, and $D_j = 3$ if $\Pi_{Cj}^{A*} \geq 3\%$. Standard errors in parentheses are robust and allow for correlation among observations from the same year. We control for merger-specific effects (full, cross-border and conglomerate mergers). The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

A. Quantifying the Effect of a Merger and Merger Decision

The estimation of the impact of a merger and merger decision proceeds in several steps. First, we estimate a market model for each firm which allows us to simulate the counterfactual scenario of what would have happened if the merger had not occurred. Using this information we then calculate the cumulative abnormal rents generated by the merger or merger decision over an event window spanning several days around the relevant dates. We then aggregate the cumulative abnormal returns for the merging firms and their rivals, to obtain a merger-specific information. Finally, we assume that market participants can - to a certain degree - foresee the merger decisions, which is priced in the stock of firms around the relevant event. Hence, to obtain a more precise measure of the competitive effect of the merger and merger decision, we correct for these market expectations.

A.1. The Market Model

Define $R_{i,j}$ as the return of firm i at date j and $R_{market_i,j}$ as the market return index of the branch of firm i.[19] The market model predicts that the daily return of a commodity i is proportional to the market index at any given point in time t. Formally: $R_{i,t} = \alpha + \beta R_{market_i,t} + \varepsilon_{i,t}$.[20] We can then estimate the coefficients of this model for all firms $i = 1, \ldots, N$ over a time period of 240 trading days, namely the period from 290 to 50 days prior to the announcement of the merger.[21] Letting the estimation window end 50 days before the announcement (that is, the date on which the financial press wrote about the proposed transaction) should yield unbiased estimates of the market model's coefficients and, hence, the 'normal' firms' return, which is our counterfactual and that is given by: $\widehat{R}_{i,t} = \widehat{\alpha} + \widehat{\beta} R_{market_i,t}$. When observing a stock market reaction to the announcement of a particular event, the change in the equity value (with respect to the 'normal' value) of firms affected by this event can then be taken as a measure of the (discounted) additional profits that are expected to accrue as a consequence of the event. This stock reaction, also called abnormal return, is a measure of the profitability of such an event and can be measured as $AR_{i,t} = R_{i,t} - \widehat{R}_{i,t}$.

[19] We use the total return index from Datastream, which accounts for dividends and corrects for stock splits.

[20] For the superiority of a market model over a constant mean return model in capturing abnormal returns see MacKinlay (1997) or Schwert (1981).

[21] For some cases the market model could not be reliably estimated in this period due to data limitations. In these cases the estimation window was shifted to 530-290 days prior to announcement.

A.2. The Event Windows

The event windows are the time intervals around the dates of the relevant events (e.g., merger or merger decision), during which new information hits the market. In the absence of any information leakages, these windows can be reduced to the event day. The larger the expectation that some information was leaked to the market prior to the event, the larger the window should be. Hence, the length of these windows is critical to the event study's ability to capture the profitability effects: if the window is too small, the effect might not be wholly captured, whereas too large a window could dilute the result.[22] To account for the structurally different circumstances of the various events we consider, we use both a *long* as well as a *short window*. The long window is the interval $[t-50, t+5]$ (where t designates the date of the event), the short window is $[t-5, t+5]$.

For the announcement and the phase 2 decision, we employ the long window. In both cases information leakages could occur substantially earlier than the date of the event in question. Rumors of mergers often circulate for weeks before definitive signs reach the financial press. The same holds for an in-depth merger investigation in phase 2, during which the Commission often contacts competitors and customers of the merging firms during its assessment and information is likely to leak to the market.[23] These prolonged processes could easily reduce uncertainty and allow the concerned parties to adjust their anticipations.

The phase 1 investigation, on the other hand, lasts only 25 working days and is conducted internally by DG Comp. Furthermore, a substantial part of this relatively short time is utilized for the appraisal of administrative issues. We therefore assume that information leakages to the market occur no earlier than five days before the decision and that the stock prices adjust in a short window around the decision. The event windows are schematically depicted in figure 2.

A.3. Aggregating the Abnormal Returns

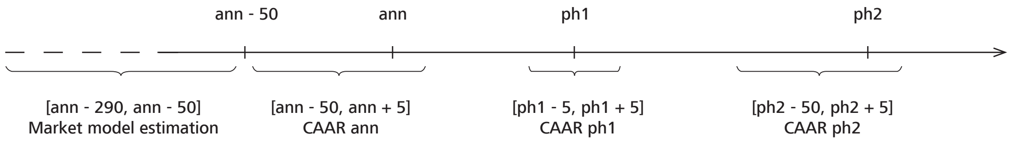
The abnormal return of firm i at date j is defined as

$$AR_{i,j} = R_{i,j} - \hat{R}_{i,j}.$$

[22]Issues concerning the length of event windows and their ability to capture the effect of regulation are more thoroughly discussed in Duso, Gugler, and Yurtoglu (2010).

[23]The EC has a time-frame of 90 working days between phase 1 and phase 2 decisions.

Figure 2: Timeline of the events



The cumulative abnormal returns (CAR) are then obtained by summing up the abnormal returns over the event window (t_1, t_2):

$$CAR_i(t_1, t_2) = \sum_{t=t_1}^{t_2} AR_{i,t}.$$

These CARs measure the profitability impact of a combination at the firm-level. Measuring firm-level effects has the advantage of allowing for asymmetric externalities of a merger.[24] While we allow for asymmetric externalities at the firm-level, the definition of an anti-competitive merger has to be done at the aggregate level, since what matters for the policy is the impact of the merger on the overall consumer surplus. Hence, to obtain a measure of the total impact of a merger, we aggregate the merging firms' as well as rivals' CARs at the merger level by using the relative market value of each firm as a weight.[25] The cumulative average abnormal returns (CAAR) at event e (announcement, decision) for firms f ($f = M$ for merging firms and C for their competitors) in merger j are then given by

$$CAAR^e_{fj} = \frac{\displaystyle\sum_{i=1}^{N_{fj}} CAR^e_i \, mv_i}{\displaystyle\sum_{i=1}^{N_{fj}} mv_i} \qquad e = ann, dec \qquad f = M, C \quad j = 1, \ldots, 368 \tag{6}$$

where N_{fj} denotes the number of merging firms or rivals for merger j and mv_i is the market value of firm i.

[24] It is an empirically well-documented phenomenon that merger targets usually experience stock market gains, whereas buyers often lose. Likewise, the externalities on rivals need not be evenly distributed as the degree of competition among firms might vary.

[25] The idea of a 'firm portfolio' weighted by market values is owed to Schwert (1981).

A.4. Correcting for Expectations

We assume that market participants can to a certain degree anticipate the decisions of DG Comp, but that there is no perfect foresight: If the market could perfectly foresee the actions of the EC, there would be no significant stock reactions around the decision dates. The fact that there are significant deviations from the market trend when news of a decision reaches the concerned market participants can be interpreted as evidence in favor of our assumption. Furthermore, the existence of prohibitions contradicts perfect foresight: if managers could perfectly foresee the actions of DG Comp, mergers that end up being blocked would not have been attempted in the first place, nor would there have been significant reactions in response to their announcements.

Using the past merger control history and the knowledge of the structural characteristics of a proposed merger, firms can form a prior of how likely it is that DG Comp will intervene. This means that the observed abnormal returns around the event dates do not measure the full effect but are the expectation-adjusted abnormal returns, which take into account that the combination might not go through or might be subjected to remedies.[26] Since we assume that the market's assessment reveals the competitive nature of a combination, we would like to remove this adjustment of expectations to obtain the market assessment in absence of merger control.

If expectations are rational, the expected value of the EC's decision is:[27]

$$E[\Pi^{dec}] = \rho\Pi^{action} + (1-\rho)\Pi^{clear} \tag{7}$$

where $\Pi^{action}(\Pi^{clear})$ denotes the merger's profitability in case of an action (a clearance) and ρ is the probability of an action. The observed abnormal returns around the announcement (Π^{ann}) therefore are equal to the truly competitive effect (Π^{ann*}) plus the expected value of the EC's final decision ($E[\Pi^{dec}]$). Assuming that an intervention by DG Comp destroys the anti-competitive rents generated by a combination ($\Pi^{action} = -\Pi^{ann*}$) in their full extent ($\Pi^{dec*} = \Pi^{ann*}$),[28] and that a clearance has no further effect on the market ($\Pi^{clear} = 0$), the impact of a merger can be written as:

[26] An (extreme) example of a prohibition might clarify the intuition. If we measure a rent of \$100 million around the merger announcement, but the ex-ante expectation of the market is that the EC will block this merger with a probability of 20%, the full extent of rents is actually $(100/(1-0.2)) =$ \$125 million.

[27] Note that, to ease notation, we eliminate the subscript for the firms' types ($f = M$ for merging firms and $f = C$ for competitors) and the merger j.

[28] We realize that this assumption might be questioned, but it is necessary for probability correction and seems less arbitrary than ex-ante assuming a certain non-zero degree of rent reversal.

$$\Pi^{ann} = \Pi^{ann*} + E[\Pi^{dec}] = \Pi^{ann*} + \rho \underbrace{\Pi^{action}}_{=-\Pi^{ann*}} + (1-\rho) \underbrace{\Pi^{clear}}_{=0} \Leftrightarrow \Pi^{ann*} = \frac{\Pi^{ann}}{1-\rho} \quad (8)$$

Similarly, the effect that we measure around the decision (Π^{dec}) is an update of the market's beliefs concerning that particular decision and, hence, the difference between the merger's competitive effect and the market expectations of the Commission decision's effect.[29]

$$\Pi^{dec} = \Pi^{dec*} + E[\Pi^{dec}] = \Pi^{dec*} + \rho\Pi^{action} + (1-\rho)\Pi^{clear} \Leftrightarrow \Pi^{dec*} = \frac{\Pi^{dec}}{1-\rho}$$

If a case goes into phase 2, the market will again update its beliefs about remedies.[30] The effect around the phase 1 decision accounts for the adjustment of market expectations to the new state of beliefs, the sum of both decision effects captures the total impact of the EC's decision. The real effect of the decision is then given by

$$\Pi^{dec*} = \frac{\Pi^{P1} + \Pi^{P2}}{1-\rho}$$

where Π^{P1} (Π^{P2}) is the measured effect around the phase 1 (phase 2) decision date.

Combining the equations for the decisions yields

$$\Pi^{dec*} = \begin{cases} \frac{\Pi^{dec}}{1-\rho} & \text{if phase 1 case} \\ \frac{\Pi^{P1}+\Pi^{P2}}{1-\rho} & \text{if phase 2 case} \end{cases}. \quad (9)$$

Thus, to account for expectations, we need to estimate the ex-ante likelihood of an intervention for every merger j (ρ_j) and correct the CAARs measured around the announcement (Π^A_{fj}) and the decision (Π^D_{fj}) of that merger according to equations (8) and (9). This refinement improves the precision of the estimate of the market competitive assessment of a merger.

B. Robustness Checks

B.1. Selection of phase 1 remedies

Our sample includes almost all phase 2 cases investigated during the sample period (except those we were forced to drop due to lack of data). These cases are complemented

[29]If the market had perfect foresight, we would measure only white noise around the decision. The surprise value of the decision is due to the private information generated during the legal proceedings.

[30]The probability of a clearance subject to conditions and obligations is much higher for phase 2 cases than for phase 1 cases; a blocking is possible only after a phase 2 investigation.

by a sample of mergers decided in phase 1, where we intentionally over-represent cases cleared with remedies in comparison to the population: while in the population only 4.1% and 4.4% of cases are subjected to phase 1 remedies in the pre- and post-reform periods, these percentages increase to 14.4% and 23.7% respectively in the sample (see table 1). This can be justified on the grounds that it makes sense to look at instances where the EC actually took actions when attempting to assess the effectiveness of the merger policy tools.

This sampling strategy might, however, introduce an element of sample selection that could potentially bias our results.[31] To rule this out, we propose the following robustness check: we retain all phase 2 cases which represent almost the entire population, but only keep a random sample of phase 1 cases which are representative of the entire population. Hence, we randomly drop cases that were cleared with remedies in phase 1 until the percentages of phase 1 remedies in the sample match those in the population.[32] After dropping 55 cases remedied in phase 1, the remaining percentage of these cases amounts to 3.6% in the pre-reform and 4.1% in the post-reform period; as close to the population values as the discrete nature of the dataset permits us to come.

We reiterate all four steps of our effectiveness analysis using this reduced sample and briefly report the results here. The finding that the ex-ante predictability of jurisdiction increases is reinforced: the pseudo R^2 increases from 0.18 to 0.28 (compared to 0.19 to 0.25 in the full sample) between the two periods and the number of correctly predicted cases increases from 72% to 84% (71% to 76% in the full sample). The number of welfare-neutral mergers still increases by 16% post-reform (17-20% in the full sample), and we still find that there are significantly fewer type 1 discrepancies after ECMR 04 (14% less in the reduced sample vs. 11-31% less in the full sample). The models on the determinants of decision discrepancies suffer from the reduced sample size, but the principal determinants remain the same. In the effectiveness regressions we still find that prohibitions effectively reverse anti-competitive rents and that the effectiveness of remedies is limited. The results on deterrence remain qualitatively unchanged: prohibitions deter anti-competitive mergers prior to ECMR 04, withdrawn notifications achieve a similar effect after the reform.

Therefore, reproducing our results in a sample that more closely resembles the population corroborates our findings on predictability, decision discrepancies, rent-reversion, and deterrence.

[31] We thank an anonymous referee for stressing this point.

[32] This was achieved by the use of a random number generator in STATA and it was repeated several times to rule out that the findings depend on the particular sample obtained. While there was some variance in the results, all the findings reported below were robust across multiple, random samples.

B.2. Purely Horizontal Mergers

The correspondence between the change in consumer surplus and competitors' profits does not necessarily hold for non-horizontal mergers. In all regressions we control for this issue by using a dummy equal to 1 for all cases in which the Commission mentioned conglomerate, vertical, or foreclosure effects as one of its leading arguments in support of the final decision. It should be noted that all mergers have a strong horizontal dimension, but these cases in addition have some non-horizontal aspects. In this section we discuss the results that we obtain by dropping these 112 cases from our sample, which leaves us with 162 mergers pre-reform and 94 post-reform.

While some of the significant results for the political dummies and market value variables change, the model of the probability of an intervention is not strongly affected by this. The results on decision errors remain qualitatively unaffected as well. The reduction in type I discrepancies after the reform, ranging between 15% and 36% remains significant for most definitions of $\bar{\pi}$, the finding that more welfare-neutral mergers are notified after the reform remains as well.

When re-estimating the rent-reversion regressions in the purely horizontal sub-sample, the finding that prohibitions achieve a large degree of rent-reversion is confirmed, but the merging parties' coefficient loses its significance due to higher standard errors. The slope coefficients of unconditional clearances in the rival regressions increase and become significant in both periods. Thus rivals seem to profit more from cleared purely horizontal mergers, which is consistent with economic theory. The slope of phase 1 remedies is significantly negative in the rival regressions in both periods and significantly negative for merging firms in the pre-reform period. It appears that phase 1 remedies are more successful when applied to purely horizontal combinations.

The deterrence of pro-competitive mergers in the purely horizontal sub-sample is largely consistent with the results from the full sample, even though some coefficients are less significant. The results on the deterrence of anti-competitive mergers hold qualitatively as well, although we find fewer significant variables in the post-reform period: only phase 1 remedies and phase 1 withdrawals significantly deter anti-competitive notifications.

Overall we conclude that focusing on purely horizontal mergers does not alter the qualitative results and even reinforces some of them.

B.3. The Timing of the Reform

To identify the effect of the reform, we choose the official date at which it legally came into force as a marking point for the pre- and post-reform periods. This choice of timing has a clear justification, since the EC could not have used the legal framework provided by ECMR 04 before this date. However, there might be reason to think that the right timing to assess the change in policy could have been before or after this date. On the one hand, it could have been before, because some of the reform's elements were implemented during the months antecedent to the legal introduction of the new merger regulation and could have affected the Commission's policy enforcement.[33] On the other hand, the right timing to start the reform's assessment could also have been after May 2004, since it might have taken time before some of the innovations brought by the reform had a clear policy impact. Hence, we propose two robustness checks for this issue. First, we date the starting of the post-reform period back to the beginning of 2003. Second, we eliminate the entire year 2004 from the sample.

In both cases, the results on the predictability of the policy pre-reform do not change substantially. The changes in the frequency of type I and type II discrepancies become small and insignificant if we define the post-reform period as starting at the beginning of 2003. This could hint at the fact that 2003 still belongs to the 'old' regime and including it in the post-reform period conceals the changes. Conversely, when dropping 2004 from the sample, the changes in the decision discrepancies remain quantitatively similar, although the significance of these findings is slightly reduced. The rent-reversion regressions are not strongly affected by either change in timing. Finally, choosing the beginning of 2003 as the introduction year makes phase 2 withdrawals lose their post-reform deterrent properties found in the main specification, while phase 1 withdrawals continue to significantly deter anti-competitive mergers. However, when we exclude the entire year 2004, the findings obtained in the main regressions are reproduced and their significance is restored.

All in all then, it seems that our qualitative results also hold if we adopt another date for the formal introduction of the merger policy reform. However, results are more significant, clear cut and in line with our main specification when we exclude the year 2004. This suggests that the change in policy around the legal introduction of ECMR 04 was substantial and supports the choice of May 2004 to identify the effects of the reform.

[33] Lyons (2004), for instance, notices that several changes in merger control were being implemented around 2003, such as the introduction of devil's advocate panels, the proposal of a clarification of the dominance test, the appointment of the first chief economist, the publishing of the draft merger guidelines, and the extension for timetables for remedies.

B.4. Different thresholds for pro- and anti-competitivity

The choice of the threshold beyond which a merger is labeled as pro- or anti-competitive - i.e., the choice of $\bar{\pi}$ - is an arbitrary one. Without measurement error, the most natural choice would be a threshold of $\bar{\pi} \lesseqgtr 0$; however, we expect a degree of noise in the data. In the main body of the paper, we reported the frequency of decision discrepancies when using either a 0, 3%, 5%, or 10% threshold, while the determinants of decision discrepancies and the deterrence regressions were restricted to the specifications with $\bar{\pi} = 3\%$. This choice of threshold was motivated by the fact that a zero threshold possibly implies too much noise and it might thus bias the results toward insignificance, while the two higher threshold choices strongly limit the number of available observations. The following paragraphs describe the regression results, when these alternative thresholds are applied.

The determinants of decision discrepancies do not qualitatively change when we choose a threshold of $\bar{\pi} = 0$; however, for thresholds of 5% or 10% the determinants cannot be estimated due to a lack of observations, particularly in the post-reform period.

The two main findings of the deterrence regressions – that prohibitions in the pre-reform period and phase 2 withdrawals in the post-reform period significantly deter anti-competitive merger notifications – are robust if we choose $\bar{\pi} = 5\%$. For a threshold of 0, only a binary probit model can be estimated, which confirms the post-reform deterrence exercised by phase 2 withdrawals. Setting the threshold at $\bar{\pi} = 10\%$ leaves too few pro- and anti-competitive outcomes for the multinomial probit model to converge.

All things considered, the choice of $\bar{\pi} = 3\%$ appears to be a good compromise between measurement error and sample size.