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AN EMPIRICAL ASSESSMENT OF THE 2004 EU MERGER POLICY REFORM*

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We evaluate the economic impact of the change in European merger legislation in 2004 and propose a general framework focusing on four different policy dimensions: predictability, decision errors, reversion of anti-competitive rents and deterrence. We find that after the reform, the predictability and the accuracy of decisions have improved. Yet, the policy shift away from prohibitions, which entail both an immediate and a deterrent effect, does not seem to be well grounded.

The modernisation 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 rigour 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 was published in December 2001.

Numerous important changes were made to achieve an approach in merger control closer to economic principles: an efficiency defence 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 favour of the 'significant impediment of effective competition test' (SIEC).[2] The reception of the new merger regulation was generally favourable, yet a first assessment of its effects is still missing.

In this article, we propose a framework for empirically identifing some robust tendencies in the effectiveness of EU merger policy along different dimensions. We analyse 368 mergers covering most of the major cases scrutinised 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

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[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).

setting, which imply that horizontal mergers benefitting competitors are detrimental to consumers (Farrell and Shapiro, 1990). To operationalise this concept empirically 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.

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 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 *et al.*, 2007). We distinguish cases in which DG Comp remedied mergers that hurt the rivals and therefore can be assumed to be pro-competitive (weak type I discrepancies) from unconditionally cleared mergers that benefited 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 *et al.*, 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 significantly hurting rivals (pro-competitive mergers), significantly benefiting rivals (anti-competitive mergers) or not significantly affecting 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.

[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.

After 2004, the deterrence properties of prohibitions are partially replaced by those of withdrawn mergers and phase I remedies.

The article proceeds as follows. Section 1 discusses the assumptions which allow us to achieve an empirically implementable identification of anti-competitive mergers. Section 2 presents the sources of the data, some summary statistics and the estimation of the merger announcement and merger decision effects by means of stock market event studies. Section 3 presents the empirical tests for the four dimensions of effectiveness pre- and post-reform and the results. Section 4 concludes the article.

1. 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, first, to define the anti-competitive nature of a merger theoretically and, second, to measure it empirically. 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, in the absence of 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. to 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 is a critical level of merger-specific efficiency gains such that the market power effect is exactly compensated 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 (CS) 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 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). Moreover, in a dynamic setting, the theoretical identification depends on the sequence of notified mergers and is therefore

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

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, that is, a stock market event study, to measure the change in profits of rival 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 actual and the predicted 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 for details).[5]

Clearly, the measured CAARs around a merger's announcement might entail effects other than the merger's pure competitive effects – that is 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 towards 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 could contain the effects of other specific forces triggering the 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). Furthermore, dynamic aspects such as the occurrence of merger waves or the information conveyed in previous EC decisions could influence measurement. 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 an in-depth antitrust investigation. This is a large advantage of our data compared to previous event studies in merger control and addresses one of the main original critiques advanced against the pioneering work by Eckbo (1983), Stillman (1983). Second, we claim that we can control for the merger's triggering events and the

[5] As we discuss in Duso *et al.* (2010), one might use different empirical methodologies to measure the change in profits. In particular, there we build on Gugler et al. (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.

allocation of roles of the firms (acquirer, target, rival), by choosing the right announcement dates and event windows. We use the date of the first merger-specific rumours 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 rumours 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 of firms (Fridolfsson and Stennek, 2010). 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, the agency's enforcement history and the number of merger notifications and then use the predictions of this model as a measure of the market expectations to calculate the 'corrected CAARs' (Duso *et al.*, 2011, and the online Appendix). This helps to control for expectations regarding the specific merger (problematic and unproblematic acquisitions), the current state of antitrust enforcement ('tough' and 'lenient' enforcement periods) as well as the overall degree of M&A activity (merger waves and troughs).[7]

A number of additional robustness checks for this identification strategy are proposed and conducted in Duso *et al.* (2011). Using a subsample of the data employed in this article, we corroborate our findings in a post-1996 sample to account for the effect of the 1996–2002 merger wave; dividing the data into subsamples of low and high merger activity to control for industry-specific merger waves; controlling for the potential contamination of CAAR measurement due to a long delay between merger announcement and official notification by differentiating between slow and speedy notifications; in a subsample of remedy-intensive industries, which are frequently subjected to antitrust actions, to control for problematic industries and learning effects; and dropping cases where the EC's decision was not in accord with the official timetable, to control for uncertainty created by early or late decisions. The results suggest that running the model in subsamples where the assumptions are less likely to hold does not seem to have a significant impact on the findings. We therefore do not replicate all checks in this article.

Given this empirical set-up, 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 summarise, from a theoretical point of view, we classify a merger as being 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 – that is Π_{Cj}^{A*} exceeds a certain threshold $\bar{\pi}$. Symmetrically, a merger is classified as pro-competitive if

[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 rumours.

[7] As an additional test on whether merger waves do influence CAARs, we correlate them with the corresponding yearly number of notifications (depicted in Figure 1). We find very low correlation coefficient for announcement CAARs (0.02) and decision CAARs (−0.017), which are not significantly different from zero (p-values of 0.698 and 0.737 respectively).

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 article, we adopt an intermediate threshold of $\pm 3\%$.[8] In the online Appendix, we discuss the robustness of our results to the use of different threshold values.

Thus, our methodology addresses a wide range of potential problems in the identification procedure. However, it would still be illusory to assume that our approach is able to completely remove all other possible sources of measurement error that would affect the individual firm's CARs. While there undoubtedly remains a degree of noise in the data, we believe that the choice of the appropriate event windows, the averaging of the individual CARs across all competitors, the probability correction procedure, the use of different threshold values to define anti-competitive merger and, most fundamentally, the use of the CAARs in a cross-sectional regression framework with many hundreds of observations, makes our identification strategy as solid as possible.

A few examples might illustrate the empirical relevance of our identification strategy. On 12 November 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 Heathrow airport, and that the merger would make matters worse particularly concerning take-off and landing slots (e.g. AFX News, 13 November 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, for example, Jordan Robertson, 12 November 2009, AP Technology, 'HP's 3Com takeover marks a shot at Cisco'.

2. Data

Figure 1 gives a representation of the evolution of notifications and actions in the population of over 3,800 mergers analysed 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.

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 number 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

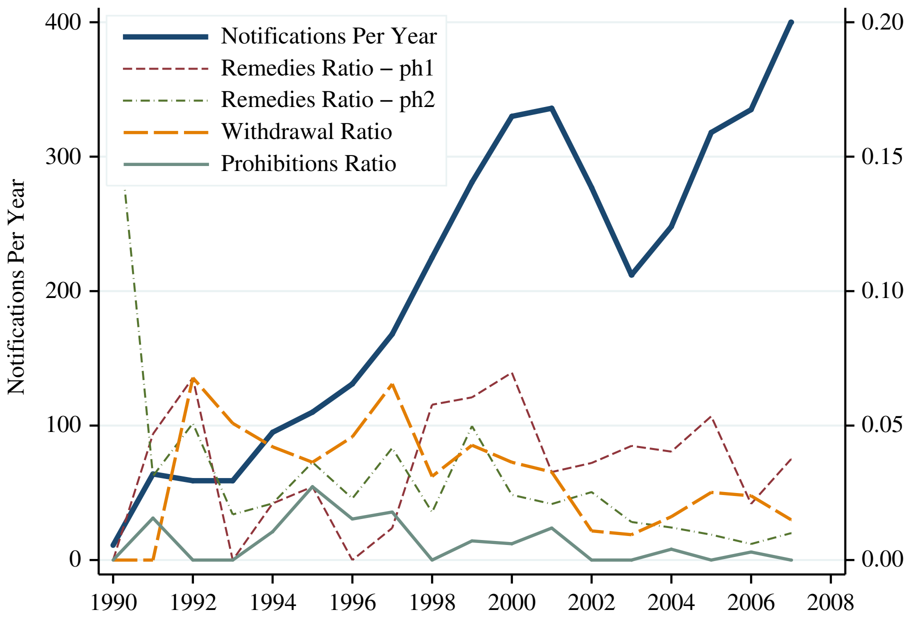

Fig. 1. *Evolution of Cases and Decisions in the Population*

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

[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 when we were unable to obtain stock market valuations.

[11] In a robustness check in the online Appendix, 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.

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 of the industry, 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 rumours about 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 summarises the variables in our data and the dynamics of the sample and population for the periods before-and-after the merger policy reform.

Table 1

Summary Statistics

	Sample				Population			
	Pre-reform		Post-reform		Pre-reform		Post-reform	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
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
Phase 2	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		2,403		1,645	

Note. Market values (MV merging, MV rivals) are reported as logs of US$000.

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, whereas 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 cleared without conditions and obligations. Thus, while we over-sample cases that 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 geographical 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 crossborder 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 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.

Table 2

CAARs of Merging Parties and Rivals by Period and Event

	Pre-reform			Post-reform		
	N	Mean	SE	*N*	Mean	SE
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

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

[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.

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.

3. Methodology and Results

In this Section, we describe the four steps of our effectiveness measurement and present the related empirical results. The objective of this article was to use this framework to measure the impact of the modernisation 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 analyses 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 analyse the frequency and determinants of the discrepancies between the EC decisions and the market predictions that we call (weak) type I and type II errors following Duso *et al* (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 behaviour of other firms. We therefore analyse 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.

3.1. *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

[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, we show that our results are robust to the use of different dates and discuss the appropriateness of our choice.

merger. Note that for the estimation of this model none of the assumptions related to theidentification 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, crossborder 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.

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 from 71% to 76% in the post-reform period. In the pre-reform period, we observe four

Table 3
Probit Model: Probability of Intervention

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

Notes. 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.

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

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 crossborder 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.

It is, however, likely that numerous other relevant variables are missing in our regression model. The firms involved in a merger would typically have detailed information on their and their competitors' market shares (which we can only proxy for), the competitive situation in the industry, structural characteristics such as ease of entry and switching costs and other factors influencing the Commission's decision process. Thus, the above estimates of predictability should be interpreted as a conservative lower bound of the firms' ability to foresee the outcome of an investigation and a causal interpretation of the individual coefficients should be avoided. Since we use the model solely for prediction purposes, this should be less problematic in our case.

3.2. *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 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 – that is, $E1_j = 1$ 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 – that is $E2_j = 1$ 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 CS. 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-maximising merger control decision is:

[15] The notion of type I errors we use here corresponds to the weak type I errors in Duso *et al.* (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 – that is, pro-competitive mergers that are blocked.

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

where Π_{Cj}^{A*} 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 concept in an empirical context, we call them discrepancies (between the Commission's decision and the stock market's assessment). Within the framework of assumptions described above, the identification of type II discrepancies is unambiguous, especially when we use a demanding threshold: if the merger rivals experience significantly positive abnormal returns in response to a merger announcement, the merger's anti-competitive effect dominates and the EC should have remedied or blocked the transaction. If it did not, a type II discrepancy occurred. 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_{Cj}^{A*}, it might still entail some anti-competitive effects which should be remedied. 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 as being welfare-neutral.

Table 4

Breakdown by Threshold

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

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

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 subsection 3.4.

On the basis of 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.

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.

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

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

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

Table 5

Type I/II Discrepancies by Period and Definition of Pro/Anti-competitivity

	Pre-reform			Post-reform			
	Cases	Mean	SD	Cases	Mean	SD	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

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

We consider a number of potential determinants of these discrepancies: as claimed by Aktas et al. (2007), the European Commission might be protectionist and favour European *versus* US firms; hence, the country of origin of the merging parties might be a determinant of the 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 (Neven *et al.*, 1993; 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 (Bresnahan and Salop, 1986; Gugler and Siebert, 2007), whereas a crossborder 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 the EU or worldwide (Neven *et al.*, 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 subsamples. While the use of previous estimates (the CAARs) in the construction of the dependent variable could induce a bias due to misclassification (Hausman, 2001), we find that our classifications do not strongly depend on the choice of threshold CAAR (Table 5) and that regression results are robust with respect to the threshold's choice as well (see online Appendix). The regression results of (2) are reported in Table 6.

If one of the merging parties is a US-based firm, the likelihood of eliciting an action in mergers that clearly penalise 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 the case of crossborder 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 no longer correlates with the likelihood of a discrepancy, 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 (3), the determinants of type II discrepancies. The marginal effects of the probit estimations are reported in Table 7.

Table 6

Probit Model: Probability of Type I Discrepancies

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

Notes. The dependent variable is one if $\Pi_{Cj}^{A*} < -0.03$ 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.

Significantly more type II discrepancies in mergers involving US firms both pre and post-reform (13% and 19% respectively) are estimated. Conglomerate mergers are 30% less likely to result in a type II discrepancy both pre and post-reform. Full and crossborder 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 geographical 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.

Table 7

Probit Model: Probability of Type II Discrepancies

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

Notes. The dependent variable is one if $\Pi^{A*}_{Cj} > 0.03$ 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.

3.3. *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 *et al.* (2011) is that there should be a reversion of the (anti-competitive) rents measured around the merger

[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). Also in this case, Duso *et al.* (2011) discuss this assumption in more depth.

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:

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

where Π_{fj}^{D*} is the probability-corrected decision CAAR of merging firms ($f = M$) and competitors ($f = C$), respectively, for merger j, while Π_{fj}^{A*} is the probability-corrected 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 *et al.* (2011) explain in depth the sizes and signs of the intercepts and slopes that 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 = clearance. 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 (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 use of generated regressors as right-hand side variables – the CAARs – might cause measurement error that leads to attenuation bias. Thus, we use bootstrapping to correct the standard errors of the regression.[17] The regression results reported in Table 8 for the pre-reform period are in line with those obtained by Duso *et al.* (2011) for the years 1990–2002.

Pre-reform, 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 estimate a positive coefficient for the rivals in the case of clearances, suggesting that cleared mergers 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 rivals. Unconditional clearances still have a positive impact on the rivals' returns.

[17] We thank an anonymous referee for pointing this out. Additionally, to check the robustness of the coefficient estimates, we re-run the regressions in 500 bootstrapped samples and calculate the average coefficients and obtain very similar estimates. These results are not reported here but are available upon request.

Table 8

Effectiveness Regressions

	Merging parties	Rivals
Pre-reform		
Clearance	0.037	−0.009
	(0.052)	(0.058)
Phase 1 remedy	0.114	−0.040
	(0.128)	(0.134)
Phase 2 remedy	−0.061	−0.028
	(0.093)	(0.080)
Prohibitions	−0.085	−0.306**
	(0.199)	(0.121)
Π_{ij}^{A*} × clearance	−0.024	0.210***
	(0.086)	(0.081)
Π_{ij}^{A*} × phase 1 remedy	0.012	0.563*
	(0.207)	(0.294)
Π_{ij}^{A*} × phase 2 remedy	0.018	−0.369
	(0.133)	(0.595)
Π_{ij}^{A*} × prohibition	−1.265**	−0.442***
	(0.624)	(0.171)
Post-reform		
Clearance	0.031	−0.028
	(0.097)	(0.098)
Phase 1 remedy	0.103	−0.043
	(0.117)	(0.090)
Phase 2 remedy	−0.008	−0.081
	(0.134)	(0.114)
Π_{ij}^{A*} × clearance	0.039	0.278*
	(0.056)	(0.165)
Π_{ij}^{A*} × phase 1 remedy	−0.109	0.009
	(0.414)	(0.186)
Π_{ij}^{A*} × phase 2 remedy	0.998	0.931**
	(0.689)	(0.411)
Observations	325	349
R^2	0.20	0.25

Notes. 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 bootstrapped (500 repetitions), 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.

3.4. *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 (Seldeslachts *et al.*, 2009; Clougherty and Seldeslachts, forthcoming). Using the theoretical identification discussed in Section 1, 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 ($\widetilde{D}_{jt}$) that takes on a value of 1 if the merger significantly hurts rivals ($\Pi^{A*}_{Cj} < -\bar{\pi}$) and hence is pro-competitive, 2 if the merger does not significantly affect rivals' profitability ($-\bar{\pi} \leq \Pi^{A*}_{Cj} \leq \bar{\pi}$) and hence is welfare-neutral, and 3 if the merger benefits rivals ($\Pi^{A*}_{Cj} > \bar{\pi}$) and hence is anti-competitive.[18] We can then analyse 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 scrutinised in the sample period with our data set to estimate a multinomial probit equation of the following type:

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

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 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 – that is, its competitive nature. Again, we control for other merger-specific determinants X_j.

The lagged number of notifications control for merger wave effects (Gugler *et al.*, 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 *et al.*, 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 *et al.*, 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

[18] 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 symmetrical.

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 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.[19] The coefficient estimates of the multinomial probit estimation where welfare-neutral cases are the reference category are reported in Table 9.

Table 9

Deterrence Regressions

	Procompetitive	Anticompetitive
Pre-reform		
Lagged notifications	0.009	−0.003
	(0.012)	(0.007)
Lagged remedies phase 1	−7.639	2.207
	(6.277)	(8.737)
Lagged remedies phase 2	0.650	1.391
	(10.116)	(10.535)
Lagged withdrawals phase 1	27.063***	40.852***
	(8.495)	(8.660)
Lagged withdrawals phase 2	38.242**	55.054***
	(16.039)	(20.016)
Lagged prohibitions	−38.672	−55.247**
	(26.428)	(24.429)
Post-reform		
Lagged notifications	0.023	−0.045***
	(0.020)	(0.006)
Lagged remedies phase 1	−6.721	−33.958**
	(5.585)	(15.826)
Lagged remedies phase 2	−48.243	32.446***
	(60.431)	(5.809)
Lagged withdrawals phase 1	−87.640***	−77.644***
	(10.941)	(20.559)
Lagged withdrawals phase 2	101.736	−106.109***
	(123.704)	(29.293)
Post-reform 2004	0.542	5.670***
	(1.067)	(1.149)
Time trend	−0.017	0.017
	(0.026)	(0.017)
Observations	347	347
Pseudo R^2	0.11	0.11

Notes. 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, crossborder and conglomerate mergers). The symbols ***, ** and * represent significance at the 1%, 5% and 10% levels respectively.

[19] The caveat on estimated dependent variables, discussed in Section 3.2, applies here as well.

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.[20]

4. Conclusion

In our attempt to assess the economic impact of the change in European merger policy in 2004, we bring forward four pieces of evidence: estimation of the determinants of intervention, estimations of the frequency and determinants of type I and type II discrepancies, estimation of rent-reversion by merger decisions and 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 summarised 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 or decreased depending on the adopted

[20] As noted by Papanikolaou and Rosenthal (2011) it is not uncommon for notifications to be withdrawn if the Commission and the firms are unable to agree on a remedies. However, since no ultimate decision is taken in the case of withdrawals, transparency and predictability may suffer.

thresholds. We analyse 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 the most 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 we focus on purely horizontal mergers; we use different thresholds to define pro and anti-competitive mergers; we select our sample to more closely mimic the population of EC mergers in terms of policy actions; and 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, results are mixed. 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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Additional Supporting Information may be found in the online version of this article:

Appendix A. Quantifying the Effect of a Merger and Merger Decision.
Appendix B. Robustness Checks.
Appendix C. Data S1.

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