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An Empirical Assessment of the 2004 EU Merger Policy Reform

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Abstract Based on a database of 326 merger cases scrutinized by the European Commission between 1990 and 2007, we evaluate the economic impact of the change in European merger legislation in 2004. We first propose a general framework to assess merger policy effectiveness, which is based on standard oligopoly theory and makes use of stock market reactions as an external assessment of the merger and the merger control decision. We then focus on four different dimensions of effectiveness: 1) legal certainty; 2) frequency and determinants of type I and type II errors; 3) rent-reversion achieved by different merger policy tools; and 4) deterrence of anti-competitive mergers. To infer the economic impact of the merger policy reform, we compare the results of our four tests before and after its introduction. Our results suggest that the policy reform seems to have been only a modest improvement of European merger policy.

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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, 7 November 2002

The modernization package of European merger control, initiated in 1999, led to the adoption of Council Regulation 139/2004 in May 2004 (ECMR 04). Several observers interpreted this major institutional change as a shock reaction to events that had happened in the early 2000s, when three prohibition decisions of the Directorate General for Competition (DG Comp) were overruled by the Court of First Instance (CFI).[1] In all three successful appeals, the CFI identified the main problems as being related to the rigor of economic analysis conducted by DG Comp and the standard of proof the decision was based upon. While these reverses certainly were an indicator of the need for reform, they were not the cause: A Green Paper calling for a revision of European merger law had been published as early as December 2001.

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] This last 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 applied if the criterion of the first part, the creation of a dominant position, was 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.

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

[2] Lyons (2004) discusses these reforms in greater detail. The problems with the DT and the advantages of the SIEC are summed up in Vickers (2004), while Boege and Müller (2002) present arguments against parting from the well-established DT test.

The reception of the ECMR 04 was generally favorable. Commentators expected the quality and effectiveness of the decisions to improve due to the new approach, which enables a more flexible analysis closer to economic principles. Yet, some commentators feared that the cost of increased flexibility stemming from the adoption of more sophisticated tools in the assessment of unilateral effects and efficiency gains could be a loss in predictability of the merger control process.

Several years have passed since the introduction of the new merger regulation, enough time to make the 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 analyze 326 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. We base our evaluation exercise on a number of maintained theoretical assumptions coming from standard merger theory in an oligopolistic setting (e.g. Farrell and Shapiro (1990)) and the use of stock-market event studies to measure the effect of mergers and merger control decisions. From this starting point, we propose four dimensions of effectiveness of EU 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.

First, we test the predictability of the European merger control procedure. We estimate a simple probit model, where the decisions of DG Comp are a function of observable characteristics. Consistent with previous studies (e.g. Aktas, de Bodt, and Roll (2007), Duso, Neven, and Röller (2007), and Duso, Gugler, and Yurtoglu (2010a)), we find that several institutional and procedural variables play a significant role in explaining the Commission's decision-making. More importantly, we do find evidence that predictability and, in particular, the ability to predict an outright clearance has substantially decreased since the introduction of the reform.

Next 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 initially assess the competitive consequences of the mergers using the abnormal stock market returns of competitors to the merging firms. We define cases in which DG Comp remedied a merger that the stock market regarded as pro-competitive (type I errors) as well as instances in which the EC failed to remedy mergers that were regarded as anti-competitive (type II errors). We

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

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observe that the overall frequency of errors did not significantly change after the reform. Yet, the EC's more pro-active attitude after the reform produced a shift toward more type I and less type II errors. In phase 2, however, it appears that the EC has become too lenient, since type II errors increase during this investigation phase compared to pre-reform.

In a third step, we estimate the degree of rent-reversion induced by the different merger control instruments used by DG Comp. Under a set of maintained assumptions, the relation between the abnormal returns around the announcement of a merger and those around the EC's decision can be interpreted to indicate the success of merger policy in eliminating anti-competitive rents created by a merger (see Duso, Gugler, and Yurtoglu (2010a)). While we find statistically significant full rent-reversion for prohibitions pre-reform, we do not find such reversion for remedies prior to ECMR 04. We cannot estimate the effects of prohibitions after the reform since there are so few, however, we find weak evidence that remedies have become more effective.

Finally, we look at a fourth dimension of effectiveness, which is related to the indirect or deterrent effects of merger control.[4] An effective competition policy should induce firms to obey antitrust rules and, hence, deter firms from proposing anti-competitive mergers. Thus, we estimate the probability of a merger to be 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 or the proportion of horizontal to total mergers (Seldeslachts, Clougherty, and Barros (2009) and Clougherty and Seldeslachts (2010)). We find that, pre-reform, prohibitions as well as phase-1 remedies have a deterrence effect on the likelihood of proposing an anti-competitive merger. Post-reform, the deterrence properties of blockings is replaced by the deterrent effect of withdrawn or aborted mergers. These findings might be explained by the post-reform policy shift away from prohibitions, which appear to have been substituted by more merger withdrawals by the firms.

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.[5] Whereas traditional techniques rely on indirect measures of market power such as concentration ratios or subjective measures of the

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[4]As pointed out by Sørgard (2009), an optimal merger policy entails deterrence, i.e. the effect a decision has on firms' future merger behavior.

[5]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 (2010a) use this methodology to evaluate EU merger control.

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.[6] However, the legitimacy of the event study approach in evaluating mergers has been put into question since it itself presents several shortcomings. Criticisms predominantly include 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 toward 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. 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 and, even more importantly, we correct for the expectations of market participants about the merger proceedings prior to events. By conditioning on all the merger-specific information available around the merger announcement, our correction for the market expectations should help us insulate the pure surprise element for any specific event and, hence, help to measure the competitive effect of the merger and the merger control decision. Finally, we conduct a comprehensive series of robustness tests to derive consistent evidence.

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 our four dimensions of effectiveness via empirical tests, which have

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

been partially developed in our previous work (e.g. Duso, Neven, and Röller (2007) and Duso, Gugler, and Yurtoglu (2010a)) and are partially newly designed in this paper. The objective of our analysis is to use this general 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).[7]

The starting point of our methodology is that merger control aims at avoiding anti-competitive (i.e. consumer welfare decreasing) mergers by either blocking or remedying them or by 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.

2.1. Assumptions

2.1.1. Theoretical Identification

We define an anti-competitive merger as one that reduces consumer welfare.[8] 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 effect triggers the response of 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 generates price increases of merging firms and rivals and a reduction of consumer welfare in models where firms compete in prices and goods are differentiated (Deneckere and Davidson (1985)). Hence, a horizontal merger creates a positive external-ity 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,

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[7]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.

[8]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).

patent-pooling, etc., allow the merged entity to produce more efficiently than before, increasing the competitive pressure on its rivals and thus exerting a negative externality on them.

In most mergers both effects co-exist and what matters for welfare is the net effect of these antipodal forces. As Farrell and Shapiro (1990) show, there exists a critical level of 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.[9] 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 before 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 endogenous 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 (2008)), the holding of our assumption depends on the nature of the sequence of mergers and the merger policy. We therefore control for the merger wave and the horizontal nature of the merger in our regressions. As a further robustness check that we discuss in 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 and merger control decision. Following an extensive literature, we do that by using stock market reactions to the merger's and decision's announcements, i.e. a stock-market event study.[10] This methodology relies on the semi-strong version of the efficient capital

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[9]Farrell and Shapiro (1990) show that these efficiency gains have to be rather substantial to outweigh the market power effect.

[10]As reported by Kothari and Warner (2007), by the end of 2006, there were more than 500 published papers utilizing the event study methodology in different areas of economics.

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 it is not possible to outperform the market index using commonly available information, or, more formally, 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.

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 (e.g. a merger or a merger control decision), 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, can be seen as 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 aggregate abnormal returns" (CAARs).[11]

The measured CAARs around a merger's announcement (or the EC's decision) also might entail effects other than the pure competitive effects and, in particular, the effects of specific forces triggering the merger (e.g. Jovanovic and Rousseau (2002)), information about the roles of merging firms and rivals (e.g. Fridolfsson and Stennek (2010)), and the market expectations about the outcome of the merger control decision (Eckbo (1992)). Hence, 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 (e.g. Banerjee and Eckard (1998). The surprise element to the stock market is likely to be largest around

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

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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 should help us to control for the uncertainty in the allocation of the roles (acquirer, target, rival). In particular, we consider different event windows for the various events to account for different information leakages. For the merger announcement, we use a long window of 50 trading days before the event's date and 5 days after.[12] 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 procedure of the EU merger control. 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).

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 the stocks of merging firms and rivals at the announcement of a merger. Thus, neither at the announcement nor at the decision we do measure the whole extent of the rents generated by the merger or the whole effect of the EC's decision, but only an update of the market's beliefs. We thus have to adjust the measured abnormal returns for the expectations of the stock market. An (extreme) example of a prohibition might clarify the intuition. If we measure a rent of 100 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. See Appendix A.4 for a detailed formal derivation. Given our setup, we are then confident that the corrected CAARs around merger j's announcement A (Π_{fj}^{A*}) can be seen as a meaningful measure of the competitive effects of the merger on merging firms ($f = M$) or competitors ($f = C$), while the corrected CAARs around the EC's decision (D) on merger j (Π_{fj}^{D*}) can be seen as a meaningful measure of the effect of the decision on the group f's profitability.

A final assumption, which is however only needed in 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

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[12]Duso, Gugler, and Yurtoglu (2010b), with a sub-sample of our data, show that the cumulative abnormal returns calculated using this large window correlate positively and significantly with an alternative ex-post measure of profitability based on accounting data. The correlation is, instead, much lower and, in the case of rivals, even negative when using shorter event windows.

merger. 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 (2010a).

2.2. Assessing Policy Effectiveness

The innovation and main contribution of this paper is to assess the economic impact of the introduction of 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, total welfare. Therefore, our 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 effectiveness as measured by 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 decisions by estimating how past interventions affect the competitive nature of currently proposed mergers.

If the reform was successful in making merger policy more effective, we would expect (1) an increase in the predictability of the merger control outcome (legal certainty), (2) a reduction in the frequency of mistakes (decision errors), (3) a larger degree of rent-reversion achieved by remedies, and finally, (4) a higher degree of deterrence of anti-competitive 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 (e.g. Smith (1957) and Elman (1965)). The benefits of legal certainty known to the literature are numerous: it increases the credibility of the authorities by encouraging them to base their decisions on accurate facts and sound economic reason-

ing, making their rulings more consistent and fair. It fosters accountability and reduces personal biases in the decision process. It allows the concerned firms to better understand the merger review process and predict its outcome with a certain reliability, thereby discouraging them from proposing clearly welfare-reducing mergers.[13] Thus, a predictable merger control process reduces the high costs and reputation losses entailed by a lengthy antitrust procedure (Neven, Nuttall, and Seabright (1993)). Moreover, the transparency of legal procedures increases the potential for harmonization among multiple regulatory authorities. Transparency can, however, also entail costs (McAfee (2010)). By encouraging simple rules it might lead to repeated consistent errors, which could be avoided by using more flexible, case-by-case criteria allowed by a rule-of-reason approach. It increases the costs borne by antitrust authorities by imposing them to disclose the outcomes and the economic and legal reasoning motivating their decisions. Finally, it might also delay innovations in the adoption of more sophisticated and precise assessment techniques by creating path dependency with respect to past decisions.

Yet, a balance of these costs and benefits reveals undoubtedly that transparency, especially with concern to the process underlying antitrust evaluation, is desirable.[14] The transparency of the proceedings of a competition authority can be classified on the basis of the comprehensiveness and public availability of documents containing the economic reasoning in merger cases, guidelines for their handling, and detailed statistics. In general, the level of transparency of European merger control has been found to be laudable in comparison with that of other competition authorities (e.g. Gelfand and Calsyn (2005)).[15]

The impact of ECMR 04 on the predictability of merger decisions, however, is am-

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[13] 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 to be proposed by firms.

[14] Shapiro (2010), for instance, reports that the US Assistant Attorney General "explained [...] that a major goal of revising the [US merger] Guidelines [in 2010] was to provide greater transparency [...] and reduce the gaps between the Guidelines and actual agency practice–gaps in the sense of both omissions of important factors that help predict the competitive effects of mergers and statements that are either misleading or inaccurate."

[15] DG Competition itself is concerned with the transparency and predictability of its proceedings. For instance, a press release on January, 6, 2010 reports: "Detailed explanations concerning how European Commission antitrust procedures work in practice have just been published by the Commission's Directorate General for Competition (DG Competition) and the Hearing Officers on the Europa website in order to further enhance the transparency and the predictability of Commission antitrust proceedings. The explanations are outlined in three documents, namely Best Practices for antitrust proceedings, Best Practices for the submission of economic evidence (both in antitrust and merger proceedings) and Guidance on the role of the Hearing Officers in the context of antitrust proceedings. The documents will make it easier for companies under investigation to understand how the

biguous. 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 (e.g. Kobayashi (1997)).[16]

As discussed by Voigt and Schmidt (2005), predictability can be defined as the "capacity to make predictions concerning the actions of others that have a high chance of turning out to be correct." Before proposing a concentration, the involved firms should be able to predict to a large extent 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.[17]

Thus 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 will 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. We measure the predictability of the decision on the basis of goodness of fit measures of the following regression:[18]

$$P_j = \alpha_0 + \alpha_1 X_j + \varepsilon_j \qquad (2)$$

We would expect changes in legal certainty to be accompanied by changes in the predictive power of model (2), measured by standard statistics such as the $pseudo-R^2$, the percentage of correct predictions, the sensitivity (i.e. the proportion of actual

positives which are correctly identified), and the specificity (i.e. the proportion of actual negatives which are correctly identified).

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 would be reduced, hence the optimal decision rule for merger j is:

$$D_j = \begin{cases} 0 & \text{(clear) iff } CS_j \geq 0 \\ 1 & \text{(intervention) iff } CS_j < 0 \end{cases}$$

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.[19] In order to measure $E1_j$ and $E2_j$, we need to measure D_j, which requires an estimate of the impact of the merger on consumer surplus. Under our maintained assumptions, consumer surplus decreases after the merger if the profits of the rivals to the merging firms increase. Hence, the consumer welfare-maximizing merger control decision is:

$$D_j = 1 \text{ if } \Pi_{Cj}^{A*} > 0$$

where Π_{Cj}^{A*} represents the corrected merger announcement CAARs of the competitors (C) for merger j.[20] Once we have defined type I and type II errors, we analyze their determinants by running the following probit regressions:

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[19]We assume that taking action is not strictly necessary in cases the market evaluates as pro-competitive. 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 will come back to this important point in section 4.2.

[20] Notice that, in order to identify whether deals are perceived as anti-competitive, we only use the sign of the expected change in the stock price. Hence, the fact that the market may anticipate the outcome of the antitrust procedure or any other bias in the *size* of the CAARs due to other market expectations, do not introduce a bias in our identification.

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

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

Clearly, if the agency is benevolent, then both errors should be completely random and, hence, neither the determinants X_j not the constants α_0 and β_0 should have significant explanatory power to predict them. However, in a political economy model of merger control (e.g. Neven and Röller (2005)), the antitrust agency maximizes an objective function containing not only the welfare of the economy 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 and institutional and political economy variables that may influence the decision of an antitrust agency (see Duso, Neven, and Röller (2007)).

In this political economy context, the gains generated by the merger for the merging firms and their rivals can be seen as the amount of resources that these firms are willing to pay to obtain their preferred outcome.[21] One would expect type I errors to decrease with the gains of the merging firms, as they are interested in obtaining a clearance for a pro-competitive merger, and to increase with the gains of the rivals, as they would like to block such mergers. The interests of both kinds of firms are, instead, aligned in the case of anti-competitive mergers. Hence, the probability of a type II error should increase with the gains of the merging firms as well as with those of the rivals, if they manage to lobby for their favorable decision.

Second, the size of the country from which the merging firms originate could also play a role in the outcome of a merger investigation, presumably because of the political pressure that can be exerted by large countries (e.g. Neven, Nuttall, and Seabright (1993) and Horn and Levinsohn (2001)). Third, 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. Fourth, the EC was often alleged to define relevant markets too narrowly, which might imply a higher frequency of errors (Neven, Nuttall, and Seabright (1993)). Fifth, 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 investigation (phase 2) might increase the likelihood

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[21]We define the gains for the merging firms and rivals as the CAARs multiplied by their market value, i.e. the firms' cumulative aggregate absolute value change due to the merger.

of errors. Sixth, 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 (e.g. Neary (2007)). Seventh, the pattern of errors may vary across the sectors in which the mergers are taking place, as some industrial sectors are more concentrated and/or have more political influence than others. Finally, the EC may learn over time how to implement more precise and effective remedial actions (Duso, Gugler, and Yurtoglu (2010a)) and, therefore, the proclivity of errors might be a function of the history of merger control decisions and of time.

To assess how the institutional changes introduced together with 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 (2010a) 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:

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

where Π_{fj}^{D*} is the probability-corrected decision CAAR of merging firms ($f = M$) and competitors ($f = C$), respectively, for merger j, while Π_{fj}^{A*} is the probability-corrected announcement CAAR. We estimate different intercepts (αs) and slopes (βs) for the different decisions (d=*clearance, behavioral remedies, structural remedies,* or *prohibition*).[22]

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[22]We distinguish remedies in two broad categories: behavioral and structural. Behavioral remedies contain all obligations concerning the conduct of the merged entity vis-à-vis its competitors. These include access to key facilities, supply guarantees and the disentanglement of intertwined directorates. Structural remedies mostly concern the divestiture of a business branch or a production site. Past empirical research suggests the superiority of structural remedies over behavioral ones - e.g. the in-house studies of the EU Commission (European Commission (2005)) and the FTC (Baer (1999)).

In all specifications we control for time and industry effects (manufacturing and communications), as well as some merger-specific characteristics (full and conglomerate mergers).

Duso, Gugler, and Yurtoglu (2010a) 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. The key points are, however, summarized below.

Prohibitions. The most extreme action taken by the EC, i.e. to block the merger, dissipates all rents, i.e. both the market power and the efficiency rents. Therefore, the slope coefficient should be $\beta_{prohib} = -1$ for merging firms and rivals. Moreover, the regression line should pass through the origin: if the merger does not generate any rents, no rents can be reverted by the decision, thus $\alpha_{prohib} = 0$.

Clearances. If the merger is cleared without commitments, we do not expect decision effects that are systematically related to announcement returns, thus $\alpha_{clear} = 0$ and $\beta_{clear} = 0$ for merging firms and rivals. 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 this case, we expect positive constants and/or slopes for the rivals if the clearing of the merger signals a green light from the EC to mergers in that particular industry. This is even more likely if the EC makes type II errors and unconditionally clears anti-competitive mergers.

Remedies. 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. Hence, for rivals, we expect a negative intercept as well as a negative slope. The former captures the shift due to the elimination of the market power rents, while the latter indicates that rent-reversion should be larger, the larger the size of the market power rents generated by the merger. For the merging firms, instead, since both market power and efficiency effects are positive, we only expect a negative slope, while we expect a zero intercept.

We run separate regressions for the pre- and post-reform periods to assess the impact of the new merger regulation.[23] The variables contained in the vector X_j are again exogenous controls, such as year and industry dummies, and other merger-specific characteristics.

2.2.4. Deterrence

Up to this point we have focused on the ex ante (predictability) and *in-fieri* effects (decision errors and rent-reversion) of merger policy. However, as pointed out by Sørgard

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[23]Only two cases were blocked post-ECMR 04, so we had to drop prohibitions from the effectiveness regressions.

(2009) an optimal merger policy also entails ex-post effects since it involves deterrence. In particular, he shows that there is an optimal level of enforcement where some actions, which in isolation would be welfare detrimental, might be optimally taken to achieve deterrence and thus increase overall welfare. Hence, the role of deterrence is especially important if competition authorities commit errors and if their remedies are not always and completely effective. If this was not the case and 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.

Very few studies have empirically analyzed deterrence in merger policy. Seldeslachts, Clougherty, and Barros (2009), by using a panel of antitrust jurisdictions over the period 1992-2003, find that prohibitions deter future merger activity, as measured by the number of merger notifications, while remedies do not. Hence, they exclusively focus on the frequency aspect but cannot say whether merger policy over-deterred pro-competitive mergers. The question of the composition of mergers which are deterred by the policy is analyzed in a sequent paper (Clougherty and Seldeslachts (2010)). They analyze the US merger policy in the period 1986-1999 and show that second-request investigations and, even more so, antitrust interventions yield significant deterrence effects for horizontal mergers, i.e. negatively affect the ratio between horizontal to total notified mergers. They conclude that a tougher merger policy makes firms move away from potentially problematic horizontal mergers toward vertical mergers that are more likely to be efficiency-increasing. Yet, such identification is quite simplistic since many horizontal mergers might involve efficiency-enhancing synergies. 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 the direction of analyzing this issue and focuses on measuring 'good' deterrence. This is enabled by the unique information contained in our dataset that allows a much cleaner definition of the competitive nature of each merger and a finer prediction on the quality of deterrence achieved by the policy. For each merger we use the indicator of its anti-competitive nature ($D_j = 1$ if $\Pi_{Cj}^{A*} \geq 0$) and test for the deterrence effects of merger policy by looking at how past decisions affects the probability of a particular merger to be anti-competitive. In particular, we look at how the complete

merger policy history of the European Commission affected the anti-competitiveness of the mergers in our sample. We thus combine measures of DG Comp's merger policy from the population of over 3,800 mergers scrutinized in the sample period with our dataset to estimate a probit equation of the following type:

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

where D_{jt} is equal to 1 if merger j proposed in quarter t was anti-competitive. The variable n_{jt-2} is equal to the total number of notifications to the EC two-quarters before merger j was notified, and $\frac{d_{jt-2}}{n_{jt-2}}$ represents the ratio of the total number of mergers with outcome d (where $d = remedies, blockings$, or $withdrawals$) over the total number of notified mergers lagged by two-quarters. Again, we control for other merger specific determinants X_j as we do in the other regressions.

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. 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 Yurtoglu (2005) 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 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. First, ineffective remedies cannot strongly deter as they do not constitute a real threat for firms which want to propose anti-competitive combinations. Second, if the antitrust authority becomes tougher and imposes remedies on mergers which were expected to be outrightly cleared, then the firms will see this as a negative signal and, potentially,

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some anti-competitive mergers will not be proposed. If, instead, the authority applies remedies on mergers which were expected to be blocked, the firms may update their beliefs, deducing that merger control has become more lenient and, therefore, propose more anti-competitive mergers. We will use the difference between phase 1 and phase 2 remedies to shed some light on these issues.

We estimate regression (6) separately for the pre- and post-reform periods to identify the potential effect of the reform. Notice that, similarly to the rent-reversion regressions, we are not able to identify the effect of prohibitions post-reform, since only two mergers were blocked after 2004.

3. Data

3.1. Data Sources

Our sample includes 326 merger cases scrutinized by the EC from the beginning of 1990 to the end of 2007. We collected information on all phase 2 mergers during the sample period, together with a randomly matched sample of phase 1 merger cases. The first half of the sample (157 mergers from 1990 to the end of 2002) was first developed by Duso, Neven, and Röller (2007), whereas the 169 cases for the 2003-2007 period were newly collected and evaluated for this paper.[24] 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 decision, and some other merger-specific characteristics.[25] From these decisions, we identified a total of 3,026 involved parties, 735 of them are merging parties and 2,291 are competitors. Due to data limitations and the requirement that firms have to be listed, the final sample contains 1,771 of these firms (522 merging parties and 1,249 competitors). Since there are overlaps in the firms' roles as merging parties and competitors, i.e. the same firm appearing as a merging party and/or competitor in multiple cases, our sample includes 1,104 unique firms.

Using the EC's merger assessment to identify the rivals represents a particular strength of this sample. It has the big advantage of being a much more realistic description of the relevant 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

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[24]While, for the first part of the sample, phase 1 cases were taken completely randomly, we over-sampled the phase 1 cases cleared with remedies in the 2003-2007 period. We did this because we were particularly interested in analyzing the effects of actions in phase 1.

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

in a merger. This is particularly true for large, integrated corporations, which are active in numerous sectors (e.g. Eckbo and Wier (1985)).

Following Banerjee and Eckard (1998), the announcement date of a merger is defined as the date on which first rumors of that particular merger leaked to the market. This is usually before the official notification to the EC as well as the official merger announcement. We used the financial press and the Dow Jones Interactive database to identify the dates at which the first definitive indications of the combination between the merging parties became known.[26] 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 dummy variables partitioning our dataset for the periods before and after the merger policy reform in our sample and in the population.

[insert Table 1 here]

In our sample, the percentage of cases that were cleared with remedies increases from 42% in the pre-reform period to 60% post-reform. If compared to the population data (7% pre-reform to 6% post-reform), remedied cases are clearly over-sampled. This is due to two facts. First, we over-sampled phase 2 cases. Second, for the period 2002-2007, we also over-sampled remedied cases in phase 1, which explains their increase from 14% to 40%, while in the population data they are constant at a rate of 4%. Our sample, however, mimics the population data for what concerns the use of remedies in phase 2. They decrease from 28% to 14% from the pre- to the post-reform period in our sample and from 3% to 1% in the population. Prohibitions represent 6% and 2% of the cases pre- and post-reform in the sample, and 0.8% and 0.06% 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% and 2% of the notified cases pre- and post-reform, respectively.

[insert Figure 1 here]

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[26]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 able to identify only 240 of our original mergers. In this sub-sample, the correlation between the first rumors and the official merger announcement is very high (0.9938). Most of the official announcements are in an interval around five days before and two days after the first rumors. For some 20% of the cases the first rumor was 15 days or earlier before the official announcement.

Figures 1 and 2 show the time evolution of notifications and actions in the sample and in the population respectively. In both the sample and the population, we observe an increasing trend in notifications with a single big drop around 2002. The proportion of remedies in phase 2 oscillates before 1999 to then take a downward trend, while the proportion of remedies in phase 1 increases substantially. The prohibitions ratio displays a downward trend, with only 2 prohibitions after the merger reform. Abortions or withdrawals stay at a rate of 4% to 6% until 2002, then slightly decrease to increase again in 2005 and 2006.

Finally, for the mergers in our sample, we have more precise information about the cases' specificities (bottom part of table 1). From the pre- to the post-reform period, we observe an increase in divestitures (from 29% to 45%), a small decrease in behavioral remedies (from 26% to 25%), a tendency to use a narrower market definition (more national and less EU markets), more conglomerate and full mergers, and less involvement of US firms.

3.3. Structure of the Cumulative Abnormal Returns

We calculate the CAARs over the event windows according to the methods presented in Appendix 6. 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% level. After the reform, mergers are still significantly profitable for merging firms yet the average CAAR drops to 1.1%. The impact of DG Comp's decisions on the valuation of merging firms is very similar pre- and post- reform and entails a negative, but not significant, drop in the firms' stock value by 0.3% and 0.5% respectively.

The competitors' merger announcement effects are positive (0.8%) but not significant prior to the reform, and they increase to 1.1% and become significant at the 10% significance level post-reform. Similar to the merging firms, rivals suffer an average negative reaction around the EC decision, which ranges between 0.3% pre-reform and 0.9% post-reform. In both cases, however, the average CAARs are not statistically different from zero.

4. Empirical Results

4.1. Probability of Intervention

The first piece of evidence on the effects of the merger policy reform relates to the predictability of antitrust intervention. The regression results of the probit model (2) are displayed in table 3. We report the marginal effects of different sets of observable factors that we expect to explain the intervention of the EC on a specific case. Before looking at the coefficient estimates, we first focus on the predictive power of the probit regressions pre- and post-reform. We then discuss in brief the main determinants of an action and how they changed over time.

[insert Table 3 here]

The first observation is that the $pseudo - R^2$ pre-reform (0.51) is more than twice as large as post-reform (0.23). This implies that our model explains the behavior of the EU Commission pre-reform much better than after the reform. We interpret this first finding as a sign that the predictability of merger policy decreased after the introduction of the new merger regulation. A merger control approach based more on a case-by-case analysis and relying less on clear-cut rules reduces the predictive power of 'external', observable determinants.

Looking at the other measures of goodness of fit, our model is able to correctly classify 87% of the observations pre-reform, whereas post-reform we correctly classify only 71% of the observations. We can also identify where this difference comes from. There is very little difference pre-reform (86%) and post-reform (83%) in the ability of our model to correctly classify the actions by the EU Commission measured by the sensitivity. On the contrary, post-reform the model is much less able to correctly classify the outright clearances, i.e. specificity. This happens in only 54% of the cases post-reform versus 87% of the cases pre-reform. Therefore, it seems that post-reform it is more difficult to predict the absence of an action by the EC based on measurable characteristics. We can conclude that this first piece of evidence supports the view that the merger policy reform decreased legal certainty.

We then look at the observable factors, which are significant predictors of the merger control outcome and identify some differences. A first set of variables that we consider is related to the 'political economy environment'. The country of origin of the firms involved in a merger case is an important driver of the EC's decision. In both periods, the probability of eliciting an action is over 42% lower if a US firm is involved. Also, there is a shift toward a significant reduction in the likelihood of an action if a big EU

country was involved in the case post-merger reform. This confirms the argument put forward by Horn and Levinsohn (2001), who point to the effectiveness of the political pressure by large countries.[27]

Merger specific characteristics play a more significant role pre-reform, with a significant increase in the probability of eliciting an action for full mergers, and mergers between large firms and with large competitors. This corresponds to the intuition that full mergers are potentially more harmful to competition than mere share acquisitions or joint ventures. It also suggests that the EC was more suspicious of concentrations among large firms pre-reform, which can be seen as in accordance with the doctrine dictated by the dominance test. Post-reform, instead, full mergers are 21% significantly less likely to be remedied or blocked. While the size of the merging firms does not play a role anymore, the size of the competitors still significantly increases the likelihood of an action, which can be seen as more in line with the new SIEC test.

The anti-competitive nature of the merger should be the main driver of the antitrust decision. We use two different proxies for this unobserved factor. Our first indicator is the size of the announcement CAARs of the rivals, as the signals upon which the market bases its evaluation of a merger might also be available to DG Comp during its assessment of the combination. Even though the CAAR-coefficient assumes the expected sign after the reform, no statistically significant relationship emerges. Instead, the fact that the merger went to phase 2, which is our second indicator of the competitive issues raised by the merger, is a very significant predictor. Yet, sizeable effects are only seen pre-reform (an increase of 78% in the likelihood of an action), while post-reform the increase is by only 5%. This corroborates the fact that in more recent years the EC increasingly started to use remedies also in the early investigation phase. The market definition is an important determinant of an intervention. If the relevant market is defined nationally, the likelihood of eliciting an action increases by over 43% pre-reform and 58% post-reform: smaller product markets are more problematic. Consistently, post-reform, if the involved firms (merging and rivals) originate from more geographic world regions, the probability of intervention is significantly reduced. Industry effects are significant pre-reform (transport and finance) and post-reform (communication).

Finally, we look at whether past decisions of the EC affect current decisions to capture possible learning or path-dependency effects. If the EC was very active in the last-but-one quarter, the probability of an action increases significantly. This points to some kind of path-dependency in the EC's decisions. The limited resources' argument seems to have bite post-reform, since the likelihood of an intervention decreases significantly with

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[27]The big EU countries are: France, Germany, Italy, Spain, and the UK.

an increase of notifications in the last quarter. Post-reform, if there are more abortions in the previous last-but-one quarter, the EC steps in less frequently. This might be an indication that abortions or withdrawals have a deterrent effect post-reform, an issue to which we return later. We also observe a positive and significant time trend toward more interventions by the EC in the post-reform period.

As we explain in the appendix A.4, we use the predicted values from these probit regressions to estimate the probability of an action $\widehat{\rho}$, which is used to build our measure of corrected CAARs that will be used in the following steps of our analysis.

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 EU Commission's possible mistakes in the enforcement of the merger regulation. We first look at simple frequencies, which are reported in table 4.

[insert Table 4 here]

Post-reform, we observe on average more type I errors (32% compared to 22%) and less type II errors (18% compared to 27%) than in the pre-reform period. This hints to the fact that the EC has become, on average, more active since the introduction of the new merger regulation: it intervenes more often in pro-competitive mergers and there are fewer anti-competitive cases without intervention. While a welfare analysis of this policy shift is not possible in our context, we want to stress that we measure *weak* type I errors. The cost generated by the application of a remedy on a merger, which is pro-competitive on average over all involved product and geographical markets, is possibly smaller than the cost of letting anti-competitive mergers through completely unchallenged.

To analyze the determinants of type I errors (i.e. the EC intervenes in pro-competitive mergers), we use the variables discussed in section 2.2.1.[28] The results of regression (3) are reported in table 5.

[insert Table 5 here]

First, we look at measures of governmental pressure. 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, 76% lower in the pre-reform period and 57% lower after the reform. This reflects the results discussed above when we presented the determinants of an action.

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

Apparently, thus, the EC is particularly cautious to avoid making type I errors when US firms are involved and this pattern does not change after the reform. We do not find any evidence of lobbying by firms, as expressed by the gains they obtain through the mergers, both before and after the reform.

Full, pro-competitive mergers are more likely to be wrongly remedied before the reform but less so after the reform. Before the reform, a type I error is significantly more likely when the merging firms and the competitors are large, while after the reform the role of competitors is not significant anymore. Post-reform, cross-border mergers are more likely to be wrongly remedied, however. The market definition is again very important. The EC tends to commit significantly more type I errors if the market is narrowly defined (nationally), since the likelihood of a mistake significantly decreases if the market is defined to be EU-wide or world-wide. This pattern is even reinforced post-reform. Additionally, post-reform, the fact that the merger affects several geographic regions reduces type I errors. Both before and after the introduction of the new merger regulation, we find significant industry effects. Only after the reform does it seem that the EC's past decisions affect the proclivity of type I errors. The likelihood decreases if the number of notifications increases: A large work-load prevents the EC from being too pro-active in general and, in particular, against pro-competitive cases. The model's predictive power is very high both in terms of $pseudo - R^2$ and in terms of correct predications. Low values for the specificity statistic mostly explain why post-reform the percentage of correctly classified cases decreases from 93% to 82%.

We then move to the estimation of equation (4): the determinants of type II errors (i.e. the EC unconditionally clears anti-competitive mergers). The marginal effects of the probit estimations are reported in table 6.

[insert Table 6 here]

We estimate significantly more type II errors in mergers involving US firms and merging parties coming from the big EU countries, but only post-merger reform. This hints at some form of political pressure by national governments. Moreover, we observe significant effects for the gains variables, which might signal lobbying activity by the involved firms. Indeed, the larger the gains for the merging firms, the lower the likelihood of eliciting an action in an anti-competitive merger, which is consistent with successful lobbying by merging parties. On the contrary, the coefficient for the rivals is negative and significant in the post-reform period. This clearly goes against the idea that the rivals were also successful in lobbying the EC to let an anti-competitive merger be cleared. The negative sign might be interpreted as revealing that the EC is increasingly careful not to

commit type II errors when the merger becomes more anti-competitive, i.e. the larger the profit gains for the rivals. Post-reform, the EC seems to be too lenient toward full mergers, cross-border mergers, mergers involving large parties, and mergers concerning many world regions and product markets. The likelihood of type II errors increase in phase 2 post-reform, while pre-reform it decreases if the merger entered the in-depth investigation phase. This indicates that the EC became too hesitant to block or remedy mergers post-reform in phase 2. Again the predictions of the model are quite accurate with a $pseudo - R^2$ of over 50% and the percentage of correct predictions are close to 90% in both periods.

To conclude, our analysis so far shows that DG Comp became more pro-active and therefore the composition of the mistakes changed toward more (weak) type I and less type II errors, particularly in phase 2. Post-reform, we find some evidence of successful political pressure by large EU countries and the US and lobbying by merging parties to get potentially anti-competitive mergers cleared.

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 the 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*, *behavioral* and *structural remedies*, and *prohibition*), and on the inter-action 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 7 for the pre-reform period are very close to those obtained by Duso, Gugler, and Yurtoglu (2010a) for the years 1990-2002.

[insert Table 7 here]

We estimate a negative and significant slope of -0.92 for outright blockings for merging firms, which is also negative but not significant for rivals (-0.53). Both coefficients are not statistically significantly different from minus one. Hence, prohibitions seem to fully 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.

Clearances have a positive and significant effect on merging firms as measured by the intercept, yet, only in the pre-reform period. This suggests that outright clearances send a positive, unexpected signal to the market. For rivals, the positive and significant slope estimates, both pre- and post-reform, imply that after an outright clearance their rents

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increase with the size of their announcement rents, which we interpret as a measure of anti-competitiveness. Hence, our interpretation is that these findings are consistent with the market evaluating the cost of a type II error: the more anti-competitive the deal is (i.e. the larger the rivals' rents at announcement), the more rivals profit from an outright clearance. The coefficient estimates for remedies are also not in line with the predictions for an effective merger control. In particular, the predicted negative shift for rivals is not observed pre-reform, since the estimated intercepts are not significantly different from zero. For merging firms, we find divestitures to even produce a positive and significant shift.

To sum up, the main results pre-reform are: 1) prohibitions achieve full rent-reversion; 2) remedies do not achieve full rent-reversion on average; 3) outright clearances increase rents for merging firms and rivals, and the larger the merger effect at announcement the more they do so. Our interpretation of these findings in terms of merger policy effectiveness is: 1) prohibitions are an effective merger control tool; 2) remedies are not perfectly effective; and 3) some outright clearances might indeed be type II errors of the EC.

Looking at the post-reform sample, we only get two significant coefficients, the intercept for behavioral remedies and the clearance slope for rivals. Notice, however, that we cannot estimate the degree of rent-reversion achieved by prohibitions, since only two mergers were blocked post-reform. The negative intercept estimates in the case of remedies for rivals are indicative that remedies achieve, on average, some degree of rent-reversion post-reform. Yet, this reversion is not connected to the size of the announcement gains, since all slope estimates in the case of remedies are not significantly different from zero. The positive clearance slope for rivals can again indicate that the market prices some of the Commission's type II errors.

The comparison of the scenarios before and after the reform suggest that, indeed, post-reform remedies have on average become a bit more effective in reverting rents. However, we definitely do not observe full rent-reversion and outright clearances remain good news for rivals, which we interpret as being their benefit from type II errors. Thus, the reluctance of DG Comp to block mergers post-reform does not seem to be well-placed.

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 always 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 anti-competitive as a function of the history of past merger control decisions. The marginal effects of the probit estimations are reported in table 8.

[insert Table 8 here]

We estimate negative and significant coefficients for the intensity of remedies in phase 1 and for prohibitions in the pre-reform period. When the EC increases the use of these kinds of policy tools in the three to six months previous a newly notified merger, its likelihood of being anti-competitive is significantly lower: these actions deter anti-competitive mergers. Prohibitions deter because they are the toughest policy tools. Remedies in phase 1 deter because they often come at the expense of expected clearances (e.g. 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 (2010a) and European Commission (2005)). Remedies and abortions in phase 2, on the contrary, do not deter anti-competitive mergers. They even increase the probability of anti-competitive mergers, possibly because they come at the expenses of a tougher action (prohibition) and therefore signal a soft antitrust stance by the EC.

Once again, we cannot test for the effects of prohibitions post-reform, as only two mergers were blocked after 2004. However, the ratios of withdrawn or aborted mergers in phase 1 and phase 2 have a negative and significant effect on the likelihood of a newly notified merger being anti-competitive. Moreover, both the remedy ratios in phase 1 and in phase 2 have a negative, though not significant, effect on the probability of a merger to be anti-competitive. After the reform, there was a clear policy shift toward a different use of merger tools. Prohibitions became a very rare event, and withdrawals appear to at least partially take over their deterrent role. One possible interpretation of these findings is that firms were pushed by the EC to withdraw particularly problematic mergers by setting the anti-competitive concerns at such a high level that any kind of remedy would have become too costly. Hence, these abortions might have been effective prohibitions.

4.5. Robustness Checks

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 used a dummy for conglomerate mergers to control for this issue, which is set equal to 1 for all those 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 all these 118 cases from our sample, which leaves us with 162 mergers pre-reform and 46 post-reform.[29]

The results for the merger policy's predictability are only minimally affected in terms of sign and significance.[30] We observe some small changes in the size of the estimated marginal effect. Post-reform, the fit of the model is slightly increased even though some variables must be dropped because of collinearity due to the extremely small sample size (46 observations). The percentage of type I errors is higher than in the full sample (22% pre-reform and 43% post-reform) and a similar pattern is observed for the type II errors (33% pre-reform and 13.6% post-reform). Hence, the tendencies of a reduction of type II errors at the expense of an increased number of type I errors that we observed in the full sample are reinforced when considering purely horizontal mergers. The determinants of both types of errors are very comparable in terms of sign, significance, and size of the marginal effect pre-reform. While the market definition variables becomes not significant in the type II regression, in both type I and type II regressions the effect for the US becomes even larger and more significant. Due to the limited sample size, it is unfortunately not possible to estimate the determinants of the errors post-reform.[31]

Also the rent-reversion regressions reflect the main findings observed in the full sample. The full rent-reversion achieved by the blocking decisions pre-reform is, as expected, even reinforced in the sub-sample of horizontal mergers – the coefficients are now -0.66 for the merging parties and -1.32 for the rivals which, in both cases, are not significantly different from -1. The sign and size of the other coefficients are comparable to those from the main specification, hence the results are not qualitatively affected. Post-reform, the slope for clearance for the rivals is still positive and significant, while the partial rent-reversion achieved by divestiture cannot be confirmed. Also in this case, the same qualitative results are attained using the second definition of horizontal mergers.

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[29] As an alternative definition for horizontal mergers, we also use a dummy that takes on the value of 1 if both merging parties share their primary SIC-classification codes. While the two definitions overlap for most of our cases (60%), in some they don't. According to this second definition, we drop 118 observations (54 pre-reform and 64 post-reform).

[30] Due to lack of space, we do not report all tables. The extensive results can, however, be obtained from the authors upon request.

[31] Using the second definition for horizontal mergers, the results pre-reform are very similar to those obtained for the full sample. The results for the post-reform regressions are in line with the main specifications, yet several coefficients are less precisely estimated and thus less significant due to the small samples size.

Concerning the deterrence regressions, the pre-reform results from the full sample are almost perfectly matched for purely horizontal mergers. Post-reform, some of the coefficients estimates become not significant due to the small sample size, even though the sign and size of the coefficients are not affected. Overall, we can therefore conclude that focusing on purely horizontal mergers does not alter our qualitative results, even though the smaller sample size in some cases affects the precision of the estimation and hence the significance level, especially post-reform.

4.5.2. The Timing of the Reform

To identify the effect of the reform, we choose the official date in 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 the legal introduction of the new merger regulation and could have affected the Commission's policy enforcement.[32] On the other hand, the right timing to start the reform's assessment could also have been after May 2004, since it might have taken time before some of the innovations brought by the reform had a clear policy impact. Hence, we propose two robustness checks for this issue. First, we date the starting of the post-reform period back to the beginning of 2003. Second, we eliminate the entire year 2004 from the sample.

In both cases, the predictability of the policy pre-reform slightly increases (pseudo-R^2 of 0.57 and 0.52 in the two robustness checks respectively) and the number of significant regressors becomes larger, which reinforces our argument that the predictability of EU merger policy started to decrease *around* the legal introduction of the new merger regulation due to the implementation of a more economic-oriented approach to the merger appraisal. This is confirmed by the post-reform results – especially when we concentrate on the post-2004 period – which show a drop in the specificity statistics to 42%.

The frequency of type I and type II errors is not strongly affected by the selection of a different date or sample for the introduction of the reform. The determinants regressions for type I errors show similar coefficients estimates to the main specification, some of

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[32]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.

which are however less significant (EU firms's involvement, regions and markets involved, industry dummies). Contrary to our expectations, this is particularly true post-reform. A similar pattern results from the type II regressions in both robustness checks. However, the post-reform results are much more consistent with our main specification when we exclude the entire year 2004. Again, this suggests that the change in the Commission's behavior around the time of the legal introduction of the reform in 2004 is particularly important in the analysis of the determinants of the the Commission's errors.

The rent-reversion regressions are not strongly affected by the change in the timing of the introduction of the ECMR 04 if not for a drop in significance in the post-reform regressions in both robustness checks, where no coefficient estimates are significant. Finally, choosing the beginning of 2003 as the introduction year does not change any of the signs of the coefficients estimates both pre- and post-reform in the deterrence regression. Yet, it leads to a drop in significance, especially for prohibitions pre-reform and withdrawals post-reform. However, when we exclude the entire year 2004, all the findings obtained in our main regressions are perfectly mirrored in the robustness checks and 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 much 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 date of the legal introduction of ECMR 04 was substantial and supports our choice as the most precise way to identify the effects of the reform.

5. Conclusion

In our attempt to assess the economic impact of the change in legislation due to the 2004 merger policy reform in Europe, we brought forward four pieces of evidence: (1) estimations of the determinants of intervention, (2) estimations of the frequency and determinants of type I and type II errors, (3) estimations of rent-reversion by merger decisions, and (4) estimations of the deterrence effect of merger decisions. These elements are thought to provide a comprehensive evaluation of the entire process of merger control: from an *ex ante* perspective on the predictability of the policy, to an *in-fieri* analysis of the effect of particular merger tools on the merger cases under scrutiny, to finish with the *ex-post* view of the effect of current policy enforcement on future firms' behavior. The identification of the reform's effect is achieved by comparing the performance of merger control along these four dimensions in the pre-reform and post-reform periods.

Our main findings can be summarized as follows. First, we find evidence of a decrease in legal certainty, since DG Comp's decisions become more difficult to predict on the basis of observable characteristics post-reform. In particular, it has become significantly more difficult to predict if the EC will unconditionally clear a merger. Second, there is a tendency toward more interventions, especially a larger use of remedies, post-reform, resulting in fewer type II errors at the expense of more (weak) type I errors. One reason is that markets are sometimes defined too narrowly. In phase 2, however, it appears that the EC has become too lenient, since type II errors increase during this investigation phase compared to pre-reform. The existence of political and firm lobbying might be a possible explanation. Third, according to our rent-reversion regressions, remedies are not effective before and only very loosely effective after the reform. 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 full rent-reversion. However, we can estimate their effect only pre-reform since only two mergers were blocked post-reform. Therefore, given the 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, these are achieved mostly via phase 1 remedies and prohibitions which is in line with these being the most effective merger control tools. Post-reform it appears that withdrawals/abortions substitute for the role of prohibitions. 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 key for understanding the change in policy brought about by the reform.

In conclusion, the introduction of the ECMR 04 seems to have changed European merger policy. Yet, in terms of effectiveness, it has been only a modest improvement. While, on the one hand, decisions are based on a more economic analysis and we observe fewer type II errors than before, we also find that the increased focus on remedies was only partially successful and cannot replace the policy tool of straight prohibitions, which solve both the competitive concerns raised by the concentration and deter future anti-competitive mergers. Clearly, this policy shift was not only the product of the reform and the belief that remedies are a more sophisticated and cleaner instrument to almost surgically appraise merger cases. Foremost, it might be the 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. 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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6. Figures and Tables

Figure 1: Evolution of cases and decisions in our sample
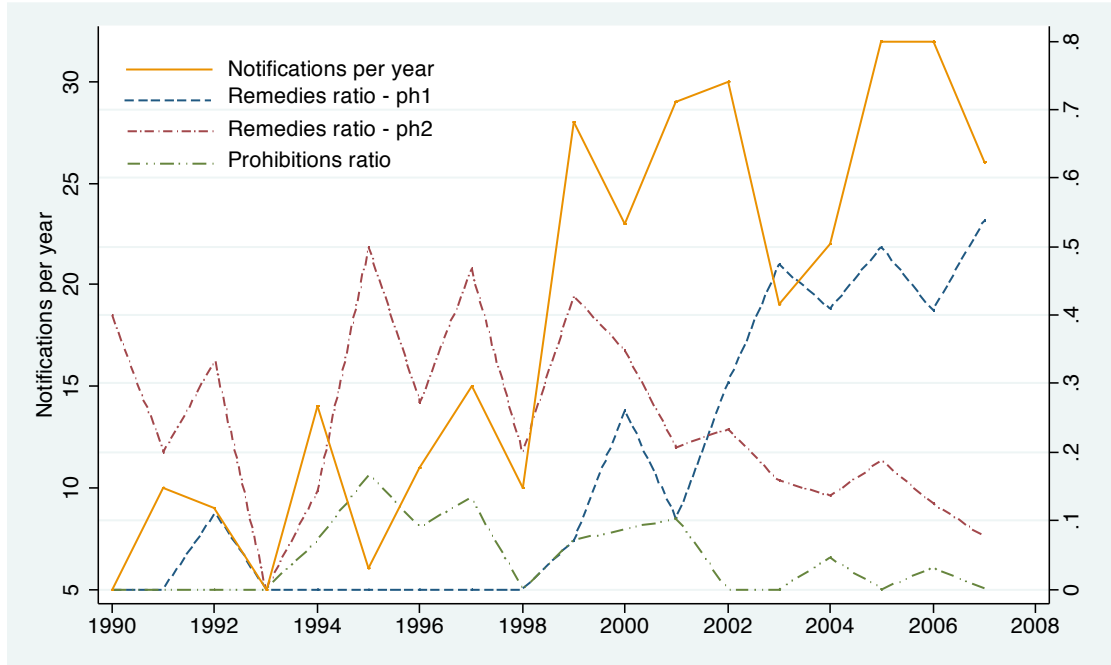
We report the yearly average of notified cases as well as the ratio of different decisions (remedies in phase 1 and phase 2, prohibitions) to the notified cases in our sample.

Figure 2: Evolution of cases and decisions in the population
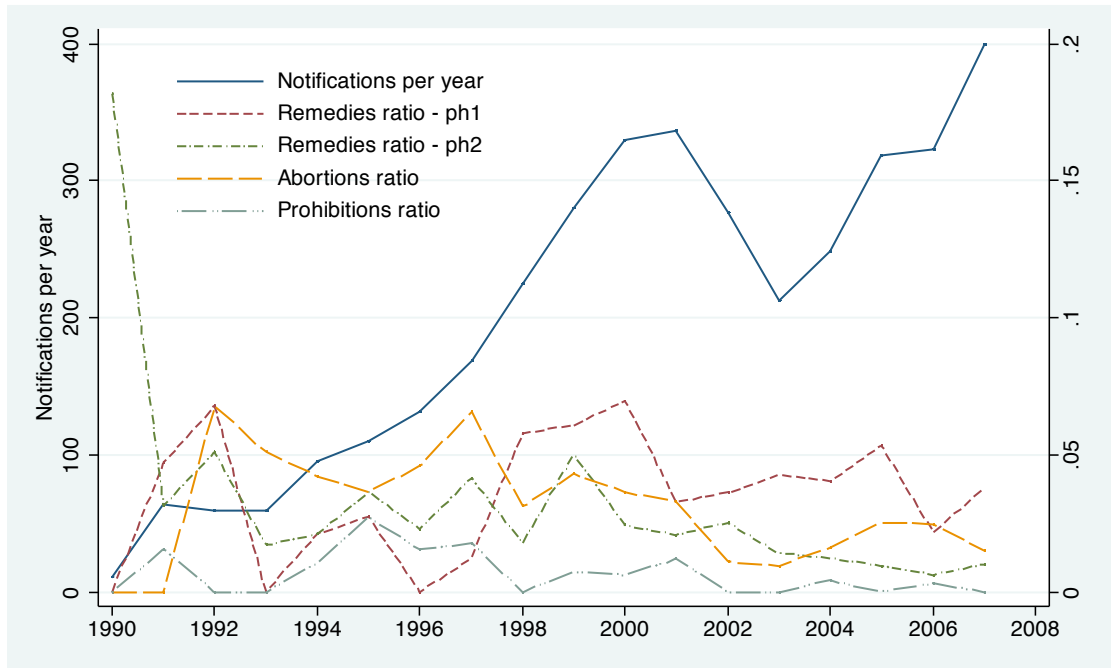
We report yearly average for the number of notified cases as well as the ratio of different outcomes (remedies in phase 1 and phase 2, prohibitions, abortions) to the notifications in the population of all EU merger cases.

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)	0.600	(0.49)	0.069	(0.05)	0.055	(0.04)
in Phase 1	0.144	(0.35)	0.464	(0.50)	0.041	(0.04)	0.044	(0.03)
in Phase 2	0.278	(0.45)	0.136	(0.34)	0.028	(0.03)	0.011	(0.01)
Cleared	0.523	(0.50)	0.382	(0.49)	0.931	(0.05)	0.945	(0.04)
Prohibited	0.056	(0.23)	0.018	(0.13)	0.008	(0.01)	0.001	(0.00)
Phase 2 cases	0.421	(0.49)	0.236	(0.43)	0.055	(0.23)	0.026	(0.16)
Aborted/Withdrawn					0.033	(0.03)	0.024	(0.01)
Remedies: Divestiture	0.287	(0.45)	0.455	(0.50)				
in Phase 1	0.097	(0.30)	0.336	(0.47)				
in Phase 2	0.190	(0.39)	0.118	(0.32)				
Remedies: Behavioral	0.259	(0.44)	0.245	(0.43)				
in Phase 1	0.065	(0.25)	0.182	(0.39)				
in Phase 2	0.194	(0.40)	0.064	(0.25)				
National markets	0.384	(0.49)	0.418	(0.50)				
EU-wide markets	0.407	(0.49)	0.373	(0.49)				
Worldwide markets	0.204	(0.40)	0.200	(0.40)				
Conglomerate merger	0.250	(0.43)	0.582	(0.50)				
Full merger	0.579	(0.49)	0.709	(0.46)				
US firms involved	0.315	(0.47)	0.191	(0.39)				
Observations	216		110		2402		1634	

Remedies represents the fraction of cases that were either cleared subject to conditions & obligations (blockings are reported under *Prohibited*). *Cleared* is the fraction of cases that were permitted without conditions & obligations, *Phase* 2 *cases* were subjected to an in-depth investigation. *Aborted/Withdrawn* are transactions that were notified to the EC, but withdrawn during the review procedure. *National*, *EU − wide* and *Worldwide* markets refer to the market definitions applied by the EC, *Conglomerate mergers* involve conglomerate, vertical, or foreclosure effects, *Full mergers* means that one of the merging firms was acquired in its entirety. *US firms involved* equals one if one of the merging firms has its headquarter in the US.

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**	(0.009)	96	0.011*	(0.008)
Decision	197	−0.003	(0.008)	96	−0.005	(0.005)
Rivals						
Announcement	208	0.008	(0.007)	105	0.011*	(0.007)
Decision	207	−0.003	(0.009)	105	−0.009	(0.007)

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

Table 3: Probit Model: Probability of Intervention

	Pre-Reform		Post-Reform	
US firms involved	-0.425^{***}	(0.098)	-0.424^{***}	(0.077)
EU firms involved	-0.291^{***}	(0.105)	0.110^{*}	(0.065)
Big EU country	0.039	(0.107)	-0.361^{**}	(0.176)
Conglomerate merger	0.455^{***}	(0.139)	0.400	(0.283)
Full merger	0.310^{***}	(0.114)	-0.209^{**}	(0.088)
Crossborder merger	0.003	(0.110)	0.130	(0.115)
Log(MV) merging firms	0.043^{***}	(0.010)	-0.012	(0.007)
Log(MV) rivals	0.034^{**}	(0.014)	0.033^{*}	(0.017)
Rival announcement CAAR	-0.525	(0.390)	0.720	(0.714)
Phase 2 Case	0.780^{***}	(0.069)	0.049^{**}	(0.023)
Worldwide markets	-0.186	(0.164)	-0.063	(0.109)
National markets	0.435^{***}	(0.088)	0.581^{**}	(0.239)
Number of regions involved	0.053	(0.066)	-0.110^{***}	(0.042)
Number of markets involved	0.103	(0.077)	0.032	(0.065)
Mining	0.287	(0.262)	-0.371	(0.255)
Manufacturing	0.024	(0.169)	-0.045	(0.248)
Transport	0.364^{***}	(0.111)	-0.240	(0.278)
Communication	0.134	(0.135)	-0.473^{***}	(0.157)
Finance	-0.339^{**}	(0.150)		
Service	-0.208	(0.211)	-0.398^{**}	(0.175)
Lagged notifications	0.001	(0.003)	-0.014^{***}	(0.005)
Lagged actions ratio	1.601^{**}	(0.693)	5.453^{***}	(0.990)
Lagged abortions ratio	0.153	(1.838)	-12.345^{***}	(1.104)
Time trend	0.002	(0.007)	0.042^{**}	(0.018)
Observations	203		105	
Pseudo R^2	0.51		0.23	
CorrClassified	87%		71%	
Sensitivity	86%		83%	
Specificity	87%		54%	

The dependent variable is action, defined as $P_j = 1$ when the merger is remedied or blocked, and zero otherwise. Marginal effects are reported. dF/dx is for discrete change of a dummy variable from 0 to 1. 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. The *EU firms* dummy indicates that one of the merging parties is headquartered in the EU. *Big EU* countries are Germany, France, Italy, Spain and the UK, *Crossborder mergers* involve at least two nations, *Log(MV)* refers to the logged market values of merging parties and rivals. For the *Rival announcement CAAR* see A.3. The number of regions and markets variables count the (geographic) regions and (product) markets concerned by a merger. *Mining*, *Manufacturing*, *Transport*, *Communication*, *Finance* and *Service* are industry dummies. For the lagged population variables, see 2.2.4. The time trend is measured in quarters. The remaining dummies are defined at the bottom of table 1.

Table 4: Type I/II errors by period

	Pre-Reform			Post-Reform		
	Mean	S.D.	Count	Mean	S.D.	Count
Type I error	0.221	(0.416)	46	0.324	(0.470)	34
Type II error	0.269	(0.445)	56	0.181	(0.387)	19

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 5: Probit Model: Probability of Type I errors

	Pre-Reform		Post-Reform	
US firms involved	−0.761***	(0.142)	−0.565***	(0.188)
EU firms involved	−0.822***	(0.148)	0.804***	(0.147)
Big EU country	0.384**	(0.152)	−0.934***	(0.093)
Profit change - merging firms	−0.000	(0.000)	0.003	(0.002)
Profit change - rivals	0.000	(0.000)	−0.000	(0.000)
Conglomerate merger	0.114	(0.409)		
Full merger	0.573***	(0.190)	−0.720***	(0.222)
Crossborder merger	0.112	(0.227)	0.805***	(0.172)
Log(MV) merging firms	0.065***	(0.022)	0.172***	(0.052)
Log(MV) rivals	0.072**	(0.028)	−0.074	(0.046)
Worldwide markets	−0.646***	(0.111)	−0.883***	(0.071)
EU-wide markets	−0.722***	(0.125)	−0.863***	(0.132)
Number of regions involved	0.102	(0.138)	−0.323***	(0.073)
Number of markets involved	−0.006	(0.239)	0.366	(0.256)
Phase 2 case	0.971***	(0.044)	0.423*	(0.245)
Manufacturing	−0.635*	(0.350)	−0.619**	(0.265)
Communication	−0.460***	(0.123)	−0.786***	(0.170)
Service	−0.467***	(0.105)	−0.414*	(0.245)
Electricity	−0.431***	(0.108)		
Finance	−0.563***	(0.097)		
Transport			−0.223	(0.415)
Lagged notifications	−0.003	(0.009)	−0.039***	(0.014)
Lagged actions ratio	5.088	(4.144)	−0.880	(7.028)
Lagged abortions ratio	0.527	(2.714)	−39.688	(39.004)
Time trend	0.012	(0.018)	0.096	(0.071)
Observations	88		49	
Pseudo R^2	0.70		0.46	
CorrClassified	93%		82%	
Sensitivity	93%		93%	
Specificity	93%		67%	

The dependent variable is type I error defined as $E1_j = 1$ if $P_j = 1$ and $D_j = 0$, and zero otherwise (action in a pro-competitive merger). Marginal effects are reported. dF/dx is for discrete change of dummy variable from 0 to 1. Standard errors in parentheses are robust and allow for correlation among observations form the same year. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

Table 6: Probit Model: Probability of Type II errors

	Pre-Reform		Post-Reform	
US firms involved	0.149	(0.093)	0.899***	(0.090)
EU firms involved	−0.162	(0.113)	−0.282**	(0.135)
Big EU country	0.154*	(0.087)	0.711***	(0.216)
Profit change - merging firms	0.000***	(0.000)	0.005**	(0.002)
Profit change - rivals	−0.000	(0.000)	−0.000***	(0.000)
Conglomerate merger	−0.236***	(0.089)	−1.000***	(0.001)
Full merger	−0.306***	(0.104)	0.389*	(0.202)
Crossborder merger	0.062	(0.112)	0.246***	(0.063)
Log(MV) merging firms	−0.029*	(0.015)	0.248**	(0.098)
Log(MV) rivals	−0.025*	(0.014)	0.031	(0.031)
EU-wide markets	−0.223*	(0.135)	−0.271	(0.234)
National markets	−0.250*	(0.133)	−1.000***	(0.001)
Number of regions involved	−0.007	(0.049)	0.119**	(0.052)
Number of markets involved	−0.036	(0.082)	0.276*	(0.154)
Phase 2 case	−0.469***	(0.141)	0.921***	(0.097)
Manufacturing	0.212	(0.170)	−0.122	(0.315)
Communication	0.009	(0.187)		
Service	0.426	(0.599)	0.847***	(0.099)
Electricity	−0.050	(0.126)		
Trade	0.094	(0.213)		
Transport	−0.105*	(0.056)	0.534	(0.444)
Lagged notifications	−0.002	(0.001)	−0.006	(0.006)
Lagged actions ratio	−0.153	(0.514)		
Lagged abortions ratio	0.073	(1.122)		
Time trend	0.008***	(0.003)		
Observations	100		42	
Pseudo R^2	0.55		0.54	
CorrClassified	87%		90%	
Sensitivity	91%		88%	
Specificity	83%		92%	

The dependent variable is type II error defined as $E2_j = 1$ if $P_j = 0$ and $D_j = 1$, and zero otherwise (unconditional clearance of an anti-competitive merger). Marginal effects are reported. dF/dx is for discrete change of dummy variable from 0 to 1. Standard errors in parentheses are robust and allow for correlation among observations form the same year. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

Table 7: Effectiveness Regressions

	Pre-reform				Post-reform			
	Merging Parties		Rivals		Merging Parties		Rivals	
Clearance	0.244*	(0.133)	0.010	(0.070)	−0.066	(0.096)	−0.071	(0.061)
Behavioral	0.107	(0.155)	0.038	(0.074)	−0.015	(0.069)	−0.102*	(0.041)
Divestiture	0.330*	(0.159)	0.035	(0.098)	−0.006	(0.055)	−0.093	(0.071)
Prohibitions	0.309	(0.177)	−0.228	(0.160)				
Π_{ij}^{A**}Clearance	0.031	(0.141)	0.331**	(0.148)	−0.069	(0.069)	0.611*	(0.250)
Π_{ij}^{A**}Behavioral	0.075	(0.247)	−0.069	(0.333)	−0.126	(0.095)	0.030	(0.056)
Π_{ij}^{A**}Divestiture	−0.496	(0.309)	−0.141	(0.372)	0.234	(0.227)	−0.016	(0.100)
Π_{ij}^{A**}Prohibition	−0.923**	(0.316)	−0.532	(0.408)				
Observations	143		135		89		88	
R^2	0.26		0.20		0.35		0.21	

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

Table 8: Deterrence Regressions

	Pre-reform		Post-reform	
Lagged notifications	−0.002	(0.004)	0.001	(0.002)
Lagged remedies ratio - ph1	−1.062*	(0.587)	−2.721	(3.143)
Lagged remedies ratio - ph2	2.909**	(1.259)	−0.554	(6.989)
Lagged abortions ratio - ph1	0.581	(1.401)	−9.673**	(4.464)
Lagged abortions ratio - ph2	6.756**	(3.350)	−14.072**	(7.009)
Lagged prohibitions ratio	−7.099*	(4.287)		
Observations	200		105	
Pseudo R^2	0.05		0.10	
CorrClassified	63%		63%	
Sensitivity	68%		61%	
Specificity	58%		64%	

The dependent variable is $D_j = 1$ if $\Pi_{Cj}^{A*} \geq 0$, zero otherwise. Standard errors in parentheses are robust and allow for correlation among observations from the same year. Marginal effects are reported. We control for industry effects (manufacturing, mining, services), merger-specific effects (full, cross-border and conglomerate mergers, relevant markets), firm-specific effects (country of origin, size) and a time trend. 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: $R_{i,t} = \alpha + \beta R_{market,t} + \varepsilon_{i,t}$.[33] 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.[34] 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

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

[34]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.

larger the window should be. Hence, the length of these windows is critical to the event study's ability to capture the profitability effects: if the window is too small, the effect might not be wholly captured, whereas too large a window could dilute the result.[35] 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.[36] 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 3.

Figure 3: Timeline of the events

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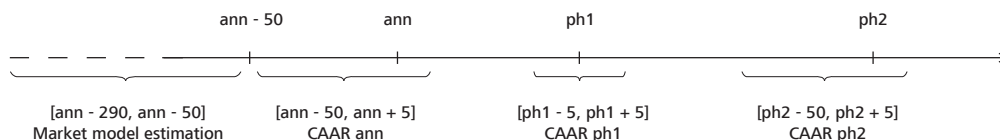
[35]Issues concerning the length of event windows and their ability to capture the effect of regulation are more thoroughly discussed in Duso, Gugler, and Yurtoglu (2010b).

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

A.3. Aggregating the Abnormal Returns

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

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

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.[37] 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.[38] 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

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

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

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[37]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.

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

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

Using the past merger control history and the knowledge of the structural characteristics of a proposed merger, firms can form a prior of how likely it is that DG Comp will intervene. This means that the observed abnormal returns around the event dates do not measure the full effect but are the expectation-adjusted abnormal returns, which take into account that the combination might not go through or 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:[39]

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

where $\Pi^{action}(\Pi^{clear})$ denotes the merger's profitability in case of an action (a clearance) and ρ is the probability of an action. The observed abnormal returns around the announcement (Π^{ann}) therefore are equal to the real effect (Π^{ann*}) plus the expected value of the EC's final decision ($E[\Pi^{dec}]$). Assuming that an intervention by DG Comp destroys the anti-competitive rents generated by a combination ($\Pi^{action} = -\Pi^{ann*}$) in their full extent ($\Pi^{dec*} = \Pi^{ann*}$),[40] 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 (Π^{dec}) is an update of the market's beliefs concerning that particular decision and, hence, the difference between

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[39]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.

[40]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.

the merger's competitive effect and the market expectations of the commission decision's effect.[41]

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

If a case goes into phase 2, the market will again update its beliefs about remedies.[42] The effect around the phase 1 decision accounts for the adjustment of market expectations to the new state of beliefs, the sum of both decision effects captures the total impact of the EC's decision. The real effect of the decision is then given by

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

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

Combining the equations for the decisions yields

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

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

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[41]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.

[42]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.