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Reform

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# An Empirical Assessment of the 2004 EU Merger Policy Reform[*]

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**Abstract** We propose a general framework to assess merger policy effectiveness based on standard oligopoly theory and stock market reactions. We focus on four different dimensions of effectiveness: 1) legal certainty, 2) decision errors, 3) reversion of anti-competitive rents, and 4) deterrence. We apply this framework to 368 merger cases scrutinized by the European Commission (EC) between 1990 and 2007. To evaluate the economic impact of the change in European merger legislation, we compare the results of the four tests before and after its introduction in 2004. Our results suggest that the 'more economic approach' resulted in improved ex-ante predictability of decisions and a reduction of the frequency of type I errors. Merger policy enforcement deters anti-competitive mergers without over-deterring pro-competitive transactions. Yet, the policy shift away from prohibitions, which are effective as a policy tool and as a deterrent mechanism, does not seem to be well-grounded.

**Keywords:** merger control, regulatory reform, EU Commission, event-study
**JEL Codes:** L4, K21, C13, D78

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# 1. Introduction

*By reforming the present merger control system as radically as needed, therefore,*
*I am determined to ensure that it remains a key instrument to foster Europe's*
*economic success in the years ahead.*
Mario Monti, EU Commissioner for Competition, November 7, 2002

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 reversals 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.

One of the major goals of the merger policy reform was to achieve what became known as a 'more economic approach' in merger control, i.e., an approach closer to economic principles. Numerous important changes were made along these lines: an efficiency defense clause was introduced, the office of the chief economist and his 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 latter point is probably the most substantive change introduced by the reform. The main problem with the old DT was that it worked as a cumulative two-part test. A merger was to be declared incompatible with the common market if it *'creates or strengthens a dominant position as a result of which effective competition would be significantly impeded'*. This implies that – as was later confirmed by the CFI – the second part of the test, the impediment of effective competition, only applies if the first part, the creation of a dominant position, is met. Mergers reducing effective competition without the creation of a dominant position could not be challenged under the old legislation, whereas the creation of a dominant position is no longer a necessary condition for intervention by DG Comp post-reform.

The reception of the new merger regulation was generally favorable. Yet, some commentators feared that the cost of increased flexibility stemming from the adoption of more sophisticated tools could be a loss in predictability of the merger control process.

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

[2] Lyons (2004) and Lyons (2009) discusses these reforms in greater detail. The problems with the DT and the advantages of the SIEC are summed up in Vickers (2004). Davies and Lyons (2007) discuss the increased role of remedies in merger control and assess their effectiveness.

Several years have passed since its introduction, enough time to make a first assessment of its effects. In this paper, we propose a comprehensive approach to empirically evaluate whether the modernization of European merger control has succeeded in attaining the goal of increasing its effectiveness.

We identify four dimensions of the effectiveness of merger policy: predictability, decision errors, rent-reversion, and deterrence. For each of these, we adopt a before-and-after approach to single out the effects of the reform. We base our evaluation exercise on a number of maintained theoretical assumptions coming from standard merger theory in an oligopolistic setting (Farrell and Shapiro, 1990) and the use of stock-market event studies to measure the effect of mergers and merger control decisions. We apply our evaluation approach to 368 mergers covering most major cases scrutinized by DG Comp until December 2007 to empirically assess the *economic impact* of the change in legislation and institutions brought about by the new ECMR 04.

First, we test the predictability of the European merger control procedure. We estimate two probit models, where the decisions of DG Comp are functions of observable characteristics. We begin with the 'ex-ante model', which only employs information that is available before the beginning of the investigation and thus emulates the firms' or markets' expectations around the notification of a transaction. Beside which, we analyze an 'investigation model' that additionally uses the information generated during the merger control procedure and it is meant to measure the degree of ex-post legal certainty in DG Comp's decisions. Consistent with previous studies (e.g. Bergman, Jakobsson, and Razo (2005), Aktas, de Bodt, and Roll (2007), Duso, Neven, and Röller (2007), and Duso, Gugler, and Yurtoglu (2011)), we find that several institutional and procedural variables play a significant role in explaining the Commission's decision-making. We also find that the ex-ante predictability of the merger review process increases post-reform, while for the investigation model one cannot observe large differences between the two periods.

Second, we assess whether the introduction of the new merger regulation has influenced the frequency and determinants of systematic mistakes made by the EU Commission (EC).[3] Following Duso, Neven, and Röller (2007), we identify the competitive impact of a merger by measuring its effect on rivals' profits (Farrell and Shapiro, 1990) by means of stock market event studies. Depending on different thresholds, we define pro- and anti-competitive mergers as well as welfare neutral mergers. We then classify cases in which DG Comp remedied a merger that the stock market regarded as pro-competitive (type I errors) or cleared mergers that were regarded as anti-competitive (type II errors). Welfare neutral cases, i.e., cases where rivals' abnormal returns neither significantly increase nor decrease are significantly more frequent after the reform. Conditional on this result, we

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[3]The terms DG Comp and European Commission (or EC) will be used interchangeably throughout this paper.

find that the frequency of type I errors significantly decreased in the post-reform period. Instead, the impact of the reform on the frequency of type II errors is not clear-cut and it depends on the threshold used to classify cases as pro- or anti-competitive.

In the 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 the 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. We cannot estimate the effects of prohibitions after the reform since there were only two prohibitions in the period 2004 - 2007.

Finally, we look at the deterrent effects of merger control. An effective competition policy should induce firms to obey antitrust rules and deter firms from proposing anti-competitive mergers. Yet, it should not over-deter, i.e.,discourage firms from proposing efficiency-increasing combinations. Thus, we estimate the probability of a merger to be pro- or anti-competitive as a function of past EC decisions. This is a novel approach and adds to the (limited) existing literature that has only looked at whether merger policy tools affect the number of notified mergers (Seldeslachts, Clougherty, and Barros, 2009) or the proportion of horizontal to total mergers (Clougherty and Seldeslachts, 2010). We find that while pre-reform prohibitions reduce the likelihood that anti-competitive mergers are notified, they do not affect the probability of pro-competitive mergers (no over-deterrence). Post-reform, the deterrence properties of prohibitions are replaced by those of withdrawn mergers and phase I remedies. This finding might be explained by the policy shift away from prohibitions which started in the early 2000s.

Combining event study methodology with econometrics – an approach pioneered by Ellert (1976) – has proven to be a fruitful empirical methodology in the assessment of business combinations and merger policy.[4] Whereas traditional techniques rely on indirect measures of market power such as concentration ratios or subjective measures of the importance of entry barriers and the prospect for coordinated effects, the event study approach allows us to directly compute an independent evaluation of the merger and the merger control decision.[5] However, the legitimacy of the event study approach in evaluating mergers has been put into question. Criticisms predominantly include

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[4]The event study analysis of mergers was first extended to rivals by Eckbo (1983) and Stillman (1983). Brady and Feinberg (2000), Aktas, De Bodt, and Roll (2004), Aktas, de Bodt, and Roll (2007), Duso, Neven, and Röller (2007), and Duso, Gugler, and Yurtoglu (2011) use this methodology to evaluate EU merger control.

[5]Monti (2008) discusses how stock market reactions could be incorporated into the EC's decisions.

the role of expectations and externalities in stock market data (e.g. McAfee (1988) and Fridolfsson and Stennek (2010)). We recognize the validity of these criticisms and propose several ways to deal with them. First, our sample yields a particularly accurate assessment of the rivals' identity, since Commission experts have carefully identified the relevant product market for every merger. We therefore reduce the potential bias towards zero of the abnormal returns earned by rivals as a group, which would be caused by including firms that are not fundamentally affected by the merger (see the discussion in McAfee (1988) and Eckbo and Wier (1985)). Second, the merger's announcements and the Commission's decisions might reveal information other than the pure competitive or profitability effect of the event, such as the effect of industry shocks triggering a merger wave, future acquisition probability, and the information about the allocation of the roles of insiders and outsiders. We tackle these issues twofold. We carefully choose the announcement date and the appropriate event window to reduce the influence of other triggering shocks. Even more importantly, we correct for the expectations of market participants regarding the eventual merger proceedings outcome. By conditioning on the merger-specific, ex-ante available information at the merger announcement, we correct for market expectations, which should help to insulate the pure surprise element of the specific event. Finally, we conduct a series of robustness tests to corroborate the consistency of our results.

The paper proceeds as follows. Section 2 is concerned with our basic framework, the methodology, and main assumptions. Section 3 presents the sources of the data, some summary statistics, and the estimations of the merger and merger control decision effects by means of stock-market event studies. Section 4 presents the results of the probability of intervention estimation, the analysis of the frequency and determinants of type I and type II errors, the rent-reversion regressions, and the deterrence regressions respectively. Section 5 concludes.

## 2. Methodology

This section provides a unified framework for assessing merger control. This framework is then used to discuss four dimensions of effectiveness via empirical tests, which have been partially developed in previous work (Duso, Neven, and Röller, 2007; Duso, Gugler, and Yurtoglu, 2011) and are partially newly designed in this paper. The objective of the analysis 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).[6]

The starting point of the methodology is that merger control aims to avoid anti-competitive (i.e.,consumer welfare decreasing) mergers by either blocking, remedying or deterring them. One of the main challenges in the empirical assessment of merger control is the ability to, first, define, and second, measure the anti-competitive nature of a merger. Next, we clearly state the assumptions needed to address these identification and quantification issues. While one can claim that these assumptions are restrictive, we try to provide evidence on whether and how each of them should affect our results by means of robustness checks.

## 2.1. Assumptions

### 2.1.1. Theoretical Identification

We define an anti-competitive merger as one that reduces consumer welfare.[7] Our basic 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). For instance, in a Cournot setting, when a subset of firms in the market merges and jointly maximizes profits, they find it optimal to reduce their production. Under mild assumptions, this induces the remaining market participants to increase their production, but by less than the merging firms. Hence, aggregate market output in the post-merger situation decreases, price rises, and consumer welfare is lower (Farrell and Shapiro, 1990). A similar mechanism is at work when firms compete in prices and goods are differentiated (Deneckere and Davidson, 1985). Hence, a horizontal merger creates a positive externality for the competitors of the merging firms: via the 'price umbrella' it increases their profits. The second externality, called 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

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[6]We chose the date in which the new merger regulation legally came into force to define the pre- and post- reform periods. However in section 4.5.2, we discuss this issue and the robustness of our results to the choice of a different date.

[7]In this paper we assume that the antitrust agency has a consumer-welfare standard, which is the standard adopted by the European Commission as well as most other competition authorities. Thus, we will not discuss the 'right' welfare criterion in merger control. For such a discussion see Motta (2004) and Neven and Röller (2005).

of these antipodal forces. As Farrell and Shapiro (1990) show, there exists a critical level of 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. Looking at this net effect thus allows us to infer the competitive nature of a merger. When the positive externalities exceed the negative externalities, i.e.,the efficiency gains are not enough to compensate for the market power effect, rivals' profits increase, while consumer surplus decreases, since prices are higher than prior the merger. The first identifying assumption of our framework is, therefore, that a post-merger increase in competitors' profits is an indication of the merger being anti-competitive.

This identification assumption is quite general and robust and holds for a wide class of oligopoly models. However, it could prove problematic in some circumstances such as vertical or conglomerate mergers and mergers in a dynamic context. Vertical mergers may cause market foreclosure, where both rivals and, potentially, consumers lose depending on the parametrization of the demand function, thereby violating our identification assumption concerning the nature of the merger. In a more general dynamic model, where horizontal merger proposals are endogenous and come over time and an antitrust authority can set its optimal policy (Nocke and Whinston, 2010), the holding of our assumption depends on the nature of the sequence of mergers and the merger policy. In our regressions, we therefore control for merger wave periods and non-horizontal mergers. As a further robustness check (section 4.5.1), we exclude from our sample mergers that are not purely horizontal and obtain qualitatively similar results as for the whole sample.

### 2.1.2. Empirical Measurement

The next step is to measure the profitability effects brought about by the merger. Following an extensive literature, we do that by using stock market reactions to the merger announcements, i.e.,a stock-market event study. This methodology relies on the semi-strong version of the efficient capital market hypothesis, which asserts that stock prices fully reflect the information available to the market on the given commodity at any point in time. This implies that the daily return of a commodity $i$ ($R_{i,t}$) is proportional to the market index ($R_{market,t}$) at any given point in time $t$:

$$R_{i,t} = \alpha + \beta R_{market,t} + \varepsilon_{i,t} \tag{1}$$

where $\varepsilon_{i,t}$ is an i.i.d. error term. The idea that markets are informationally efficient is central to the entire event-study literature starting from Fama (1970) and constitutes our second crucial assumption.[8]

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[8]In previous work based on a sub-sample of the data used in this paper (Duso, Gugler, and Yurtoglu, 2010), we show that the ex-ante profitability measures based on event studies are positively and highly correlated with ex-post measures of profitability based on accounting data.

Under this assumption, model (1) can be used to estimate the 'normal' return of a firm at any given point in time as $\widehat{R}_{i,t} = \widehat{\alpha} + \widehat{\beta} R_{market,t}$. 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 $AR_{i,t} = R_{i,t} - \widehat{R}_{i,t}$.

Since there might be information leakages, which influence firm $i$'s return before (or after) the merger announcement, the total valuation effect of the event is defined as the sum of the daily abnormal returns within a window of several days around the event: the cumulative abnormal return (CAR). Finally, we aggregate these measures to obtain a profitability measure for the merging firms and the competitors by taking a weighted sum of the individual CARs, where the weights are represented by the relative market value of each firm. We call these measures 'cumulative average abnormal returns' (CAARs).[9]

The measured CAARs around a merger's announcement might entail effects other than the pure competitive effects: In particular, the effects of specific forces triggering the merger (Jovanovic and Rousseau, 2002), information about the roles of merging firms and rivals (Fridolfsson and Stennek, 2010), and the market expectations about the outcome of the merger control decision (Eckbo, 1992). The third important assumption of our methodology is that we can effectively 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). 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).

Finally, to tackle the issue of market expectations about the merger proceedings, we estimate the probability of intervention and use it to correct our CAAR measures. The logic of this correction is as follows: The stock market builds expectations on the likely outcome of the antitrust procedure, which should already be priced in at the announcement of a merger. Hence, we do not measure the whole extent of the rents generated by the merger, but only an update of the market's beliefs. We thus have to adjust the measured abnormal returns for the stock markets' expectations about the merger control procedure. [10]

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[9]In the appendix, we provide a formal derivation and a discussion of the CAARs.

[10]An (extreme) example of a prohibition might clarify the intuition. If we measure a rent of 100

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Given our setup, we are then confident that the corrected CAARs around merger $j$'s announcement $(\Pi_{fj}^{A*})$ can be seen as a meaningful measure of the competitive effect of the merger on merging firms $(f = M)$ or competitors $(f = C)$. While this measure of the competitive impact of a merger is not the main subject of analysis in this study, it allows us to empirically achieve the theoretical identification discussed in 2.1.1: we classify a merger as anti-competitive if its impact on competitors' profits is *sufficiently* positive, i.e., $\Pi_{Cj}^{A*}$ exceeds a certain threshold $\bar{\pi}$. Symmetrically, a merger is classified as pro-competitive if $\Pi_{Cj}^{A*}$ is smaller than $-\bar{\pi}$.

Since the choice of the threshold level $\bar{\pi}$ is necessarily arbitrary, we consider different values for $\bar{\pi}$, namely $\bar{\pi} = 0$, $\bar{\pi} = 3\%$, $\bar{\pi} = 5\%$, and $\bar{\pi} = 10\%$. The first definition for when a merger is anti-competitive is quite liberal but allows us to use the maximum number of observations. Conversely, requiring the competitors' CAAR to exceed 5% or even 10% (resp. be smaller than -5% or -10%) constitutes a rather demanding criterion. In the main regressions reported in the paper, we adopt an intermediate threshold of $\pm 3\%$.[11] In section 4.5.3 we discuss the robustness of our results to the use of different threshold values.

A final assumption which is, however, only needed for the rent-reversion test, 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 $j$ $(\Pi_{fj}^{D*})$ can be seen as a meaningful measure of the effect of the decision on profitability. 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 again 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 Appendix A.2). All of these assumptions as well as the consequences of their failure are discussed in length and justified in greater detail in Duso, Gugler, and Yurtoglu (2011).

## 2.2. Assessing Policy Effectiveness

The key contributions of this paper is to propose a comprehensive evaluation framework for merger policy and attempt to assess the economic impact of the introduction of

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million US dollars 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 $(100/(1 - 0.2)) = US\$125$ million. In Appendix A.4 we provide a detailed formal derivation of our approach.

[11] 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 US dollars.

ECMR 04. To achieve this goal, we look at four different dimensions of effectiveness and explicitly analyze the differences in the performance of the EC before and after the 2004 reform. These dimensions of policy effectiveness can be seen in a natural chronological order. First, before the announcement of a merger, legal certainty and predictability of the merger control procedure are important determinants of firms' choices on the kind of merger they propose and, hence, its welfare consequences. 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 reduce mistakes. Thus, we analyze the frequency and determinants of type I and type II errors committed by the EC. 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, a particular decision might have consequences on the future merger behavior of other firms. We therefore analyze the deterrence effects of the EC's merger policy by estimating how past interventions affect the competitive nature of currently proposed mergers.

### 2.2.1. Legal Certainty, Transparency, and Predictability

The predictability of the merger control procedures is a key issue for judges, competition lawyers, authorities and, of course, the firms. Since legal certainty and transparency of the proceedings reduce the welfare-detrimental risk of political influence and decrease uncertainty for the firms, the desirability of a merger control system comprised of clear-cut, transparent and traceable rules and proceedings has long been stressed by scholars and practitioners (Smith, 1957; Elman, 1965). The benefits of legal certainty known to the literature are numerous: it increases the credibility of the authorities, fosters accountability, reduces personal biases, and allows the concerned firms to better understand the merger review process.[12] Moreover, the transparency of legal procedures increases the potential for harmonization among multiple regulatory authorities.

The impact of ECMR 04 on the predictability of merger decisions, however, is ex-ante ambiguous. On the one hand, the publication of merger guidelines and several institutional changes were clearly aimed at augmenting legal certainty. On the other hand, the more intensive use of specific theoretical and econometric tools, aimed at accurately pinning down the specificities of each single case, makes singular decisions more difficult to be anticipated, since the decision process is less anchored on simple, general rules (Kobayashi, 1997).[13]

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[12]In a slightly different setting, Barros (2003) theoretically proves that an increase in the uncertainty of the antitrust policy's implementation leads to more anti-competitive agreements being proposed by firms.

[13]As noted by Christiansen (2006): '[...] with the simultaneous introduction of unilateral effects analysis

Before proposing a concentration, the involved firms should be able to to a large extent predict the reaction of the competition authority on the basis of observable characteristics related to the merger. Therefore, one testable implication of legal certainty in merger control is the predictability of the EC's decisions.[14]

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, which we call *action*, and zero otherwise (*clear*). Let $X_j$ be a set of observable characteristics related to the specific merger. These might be characteristics of the merging firms, the product and geographical markets where they operate, the nature of the merger they propose, as well as the merger policy history up to the point in time when merger $j$ is proposed. Note that for the estimation of this model none of the assumptions related to event-studies are required. We measure the predictability of the decision on the basis of goodness-of-fit measures of the following regression:[15]

$$P_j = \alpha_0 + \alpha_1 X_j + \varepsilon_j \tag{2}$$

We estimate model (2) using two different sets of explanatory variables, $X_j$. The first one includes only ex-ante observables, i.e.,information that is available to the firms and the market at the time of the merger's notification. This 'ex-ante' model is supposed to provide a measurement of how well the parties notifying a merger and the market 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 large EU countries), measures of past decision records of the EC (lagged notifications, antitrust actions, and merger withdrawals) as well as industry dummies and a time trend. If the ex-ante predictability of the European merger control procedure was improved by the reform, we would expect to observe an improvement in the predictive power of this model, measured by standard statistics such as the $pseudo - R^2$ and the percentage of correct predictions.

The second model includes, in addition, information generated in the course of the

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and the 'efficiency defense', it is possible not only for mergers 'below' the previously relevant market dominance threshold to be prohibited but also for mergers 'above' it to be approved. This boils down to a wider margin of discretion in decision-making, thus making the Commission's decisions permanently more difficult to predict.'

[14]Similar analyses have been performed by Khemani and Shapiro (1993), Bergman, Jakobsson, and Razo (2005), and Bougette and Turolla (2006). Yet, the logic of their work is not motivated by the concept of predictability but rather by the aim of providing a test of whether the antitrust authorities give appropriate weights to the factors that they regard as important ex-ante, such as market shares, concentration, and barriers to entry.

[15]Since we assume that the error terms $\varepsilon_j$ are correlated over time, we allow for clustering at the year level.

Commission's investigation. Hence, the 'investigation model' also uses several variables obtained from the decision files: a variable to indicate whether the EC identified barriers to entry, a variable denoting the existence of a dominant player in the market, a variable distinguishing phase 1 from phase 2 cases, and three dummies measuring whether the relevant market was either national, EU-wide or worldwide. We would expect that the existence of barriers to entry and a dominant market participant positively increase the probability of an intervention. In particular in the pre-reform period, the opening of a phase 2 investigation should also be a good predictor for an action. This is not obvious in the post-reform period, since interventions in the form of remedies during phase 1 have become increasingly more common.[16] With respect to the definition of the relevant market, we would expect small (national) markets to increase the likelihood of an action if compared to EU-wide markets (the reference category), while large (worldwide) markets should be less problematic and thus be associated with less interventions.

### 2.2.2. Type I and Type II Errors

The first assessment of a particular decision is whether it conforms to the objectives of merger control and, hence, whether the Commission committed mistakes. According to our discussion in section 2.1, a benevolent agency intervenes 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.[17]

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,

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[16]Only since 1997 has the Commission had the legal power to impose conditions and obligations (Lyons (2009)). See revision to Art. 6(1) in Council Regulation (EC) No 1310/97 of 30 June 1997 amending Regulation (EEC) No 4064/89 on the control of concentrations between undertakings (OJ L 180, 9.7.1997, p.1Ŭ6).

[17]The notion of type I errors we use here corresponds therefore to the *weak* type I errors in Duso, Neven, and Röller (2007). Given that prohibitions were 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 were blocked. We come back to this point in section 4.2.

consumer surplus decreases after the merger when the profits of the rivals to the merging firms increase. Hence, the consumer welfare-maximizing merger control decision is:

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

where $\Pi_{Cj}^{A*}$ represents the corrected merger announcement CAAR of the competitors ($C$) for merger $j$ and $\bar{\pi}$ is either 0%, 3%, 5%, or 10%.[18]

Under our assumptions, the definition of type II errors is not problematic, especially when we use a demanding threshold: these are anti-competitive cases where the EC did not intervene. The definition of type I errors, instead, might be more cumbersome. Even if a merger is *on average* pro-competitive as captured by a negative value for $\Pi_{Cj}^{A*}$, 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 error. 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 errors. Mergers that are clearly pro-competitive are less likely to entail anti-competitive elements. Because of these considerations, we will base our analysis on a threshold $\bar{\pi} = 3\%$. Results based on other thresholds are discussed as robustness checks.

Once we have defined type I and type II errors, we analyze their determinants by running the following probit regressions:

$$E1_j = \alpha_0 + \alpha_1 X_j + \varepsilon_j \text{ if } D_j = 1 \tag{3}$$

$$E2_j = \beta_0 + \beta_1 X_j + \varepsilon_j \text{ if } D_j = 0 \tag{4}$$

Clearly, if the agency is benevolent and does not systematically commit mistakes, then both errors should be completely random and, hence, neither the determinants $X_j$ nor the constants $\alpha_0$ and $\beta_0$ should have the significant explanatory power to predict them. However, in a political economy model of merger control (Neven and Röller, 2005), the antitrust agency maximizes an objective function containing not only consumers' welfare but also the additional utility that it can obtain from third parties. These include the involved firms and other agents such as member states' governments, which provide contingent perks or, more generally, other kinds of private benefits. The determinants of errors $X_j$ are thus merger-specific characteristics as well as institutional and political

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[18]This means that, for any positive value of $\bar{\pi}$, we define a symmetric interval around 0, where mistakes cannot be defined since the mergers are considered to be welfare neutral.

economy variables that may influence the decision of an antitrust agency (Duso, Neven, and Röller, 2007).

We consider a number of potential determinants of decision errors: as shown 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 mistakes. 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 define relevant markets too narrowly, which might imply a lower frequency of errors 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 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 errors.

To assess how the new merger regulation affected the likelihood and determinants of the EC's mistakes, we run the basic regressions 3 and 4 separately on the pre- and post-reform sub-samples.

### 2.2.3. 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. 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. We therefore assess the effectiveness of an antitrust action by running the following regression separately for merging firms and rivals:

$$\Pi_{fj}^{D*} = \sum_d \alpha_{fd} + \sum_d \beta_{fd}\Pi_{fj}^{A*} + \gamma_f X_j + \epsilon_{fj} \qquad (5)$$

where $\Pi_{fj}^{D*}$ is the probability-corrected decision CAAR of merging firms ($f = M$) and

competitors ($f = C$), respectively, for merger $j$, while $\Pi_{fj}^{A*}$ is the probability-corrected announcement CAAR. We estimate different intercepts ($\alpha$s) and slopes ($\beta$s) for the different decisions ($d$=*clearance*, *phase1 remedies*, *phase2 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 all rents, that is both the market power and the efficiency rents (i.e.,$\alpha_{fd} = 0$, $\beta_{fd} = -1$ if $d = prohibition$). If a merger is *cleared* without commitments, we do not expect decision effects that are systematically related to announcement returns (i.e.,$\alpha_{fd} = 0$, $\beta_{fd} = 0$ 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 the rivals would profit. The situation is more complex 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 (i.e.,$\alpha_{fd} < 0$, $\beta_{fd} < 0$ if $d = remedies$). We again run separate regressions for the pre- and post-reform periods to assess the impact of the new merger regulation.

### 2.2.4. Deterrence

As pointed out by Sørgard (2009), an optimal merger policy involves deterrence. In particular, he shows that there is an optimal level of enforcement where some actions, which in isolation would be welfare detrimental, can be optimal to achieve deterrence and thus increase overall welfare. Hence, 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 and that over-deterrence would not be an issue. Hence, the existence of decision mistakes is a key ingredient in a deterrence model.

Key to the analysis of deterrence in merger control is that a good policy should deter firms from proposing socially detrimental mergers but it should not over-deter and hence discourage firms from proposing efficiency-increasing combinations. Our analysis takes an important step in this direction if compared to the limited existing literature (e.g.,Seldeslachts, Clougherty, and Barros (2009) and Clougherty and Seldeslachts (2010)). This is made possible by the unique information contained in our dataset that permits us to infer the competitive nature of each merger and, hence, present a finer

prediction on the quality of deterrence achieved by the policy. For each merger, we can generate a categorical variable $(D_{jt})$ which takes on a value of 1 if the merger is pro-competitive $(\Pi_{Cj}^{A*} < -\bar{\pi})$, 2 if the merger is welfare neutral $(-\bar{\pi} \leq \Pi_{Cj}^{A*} \leq \bar{\pi})$, and 3 if the merger is anti-competitive $(\Pi_{Cj}^{A*} > \bar{\pi})$. We can then analyze how past decisions affect the odds of a particular merger being pro- or anti-competitive when compared to the reference category of welfare neutral mergers. Hence, we can separately look at whether merger policy enforcement deters anti-competitive mergers without over-deterring welfare increasing combinations.[19] In particular, we look at how the complete merger policy enforcement's history of the EC affected the competitive nature of the mergers in our sample. 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 logit equation of the following type:

$$D_{jt} = \alpha_0 + \alpha_1(n_{t-1} + n_{t-2}) + \sum_d \alpha_{2d} \frac{d_{t-1} + d_{t-2}}{n_{t-1} + n_{t-2}} + \alpha_3 X_j + \epsilon_j \qquad (6)$$

The variable $n_{t-i}$ is equal to the total number of notifications to the EC $i$ quarters before merger $j$ was notified, and $d_{t-i}$ is the total number of mergers with decision $d$ ($d = remedies, blockings,$ or $withdrawals$) $i$ quarters ago. We thus regress the indicator of the merger's competitive nature on the total number of notifications in the last two quarters and on the ratios of possible actions over total notifications. Again, we control for other merger-specific determinants $X_j$.

The lagged number of notifications controls for merger wave effects. While several studies show that merger waves can be driven by periods of over- and undervaluation of the stock market (e.g.,Gugler, Mueller, Weichselbaumer, and Yurtoglu (2012), Rhodes-Kropf and Viswanathan (2004) and Harford (2005)), very few studies have looked at how merger waves might impact the competitive effects of a merger (e.g.,Gugler, Mueller, and Weichselbaumer (2012), and Clougherty and Duso (2009)). One might argue that on the wave-crest the quality of the targets and the fit of the match between acquirer and target is worse. Hence, less efficiency-enhancing and potentially more anti-competitive mergers will be proposed.

More importantly, the kind of merger policy decisions and their effectiveness send signals to firms about the toughness of the authority. If merger policy deters anti-competitive mergers, one should expect negative coefficients for all kinds of actions. Yet, as shown by Seldeslachts, Clougherty, and Barros (2009), 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. It is quite clear that

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[19]Clearly, for $\bar{\pi} = 0$ 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.

prohibitions 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 or abort 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 are effective and 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 and, potentially, less anti-competitive mergers will not be proposed. If, instead, the EC remedies mergers which were expected to be blocked, the firms can infer that merger control has become more lenient and could propose more anti-competitive mergers. Finally, if the policy achieves 'good deterrence', then none of the EC's actions should negatively affect the likelihood of pro-competitive mergers.

We estimate the model (6) on the full sample interacting the independent variables with the pre- and post-reform dummies and adding a time trend and a post-reform dummy. This latter variable should capture whether the reform per se had direct deterrence effects and, hence, influenced the kind of mergers proposed.

## 3. Data

### 3.1. Data Sources

Our sample includes 368 merger cases scrutinized by the EC from the beginning of 1990 to the end of 2007 and was designed to mimic the dynamics of the population of EC merger cases prior to and after ECMR 04. We collected information on as many phase 2 mergers as possible, together with a randomly matched sample of phase 1 merger cases. By carefully reading the text of publicly available merger cases 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 decisions, and some other merger-specific characteristics.[20]

Using the EC's merger assessment to identify the rivals represents a particular strength of this sample. It has the major advantage of being a much more realistic description of the relevant markets than, say, using 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 of that particular merger leaked to the market.

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[20]All documents are publicly available at http://ec.europa.eu/competition/mergers/cases/.

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 on which the first definitive indications of the combination between the merging parties became known.[21] The total return index, market value and branch index time series for the identified parties were downloaded from the Thomson Reuters Datastream database, providing daily data for the variables in question.

## 3.2. Summary Statistics

Table 1 summarizes the variables in our dataset for the periods before and after the merger policy reform, information on the population of EC mergers is included where available.

[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 exactly 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%). Thus, while we over-sample cases with remedies – which are our main interest – our sample exhibits the same dynamics as the population of EC merger cases does. Prohibitions represent 5.2% and 1.3% of the cases pre- and post-reform in the sample, and 0.8% and 0.01% in the population. All other cases have been cleared without conditions and obligations. For the population data, we also have information on aborted or withdrawn cases. 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 proportion of geographic market definitions (national, EU-wide, worldwide) do not change much between the two periods. The proportion of conglomerate and full mergers increases, that of cross-border mergers slightly increases, while barriers to entry are found less often in the post-reform period. Dominant firms (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 large EU country (Germany, France, Italy, Spain or the UK) are observed with approximately the same frequency before and after ECMR 04. The

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

dummy for the involvement of EU firms decreases, while the average market values of both merging firms and rivals increase.

[insert Figure 1 here]

Figure 1 shows the evolution of notifications and actions in the population of EU mergers. We observe an increasing trend in notifications 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. The mergers in our sample display similar dynamics.

## 3.3. Structure of the Cumulative Average Abnormal Returns

We calculate the CAARs over the event windows according to the methods presented in Appendix A. We use the total return index from Datastream, which accounts for dividends and corrects for stock splits. Table 2 reports mean values 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%, significant at the 10% level post-reform.

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% confidence level post-reform. Similar to the merging firms, rivals suffer an average negative reaction around the EC decision date: an insignificant effect of -0.3% increases to -0.8% post-reform and becomes significant at the 10% level.

## 3.4. Pro-competitive, Anti-competitive, and Welfare Neutral Mergers

As discussed in section 2.1.1, mergers are categorized on the basis of their effects on rivals' profits. In section 2.1.2 we propose using the estimated CAARs and a threshold $\bar{\pi}$ to better identify the clearly anti-competitive or pro-competitive mergers: The larger the interval $(-\bar{\pi}, \bar{\pi})$, the more mergers are defined to be welfare neutral. Table 3 reports the composition of mergers in our two sub-samples.

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[insert Table 3 here]

Post-reform, the percentage of welfare neutral mergers 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 the EC should commit less type I and type II errors after the reform, due to the change in the nature of proposed mergers. Second, whether this composition change is due to the changes in merger policy enforcement or other determinants can be analyzed in the analysis of deterrence.

## 4. Empirical Results

### 4.1. Probability of Intervention

First we investigate the predictability of European merger control. Both the 'ex-ante' and the 'investigation' specifications of the probit model (2) are displayed in table 4. We report the marginal effects of the two sets of observable factors that we expect to explain the intervention of the EC prior to the introduction of the reform and after ECMR 04 was implemented.

[insert Table 4 here]

The first two models in table 4 investigate how easily the Commission's decision on a notified merger can be predicted prior the beginning of the investigation. In the post-reform period, both the $R^2$ and the percentage of correct predictions increase by over 5%. This suggests that following the reform it is easier to anticipate DG Competition's decisions based on commonly observable factors. Although the pseudo R-squared 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 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 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 intervenes 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.

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The models including investigation-specific information corroborate several of the findings of the simpler model. While the $R^2$ of the investigation model decreases by 9% between the periods (from 68% to 59%), the number of correctly classified cases slightly increases and stays around 90%. Thus, even though the adaption of a new set of rules seems to have created some additional noise, legal certainty does not appear to have significantly decreased. The finding of barriers to entry, the existence of a dominant firm, as well as the initiation of a phase 2 investigation significantly increase the likelihood of an action by 22%, 18% and 16% respectively pre-reform. As expected, a (narrow) national market definition increases the probability of an intervention by 7% and a (broad) worldwide definition decreases it by 6% if compared to the reference group (EU markets). After the reform, the presence of barriers to entry and dominant firms are still strong predictors for intervention (14% and 29% higher probability), yet the opening of a phase 2 investigation is not positively correlated with the likelihood of an intervention anymore. This is in line with the more economic approach the reform was supposed to achieve. After the reform, the geographic market definitions seem to play an even more important role: a national market definition increases the probability of an intervention by almost 30% and a worldwide definition decreases it by almost 15%.

As explained in the appendix A.4, the predicted values of the ex-ante model are used to correct the CAARs for the expectations of the market.

## 4.2. Type I and Type II Errors

The next piece of evidence on the effects of the merger control reform is the analysis of the EC's possible mistakes in the enforcement of the merger regulation. As discussed in section 3.4, due to changes in the composition of notified mergers, the unconditional percentage of type I and type II errors must have decreased. However, this might not be due to the merger policy but to other, external factors. We come back to this issue when discussing deterrence. Here we focus on the conditional likelihood of mistakes, i.e., conditional on the mergers not being welfare neutral.

Table 5 reports the frequencies of decision errors within the pro- and anti-competitive sub-samples using different thresholds for the definition of pro- and anti-competitiveness.

**[insert Table 5 here]**

The propensity of committing type II errors (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 committing type I errors (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 errors 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 section 4.5.3.

To analyze the determinants of type I errors, we use the variables discussed in section 2.2.2.[22] The results of regression (3) are reported in table 6.

<div align="center">[insert Table 6 here]</div>

If one of the merging parties is a US-based firm, the likelihood of wrongly eliciting an action in pro-competitive mergers is, ceteris paribus, 20% lower in the pre-reform period and 25% lower after the reform. Apparently, thus, the EC is particularly cautious to avoid making type I errors when US firms are involved. Similarly, type I errors are almost 20% less likely cross-border merger cases both pre- and post-reform. Full mergers and mergers involving large firms and large competitors in terms of market value are more likely to elicit a type I error pre-reform. Post-reform, pro-competitive conglomerate mergers 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 investigation variables (barriers to entry, phase 2, and national markets) significantly increase the likelihood of a type I error pre-reform. These dimensions of the decision are therefore possible explanations of why the EC might have made a mistake. For instance, this is evidence that DG Comp might sometimes define the relevant antitrust markets too narrowly. Post-reform most results remain the same, yet the phase 2 dummy does not correlate with the likelihood of an error anymore. The model's predictive power is high both in terms of $pseudo - R^2$ (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 (4): the determinants of type II errors. The marginal effects of the probit estimations are reported in table 7.

<div align="center">[insert Table 7 here]</div>

We estimate significantly more type II errors in mergers involving US firms both pre- and post-reform (13% and 19% respectively). It seems that American firms again, ceteris paribus, are treated more leniently. Conglomerate mergers are 30% less likely to elicit a type II error both pre- and post-reform. Again, this might be due to the fact that we wrongly identify the competitive nature of such transactions. Full and cross-border mergers significantly increase the likelihood of a type II error only post-reform by 23%

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[22]For the post-reform period, we were forced to drop some variables because of collinearity problems due to the small sample size.

<div align="center">21</div>

and 14% respectively. Merging parties' market values negatively and significantly affect the probability of type II errors pre-reform, while increasing it post-reform, potentially indicating lobbying. On the contrary, the coefficient for rivals is negative and significant in both periods. This goes against the idea that the rivals were also successful in lobbying the EC to allow an anti-competitive merger to be cleared. This might also indicate that the EC takes stock market reactions increasingly into account in its decisions (Monti, 2008).

Except for barriers to entry, which significantly reduce the likelihood of type II errors pre- and post-reform, the effects of the other investigation variables changed after the introduction of ECMR 04. A narrow geographic market definition does not reduce the probability of a mistake. Moreover, conditional on a merger being anti-competitive, the opening of a phase 2 investigation significantly increases type II errors post-reform while it significantly decreased it before. Again the predictions of the model are quite accurate with a pre-reform $pseudo - R^2$ 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 Estimations

We now turn to the assessment of the effectiveness of different merger policy tools to reduce the anti-competitive rents generated by a merger. We estimate equation (5) for the merging parties and their rivals separately. The dependent variable, the probability-corrected decision CAAR, is regressed on different constants for the different decisions (*clearance*, *phase*1 and *phase*2 *remedies*, and *prohibition*), and on the interaction terms of decision type and probability-corrected announcement CAARs. These coefficients measure the rent-reversion achieved by the respective decisions of DG Comp. 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 (-1.3) and rivals (-0.48), where the less negative slope of rivals is reinforced by the significantly negative prohibition constant (-0.36). Hence, prohibitions seem to substantially reverse the rents measured by the stock market around the announcement of the merger, and can be interpreted as being an effective merger policy tool. Furthermore, we find that clearances and remedies in phase 1 have a positive coefficient for the rivals, suggesting that they were a positive signal, possibly because they suggest that profitable future merging activity in the sector would go unchallenged as well. Remedies in phase 2 have a positive impact on merging firms' decision CAARs contrary to the expectations. Remedies seems, therefore, to be ineffective in reverting anti-competitive rents.

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Post-reform, we cannot estimate the degree of rent-reversion achieved by prohibitions, since only two mergers were blocked. Among the other variables, only the phase 1 remedies constant in the merging parties' regression and the phase 2 remedies slope in the rivals' regression are significantly positive, indicating that remedies even increase the final rents gained by the firms. Since all other coefficients are insignificant, we conclude that the associated policy tools do not systematically affect the market and should hence be considered ineffective. Thus, post-reform, no policy tool seems to have been an adequate replacement to outright prohibitions, the only tool capable of reverting rents.

## 4.4. Deterrence Estimations

The last piece of evidence we propose relates to the deterrence properties of EU merger control. As mentioned in section 2.2.4, the deterrence properties of merger control are particularly relevant if type I and type II errors occur or remedies imposed by the antitrust authority are not effective in reverting the anti-competitive rents generated by the merger. This is exactly the situation that emerges from our results so far. To estimate the degree of 'good' deterrence achieved by the policy, we estimate model (6) and assess the likelihood that a newly notified merger is either anti-competitive or pro-competitive instead of welfare neutral as a function of the history of past merger control decisions. The coefficient estimates of the multinomial logit estimation 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, its likelihood of being anti-competitive is significantly lower: prohibitions deter anti-competitive mergers because they are the toughest policy tool. This is not the case for clearly pro-competitive mergers. Hence, prohibitions do not seem to over-deter. Unexpectedly, the ratio of mergers withdrawn in phase 1 as well as in phase 2 increases both pro- and anti-competitive notifications. It might be that withdrawn mergers encourage further combinations, which are perceived as alternative to the failed ones. This might be especially true during a merger wave like the one observed in our sample pre-reform.

While remedies do not affect the odds of pro-competitive mergers post-reform, phase 1 remedies deter anti-competitive mergers and phase 2 remedies seem to encourage them. Remedies in phase 1 deter 'bad' mergers because they often come at the expense of expected outright clearances (Seldeslachts, Clougherty, and Barros, 2009) and because they are more clear cut and easy to implement and, hence, more effective than phase 2 remedies (e.g.,Duso, Gugler, and Yurtoglu (2011) and European Commission (2005)).

Remedies in phase 2, on the contrary, possibly promote anti-competitive mergers because they come at the expense of prohibitions and therefore signal a soft antitrust stance by the EC. Since these policy tools do not affect pro-competitive mergers, they do not seem to over-deter.

Both the phase 1 and phase 2 withdrawal ratios significantly deter anti-competitive mergers, even though the coefficient of phase 2 abortions is much larger. This is presumably due to the fact that a withdrawal at such a late stage of the investigation almost amounts to a prohibition. Once again, we cannot test for the effects of prohibitions post-reform, as only two mergers were blocked after 2004. Prohibitions became a very rare event, and withdrawals or abortions 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/abortions might have been effective prohibitions.[23]

## 4.5. Robustness Checks

In this section we report several robustness checks. Due to lack of space, we do not report all tables but just briefly discuss the main results. The extensive results can, however, be obtained from the authors upon request.

### 4.5.1. Purely Horizontal Mergers

As discussed in section 2.1, 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. 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.

The results for the probability of an intervention are not strongly affected: political dummies and market value variables are in some cases less significant and – contrary to the specification reported – the goodness-of-fit measures of the investigation model increase in the post-reform period. This suggests that some of the policy changes made it even easier to predict the outcome for purely horizontal mergers.

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[23] 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 event of withdrawals, transparency and predictability may suffer.

The results on decision errors remain qualitatively unaffected as well. The reduction in type I errors 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 also remains.

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 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 horizontal sub-sample is largely on par with the results from the full sample, even though some results are less significant due to the smaller sample size. The findings on 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 abortions 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.

### 4.5.2. 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.[24] 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,

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[24]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 timetable for remedies.

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 errors 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 errors remain quantitatively similar, while their significance is in some cases 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.

### 4.5.3. Different thresholds for pro- and anti-competitivity

As noted in 2.1.2, 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 $\bar{\pi} \lessgtr 0$; however, we expect a degree of noise in the data. In the main body of the paper, we reported the frequency of decision errors when using either a 0%, 3%, 5% or 10% threshold, while the determinants of decision errors and the deterrence regressions were restricted to the $\bar{\pi} = 3\%$ specification. This choice of threshold was motivated by the facts that a zero threshold implies a lot of noise and thus biases results towards insignificance, while the two higher threshold choices strongly limit the number of available observations. The following paragraphs describe the regression results, when these thresholds are applied.

The determinants of decision errors 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 in particular phase 2 abortions 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 de-

terrence exercised by phase 2 abortions. However, the negative effect of prohibitions is not significant in this case. Setting the threshold at $\bar{\pi} = 10\%$ leaves too few pro- and anti-competitive outcomes for the multinomial logit model to converge.

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

## 5. Conclusion

In our attempt to provide an unifying framework to assess merger control decisions, we identified four distinct dimensions of effectiveness: (1) the determinants of intervention, (2) the frequency and determinants of type I and type II errors, (3) rent-reversion by merger decisions, and (4) the deterrence effect of merger decisions. These elements are meant to provide a comprehensive evaluation of the entire process of merger control from an ex-ante to an ex-post perspective. Based on this framework we assess the economic impact of the change in legislation due to the 2004 merger policy reform in Europe. The identification of the reform's effects is achieved by comparing the performance of merger control along the 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 from an ex-ante point of view has improved. We observe an increase in the number of significant predictors of the probability of an action, as well as increases of the $R^2$s 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. This is even more so if we focus on purely horizontal mergers. We also estimate a second model that uses information from the Commission's assessment, finding that dominant firms, entry barriers, market definitions and a phase 2 investigations significantly predict the decision, as one should expect. Market definition becomes more important post-reform, while – consistent with the pursuance of a more economic approach – the initiation of a phase 2 investigation is no longer an significant indication for an intervention. The goodness-of-fit measures of the second model are only slightly lower in the post-reform period.

Second, we observe that more welfare neutral mergers have been proposed post-reform. This implies that the EC commits less type I and type II errors. Conditional on this, the percentage of type I errors significantly decreased after the introduction of ECMR 04, independent of the threshold used to define pro-competitive mergers. The percentage of type II errors, instead, slightly increases or decreases depending on the the adopted thresholds. We analyze the determinants of the errors and find that merger characteristics as well as procedural issues systematically affect mistakes. In particular, US firms seem

to be treated more leniently than other firms and a too narrow market definition increases type I errors.

Third, according to our rent-reversion regressions, remedies seem to be ineffective before as well as after the reform. Only phase I remedies seem to work properly when adopted in purely horizontal mergers. Some outright clearances are seen by the market as good news for the rivals, possibly indicating the cost of type II errors by the EC. Only prohibitions achieve substantial rent-reversion. However, we can estimate their effect pre-reform since only two mergers have been blocked post-reform. Given the undisputed effectiveness of this merger policy tool compared to remedies, it appears that the EC blocks too few mergers.

Finally, we measure significant deterrence effects pre- and post-reform. Pre-reform, deterrence is achieved via prohibitions, which confirms their role as the most effective merger control tool. Post-reform it appears that withdrawals/abortions substitute the role of prohibitions. We do not observe over-deterrence due to the enforcement of the merger policy since no policy tools affect the likelihood of a merger to be pro-competitive.

Our robustness checks support two of our main identification assumptions: to identify anti-competitive mergers via the rivals' change in profits seems to be a good approximation as confirmed by the regressions from the sample of purely horizontal mergers, where this identification strategy is more likely to hold. Moreover, to identify the introduction of the reform with the date of its legal implementation in May 2004 seems to be important for understanding the change in policy brought about by the reform. We also try to control for the noise in the data by considering different thresholds for pro- and anti-competitiveness and find the results to be quite robust with respect to the choice of thresholds.

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. While, on the one hand, decisions are based on a more economic analysis and we observe a decline in the frequency of type I errors post-reform, we also find that the increased focus on remedies was only partially successful and cannot replace the policy tool of straight prohibitions. They solve both the competitive concerns raised by the concentration and deter future anti-competitive mergers. Clearly, this policy shift was not only the foremost product of the reform, it might be a persistent reaction to the substantial shock and political climate which originated from the Court of First Instance's reverses of three prominent cases in the early 2000s. Yet, an approach to merger control that is more clearly based on economic principles does not necessarily mean abandoning the use of prohibitions, as shown by US antitrust authorities that are far less hesitant to block mergers than their European counterpart. The belief that remedies are a more sophisticated and cleaner instrument to almost surgically appraise merger cases seems,

at least partially, misplaced. Thus, according to our analysis, while some of the changes brought about by the reform seem to go in the right direction, the positive impact on the efficiency of European merger control is dampened especially by the fact that DG Comp deprives itself of its most powerful tool: prohibitions.

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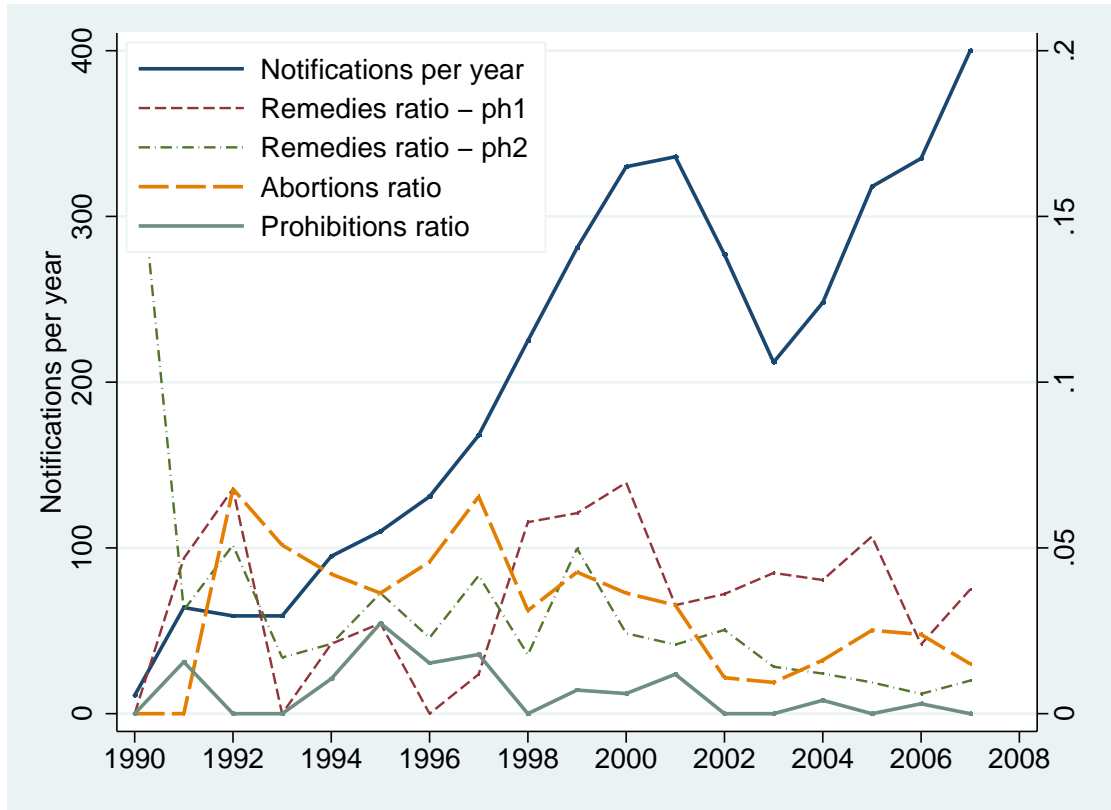
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# 6. Figures and Tables

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 of Dummies

| | 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) |
| Aborted/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: 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 4: Probit Model: Probability of Intervention

| | Ex-ante model | | Investigation model | |
|---|---|---|---|---|
| | Pre-Reform | Post-Reform | Pre-Reform | Post-Reform |
| US firms involved | −0.258*** (0.069) | −0.340*** (0.098) | −0.078* (0.044) | −0.188*** (0.046) |
| Big EU country | 0.005 (0.073) | −0.068 (0.096) | 0.010 (0.045) | 0.006 (0.109) |
| Conglomerate Merger | 0.174* (0.092) | 0.200*** (0.050) | 0.127** (0.055) | 0.332*** (0.088) |
| Full merger | 0.212*** (0.043) | −0.055*** (0.016) | 0.036 (0.039) | −0.058*** (0.022) |
| Crossborder merger | −0.101 (0.078) | −0.070*** (0.027) | −0.063* (0.038) | 0.003 (0.028) |
| Log(MV) merging firms | 0.015*** (0.006) | −0.006** (0.003) | 0.013*** (0.005) | −0.005* (0.003) |
| Log(MV) rivals | 0.012 (0.009) | 0.019* (0.011) | 0.005* (0.003) | 0.006 (0.004) |
| Lagged notifications | 0.002 (0.002) | −0.001** (0.001) | −0.001 (0.001) | −0.002 (0.001) |
| Lagged actions ratio | 0.501 (0.622) | 1.592 (1.010) | 1.192*** (0.301) | 0.575 (0.956) |
| Lagged abortions ratio | −0.109 (1.137) | −1.106 (3.319) | 0.254 (0.586) | −3.195 (2.550) |
| Barriers to entry | | | 0.216*** (0.039) | 0.143** (0.057) |
| Dominant firm | | | 0.175*** (0.038) | 0.286*** (0.056) |
| Phase 2 Case | | | 0.163*** (0.062) | −0.037 (0.038) |
| Worldwide markets | | | −0.063* (0.034) | −0.144** (0.066) |
| National markets | | | 0.066** (0.032) | 0.295*** (0.090) |
| Observations | 211 | 152 | 210 | 152 |
| Pseudo $R^2$ | 0.19 | 0.25 | 0.68 | 0.59 |
| Correctly classified | 70.6% | 76.3% | 89.5% | 90.1% |

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

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

## Table 6: Probit Model: Probability of Type I errors

|  | 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 $\Pi_{Cj}^{A*} < -0.03$ 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.

## Table 7: Probit Model: Probability of Type II errors

|  | 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

| | Pre-reform | | Post-reform | |
|---|---|---|---|---|
| | Merging Parties | Rivals | Merging Parties | Rivals |
| Clearance | 0.023 (0.065) | −0.005 (0.063) | 0.108 (0.060) | 0.007 (0.155) |
| Phase 1 Remedy | 0.131 (0.154) | −0.038 (0.134) | 0.199** (0.067) | −0.014 (0.151) |
| Phase 2 Remedy | −0.086 (0.110) | −0.038 (0.085) | 0.082 (0.110) | −0.007 (0.118) |
| Prohibitions | −0.096 (0.152) | −0.364** (0.147) | | |
| $\Pi^A_{ij}$*Clearance | −0.053 (0.049) | 0.256*** (0.079) | 0.050 (0.056) | 0.296 (0.195) |
| $\Pi^A_{ij}$*Phase 1 Remedy | 0.101 (0.186) | 0.408*** (0.119) | −0.155 (0.147) | 0.010 (0.228) |
| $\Pi^A_{ij}$*Phase 2 Remedy | 0.208** (0.091) | −0.337 (0.495) | 0.815 (0.613) | 0.911* (0.361) |
| $\Pi^A_{ij}$*Prohibition | −1.299** (0.461) | −0.480*** (0.115) | | |
| Observations | 192 | 202 | 133 | 147 |
| $R^2$ | 0.16 | 0.20 | 0.24 | 0.45 |

The dependent variable is the decision corrected CAAR in merger $j$ ($\Pi^{D*}_{fj}$) 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 abortions ph1 | 27.063*** | (8.495) | 40.852*** | (8.660) |
| Lagged abortions 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 abortions ph1 | −87.640*** | (10.941) | −77.644*** | (20.559) |
| Lagged abortions 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.

# Appendix

## 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 occured. 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 $i$. 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: $R_{i,t} = \alpha + \beta R_{market,t} + \varepsilon_{i,t}$.[25] We can then calibrate 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.[26] 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: $\widehat{R}_{i,t} = \widehat{\alpha} + \widehat{\beta} R_{market,t}$.

### 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 expectations 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

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

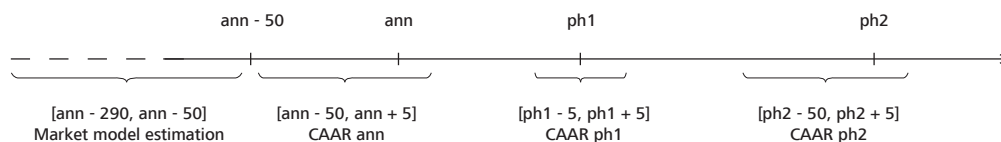[26] For some cases the market model could not be reliably estimated in this period due to data limitations. In these cases the estimations window was shifted to 530 - 290 days prior to announcement.

might not be wholly captured, whereas too large a window could dilute the result.[27] 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.[28] 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 5 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.

Figure 2: Timeline of the events



## A.3. Aggregating the Abnormal Returns

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

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

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[27]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).

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

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.[29] 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.[30] 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

$$CAAR_{fj}^e = \frac{\sum_{i=1}^{N_{fj}} CAR_i^e \, mv_i}{\sum_{i=1}^{N_{fj}} mv_i} \qquad e = ann, dec \qquad f = M, C \quad j = 1, \ldots, 326 \qquad (7)$$

where $N_{fj}$ denotes the number of merging firm or rivals for merger $j$ and $mv_i$ is the market value of firm $i$. The CAARs, as an aggregate measure of the implications of a merger, are used to classify pro- and anti-competitive mergers and serve in the probability of intervention estimation.

## 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

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[29]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.

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

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 be subjected to remedies. 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:[31]

$$E[\Pi^{dec}] = \rho\Pi^{action} + (1-\rho)\Pi^{clear} \tag{8}$$

where $\Pi^{action}(\Pi^{clear})$ denotes the merger's profitability in case of an action (a clearance) and $\rho$ is the probability of an action. The observed abnormal returns around the announcement ($\Pi^{ann}$) therefore are equal to the real effect ($\Pi^{ann*}$) 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 ($\Pi^{dec*} = \Pi^{ann*}$),[32] and that a clearance has no further effect on the market ($\Pi^{clear} = 0$), the impact of a merger can be written as:

$$\Pi^{ann} = \Pi^{ann*} + E[\Pi^{dec}] = \Pi^{ann*} + \rho\underbrace{\Pi^{action}}_{=-\Pi^{ann*}} + (1-\rho)\underbrace{\Pi^{clear}}_{=0} \Leftrightarrow \Pi^{ann*} = \frac{\Pi^{ann}}{1-\rho} \tag{9}$$

Similarly, the effect that we measure around the decision ($\Pi^{dec}$) 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.[33]

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

If a case goes into phase 2, the market will again update its beliefs about remedies.[34] The effect around the phase 1 decision accounts for the adjustment of market expectations

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[31] 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 $j$.

[32] 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 nonzero degree of rent reversal.

[33] 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.

[34] 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.

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 $\Pi^{P1}$ ($\Pi^{P2}$) is the measured effect around the phase 1 (phase 2) decision date.

Combining the equations for the decisions yields

$$\Pi^{dec*} = \begin{cases} \frac{\Pi^{dec}}{1-\rho} & \text{if phase 1 case} \\ \frac{\Pi^{P1}+\Pi^{P2}}{1-\rho} & \text{if phase 2 case} \end{cases} . \tag{10}$$

Thus, to account for expectations, we need to estimate the ex-ante likelihood of an intervention for every merger $j$ ($\rho_j$) and correct the CAARs measured around the announcement ($\Pi^A_{fj}$) and the decision ($\Pi^D_{fj}$) of that merger according to equations (9) and (10). This refinement improves the precision of the estimate of the market competitive assessment of a merger.

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