European Commission

A review of merger decisions in the EU: What can we learn from ex-post evaluations?

Competition

EUROPEAN COMMISSION

Directorate-General for Competition
E-mail: comp-publications@ec.europa.eu

European Commission
B-1049 Brussels

European Commission

A review of merger decisions in the EU: What can we learn from ex-post evaluations?

Report by Peter Ormosi, Franco Mariuzzo, and Richard Havell

with Amelia Fletcher, Bruce Lyons

July, 2015

Luxembourg, 2015

Peter Ormosi, Franco Mariuzzo, Richard Havell, Amelia Fletcher, Bruce Lyons

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Luxembourg: Publications Office of the European Union, 2015

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ISBN 978-92-79-51935-2

doi: 10.2763/84342

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Executive summary

DG COMP of the European Commission commissioned a team of academics (lead by Peter Ormosi) at the Centre for Competition Policy, University of East Anglia, to deliver a report, which systematically reviews ex-post evaluations of the impact of merger decisions by EU competition authorities. Ex-post merger evaluations (or merger retrospectives) estimate the impact (typically on price) of mergers by using different econometric techniques. The objective of this report was to review the relevant literature of these merger retrospectives, to discuss what the findings of these studies may imply about the quality of merger decisions, introduce the relevant methodologies, and provide a framework for identifying errors in merger decisions.

The price effect of mergers

(1) *Given the size of the study sample and the likely non-random nature of selecting the mergers to be evaluated, the findings of this study should be treated with caution as any conclusions drawn from this study are specific to the analysed sample.*

An important limitation of this report is that the sample of relevant merger retrospectives is small and the mergers they evaluate are unlikely to be representative of the population of mergers (we looked at 27 price-effect estimating studies and 50 studies evaluating effects other than price). For this reason we cannot generalise the findings of this report and extrapolate them to the population of all merger decisions. Nevertheless, this exercise is still useful as it allows an in-depth analysis of available studies and their findings. This can reveal to us where potential weaknesses of merger decisions lie, and also identify if merger retrospectives – in their current form – can contribute to the process of merger control, or whether they also need readjustment to better inform policy. In contrast to the wide use of merger retrospectives in the US the relative scarcity of similar studies in the EU is even more striking. Knowing how much these works could contribute to the optimisation of merger decisions, EU competition authorities should be given the right incentives to engage in such exercise on a more regular basis.

(2) *On average, mergers in our sample were followed by a price increase, although this remained under 5 per cent in the large majority of cases. The average price increase in unconditionally approved mergers was just under 5 per cent and in remedied mergers between 1 and 2 per cent. In half of the unconditionally approved cases post-merger prices increased. The majority of remedied mergers are associated with a price increase despite the remedy, although this increase is very small.*

Post-merger prices increased in around 60 per cent of the analysed sample. The price increase was worse (5%) in unconditionally approved mergers. This finding is similar to a large study conducted on US ex-post merger studies (which also found that unconditionally remedied mergers are followed by around 7% price increase). One the other hand, remedied mergers were followed by a very small price increase (around 1%), which is in contrast to the findings of the US study, which found that remedies have been largely ineffective in preventing a price-increase. Nevertheless, we caution against making ambitious comparisons between the two studies as it is likely that the two samples contain very different cases (probably due to different sample selection mechanisms).

Where a price increase follows an unconditionally approved merger, it seems tempting to jump to the conclusion that the competition authority 'made an error'. Similarly, a price increase following a remedied merger would suggest that the authority was right in imposing a remedy, but the remedy did not fully eliminate the price-increase. When further investigating the price-increase cases, we point out that some of these are likely to represent a genuine error in the decision, and others are possibly a result of other factors (non-price effects were given priority over price effects, faulty evidence, or random error).

(3) *Average estimates of post-merger price increase were around zero where the market was less concentrated. In more concentrated markets the average estimated price-*

increase was large (between 10% and 20%), but only if the merger had been unconditionally approved. In the sample, remedies had been able to reduce post-merger price-increases even in concentrated markets.

Market structure is still very much in the focus of merger control as an indicator of the likely market power increasing effect of the merger. For this reason we collected information on three different measures of market concentration (number of firms, market share of the largest merging firm, and HHI). We found that market concentration is a strong driver of the estimated price-effect of the merger. The average price increase in markets that are considered un-concentrated based on conventional measures of concentration is around zero. In concentrated markets, on the other hand, the average price increase is large, although the remedies managed to mitigate the post-merger price hike even in concentrated markets.

The methodology used

(4) *In Difference-in-Differences studies the change in the price of the merging firms (Treatment) is compared with the change in the price of a counterfactual market (Control). The following assumptions have to be satisfied: the prices in the Control and Treatment markets follow a parallel trend; price at one period is not correlated with price at another period; firms' decision to merge is not correlated with unobserved characteristics that also affect the relevant prices; prices in the Control group are not affected by the merger; and there is sufficient similarity between the Treatment and the Control markets.*

Difference-in-Differences methods (18 studies in our sample) are the most frequently used tool for evaluating the impact of individual merger decisions. It compares how the prices in the merger market (Treatment) change, with how prices change in a sufficiently similar market (Control). This is based on the assumption that the Control market is what the merger market would have been in the absence of the merger. Of course, for the method to provide unbiased estimates, certain assumptions have to hold. One key contribution of this report is that it catalogues these assumptions and explain them in the context of evaluating merger decisions. It highlights the possible biases in estimates that violate these assumptions. Therefore the methodological discussion in this report also serves as a short reference guide for conducting merger ex-post evaluations.

(5) *In Difference-in-Differences studies the Control group is typically composed of rival firms or local markets not affected by the merger. The selection of the Control group should follow a formalised procedure, ensuring that the Control is sufficiently similar to the Treatment (the merger market), and that there are no spill-over effects. The possibility of spill-over effects (the merger affecting competitors' prices) is more likely to be an issue where rivals are used as Control.*

One key condition of reliable price-change estimates is that the Control group (the group against whom the merger prices are compared) is selected in a way that best satisfies the assumptions above (sufficiently similar to the Treatment group but not affected by the merger). There are formal procedures for selecting the best Control group, such as propensity score matching and iterative techniques such as those adopted in Chone et al (2012), and these are discussed in sufficient detail in this report.

(6) *In Difference-in-Differences studies, if available, the data should span over longer than a year following the merger in order to allow any market self-correction to take place. In our sample we found that a third of the studies looked at price effects within a year after the merger.*

The time-span of the data used for assessing the impact of a merger is a surprisingly under-discussed part of merger retrospectives. This report argues that sufficiently long time has to be allowed to pass after the merger, before the impact evaluations can be done. The rationale is simple. It is possible that the immediate price-shock, caused by the merger, is self-corrected by the market within few years following the merger. It is also

possible that in the immediate aftermath of the merger prices increase only in markets directly affected by the merger, but then the effect spreads on to other markets later on. Both of these possibilities have to be accounted for when designing an ex-post study. It seems unlikely that a competition authority would choose to intervene if it believed that market self-correction would offset the price increase within one or two years after the merger. It is also unlikely that the competition authority would refrain from intervention if it judged that the anti-competitive effects of the merger might take a few years to fully develop. For these reasons it is vital that the ex-post study uses data that spans sufficiently far following the consummation of the merger. On the other hand, longer spanning data is more likely to contain confounding effects (i.e. effects other than the merger). The best practice appears to be to estimate the effect of the merger in each subsequent year following the merger.

(7) *Merger simulations can be done using ex-ante or ex-post data. The former can inform us how far the competition authority's decision fell from the best possible prediction (a full-fledged merger simulation). The latter can tell us how the merger affected the market, whilst accounting for various post-merger market developments. The data used in this exercise should include: (1) post-merger price, market share and product characteristics to estimate own-, and cross-price elasticities in the post-merger equilibrium; (2) marginal costs and information on efficiency gains; and (3) how market structure evolved post-merger through entry and exit. If both ex-ante and ex-post data are available and have similar length, then the research can test if demand characteristics change with the merger.*

Merger Simulations (9 studies in our sample) are a widely used method for predicting the effects of the merger during the competition authority's investigation. This report argues and demonstrates that they can also be used for the ex post assessment of merger decisions. If the simulation uses data that was already available during the investigation (ex-ante data) then the findings of the simulation can show us how far the authority's merger decision fell from the best possible prediction (this is based on the assumption that a full-fledged simulation would provide the best prediction pre-merger). If the simulation uses data from after the merger, then it can work as a genuine ex-post evaluation tool.

Merger Simulations have another important characteristic: unless accounting for efficiency gains they always estimate a post-merger price increase – simply because the simulation compares two scenarios, one with *n* number of firms (pre-merger), and the other one with *n*–1 firms (post-merger). For this reason it is vital to incorporate efficiency gains in the model and, possibly, entry of new products or firms.

(8) *If possible, the ex-post assessment of merger decisions should use both Difference-in-Differences and Merger Simulation to learn from the comparison between the two methods.*

Although this is very demanding task, given the amount of complementarity between Difference-in-Differences and Merger Simulation estimates, the two estimates together could give a more complete picture of the effect of mergers. Difference-in-Differences estimates reveal what happens to measurable factors such as post-merger price, on the other hand Merger Simulation can estimate the effect of unobserved scenarios (counterfactuals), such as alternative merger remedies or a non-remedied merger, or blocked mergers, and it can also provide welfare estimations.

How to evaluate the merger decision?

(9) *Because of its relative simplicity the Difference-in-Differences method is typically preferred to Merger Simulations for analysing approved mergers with no intervention. On the other hand, Difference-in-Differences methods are less suitable for detecting if the competition authority made an unnecessary intervention (Type I error). Ex-post Merger Simulations are capable of fully identifying decision errors in the merger intervention (remedy or block) provided that efficiency gains are incorporated in the*

simulations. Merger simulations are also able to estimate the welfare impact of mergers.

The report provides a framework for evaluating the competition authority's merger decision. It distinguishes between two type of errors: (1) where the authority made an unnecessary intervention (Type I error), and (2) where the authority's intervention was not sufficient (Type II error). In general, because of its simplicity, the Difference-in-Differences method is preferred if the merger was unconditionally approved. However, because it always compares the merger price with the non-merger price, it is more limited in its assessment of whether a Type I error was made in a remedied or blocked merger decision. To give an example: a price drop or no price change after a remedied merger would imply that there was no Type II error (the intervention was not deficient). However, without knowing what would have happened under an un-remedied merger (or under a less intervening remedy) we cannot tell if there was a Type I error. It is possible that the merger would have led to a price drop even without the remedy – in which case it was an error to intervene (Type I error). It is also possible that the un-remedied merger would have pushed up prices and the remedy eliminated this threat (No error). For the same reason, using Difference-in-Difference methods are less able to identify whether the competition authority made an error in the design of the remedy. Merger Simulations are preferred in these cases.

(10) *In more than half of the analysed sample, prices increased after the competition authority's intervention. A post-merger price increase may not imply an erroneous merger decision: (1) if non-price effects dominated price effects and the authority recognised this, (2) if the decision was based on faulty facts, or (3) if the post-merger price increase could have been seen as random variation at the time of the authority's decision.*

A central message of this report is that an estimate that shows increased post-merger prices does not necessarily mean that the competition authority had made an error. To be able to assess whether there was an error one has interpret the findings of the study in the context of the authority's decision. The only time that we can conclude that the decision was erroneous is when the authority had all information at hand that should have enabled it to predict the price increase, but despite this it made an erroneous assessment leading up to a decision that did not stop the negative effects of the merger. On the other hand, if effects – other than price – inspired the authority's decision then even if the authority had perfectly foreseen a small price increase, these could have been tolerated for the sake of non-price effects. Similarly, it is possible that a small price increase would have appeared as a random error at the time of the decision. It is also likely in many instances that authorities would not intervene a merger with a very small price increase, if it can be reasonably expected to be self-corrected by the market.

(11) *The competition authority was more likely to have made a decision error in more concentrated markets.*

The study also looked at the observable characteristics of cases where there was a potential error in the merger decision. We found that potential errors were more likely in concentrated markets. This seems to imply, that CAs should pay more attention to highly concentrated markets, which is in line with current thinking as expressed in the European Commission's guidelines.

The non-price effect of mergers

(12) *Looking at how market structure changed post-merger may provide useful information for assessing the competition authority's decision. Developments in the joint market share of the merging firms, the level of rivalry, the level of concentration, and the size of the market are all informative for this purpose. We found that there is a non-trivial number of cases where the merger was followed by higher concentration, less rivalry, or larger market power of the merging firms. Time also seems to play an important*

role; studies that are conducted more than 5 years after the merger were less likely to find similar concerns.

Some competition authorities regularly assess what happened in the merger markets in the years following the merger. These evaluations, which are typically based on interviews with market participants, offer the possibility to assess how factors such as market structure, market entry, or innovation developed after the merger. When focusing on market structure, these ex-post qualitative studies can tell us if the market became more concentrated or less conducive to competition after the merger. The merger decision can be re-examined in light of these findings and assessed if there are systematic errors in the decision-making process.

(13) *Very few studies looked at how dynamic effects develop post-merger. This is somewhat surprising because these dynamic effects are typically the most debated part of merger decisions, and therefore it would be useful to improve our knowledge on how these effects unfold after the merger. Most of what we know is on market entry, in which case the sample of studies suggests that in general CAs do a good job in predicting where entry can potentially eliminate short-term competition concerns in the market.*

When it comes to the non-price effects of the merger, one of the most important finding of this report is that there is very little information on how dynamic factors, such as innovation and efficiencies, developed after the merger. Given that the competition authorities are in a good position to conduct these evaluations, increasing the number of such studies would be a welcome development.

1 Introduction

The last two decades have seen an increasing amount of works by academics and competition authorities, trying to improve our understanding of the political economy of competition policy in the EU. These works have focused on the activities, the legal procedures, the underlying economic behaviour and the institutional framework of EU competition authorities.

Merger control is no exception, especially given that it is an ex-ante policy instrument – i.e. the competition authority (CA) has to assess the market effect of a transaction before the transaction takes place. In this respect even if it is based on the best available evidence, there is an inevitable uncertainty in the appropriateness of any intervention that follows. Because of this, it is crucial that merger control decisions are subjected to rigorous ex-post evaluation exercises in order to assess how well they are achieving their task of filtering out anti-competitive mergers. If done correctly, lessons learned from these ex-post studies can be channelled back into policymaking to affect how mergers are assessed in the future. As Ashenfelter, Hosken and Weinberg (2008) pointed out "*Because economic models generate explicit predictions of the competitive effects of mergers, it is relatively straightforward (though resource intensive) to evaluate their performance with retrospective evidence. If these tools are proven effective, they could lead to a more efficient, objective, and accurate merger review process.*" For example in the US hospital sector a series of ex-post merger studies revealed some of the merger analytical issues that had been systematically misunderstood by the courts and other policy analysts,[1] and which likely have led to systematically biased judicial decisions.[2] These studies led to an improvement of the FTC's merger enforcement programme in the hospital sector.[3]

Since Neven et al's (1993) pioneering book we have seen a steady increase in the number of papers that evaluate merger decisions ex-post (merger retrospectives).[4] The objective of any such work has typically been two-fold: (1) to assess whether the decisions in question were right, and, (2) if it was found to be wrong, to understand why, with a view to improving merger analysis or decision-making going forward, either in general or in the context of that market.

A study that brings together this body of literature can be useful for numerous reasons. Firstly, this would provide a concise document that consistently summarises the corpus of relevant studies and conclude what we have learnt from EU merger control. Secondly, it is an obvious way of providing evidence whether merger control works. Thirdly, it can identify areas where changes would be most pressing. Fourthly, it could help assess the effectiveness of relevant methodological tools. Finally, it could contribute to the design of a better evaluation framework.

The most relevant paper for our purposes is an analysis of US merger retrospectives by Kwoka (2013). Kwoka collates 60 high-quality studies estimating the price effects of 53 transactions (46 mergers) in the US and presents the average price effects estimated by this sample of studies, broken down to various sub-samples. For a smaller sample (23 mergers), where he had information on the CA's decision, he reports price-change estimates according to the action taken by the CA as an attempt to identify if the CA made an error in its decision. He finds that a large proportion ex-post studies estimated that the merger led to a price increase, even when the competition authority had imposed remedies upon the merger. Despite this, the strength of remedies is linked to the price effects of

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[1] For example the Elizinga-Hogarty market definition that had been applied in these cases generated markets that were much too large, or that not-for-profit hospitals were incorrectly assumed to not have exercised market power.

[2] Ashenfelter et al. (2011).

[3] A presentation by Daniel Hosken (FTC) at the OECD Competition Committee's Capacity building workshop on the ex-post evaluation of Competition Authorities' enforcement decisions (22 April 2015).

[4] In the report we use 'merger retrospectives' and 'ex-post merger evaluations' interchangeably.

mergers, indicating that the authorities are capable of identifying those mergers, which pose the largest competitive concerns, and apply stronger remedies in such cases. Kwoka argues that the key problem faced by US competition authorities is the effectiveness of remedies, especially non-structural remedies, rather than the identification of problematic mergers. Examination of the subset of mergers for which information on prior structural conditions is available indicates that US competition authorities are more inclined to allow increases in concentration that the Merger Guidelines would find problematic.

The report by the OECD (2011)[5] on ex post merger studies is more of a methodological survey as it reviews the methods used in performing such retrospectives. It comprehensively discusses the relative advantages and disadvantages of the main methods: difference-in-differences, merger simulations, event studies, and surveys. The study concludes that there is no one-size fits all method, and specific circumstances justify different choice of methods.

Both Kwoka and the OECD emphasise the selection problem in meta-reviews of merger retrospectives: that academic studies will be biased towards studying mergers with available data and which were on the margins of being accepted or rejected. For this reason one has to be very cautious when using these studies to derive general conclusions on the CAs merger decisions. Coate (2014) attempts to control for this problem by estimating challenge probabilities for the Federal Trade Commission. Challenge probabilities are estimated using variables such as the post-merger HHI, the change in HHI, the number of pre-merger rivals, entry information, efficiency information, buyer sophistication, and industry fixed effects. Having established which conditions predicted high probabilities of challenge it was possible to interpret the merger effects in light of how likely there were to lead to anti-competitive problems. Coate then applied these challenge probabilities to his sample of 22 studies of 19 mergers challenged by the FTC in order to infer the effectiveness of the FTC's actions. He finds that price increases were estimated in cases where there was a high challenge probability. On the other hand, it was less likely that the study found a price increase in mergers with low challenge probabilities. Coate interprets this as evidence that the current policy is efficient – although the intervention may not be. Because of the correlation between challenge probability and the result of the merger retrospective, he adds that the decision of the competition authority should also serve as a guide to the credibility of the results of the retrospective study. If the authority, based on strong evidence, did not challenge the merger, then the "the analyst must take a very close look at a retrospective study that finds an anticompetitive effect to be sure that the study is not flawed." Another specific finding of Coate is that price increases did not result from any of the low-concentration cleared mergers where the FTC feared coordinated effects.

This report contributes to the already existing works in various ways. Its main objective is to provide a systematic review of ex-post studies that evaluate the impact of merger decisions by EU competition authorities. We use this exercise to overview what has been done in this area, introduce the relevant methodologies and provide a framework for identifying errors in merger decisions.

The first part of this report replicates Kwoka's work and applies his method to a sample of EU mergers. We look at the estimated price impact of these mergers and briefly assess how they vary with the type of the CA's decision. We find that in half of the unconditionally approved mergers in our sample prices increased post-merger. We also found that the majority of remedied cases led to a price increase, although the magnitude of this increase was typically small.

[5] Organization for Economic Co-operation and Development, *Impact Evaluation of Merger Decisions* (2011), DAF/COMP(2011)24

Instead of stopping here and concluding that CAs work with a high rate of errors (at least for the analysed sample) we propose that a more thorough investigation of these findings is necessary in order to make any conclusions on the errors in the CA's decisions.

Firstly, we discuss the methods used in merger retrospectives and explain the potential sources of bias that may arise from poor research design or inadequate choice of estimation method, and highlight how this bias would affect the estimated price change. In this exercise we find that the studies in our sample are designed in a way to avoid the most obvious biases. However, some problems still remain and, when these are not addressed, the implications of potential biases are not always discussed. We also find that a strikingly large proportion of studies look at the merger's price effect within a year from the merger, which is likely to be inadequate for picking up more dynamic effects and any potential market self-correction that typically takes longer to unfold.[6]

Regarding the methods, we make the recommendation that Difference-in-Differences methods are better suited (being less resource demanding) for analysing unconditionally approved mergers and Merger Simulations are better for studying the impact of interventions (remedies or prohibition). Another important thing that stood out from our analysis is that Difference-in-Differences estimates are less able to identify errors where the CA's intervention was overly harsh (Type I error), and that merger simulations always estimate a post-merger price increase unless efficiencies are taken into account.

We evaluate the merger decision in the light of the price-change estimates. First, we set out the criteria for which the combination of the decision and the retrospective estimate implies that there was potentially an error in the merger decision (for example, without further knowledge on the CA's decision, an unconditionally approved merger followed by a price increase might imply such an error). In around half of the cases the retrospective estimate and the decision together suggest that the CA's decision was potentially erroneous. When further investigating these cases, we point out that only a small number of these are likely to represent a genuine error in the decision, and others are a result of other factors (non-price effects were given priority over price effects; faulty evidence; or random error).

Having concluded the analysis of price effects, we turn to the non-price effects of mergers. These studies are typically conducted by the CA's themselves and they look at a wide range of factors such as market structure (e.g. concentration, entry, exit), dynamic effects (e.g. innovation, investment) and other factors (e.g. service quality, buyer power). We show that assessing these effects is a useful exercise as it complements the price-effect studies, helps identify the cases where the CA had potentially reached an erroneous merger decision, and thus inform a better design of merger control decision-making.

1.1 Sample Description

To conduct our review, we looked at all retrospective ("ex-post") studies of mergers in Europe. Our sample consists of quantitative and qualitative studies, studies, which estimate the price effects of mergers and those which examine non-price effects. The key selection mechanism that we used for generating this sample is that the merger and its effect had to be individually identifiable in the study. For this reason aggregate studies that look at a sample of mergers and study their characteristics as a sample were excluded from this work.

Kwoka's work has been highlighted as an important precursor to this study. Our sample size, where both the price-change estimate and the agency's decision is available, is similar to Kwoka's. Kwoka's findings are based on 23 Difference-in-Differences (DiD) estimates, our study has 18 Differences-in-Differences and 9 Merger Simulation (MS) estimates. This of course puts some obvious limitations on the conclusions we can make from our findings. On the other hand we go beyond the questions analysed by Kwoka. We look at

[6] Market self-correction typically refers to the market mechanism, whereby the monopoly profit attracts entry, and thus more competition, consequently reducing prices.

heterogeneity across the estimates, offer a review of the details of the methods used, an assessment of the decisions of the CA, and a wide-ranging analysis of non-price effects.

1.1.1 Price effect estimating studies

Kwoka (2013) focuses on a particular category of research: quantitative studies, which use DiD methods in order to estimate the price effects of specific, individual horizontal mergers in the USA. He ensures that the studies in his sample are of sufficient quality by including only those published in peer-reviewed journals or respected working paper series. Kwoka finds sixty papers meeting these standards, examining fifty-three mergers, joint ventures and airline code-sharing agreements (some papers examine the same mergers). Finally, in 23 of the studies he found sufficient information on the agency's decision to carry out a more detailed assessment.

Taking the same approach to collect a sample of studies of EU mergers yields only four studies published in academic journals. Therefore we broadened our selection criteria to include university working paper series and works by national competition authorities. The sample of papers estimating price effects using Difference-in-Differences is then 11 studies examining 18 mergers. This set of papers consists of the four journal articles, two studies commissioned by national competition authorities, one study performed by a national competition authority and four working papers from university working paper series.

To expand our sample we also included studies that used merger simulations (MS) to estimate the price effect of mergers. Our justification is simple: as we will argue below, some types of merger decisions can be better assessed by using MS methods. Therefore the inclusion of MS studies offers us the possibility to discuss the method and highlight the circumstances when they have a comparative virtue over DiD studies. Our study selection criteria was that the study was conducted after the merger, rigorous structural models were used to estimate parameters of the demand system, and these parameters were then plugged into a merger simulation to estimate a quantified price effect of the merger. This added 7 studies and 9 mergers to our sample. Two of the studies estimated price effects through both DiD and MS techniques in order to compare the results and evaluate the accuracy of the simulation so altogether we had 16 distinct studies including DiD and MS estimates.

In the Appendix we provide a set of tables with detailed information on each of the studies. The *main findings* table gives the authors/title/year of the study, the jurisdiction, the year of the merger decision, the names of the merging parties, and the relevant NACE industries. The *market structure* table contains the market shares of the parties, the market HHI, and the number of firms. The *data table* presents the main source of the data, the time span of the data, the number of time periods, and the number of observations. The *estimation* table presents the methodology used, the counterfactual used, whether there were placebo treatments, multiple control groups, and multiple model specifications, and whether the estimated model controlled for costs, firm characteristics, product characteristics, and market characteristics. The *results* table presents the main estimates, the potential biases, and if the data implied an error, and if following our analysis we believed that there was an error.

1.1.2 Non-price effect and qualitative studies

In addition to price outcomes, we also look at studies, which examine the non-price effects of mergers. This is beyond the scope of the work of Kwoka and Coate[7] so an original specification for this sample was necessary.

There are many studies, which use large samples of mergers in order to look at various aspects of merger policy such as the probability of intervention, the differences between

[7] Coate, Malcolm B. "A Meta-Study of Merger Retrospectives in the United States." *Available at SSRN 2333815* (2014).

the US and EU, and the characteristics of merger waves. However, as explained above, we are concerned with only those works that look at specific, individual mergers and which use empirical data gathered after the consummation of the merger. Our justification is that aggregate studies typically assume homogeneity for unobserved characteristics, and any of their findings is only true on the average. Therefore to allow us to explore cross-merger heterogeneity as much as possible, we need to look at studies that evaluate these cases one-by-one.

The bulk of these non-price impact evaluations had been conducted by the competition authorities themselves.[8] Only three suitable academic studies, analysing three different mergers, were found. An OECD policy roundtable on evaluating the impact of merger decisions[9] collated reports from competition authorities on the research they have performed and commissioned in order to assess the effects of mergers in their jurisdictions.

Nine studies performed by competition authorities into their own merger decisions were found, and one study by a consultancy, which was commissioned by a competition authority. Most of these studies analysed multiple mergers, meaning that our sample includes 50 mergers. The UK's Office of Fair Trading and Competition Commission are the most prolific authors of this research, publishing six studies into thirty-nine merger cases between 2003 and 2010. The Swedish, Romanian and Netherlands competition authorities also published retrospectives of their merger decisions. The European Commission commissioned the consultancy-performed study.

1.1.3 Potential Biases

Any attempt to reach conclusions about the overall nature of merger enforcement through the analysis of a group of ex-post studies must acknowledge that the mergers analysed by such studies are not a representative sample of all mergers. Kwoka suggests that the mergers of most interest to academics are those which raised the most antitrust questions and where the antitrust authorities faced a hard decision. He also points out that markets with publically available data will be most attractive to academics for study. This biases Kwoka's sample towards regulated industries with data reporting requirements such as airlines and industries such as petrol retailing where data is often collected and released publically by third parties.

The sample of EU merger retrospectives collected by this study is naturally susceptible to the same biases, although given the large proportion of unconditionally approved mergers in our sample we believe it is much less likely that case selection was motivated by the complexity of the merger investigation (rather the availability of data). In any case, we acknowledge that non-random issues, such as data availability could have been driving sample selection. Moreover, our findings also suffer from the consequences of small sample size.

For this reason the interpretation of our findings and conclusions should be limited to this sample and we warn strongly against any generalisation. For example, the finding that in our sample the mean price effect was higher in unconditionally approved cases than in remedied cases must not be interpreted that in general EU CAs are prone to make mistakes when deciding which cases to intervene, but are good at designing a remedy that eliminates anticompetitive problems.

Summary remark 1: *Given the size of the study sample and the likely non-random nature of selecting the mergers to be evaluated, the findings of this study should be treated with caution as any conclusions drawn from this study are specific to the analysed sample.*

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[8] For a list of non-price evaluations see the Table 31 in the Appendix.

[9] Organization for Economic Co-operation and Development, *Impact Evaluation of Merger Decisions* (2011)

2 Estimating the price impact of mergers

For the purposes of this section we follow Kwoka's methodology and apply it to our sample of EU merger retrospectives. The main difference in comparison to Kwoka is that on top of Difference-in-Differences (DiD) studies, we also include ex-post impact estimates using Merger Simulations (MS) that are based on rigorous structural estimates of demand. We acknowledge that the two methods have different merits and limitations, and in reporting the estimated price-effects we refer to these differences where relevant. On top of a Kwoka-type assessment we also discuss the heterogeneity across the estimates.

2.1 Description of studies in the sample

This section gives an overview of the studies in our sample. There were 18 relevant studies, examining 25 mergers. Two of the mergers were analysed using two different methods (DiD and MS) and we assess these separately, which increases our merger sample size to 27. Out of the 18 studies 7 were published in peer-reviewed journals or as book chapters, 11 are available as working papers (at the time of drafting this report).

Table 1: List of mergers included in this study (ordered by jurisdiction)

Merger	Jurisdiction	Decision year	Industry	CA decision	Method*	Price effect (%)
DISA/Shell	ES	2004	Petrol retail	Approved	DiD	0
Easyjet/Go Fly**	EU	2002	Airlines	Approved	DiD	-
Ryanair/Buzz**	EU	2003	Airlines	Approved	DiD	-
Agip/Esso	EU	2007	Petrol retail	Approved	DiD	0.7
Lukoil/Jet	EU	2007	Petrol retail	Approved	DiD	0.8
Volvo/Scania	EU	2000	Car making	Blocked	MS	5.3
Rio Tinto/North	EU	2000	Iron ore mining	Approved	MS	2.6
CVRD/Caemi	EU	2008	Iron ore mining	Remedied	MS	4.6
Carrefour/Promodes	FR	2000	Supermarkets	Remedied	DiD	-2.4
Vinci/GTM	FR	2001	Car parking	Remedied	DiD	3
Ziekenhuis Hilversum/ Gooi-Noord	NL	2005	Hospitals	Approved	DiD	4
Erasmus MC/Havenziekenhuis Rotterdam	NL	2005	Hospitals	Approved	DiD	-1.3
MC Alkmaar/Gemini Ziekenhuis	NL	2007	Hospitals	Approved	DiD	10
St. Lucas/Delfzicht ziekenhuis	NL	2008	Hospitals	Approved	DiD	4.5
Walcheren/Oosterscheldeziekenhuizen	NL	2009	Hospitals	Approved	DiD	-1
Bethesda/Scheperziekenhuis	NL	2009	Hospitals	Approved	DiD	-2.1
GSK/AstraZeneca	SV	2009	Pharmaceuticals	Approved	DiD	42
Carlsberg/Pripps	SV	2001	Brewing	Remedied	DiD	-2
GSK/AstraZeneca	SV	2009	Pharmaceuticals	Approved	MS	34
Cerealia/Schulstad	SV	2003	Bread making	Approved	MS	2.7
Carlsberg/Pripps	SV	2001	Brewing	Remedied	MS	5
Game/Gamestation	UK	2008	Video games	Approved	DiD	-20
Waterstones/Ottokars	UK	2006	Books	Approved	DiD	0

Shell/Rontec	UK	2012	Petrol and diesel retail	Remedied	DiD	1.2
Scottish&Newcastle/Courage	UK	1995	Brewing	Remedied	MS	-15.5
Bass/Carlsberg–Tetley	UK	1997	Brewing	Blocked	MS	56
Morrisons/Safeway	UK	2004	Supermarkets	Remedied	MS	0.26

* Difference-in-Differences (DiD) or Merger Simulations (MS).
** Price effect only expressed in absolute terms.

Table 1 lists all the mergers discussed in this study. All of these are true mergers (e.g. no joint ventures are included in the sample). The mergers span over eleven different industries. These industries are: hospitals with six mergers studied; brewing and petrol retailing with three mergers each; supermarkets, iron ore mining, and airlines with two mergers each; pharmaceuticals with one merger studied with both methods; and book retailing, video game retailing, bread making and lorry trailers all with one merger each.

There is also some variation in the geographical distribution of sample cases. The main three jurisdictions in the sample account for 19 of the mergers. The European Union assessed 7 of the mergers, the United Kingdom 6 and Netherlands 6. Sweden has 3 mergers in the sample[10], France has 2, and Spain has 1. The sample description is given in Table 2.

Two of the mergers in the sample were decided upon by competition authorities in the 1990s, the remaining 21 were in the 2000s. The earliest merger decision studied was in 1995 and the most recent in 2012. The mean lag between a merger decision and a study of that merger decision is 7.3 years. This lag accounts for the lack of recent mergers in the sample. The mean lag between the merger and the analysed year is 1.3 years (with a median of 1).[11]

In 18 mergers the price effects were estimated using DiD. Structural model based merger simulation (MS) was used in 9 of the mergers.[12]

Table 2: Describing the sample

		Total sample		DiD		MS	
		N	Proportion	N	Proportion	N	Proportion
Country	EU	7	0.26	4	0.22	3	0.33
	Netherlands	6	0.22	6	0.33	0	0
	UK	6	0.22	3	0.17	3	0.33
	Sweden	5	0.18	2	0.11	3	0.33
	France	2	0.07	2	0.11	0	0
	Spain	1	0.04	1	0.06	0	0
Method	DiD	18	0.66				
	MS	9	0.33				
Decision	Approve	18	66.67	15	83.33	3	0.33
	Remedy	7	25.93	3	16.66	4	0.44
	Block	2	7.41	0	0	2	0.22
Industry	Hospitals	6	0.22	6	0.33	0	0
	Brewing	4	0.15	1	0.06	3	0.33

[10] Two of these cases (*GSK/AstraZeneca* and *Carlsberg/Pripps*) were assessed by both DiD and MS.

[11] The year of the price-change estimate is the year of the data that is used as the factual in the estimation.

[12] This includes the 2 mergers for which both DiD and MS were used.

	Petrol retail	4	0.15	4	0.22	0	0
	Airlines	2	0.07	2	0.11	0	0
	Pharmaceuticals	2	0.07	1	0.06	1	0.11
	Supermarkets	2	0.07	1	0.06	1	0.11
	Iron ore mining	2	0.07	0	0	2	0.22
	Books	1	0.04	1	0.06	0	0
	Video gamesl	1	0.04	1	0.06	0	0
	Bread making	1	0.04	0	0	1	0.11
	Car parking	1	0.04	1	0.06	0	0
	Car making	1	0.04	0	0	1	0.11
Years between decision and price estimate	Fewer than 1	9	0.33	1	0.06	8	0.89
	1	8	0.30	8	0.44	0	0
	2	6	0.22	5	0.28	1	0.11
	3	2	0.07	2	0.11	0	0
	4	0	0	0	0	0	0
	5	2	0.07	2	0.11	0	0

Finally, 18 of the estimates looked at unconditionally approved mergers, 7 estimates related to mergers that the CA conditionally approved (remedy), and 2 mergers in the sample were blocked by the CA. It appears that DiD methods are used predominantly for unconditionally approved mergers, interventions (remedy or block) were more likely looked at using MS. This will be further discussed in Section 1.4.

As far as market structure is concerned, for those studies where the relevant information was available, the cases in the sample display significant variation, including markets with low and high concentration alike. Information on the market structure is documented in Table 3.

Table 3: Market structure in the sample mergers[13]

		N	mean	std.dev	min	max
Number of post mergers firms		21	7.33	3.51	1	15
Market shares (%)	Combined post-merger	24	0.34	0.18	0.07	0.87
HHI	Pre-merger	14	1812	783	656	3199
	Post-merger	14	2347	1055	693	3867

2.2 Price effect of the transactions

The main variable of interest in our sample of studies is the estimated price effect of the merger. In order to summarise the findings of this largely diverse set of works, it is useful to reduce each study to a single figure that captures the price effects estimated. Kwoka carefully details the procedure he used to solve the same problem which we follow.

Where a single figure is reported as the measured effect, this figure is taken. When the author presents multiple figures from various specifications and indicates that a particular figure is preferred, this figure is taken. Where multiple results are presented alongside each other, then a simple mean is calculated and this is taken as the effect measured by the study.

[13] Because of missing values the frequency is lower than the total number of mergers in our sample.

In some cases it was necessary to convert nominal price changes into percentage figures. For two mergers (*Easyjet*/*Go Fly*, *Ryanair*/*Buzz*) it was not possible to make this conversion so this study is excluded from discussions that relate to the magnitude of the price change (but were still included in the analysis of the sign of the price change). This means that where the magnitude (and not just the sign) of the price-change is discussed our sample only has 25 observations.

Table 4 displays the price effects found, broken down by methodology and whether they found a price increase or decrease. In the 27 price estimates in our sample, two relate to blocked mergers. In these cases the factual is the no merger world against the estimated counterfactual of an unconditionally approved merger.[14] In all of the other cases, the counterfactual is the merger not occurring (i.e. comparing the actual realised price with the counterfactual price). Because of this, a positive price increase implies an undesired outcome for an unconditionally approved merger, and a desirable estimate for a blocked merger (in which case the estimate refers to the avoided price-increase). For this reason we judged that it would be misleading to include blocked merger estimates in this table. For the same reason we have taken out the blocked mergers from all discussion where mean price effects are reported (we always make a note of this).

We also report summary statistics for samples where we removed three 'outliers' (a DiD and MS estimate for *GSK*/*Astra Zeneca*, 42 and 36% respectively; and -20% in *Game*/*Gamestation*); these are displayed as (no outliers) in the tables below. The use of the term outlier might be somewhat misleading in this case as these large price change estimates are exactly the ones that we should be most interested in. For this reason, in the narrative, we refer to the sample including these 'outliers'. Nevertheless the tables below also report a sub-sample where these cases were removed.

Table 4: Mean price effects by direction of price change and type of study[15]

	Difference-in-Differences			Merger Simulations			Total sample		
	Mean (%)	Std.dev (%)	N	Mean (%)	Std.dev (%)	N	Mean (%)	Std.dev (%)	N
Overall	2.34	12.22	16	6.93	12.11	7	3.74	12.11	23
(no outliers)	1.10	3.37	14	2.43	2.50	6	1.50	3.08	20
Increases	8.27	13.96	8	8.19	12.75	6	8.24	12.94	14
(no outliers)	3.46	3.27	7	3.03	1.89	5	3.28	2.68	12
Decreases	-4.80	7.46	6	-0.60		1	-4.20	6.99	7
(no outliers)	-1.76	0.59	5	-0.60		1	-1.57	0.71	6
No effect	0	0	2	0		0	0	0	2

The average price effect was an increase of 3.74%. The overall mean price increase is larger when the MS method was used. This is not surprising because MS for approved mergers will always give a positive price change estimate if no efficiencies are incorporated in the simulations.[16]

[14] MS methods are used to model the approved merger, which in fact did not happen.

[15] As explained above, estimates for the two blocked mergers were excluded from this table. Percentage estimates for the *Easyjet*/*Go Fly*, *Ryanair*/*Buzz* mergers were not available.

[16] Almost all MS studies discuss efficiencies. However the typical way of reporting them is to simulate the level of efficiencies that would be needed to eliminate any negative welfare effects. The MS price estimates reported in this study do not contain such consideration. As these MS studies were conducted after the merger we feel that this was a forgone opportunity to incorporate post-merger information on cost savings.

The average price increase seemed to be the same (8%) irrespective of the method used. To show the distribution of estimated price-changes, we provide histograms of the estimates, broken down by the method used.

Figure 1: Distribution of price effects (%) by method used

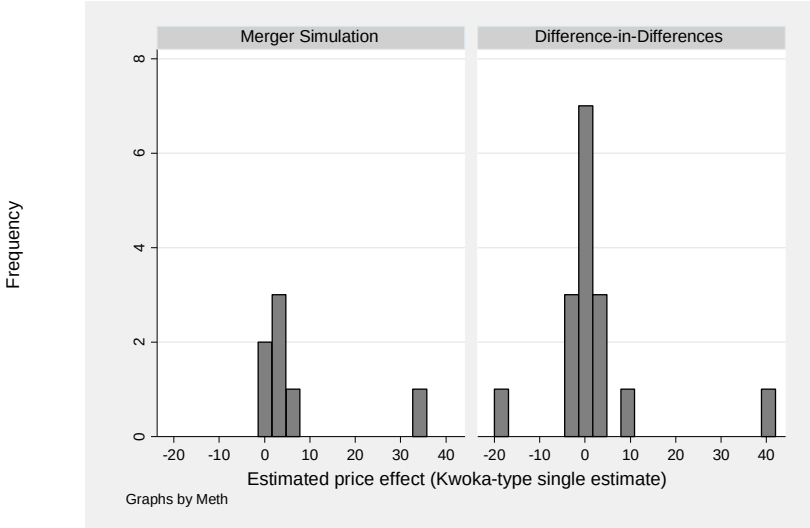

Figure 1 reveals that an overwhelming proportion of estimates are smaller than ±5%. The histograms also show the 'outliers' in our sample. In the DiD sample, at the bottom end, the estimated price change in the *Game/Gamestation* merger was -£5 for mint-games, and -£4.5 for pre-owned games (this was translated to an average 20% price-drop). The authors of the study attributed the large price-drop to the fact that the merger increased the parties' efficiency and, in particular, their ability to obtain better terms from publishers and manufacturers. On the other end of the spectrum, both in the DiD and the MS samples the outlier is the *GSK/Astra Zeneca* merger (42% and 36% price increase respectively). This was due to the merging parties' dominant position and the lack of entry that the CA anticipated at the time of approving the merger. The price increase in this case was likely to have been made worse by a regulation that required pharmaceutical products to be sold in smaller packages. The reduction in the size was not accompanied by a proportionate reduction in the prices, which made the large increase in the unit price a lot less conspicuous.

Purely for expositional purposes (and not as a comparison) we also report the estimates from the Kwoka study.[17] We caution against any comparison of these figures with our sample as the characteristics of the two samples of mergers might be very different in the two studies. For example, it is possible (given the higher proportion of remedied mergers in the sample) that US studies are more likely to be on more concentrated markets.

Table 5: Kwoka's price estimates broken down by the sign of price-change

	Mean price change	Number of Cases
Overall	7.29%	46
Increases	9.85%	38
Decreases	-4.83%	8

[17] Note that in Kwoka's study all price effect estimates are reported irrespective of their statistical significance. We treated non-significant estimates as no-effect mergers.

2.3 Price effects and competition authority actions

Kwoka divides merger decisions into five categories: Cleared, Opposed,[18] Divestiture, Conduct/Conditions, and No information. There are no mergers in our dataset where we do not know the competition authority's decision. Only one merger in our sample was given a behavioural remedy (the weakening of vertical ties in the *Scottish and Newcastle*/*Courage* merger) so we treated all remedied mergers as a single category, leaving us with three categories: approved, remedied and blocked. Kwoka uses the same three categories later in his analysis.

Table 6: Mean price effects (%) by competition authority intervention and type of study[19]

	Difference-in-differences			Merger Simulations			Total sample		
	Mean (%)	Std.dev (%)	N	Mean (%)	Std.dev (%)	N	Mean (%)	Std.dev (%)	N
Approved	2.71	13.60	13	13.10	18.10	3	4.66	14.46	16
(no outliers)	1.20	3.66	11	2.65	0.07	2	1.42	3.39	13
Remedies	0.73	2.53	3	2.31	2.90	4	1.64	2.65	7
(no outliers)	0.73	2.52	3	2.31	2.90	4	1.64	2.65	7
Blocked			0	30.65	35.85	2	30.65	35.58	2

Table 6 shows the price outcomes of mergers by methodology and CA action. The average price increase in unconditionally approved mergers is around 5%. In Kwoka's sample (shown in Table 7) unconditionally approved mergers had a similarly high (7.4%) price increasing effect. Remedied mergers had a more moderate effect. On average the price after a remedied merger increased marginally (by 1.64%). In contrast, Kwoka's estimates for the various decision types reveal that in the US sample of studies, remedied mergers were followed by a large price increase. We will discuss this discrepancy below.

Table 7: Kwoka's estimates broken down by decision type

	N	Mean price change (%)
Approve	8	7.40
Conduct remedy	4	16.01
Divestiture	6	7.68
Blocked	5	1.86
No information	23	6.82

To complement the above discussion we also look at the direction of the price change (increase, decrease, no effect) in Table 8.

Overall, 16/27 (around 60%) of the sample mergers led to a price increase.[20] It appears that there are proportionately more price increases in intervened mergers – although, as shown above, the mean magnitude of these price increases is smaller than in non-

[18] The prosecutorial system in the US allows for a class of mergers that we see only rarely in the EU – mergers that the authority opposed but which they were unable to get the Court to block. These cases offer the potential to test for CA Type I errors – i.e. if prices stay the same or go down following, then this suggests a Type I error in respect of the authority's original decision. In the EU, we rarely have such cases (although we could in very specific circumstances – e.g. where mergers are cleared only on the basis that the turnover concerned is too small or whether authorities make procedural errors which allows the merger to go through (e.g. missing deadlines). We had neither in our sample of cases.

[19] Percentage estimates for the *Easyjet*/*Go Fly*, *Ryanair*/*Buzz* mergers were not available.

[20] 75.5% of Kwoka's transactions were estimated to have increased prices.

intervened ones. In these cases it seems that the CA was right in that intervention was necessary – as prices increased even with the remedy. But the scope of the intervention was insufficient to completely eliminate the anticompetitive effects. It also stands out, from Table 6, that in comparison to non-intervened mergers the price increase is very small in remedied cases, implying that the remedies were effective at eliminating most of the problems.

Table 8: Direction of price change by CA intervention type

	Increase		Decrease		No effect		Total	
Approve	9		7		2		18	
	DiD	MS	DiD	MS	DiD	MS	DiD	MS
	6	3	7	0	2	0	15	3
Remedy	5		2		0		7	
	DiD	MS	DiD	MS	DiD	MS	DiD	MS
	2	3	1	1	0	0	3	4
Block	2		0		0		2	
	DiD	MS	DiD	MS	DiD	MS	DiD	MS
	0	2	0	0	0	0	0	2

Kwoka's main conclusion is that US authorities were right to intervene in the remedied cases, but the remedies were insufficient to eliminate competition concerns. Moreover, on average, they misjudged the effects of the merger in the unconditionally approved cases (where intervention should have been made). A similarly stylised conclusion on the EU sample would be that remedies are effective in eliminating post-merger price increases (hence the small price increase in remedied mergers), however EU authorities are also not so good at identifying which cases to intervene (hence the high estimate price-effect of unconditionally approved mergers).

In any case, a comparison of the effectiveness of the jurisdictions would be misleading without knowing more of the characteristics of the cases. Firstly, the limitations of small sample size apply to both samples. Secondly, it is possible that the US sample is composed predominantly of high-concentration, problematic mergers, whilst the EU sample contains more benign cases. The cases studied for ex post evaluation are highly unlikely to represent a random sample drawn from the full set of merger decisions. Cases might have been chosen for ex post evaluation because the original decision was seen to be marginal, or because there are suspicions that the merger may in fact have been harmful, or more prosaically because of data availability. Moreover, the price effect is only one of the variables (yet, an important one) upon which the CA draws conclusions on the merger. It is equally possible – as discussed in Section 4.2 – that other considerations (such as quality or efficiency improvements) dominated the merger decision. For this reason we would argue that the difference in the main conclusions of Kwoka's work and this study is likely to be due to the differences in the way the two samples were selected.

Summary remark 2: *On average, mergers in our sample were followed by a price increase, although this remained under 5 per cent in the large majority of cases. The average price increase in unconditionally approved mergers was just under 5 per cent and in remedied mergers between 1 and 2 per cent. In half of the unconditionally approved cases post-merger prices increased. The majority of remedied mergers are associated with a price increase despite the remedy, although this increase is very small.*

For the remainder of this report we diverge from Kwoka and look at these price estimates in more detail.

2.4 Examining heterogeneity across merger studies

Kwoka's study stops short of examining the heterogeneity across the mergers (and across estimates). It is possible that the high price-increases were typical to very concentrated, and thus difficult to handle markets. It is possible that the time lag between the merger and the observed price data also matters (reading the underlying studies it appears that there is considerable variation in this time lag). Finally, industry-specific conditions in some markets might be more/less susceptible to a larger/smaller price increase.

Reporting on the authority's merger decisions tends to be more transparent in Europe, which allows us to examine some of this heterogeneity across our sample of mergers, and provide some simple findings.

2.4.1 Market structure

Market structure can serve as an indicator of the likely market power increasing effect of the merger. For this reason market structure considerations have a prominent place in EU merger control guidelines. We collected information on three different measures of market concentration (number of firms, market share of the largest merging firm, and HHI), and these were introduced in Table 3 above. Although our sample size is prohibitive in terms of making general conclusions about EU merger control, some clear patterns emerge.

Table 9 reports the average estimated price-effect of mergers in our sample. For each measurement of market concentration we broke down the sample into two groups, around the median measure of concentration. For example the median of the combined post-merger market shares is 35%, therefore we report the average of the price-change estimates for cases where the combined post-merger market share is less than 35% and cases where it is more. In this context, a market is less concentrated if the combined market share is less than 35%, if the pre-merger HHI is less than 1700, if the post-merger HHI is less than 2400, or if the number of firms in the market is more than or equal 5.

Table 9: The estimated price-effect of mergers by market concentration

		Total		Unconditional		Remedied	
Measure of concentration		N	Mean price-change (%) (std.err)	N	Mean price-change (%) (std.err)	N	Mean price-change (%) (std.err)
Combined post-merger market share of merged firms (%)	< 35%	11	0.59 (2.29)	9	0.76 (2.52)	2	-0.17 (0.61)
	> 35%	11	7.12 (17.12)	7	9.67 (21.50)	4	2.65 (3.22)
Pre-merger HHI	< 1700	5	-0.13 (1.31)	4	-0.22 (1.49)	1	0.26
	> 1700	7	12.10 (17.98)	4	19.67 (21.43)	3	2.00 (3.61)
Post-merger HHI	< 2400	7	-0.13 (1.31)	4	-0.22 (1.49)	1	0.26
	> 2400	7	12.1 (17.98)	4	19.67 (21.44)	3	2 (3.61)
Number of firms	< 6	6	12.5 (8.16)	6	12.5 (19.99)	0	
	≥ 6	11	1.67 (0.69)	5	1.62 (1.64)	6	1.71 (2.90)

Table 9 reveals that market concentration is a strong driver of the estimated price-effect of the merger. The average price-increase in markets that are considered un-concentrated based on conventional measures of concentration is around zero. In concentrated markets, on the other hand, the average price increase is large, although the remedies managed to mitigate the post-merger price hike even in concentrated markets.

Summary remark 3: *Average estimates of post-merger price increase were around zero where the market was less concentrated. In more concentrated markets the average estimated price-increase was large (between 10% and 20%), but only if the merger had been unconditionally approved. In the sample, remedies had been able to reduce post-merger price-increases even in concentrated markets.*

2.4.2 The timing of the study

The other characteristic of the sample estimates that we observe is the time-gap between the merger and the analysed year (this is the year in which the post-merger price is observed). Although all the studies highlighted a single price-change as a preferred estimate, some of them estimated the post-merger price impact for various years following the merger. For example the *Dutch hospital mergers* study estimates how post-merger prices changed in the first to the fifth year after the merger, and it uses the over-time average estimated price-change as its preferred one (although the study also reports price-change estimates for each year following the merger). For the purposes of analysing how post-merger prices evolve over time, we treat the estimates for the various years as separate. In the 18 DiD studies we had 28 such estimates.

We argue that it is crucial which post-merger year the research estimates the price-change for. It seems unlikely that a CA would choose to intervene if it believed that market correction would offset the price increase within one or two years after the merger. It is also unlikely that the CA would refrain from intervention if it judged that the effects of the merger would slowly materialise. For these reasons it seems to matter that the ex-post study uses data that spans sufficiently far following the consummation of the merger (on the other hand, longer spanning data is more likely to contain confounding effects, i.e. effects other than the merger).

Table 10 suggests a quadratic relationship between the time passed after the merger and the price-change estimate, peaking at around 2-3 years after the merger. Accepting the figures in Table 10 at their face value would suggest that prices only increase for a few years after the merger, and eventually they drop to near pre-merger levels. But even if one was to jump to such conclusions, it still would not imply that market self-correction forces fix un-remedied mergers and therefore there is no need for merger control. The un-remedied mergers were approved, exactly because the CA may have anticipated these correcting forces to eliminate any price increase. On the other hand, in more problematic mergers the CAs intervention was seemingly effective in eliminating most post-merger price increase by requiring a remedy.

Of course given the sample size we cannot statistically establish a significant difference between the years. Nevertheless it appears that it does matter when the study is conducted. A study using data from the immediate aftermath of the merger may not fully reflect all the effects of the merger. A study with a sufficiently long delay after the merger might already reflect the effects of market self-correction.

Table 10: The estimated price-effect of the merger by the year of the price estimate[21]

	All cases			Unconditionally cleared			Remedied		
Number of years after the merger	N	Average price-increase (%)	Std.dev. (%)	N	Average price-increase (%)	Std.dev. (%)	N	Average price-increase (%)	Std.dev. (%)
0	1	3	0.34	0			0	1	3
1	13	0.301	1.60	11	0.42	1.58	1	2	-0.35
2	7	6.98	19.00	6	8.48	20.36	2	1	-2
3	3	4.63	5.49	3	4.63	5.49	0		
4	2	0.88	3.07	2	0.88	3.07	0		
5	2	3.24	3.17	2	3.24	3.17	0		

The second and third block of columns in Table 10 show the price-change estimates but only for unconditionally approved and remedied mergers respectively. Unconditionally approved mergers follow the same pattern as the total sample. The few remedied mergers do not show much variance over time, in the price-change estimate. Of course these numbers have to be handled with caution. The sample size is very small for formal testing, and it is also possible that the high price-increase estimates are simply to do with the market concentration in those cases and not the timing of the study. Without a larger sample and a more detailed multiple regression analysis we cannot statistically confirm if there is indeed a time-effect on the price-change estimate.

2.4.3 The relevant industry

Although we only have a handful of price-change estimates for each industry, there were three industries where 4 or more studies were conducted. From these, some simple observations can be made. For hospitals and brewing, the price-change estimates show considerable variation. On the other hand, for petrol and diesel retail all estimates are around or less than 1 per cent. This raises an important point. Based on price-change estimates one would be inclined to conclude that petrol retail mergers do not harm consumers once remedies have been imposed. Any such conclusion would ignore the possibility that even a small price change can have large welfare implications in certain circumstances. Such is the case in petrol retail, as petrol prices are likely to represent a large proportion of household expenses, demand is probably very inelastic, and at retail level it is unlikely that mergers would lead to large cost savings.

Table 11: Price effect by industry

Industry	N	average price change	std.dev.	min	max
Hospitals	6	2.35	4.69	-2.1	10
Petrol retail	4	0.675	0.50	0	1.2
Brewing	3	0.8	3.70	-2	5
Supermarkets	2	-1.07	1.88	-2.4	0.26
Pharmaceuticals	2	38	5.66	34	42
Iron ore mining	2	3.6	1.41	2.6	4.6
Video games	1	-20		-20	-20
Books	1	0		0	0
Car parking	1	3		3	3
Bread	1	2.7		2.7	2.7

[21] Because the timing of the study only varied in the case of DiD estimates (and not for simulations), this table only contains DiD estimates.

Of course we would not like to use the findings of a handful of merger price-effect estimates to make general policy conclusions. Nevertheless, it appears that the characteristics of the analysed industry have to be taken into account when interpreting the price effect estimates. A full-fledged merger simulation would of course allow the analysis of the full welfare impact of the merger.

2.5 Why are there fewer DiD studies in the EU?

Throughout this section we continuously emphasised the limitations of our results due to the small number of relevant EU merger evaluations. This becomes even more conspicuous if one makes a comparison with the number of US DiD studies. Kwoka (2013) starts from a sample of 53 studies that studies 62 mergers (some mergers are covered more than once), and these are only the peer-reviewed studies. There is at least the same number of (yet) unpublished merger retrospectives in the shape of working papers. In contrast, in Europe we could altogether only find 11 distinct DiD studies, looking at 18 mergers.[22]

This of course raises the question: why is there such a gap between the two sides of the Atlantic, why are there much fewer merger retrospectives in Europe? Of course we cannot provide a definitive answer to this question, but we can look at some of the differences between the two sets of studies that might provide at least a partial explanation.

Industry analysed

The US studies are concentrated around a few industries: petroleum (petrol retail), academic publishing, airlines, and hospitals giving most of the retrospective studies. In Europe there is also some concentration but it is difficult to observe similar patterns in the studied industries in Europe as in the US – although this may be due to the sample size. Some industries that had more than one mergers studied were covered by the same study (e.g. the 6 Dutch hospital mergers, the 2 airline mergers, or the 2 mining mergers).

Table 12: Number of mergers studied using DiD in the US and in Europe (by industry)

Number of mergers retrospectives in the US	Industry	Number of merger retrospectives in Europe
13	Petroleum/petrol retail	4
10	Academic journals	0
18	Airlines	2
3	Railways	0
6	Hospitals	6
0	Brewing	3
0	Mining	2
2	Microfilms	0
0	Pharmaceuticals	1
0	Book retail	1
0	Video game retail	1
0	Bread making	1
0	Car parking	1
0	Supermarkets	1
1	Banking	0
1	Breakfast syrup	0
1	Cement	0
1	Conventional motor oil	0
1	Corrugating medium	0
1	Feminine hygiene products	0

Data used

The high concentration of sampled industries in the US is partially due to data availability. For example 9 studies looked at 18 mergers in the airline industry. A large proportion of these relies on the dataset published by the U.S. Department of Transportation (DOT), which is a quarterly 10% sample of all flight itineraries in the U.S. Similarly, merger

[22] As we do not have an inventory of US MS studies we only make comparison on DiD studies.

retrospectives on petrol retail mergers almost invariably use data from the Oil Price Information Service (OPIS), which is high-quality, proprietary data.

Contrary to the US, we did not find two studies in the EU sample that used data from the same source. As there are no similar EU-wide industry regulators, it is less likely that data, similar to the DOT dataset is available in Europe. However, at member state level industry regulators would always be a good potential data source, which was made use of in the *Dutch hospital merger* studies. Of course, proprietary data would be available at EU-level, but its price might form an imperative obstacle to using it for merger studies. In places, where the CA is bound by a commitment to regularly deliver ex-post evaluations (e.g. CMA UK), the CA might have budgetary allocation for buying such data for ex-post studies. On the other hand, smaller authorities are understandably more constrained by their scarce resources.

Who conducted the study?

When it comes to conducting these evaluations, CAs would be well positioned given their better access to relevant data, information about the decision, and industry sources. However, academics are more likely to be seen as representing independent opinion. Moreover, conducting a fully-fledged ex-post assessment of mergers is a resource-demanding exercise, which the authorities typically lack the time for.

In the US more than 4/5 of the merger retrospectives (used in Kwoka) had officials from the FTC, DOJ, or the Government Accountability Office as authors or co-authors. In the EU, only a third of the studies (9 out of the 27) were authored or co-authored by officials from competition authorities.

This difference may be explained by simple organisational anomalies between authorities on the two sides of the Atlantic. Alone in the FTC there are approximately 80 staff with PhD in applied microeconomics.[23] This figure dwarfs the number of staff with similar qualifications in EU CAs. From anecdotal stories we also know that the FTC strongly supports staff research activities. Similar considerations might be less prevalent in some EU CAs.

Judicial review

It is also possible that the difference in the CA's commitment to ex-post evaluations also matters. In the EU, the Court can challenge the CAs decision ex-post. This might explain why these authorities could be more reluctant to publish the results of ex-post evaluations. On the other hand, in the US, the Court procedure plays a much more central role in the decision-making, therefore the analysed mergers are typically not subject to further judicial review. This may explain why US authorities have more incentives (or less disincentives) to assess the decisions.

For all of the above reasons it would be important to provide more incentives to deliver ex-post merger studies in Europe. A formal commitment of the CA to such activities could help ensure that some of the budget is allocated to these activities and that political disincentives do not obstruct the decision to conduct retrospective merger analyses.

On the plus side, there are signs of more agency involvement in conducting merger retrospectives in the EU. The CMA in the UK has already been regularly conducting these studies: as it "has a public commitment to evaluate each year at least one of its previous interventions".[24] Recently, the European Commission (both by commissioning external experts, and through its Chief Competition Economist's team), and the Dutch competition authority (by officials of the authority) have also been active in delivering ex post assessments of their merger decisions.

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[23] https://www.ftc.gov/about-ftc/bureaus-offices/bureau-economics/careers-bureau-economics

[24] See *Shell*/*Rontec* study, p.4.

3 Methodologies used in merger retrospectives

In this section we introduce two alternative econometric methodologies that are used for the evaluation of mergers. The first belongs to the causal effects literature and is known as Differences-in-Differences (DiD). The second procedure builds on the structural modelling of the market and we refer to it as Merger Simulation (MS).

Both DiD and MS techniques have their relative strengths and limitations and each can be more fitting in certain situations than the other. One problem to be aware of from the beginning is that if the underlying assumptions are not fully understood and mastered, both methods can lead to biased estimations of relevant parameters. Unfortunately there is no one-size-fits-all method to use, though under certain circumstances, one or the other method may be preferred.

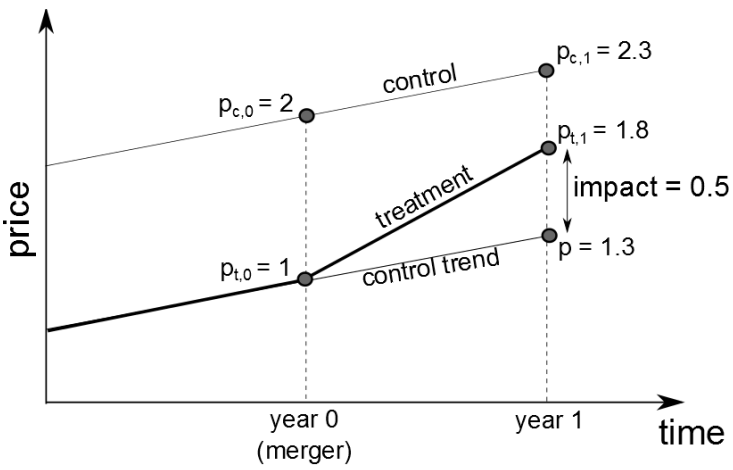
We examine the main assumptions required under each method and then discuss issues related to the data, the counterfactual, the model specification, and robustness checks. Where relevant we refer to the individual studies for illustrational purposes. This section is accompanied by a Technical Appendix to be found at the end of this document. This Section, together with the Appendix can be used as a brief standalone guide for conducting DiD or MS estimations.

The discussion below also reflects how we believe ex post merger assessments should be conducted using either DiD or MS. We highlight the weaknesses of some of the past studies and discuss how erroneous research design and implementation can lead to biased estimates. We also examine characteristics of research design (such as the choice of data of counterfactual) in our sample studies and look at how they affect the post-merger price-change estimates.

3.1 Difference-in-differences (DiD)

The simplest forms of causal analyses are before and after price comparisons and cross-section price comparisons. In the context of merger analysis this means comparing prices before and after the merger or comparing post-merger prices over different geographical areas or products. The main problem with either of these methods is that the estimated difference is likely to be a biased estimator of the effect of the merger.

Figure 2: Interpretation of the DiD estimator



We provide a technical examination of this bias in the Appendix but Figure 2 gives a simple illustration. Following the usual notation in causal analysis, one group of individuals (i.e. the merging firms, or the affected market) is given a (t)reatment (i.e. the merger) and another group, as similar as possible to the treated group, is used as (c)ontrol. Figure 2 shows how prices change for these two groups before and after a merger (year 0).

At the time of the merger, the price in the two groups was 1 and 2 respectively. One year after the merger the price in the Treatment group was 1.8, and the price in the Control

was 2.3. A before-after analysis would compare the post-merger Treatment price $p_{t,1}$, with the pre-merger treatment price $p_{t,0}$, and would suggest that the merger increased prices by $1.8 - 1 = 0.8$. A cross-sectional analysis would compare the post merger prices in the Treatment $p_{t,1}$ and the Control groups $p_{c,1}$, and would estimate that the merger increased prices by $2.3 - 1.8 = 0.5$. Both of these would be biased estimates and the slopes of the Treatment and the Control explain why. As Figure 2 shows, there is a price-increasing trend in the market before the merger and this trend continues in the Control group even after the merger. If the only difference between the Control and the Treatment is the fact of the merger, then the Treatment price-trend would be exactly the same as the Control trend. To take this trend effect out of the estimate, DiD compares the difference across Treatment and Control in the differences between pre-, and post-merger prices; i.e. $(p_{t,1} - p_{t,0}) - (p_{c,1} - p_{c,0}) = 0.3$. In Figure 2 $p = 1.3$ shows what the price would have been in the Treatment market, in the absence of the treatment.

The DiD method uses variation over groups of individuals and over time to mitigate the issue of selection bias. The Control group is used as benchmark to evaluate what would have been the situation without treatment (the counterfactual).[25] The identification relies on the assumption that the Control group is similar to the Treatment group and that the two groups have similar behaviour over time. When it comes to mergers it can be challenging to find proper control groups, as reiterated later on.

3.1.1 Assumptions needed for DiD

This section provides a detailed list of assumptions used by DiD methods.[26] It often comes down to the choice of the Control group (counterfactual) to ensure that these assumptions are not violated. For this reason the discussion is strongly related to the choice of the Control. In Section 3.1.2 we give a detailed discussion of some of the practical aspects of this choice.

3.1.1.1 Parallel trends

This assumption states that the output variable of interest (typically price) follows parallel trends over time for Treatment and Control. Strictly speaking, this assumption is not directly testable because we never observe the Treatment group without treatment (i.e. we cannot observe the merging firms in a 'post-merger' world without a merger). Nevertheless, we can compare trends over time, if we have data on more than two time-periods, by estimating a model with dummy variables for each time period (e.g. year) pre-merger, differentiated for the treatment and control groups. This is now widely used practice, however in our sample only 3 of the studies looked at this question formally.[27]

Adding time periods to estimate trends for the treatment and the control before and after the merger is good practice to validate the hypothesis that the distance between the two groups is constant over time, but the researcher has to be aware that longer time series bring in issues of serial correlation, which may lead to biased standard errors (typically underestimated standard errors) as discussed below.

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[25] For the purposes of this section we use the shorthand term Treatment to refer to the factual scenario (i.e. the merger price) and Control to infer the counterfactual world of no merger.

[26] More detailed technical discussion these assumptions is given in the Technical Appendix.

[27] In *Shell*/*Rontec* prices for the Treatment and Control are compared for a 2.5 year period – starting from 1 year before the merger. In the *DISA*/*Shell* study the authors estimated the same models incorporating dummy variables for each year prior to the merger, differentiated for the islands affected and unaffected by the merger and eliminating the variables of the DiD estimator. In *Vinci*/*GTM* the authors also used price data starting from 1 year before the merger to test for parallel trends.

3.1.1.2 No serial correlation

We talk about serial correlation (or autocorrelation) if in the estimated model the error terms across time periods are correlated. In the case of mergers, where typically price effects are estimated, this would mean correlation between prices at different time-periods due to an unobserved factor.

DiD models often rely on panel data that spans over many time-periods and cross-sections. Not all panel data necessarily suffers from autocorrelation. If the number of time series is small (e.g. two periods, pre- and post-merger) then autocorrelation may not be so much of an issue. However, when the panel data is dominated by its time-series dimension then all issues of a time series analysis (including autocorrelation) have to be accounted for. Bertrand et al. (2004) give a general rule of thumb: DiD using panel data with more than five time-periods is likely to suffer from autocorrelation.

If there is positive serial correlation in the price data, then the standard errors of the OLS coefficient estimates will be lower than the unbiased standard errors. This would imply that the effect of the merger might be found significant even when, in an unbiased model, it would not be. Similarly, negative serial correlation in the price data may overestimate the standard error of the merger effect.[28]

It is therefore advised that, if there are many time-periods in the data, the research should test for autocorrelation in the estimated model. All graduate level econometrics textbooks contain explanations of how to run these test; we provide more details in the Technical Appendix and also offer an overview of the most commonly used ways to deal with autocorrelation.

Table 29 in the Appendix contains details of the data used for each study. The most relevant column, from the perspective of autocorrelation, is the number of time periods used in the estimations. The average number of time periods in our sample was 30 (with a median of 18 periods). The shortest time series was 2 periods (before and after the merger, *Game/Gamestation* and *GSK/Astra Zeneca*), the longest was 118 periods (*Shell/Rontec*). We broke down our sample into two groups around the median number of time periods in the data. We found no difference in the average price-change estimates for these two groups. However, the variance of these price estimates is much higher in the group with few time-series periods (all the large negative and positive price change estimates fall in this group). The standard deviation of price-change estimates in this group is around 10 times the standard deviation of the other group.

Two of the studies in the sample with a high number of time periods tested for autocorrelation and made attempts to address them. The *Waterstones/Ottokar* study imposed an autoregressive structure (the price variable depends on its own previous values) on the model, and – in different model – also tried using monthly dummies as time fixed-effects instead of the monthly trend.[29] The *Shell/Rontec* study provides the most detailed discussion of the issue of autocorrelation. They included in the model lags of the dependent variable to eliminate serial correlation from the residuals. As an alternative treatment they also used Newey-West and Prais-Winsten estimators and Feasible GLS estimators. The study discussing *Lukoil/Jet* and *Agip/Esso* acknowledged the possibility of non-stationarity, and used a proxy for profit margins instead of retail prices as dependent variable to eliminate non-stationarity in the data.

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[28] The magnitude of the bias depends on the length of the time series and also on how serially correlated the independent variable (the merger dummy) is. This is an important point – emphasised by Bertrand et al. (2004) – because serial correlation is likely to be high for the merger dummy (as Bertrand et al. put it, this variable is 0 until the merger happens and then remains 1 afterwards).

[29] These are typical tools for eliminating non-stationarity in the data, however autocorrelation could have remained even in stationary data.

3.1.1.3 Exogenous mergers

The assumption that treatment is exogenous is often violated in the field of medicine (where the terminology of treatment/control originates from). However, it may also be an issue in the analysis of mergers, where the incidence of a merger may be endogenous to various market conditions. One main source of endogeneity is when the estimated model does not control for important variables (omitted variables). For example unobserved demand shocks or previous mergers[30] might trigger the merger decision. If the dependent variable is price and these omitted market conditions affect both price and the decision to merge then we have a problem of endogeneity and the estimated effect of the merger will be biased. Besley and Case (2000) discuss this type of endogeneity in DiD models.

Another source of endogeneity is where there is reverse causality between the dependent variable (price) and the independent variable (the merger). For example a slump in prices might trigger mergers in order to generate cost savings but in turn the merger will affect the price in the market.

It is possible – and this is also argued by some studies in our sample – that the merger is truly exogenous. For example the *Lukoil/Jet* and the *Agip/Esso* studies argue that the decision to merge was done at international level, making it exogenous when looking at the impact of the merger on prices (mark-up) in a relatively small geographical area (Hungary). Similar argument is used in the *Vinci/GTM* study. In the *Shell/GTM* merger the authors argue that the merger was triggered by an identifiable exogenous event, Shell's exit from Spain and private negotiations to sell the company's assets in the archipelago.

One way of dealing with omitted variables (unobserved heterogeneity) is to include individual (market) level fixed effects in the estimated models. This way we can rule out the part of unobserved heterogeneity that is time-constant. To give an example, it is reasonable to think that the Control market did not have a merger because the same economic conditions were not present as in the Treatment market.[31] For example, assume that a supermarket merger, with local effects, is being assessed. Different (unobserved) demand characteristics might explain why a supermarket does not have a store in some local markets. These demand conditions will obviously affect price and will also affect whether the merger happens in the market. Using local market dummies (fixed effects) as control would account for the time-invariant part of the unobserved differences between local markets. Almost invariably all studies in our sample use fixed effects dummies to capture some unobserved heterogeneity.

Another way of dealing with endogeneity is by using instruments. This means finding variables that are exogenous to the model but are correlated with the merger decision. For example, in a US study Dafny (2005) uses rival co-location as instrument, i.e. the distance of a rival from the merging firm. None of the studies in our sample instrumented the merger decision. This is no surprise, finding variables that affect the decision to merge but not the outcome (price) variable is not a trivial exercise. Having looked through a large body of US merger retrospectives, it appears that they typically assume that the merger is exogenous, or at best treat it with fixed-effect dummies.

The endogeneity of mergers in DiD is certainly a topic that has not been explored in great detail. For this reason we do not give any recommendations, instead we offer the following pointers:

1. Try and identify what triggered the merger and think how this would have affected the prices. Controlling for these might help eliminate the omitted variable bias.
2. Include a set of fixed effect dummies for each individual/geographical area/etc. to eliminate some of the unobserved heterogeneity.

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[30] For literature on endogenous mergers see Qiu and Zhoum (2007).

[31] Besley and Case (2000) make the same suggestion, p688.

3. In exceptional cases there might be suitable instruments. This may vary case by case but intuitively we think that in the case of large diversified mergers a possible set of instruments could be gained from characteristics of parts of the merging firm that are not directly involved in the merger.

3.1.1.4 No spill-over effects

A spill-over effect occurs where the effect of the treatment spills into the Control group, nibbling away at the difference between Control and Treatment, hence underestimating the effect of the merger. This may be problematic in markets with strategic interaction, as typically are those studied in most of the merger literature. A violation of this assumption would mean that the estimated post-merger price change is downward biased in its absolute value (it will be the same sign but smaller absolute value). More detail on this assumption and how it is related to the choice of Control (counterfactual) is given in Section 3.1.2.

3.1.1.5 Treatment and Control are sufficiently similar

The only difference between the two groups is the treatment (merger). Simply, with the violation of this assumption the price-effect estimates measure not the impact of the merger, rather the difference in price changes between two different markets. There are now widely used formal methods for selecting counterfactuals that adhere to this assumption (e.g. propensity score matching); this is discussed in detail in Section 3.1.2.

3.1.1.6 Grouped error terms

Bertrand et al. (2004) highlights another source of potential bias: failure to use grouped error terms in cases where the unit of observation is more detailed than the level of the variation. For example, the effect of the same merger is estimated across many local markets or different firms. Clustering the error terms at these local levels or the level of firms is a straightforward solution to this and is offered as an option by all mainstream statistical packages.

Summary remark 4: *In Difference-in-Differences studies the change in the price of the merging firms (Treatment) is compared with the change in the price of a counterfactual market (Control). The following assumptions have to be satisfied: the prices in the Control and Treatment markets follow a parallel trend; price at one period is not correlated with price at another period; firms' decision to merge is not correlated with unobserved characteristics that also affect the relevant prices; prices in the Control group are not affected by the merger; and there is sufficient similarity between the Treatment and the Control markets.*

3.1.2 Choice of control

The validity of all the above assumptions hinges on the choice of the Control. The two most frequently chosen Controls in literature are competitors' prices and prices in local markets not affected by the merger.

Table 13 shows the mean price-change depending on the two main types of Control. The size of the sample does not allow us to do much formal testing, nevertheless, it appears that in almost all the cases where a post-merger price increase was estimated local markets were used as Control (9/10 cases). Below we look at each of these methods in detail.

Table 13: The direction of price change by choice of Control

		Price change (%)			
Control	Mean	Negative	Zero	Positive	Total
Competitor	6.67	2	1	1	4
		50%	25%	25%	100%
Local market	1.34	4	2	9	15
		26.67%	13.33%	60%	100%
Total	2.34	6	3	10	19
		31.58%	15.79%	52.63%	100%

Note: The *Waterstones/Ottakar* study used both local markets and competitors' prices as Control; the Table includes separately both of them, therefore a total number of DiD estimates increases from 18 to 19.

3.1.2.1 Competitors' prices

A simple Control group could compose of the merging firms' competitors (i.e. firms in the same market as the merging firms but not involved in the merger). This is of course based on the assumption that the merger had no impact on competitors' prices. There is a wide range of theoretical and empirical evidence refuting this assumption.[32] This would imply that using competitors' prices as Control may provide biased price-effect estimates. For example if the merger resulted in a price increase and it had a spill-over effect on the rivals, the rivals' prices would have also increased (although to a lesser extent[33]). Therefore any post-merger price-change estimate will be biased downwards. Nevertheless the price effect estimate can still be interpreted as a lower bound to the real effect.

In our sample there were 4 cases where competitors' prices were used as control.[34] It is important to add that in two[35] of these the primary choice of estimation method by the researchers was MS and a simple DiD was conducted to verify the MS findings. Out of the other two studies, in *Game/Gamestation* prices were set at national level in the UK, therefore using UK local markets was not an option (using other countries would have meant very dissimilar Control). Moreover, the *Game/Gamestation* study – which estimated that the merger led to a large post-merger price-drop – acknowledges the limitation of using competitors' prices as Control: "*competitors' prices may also be affected by the merger and, therefore, they may not fully reflect the counterfactual scenario that would have occurred absent the merger*". In *Waterstones/Ottakar* the authors re-estimated their model at the national level, using competitors as Control. This was done as a robustness check, which supported the main findings of the study.

Of the 4 relevant studies, prices were estimated to drop in two cases (by 2%[36] and 20%[37] respectively); in one of them no change was estimated[38] and in one of them the prices were estimated to rise (by 42 percent). If we follow the argument, that using competitors'

[32] For a discussion in the literature, see Borenstein (1990), Clougherty and Duso (2009), and Ashenfelter et al. (2013).

[33] See for example Deneckere and Davidson (1985, RAND).

[34] *Carlsberg/Pripps*, *Game/Gamestation*, *Waterstones/Ottakar* (as part of a robustness check) and *GSK/Astra Zeneca* mergers.

[35] *Carlsberg/Pripps* and *GSK/Astra Zeneca*.

[36] *Carlsberg/Pripps*.

[37] *Game/Gamestation*.

[38] *Waterstones/Ottakar*.

prices may give a lower-bound estimate only, the real effect of these mergers could have been larger in absolute terms.

3.1.2.2 Local markets

The Control can also be constructed using local markets, i.e. different geographical markets for the same product but not affected by the merger.[39] This method explores the local variability of prices and assumes that there are geographical markets for the same product that have similar characteristics to the merger market but are unaffected by the merger (e.g. because only one of the merging firms are present in the market). For example, in *Waterstones/Ottakar* the authors use local markets where only one of the merging firms is present.

The idea behind using local markets is to populate the Control group with price data from sufficiently similar markets that were not affected by the merger. In 15 of our sample cases local markets were used as Control. There was some variation in how the Control groups were created. The simplest – although probably not the most efficient – of these is when all local markets where data was available are included in the study.[40] More sophisticated methods have been developed for improving the choice of Control (i.e. to select local markets that are sufficiently similar and unaffected by the merger). Here we introduce three of these methods that are most relevant for the analysis of mergers.

- **Propensity score matching (PSM)**: PSM is a method that allows the selection of *a Control group that is most similar* to the Treatment group bar the merger. In many applications, especially in health studies, PSM uses variables that predict the reception of treatment. These variables are applied to decide which individuals are equally likely to receive the treatment (i.e. which one is most similar to the Treatment group). In our sample, 4 studies used PSM, in two of these the estimated post-merger price decreased,[41] although the change was not expressed in relative terms. In 1 case the merger was estimated to have no effect on price[42] and in another case a small price increase (1.2%) was estimated.[43]
- **Iterative technique**: Whereas PSM is used to find similar Control groups, one of the studies in our sample, Chone et al (2012) proposes a method for identifying a suitable *Control group that is not affected by a spill-over effect* of the merger. The authors show that even local markets that are only indirectly affected by the merger still experience some spill-over effect (particularly in neighbouring markets). To compose the Control therefore they use a method, which combines data on the physical distance from either of the merging firms and 'the order of exposure'. They show that including indirectly affected firms in the Control means that the estimated price effect of the merger between two Parisian parking companies was zero. After these firms were removed from the Control, the estimated price increase caused by the merger was 3 percent.
- **Synthetic control**: Although we do not have an example of this in our sample, another way to compose an unbiased Control group is suggested by Abadie et al (2010). This is based on the idea that a combination of potential Controls is a more efficient counterfactual than a single Control. This is particularly useful when the number of potential Controls is small or when it is difficult to find a single Control that is unaffected by the treatment. Instead of using a single

[39] These could potentially be different national markets, so long as the markets are sufficiently similar absent the Control.

[40] *Dutch hospital mergers.*

[41] *Easy Jet/Go Fly* and *Ryanair/Buzz*.

[42] *Waterstones/Ottakar.*

[43] *Shell/Rontec.*

Control, the synthetic control method proposes a weighted combination of possible Controls. The weights are derived by using the observable characteristics of the Treatment and the Controls, and determining which combination of Controls gives the closest match to the Treatment when all characteristics are considered.

Two additional caveats have to be made about the use of local markets as control. Firstly, local markets are often not available (when prices are set at national level and local variation in the price data cannot be explored), so this method cannot offer a general solution. Second, as Chone et al. (2012) show, even local markets that are not directly affected by the merger might still fall under the merger's umbrella effect, therefore their choice must always be based on careful consideration.

Summary remark 5: *In Difference-in-Differences studies the Control group is typically composed of rival firms or local markets not affected by the merger. The selection of the Control group should follow a formalised procedure, ensuring that the Control is sufficiently similar to the Treatment (the merger market), and that there are no spill-over effects. The possibility of spill-over effects (the merger affecting competitors' prices) is more likely to be an issue where rivals are used as Control.*

3.1.3 The dataset

3.1.3.1 Sources of data

Data on the dependent variable (price)

Various sources were used to acquire price data. In the majority of studies in our sample it came from proprietary sources (from Nielsen in *Waterstones*/*Ottokars*, from TNS Worldpanel in *Carrefour*/*Promodes*, from Apoteket AB in *GSK*/*Astra Zeneca*, from Systembolaget in *Calrsberg*/*Pripps*, from Experian in *Shell*/*Rontec*, and from City of Paris in *Vinci*/*GTM*). Other sources included data from the industry regulator (Dutch Health Authority in the *Dutch hospital mergers*); data collected by the CA during the investigation (*Game*/*Gamestation*); date collected by the researcher from various websites (using a web-crawler in *Easy Jet*/*Go Fly* and *Ryanair*/*Buzz*), a range of retail websites (*Game*/*Gamestation*), and from price comparison websites (*Agip*/*Esso*, *Lukoil*/*Jet*).

Data on independent variables

Besides the ones noted above, other sources were also used for various independent variables. This included data collected by the researcher (from Dutch newspaper Algemeen Dagblad in the *Dutch hospital mergers*, from IGN Entertainment UK in *Game*/*Gamestation*,); from proprietary data (from Panorama Tradedimensions in *Carrefour*/*Promodes*, from OPEC in *DISA*/*Shell*, from MOL in *Agip*/*Esso* and *Lukoil*/*Jet*, from Bloomberg Finance LP in *Shell*/*Rontec*); and from census data (in *Carrefour*/*Promodes*, from Instituto Canario de Estadística in *DISA*/*Shell*).

3.1.3.2 The time-range of data

As discussed in Section 2.4 the time-range of data used for DiD is also important when assessing how well these studies evaluate the merger decision. It is possible that the immediate price-shock, caused by the merger, is self-corrected by the market within few years following the merger. It is also possible that in the immediate aftermath of the merger prices increase only in markets directly affected by the merger, but then spreads on to other markets later on. Both of these possibilities have to be accounted for when designing the ex-post study.

In choosing the appropriate time-span of the data one should take into account what the CA's decision was. If the CA unconditionally approves a merger it may be that it expected market forces to self-correct for the increase in market concentration. As these mechanisms may take longer than a year to get in full swing, it is advisable in these cases

to look at price data that spans longer than the first year following the merger. A dataset that does not span longer than a year after the merger may only pick up short-term price increases (and would imply that the CA's decision was wrong). On the other hand, longer spanning data would be more able to verify if market self-correction did take place. If data is available, it is good practice to report how prices evolve post-merger by reporting estimates for each post-merger year (similar to the *Dutch hospital merger* study), in order to get a fuller picture of the dynamics of post-merger price-changes.

Summary remark 6: *In Difference-in-Differences studies, if available, the data should span over longer than a year following the merger in order to allow any market self-correction to take place. In our sample we found that a third of the studies looked at price effects within a year after the merger.*

3.1.4 What variables to control for

Invariably for our sample – but also for most other merger retrospectives – the dependent variable is the price of the relevant product. Therefore it is inevitable in the model to try to control for as many factors as possible that may have affected prices.

Cost variables: The obvious candidate for this purpose is information on the cost of the relevant product. A typical approach is to include information of the price of one or more input used in the production of the relevant product. If the merger happens in the retail market then the model could include wholesale prices as a control for costs. Because mergers often happen to reduce costs, it would be important to control for costs in order to avoid endogeneity bias in the DiD estimates. Yet, only five studies in our sample controlled for cost factors.[44]

Firm characteristics: This could be the merging firms, or firms in the Control group. Sometimes it may be relatively simple to find information on the characteristics of these firms (firm size, market share, etc.). One way to control for some (the time-invariant part) of the unobserved heterogeneity is by including a simple set of fixed effect variables (dummy variables for each individual). Almost all of the studies in our sample controlled for some type of firm characteristics

Product characteristics: In the case of differentiated products it is advisable to control for product characteristics. Fixed effect dummies can also be included to control for some of the unobserved product heterogeneity. In our sample, the *Dutch hospital mergers*, or the *Waterstones/Ottokars* and *Game/Gamestation* mergers are good examples of controlling for a wide range of product characteristics.

Market characteristics: This is especially useful if the data exploits cross-market price variation to estimate the effect of the merger. Again, some market characteristics are relatively easy to observe (market structure, size of the market – population, etc.). Fixed effect dummies can also be included to control for some of the unobserved market heterogeneity.[45]

3.1.5 Robustness checks

We have now covered the most important components of a DiD merger retrospective study. As the above discussion shows, the researcher often has a large amount of discretion when designing the study. Decisions have to be made on the choice of Control, the time period used in the estimates, and the exact model specification. To demonstrate that the findings of the study are robust to these choices, it is prudent practice to run some 'robustness checks', and re-estimate the merger effect using a different Control, a different span of data, or a different model specification. In our sample we found three categories of robustness checks. (Note, where we refer to our sample we only mention

[44] *Waterstones/Ottokars*, *DISA/Shell*, *Agip/Esso*, *Lukoil/Jet*, *Shell/Rontec.*

[45] In our sample the *Waterstones/Ottokars*, *DISA/Shell*, *Agip/Esso*, *Lukoil/Jet*, *Easy Jet/Go Fly,* and *Ryanair/Buzz* mergers control for various market characteristics.

robustness checks to the extent they were reported in the study. The researchers could have performed other checks and not report them.)

- *Placebo treatment*: In this case the model is re-estimated using a different (false) date of the merger. Placebo treatments are typically used in DiD models in order to show that the effect estimated for the merger is not spurious. The interpretation is simple, if the placebo treatment returns equally significant (and similar in magnitude) estimates for the effect of treatment, then one should be suspicious that the initial estimate was biased by issues such as autocorrelation. In our sample five studies used placebo treatments.[46] Interestingly in the *Carrefour/Promodes* study the placebo treatment was also found significant and the authors interpreted it as a pre-adjustment price of the market that anticipated the forthcoming merger.
- *Changing the Control groups*: Another way to test the robustness of results is by changing the Control to an alternative – often equally suitable – counterfactual. If the results remain in the same ballpark as the original estimate even with different controls, it would be interpreted as confirmation of the robustness of the original estimate. In our sample three of the studies used alternative Controls as robustness check.[47]
- *Changing model specifications*: The goal of this exercise is to demonstrate that the results are not sensitive to the exact choice of model specification. In practice this most often means running the estimation with a different set of dependent variables. Most studies in our sample performed such checks.

[46] *Carrefour/Promodes*, *Agip/Esso*, *Lukoil/Jet*, *Shell/Rontec*, and *Carlsberg/Pripps.*

[47] *Shell/Rontec*, *Waterstones/Ottokars*, *Vinci/GTM.*

3.2 Merger Simulation (MS)

While DiD is a relatively simple method to conduct a retrospective merger analysis, we will show below that the MS method, despite being much more involved, has benefits over DiD in some circumstances. For example, it is much more suitable for estimating certain counterfactuals, such as alternative merger remedies or remedy divestures when remedies were imposed, or divestures in general when mergers are approved. In a way, it is much more suitable than DiD to identify a Type I error by the CA. Also, it is the only method able to provide welfare calculations.

Merger simulations have originally been employed as ex-ante tools, i.e. as screening devices in merger control to predict the unilateral effect of the merger, but have recently been more and more frequently deployed for retrospective merger analysis. For the purposes of this report we focus on simulations that are done ex-post. We distinguish between MSs that are executed ex-post, but using ex-ante data, and those that are implemented ex-post and utilising ex-post data.

3.2.1 Three steps of Merger Simulations

To commence we provide a brief summary of the main components of MS studies. The procedure consists of three steps:

1. In the first step a *system of demand equations is estimated* either simultaneously with a system of pricing equations or, under certain assumptions, on its own, in which case parameters of the pricing equation are derived from the estimates of the demand equations. Two alternative approaches are available in the literature. The first one is the continuous choice approach, in which consumers decide how much of a good to consume. The second one is the discrete choice approach, where individual consumers choose whether or not to buy a product. In this report we discuss the discrete choice method, as this one is the most popular. Demand coefficients are estimated using one of the discrete parametric specifications: logit, nested logit, random coefficient logit, random coefficient nested logit models and distance metrics. The technical appendix provides a brief overview of the most widely used alternative econometric discrete choice models and discusses pros and cons of the competing methodologies within the discrete choice framework. Under certain parametric specifications, demand can be estimated with a linear estimator that deals with the endogeneity related issues.[48]
2. In the second step, *the marginal cost is retrieved* by assuming some model of oligopoly competition (typically Bertrand differentiated products) and by filling in an ownership matrix, which is a matrix of zeros and ones, depending on the portfolio of products the firms have control on. For example in a simple market with three firms, each owning a single product, the ownership matrix will be a 3x3 identity matrix (with ones along the diagonal and zeros off-diagonal). This second step of the procedure can be pushed as far as to estimate the market conduct parameter, if marginal costs were to be observed.
3. In the third step, holding the marginal cost (observed or backed-out) and consumer preferences (the estimated demand coefficients) fixed, a merger can be evaluated. The merger is simulated via an adjustment of the ownership matrix. The new ownership matrix accounts for the change in ownership after the merger. The ownership matrix does not necessarily have to be full, the *GSK/Astra Zeneca* merger is an example that allows for partial ownership. Then, using the updated ownership matrix and maintaining the marginal cost and demand parameters fixed, new equilibrium prices can be computed. The new equilibrium prices can be compared with the old ones and

[48] The price endogeneity is handled with instrumental variables constructed along the recommendations proposed by Berry et al. (1995), Hausman (1996), and the very recent insight provided by Berry and Haile (2014). The endogeneity of the within market share is tackled with counts and sums of product characteristics by subgroups and groups (see Verboven 1996).

the difference is the effect of the merger on prices. A good rule of thumb when estimating the price-change of a merger is to include a discussion of efficiency, and possibly provide the price change in presence of cost efficiency. If potential efficiency gains are not taken into consideration prices are never expected to show a price drop for an un-remedied merger (see below).[49] Finally, the simulation can also include potential entry and exit. It is always difficult to anticipate entry and exit, particularly when dealing with a static model, as is a typical merger simulation model. For example, the Swedish competition authority got a wrong decision on the merger between *GSK* and *AstraZeneca* because it was expecting entry in the market as a result of the deregulation of the pharmacy monopoly; such entry did not occur. When the simulation is done ex-post, it has the advantage that entry/exit (if there was one) can be observed.

The valuation of the merger can be done at the market-level or at the sub-market level. Similarly it can be computed for a whole country or for more countries (as in *Volvo*/*Scania*), or limited to a specific area. Following the same line of reasoning, the counterfactual can be restricted to specific segments of the market, to certain areas, countries, etc. This flexibility of the methodology is particularly convenient to study remedies, but can also be exploited to evaluate certain peculiar aspects of mergers.

3.2.2 Types of merger simulations

Regarding their use in the evaluation of merger decisions, we distinguish between the following ways merger simulations can be used:

- *Ex-post MS using ex-ante data*: MS methods – using ex ante data – can be applied to inform the CA during the investigation. However, given the complexity of the estimation process, and of the data requirement (typically product-level data with information on prices, unit sales and product characteristics, and when possible, the cost side), it is more likely that these full-fledged merger simulations are conducted post-merger, unconstrained by the CA's strict procedural deadlines. Although one might argue that strictly speaking these are not ex-post evaluations, we believe that they should play an important role in evaluating the CA's merger decisions. During the merger investigation, a full-fledged merger simulation is probably the best approximation of what the post-merger world would be like. Doing this exercise ex-post (using ex ante data) can tell us how close the CA's decision was to this best approximation.[50]

- *Ex-post MS using ex-post data*: This type of MS is useful because it relies on an estimation of the post-merger equilibrium, which might deliver different demand and supply parameters and can also plug-in realistic efficiency information. It is also helpful as it allows us to evaluate divestures and account for adjustments to market structure (via observed entry and exit). The data needed to implement ex-post MS have to be those that satisfy the three procedural steps listed above. For the first step it is post-merger price, sales, and product characteristics. For the second step this would be post-merger cost data and efficiency developments. Finally, for the third step, it would include information on the ownership structure of the market that reflects how market structure evolved post-merger through entry and exit. In this

[49] Merger decision-making often implicitly assumes some level of merger efficiencies. This is why the question asked is not whether the model predicts a price rise but whether that price rise really represents a 'significant impediment to effective competition'. In practice, authorities will usually allow a merger if the estimated price rise is less than 5% (or sometimes the cut off is a little less for very low margin or large markets).

[50] In our sample we have a mixture of ex-post MS studies that use ex-ante information (*Volvo*/*Scania*, *Bass*/*Carlsberg Tetley*, *Rio Tinto*/*North*, and *CVRD*/*Caemi Lundmark*), and ex-post data (*Cerealia*/*Schulstad*, *Scottish*/*Courage*, and *Morrisons*/*Safeway*) or a combination of the two (*GSK*/*Astra Zeneca*).

case of course one would have to simulate the pre-merger world as the counterfactual.

- *Ex-post MS that combines ex-ante and ex-post data:* These studies would be probably the most powerful because in addition to the merits described for the ex-ante and ex-post data on their own, combining the data allows the researcher to produce important in-sample predictions. For example, the researcher can test one important assumption of the MS method: that the demand coefficients (consumer preferences) are time invariant and hence invariant after the merger (by estimating and comparing demand coefficients with ex-ante data and then with ex-post data). This type of statistical exercise was not done in any of our sample studies, but it is perhaps a suggestion for future research.

MS methods have another important characteristic: unless accounting for efficiency gains they always estimate a post-merger price increase – simply because the simulation compares two scenarios, one with n number of firms (pre-merger), and the other one with n–1 firms (post-merger). For this reason it is vital to incorporate efficiency gains in the model and, possibly, entry of new products or firms.

Summary remark 7: *Merger simulations can be done using ex-ante or ex-post data. The former can inform us how far the competition authority's decision fell from the best possible prediction (a full-fledged merger simulation). The latter can tell us how the merger affected the market, whilst accounting for various post-merger market developments. The data used in this exercise should include: (1) post-merger price, market share and product characteristics to estimate own-, and cross-price elasticities in the post-merger equilibrium; (2) marginal costs and information on efficiency gains; and (3) how market structure evolved post-merger through entry and exit. If both ex-ante and ex-post data are available and have similar length, then the research can test if demand characteristics change with the merger.*

3.2.3 Assumptions used in merger simulations

MS methods rely on a variety of assumptions: about the structure of demand, the identification of relevant parameters (via valid and strong instruments), the nature of competition in the market, the absence of entry and exit, and – if cost data is unavailable – the likely change in marginal cost induced by merger-related efficiency gains. Variations of these assumptions can lead to different estimates of post-merger price change.

Below we list some of the key assumptions, as well as the weaknesses of MS. If these assumptions were not to hold, then the estimated post-merger price would be biased.

- *Identification of price:* One of the key variables of interest in a structural model is price. However, prices are variables that reflect the choice of firms, which means that they are likely to be driven by components of demand that are not observed to the researcher. This means that in structural models prices are endogenous. To deal with this endogeneity, researchers use instruments (i.e. they find variables that are correlated with price but not with other factors in the model). Such an instrument could be prices of the same product in different markets (assuming that they are driven by the same cost factors as the prices of interest).[51] The preferred choice of instruments depends on the variability of the characteristics and prices across markets. Good practice is to include an over-identification test to verify the validity of the instruments.[52] In our sample of papers the choice of instruments, is provided in

[51] Hausman (1996). Other instruments for the price endogeneity are offered by the sum (or average) of product characteristics of other products produced by the same firm, if multiproduct, and by competing firms (Berry et al.'s 1995 instruments).

[52] The over-identification test is statistical test that is used to test for over-identifying restrictions on the coefficients. It is often known as Sargan test or Hansen test or J-test. Under the null hypothesis that the over-

Volvo/*Scania*, *Scottish & Newcastle*/*Courage*, *Bass*/*Carlsberg Tetley*, *GSK*/*Astra Zeneca*, and *Carlsberg*/*Pripps*. However, a test on the validity of the instruments (and possibly strength) is only supplied in *Carlsberg*/*Pripps*. When instruments are not valid the estimated coefficients are biased and that has implications on the calculation of the predicted price, possibly leading to an error in the competition authority's decision.[53]

- *Identification of product characteristics*. Many papers assume that product characteristics are exogenous. One problem with this assumption, is that if it does not hold, the coefficients associated with the product characteristics will be biased. One additional issue is that endogenous product characteristics, when used in the instrumentation of prices and market shares, will lead to biased estimated coefficients with implications on the final results.[54] In our sample of retrospective studies either there is no mentioning of the possible endogeneity of product characteristics or when there is a discussion they are assumed exogenous, as in *GSK*/*Astra Zeneca*, and *Carlsberg*/*Pripps*.

- *Assumption on the type of competition*: Nash-Bertrand competition is explicitly assumed in *Rio Tinto*/*North*, *CVRD*/*Caemi Lundmark*, *Carlsberg*/*Pripps* and *GSK*/*Astra Zeneca* and is not explicitly mentioned in most other studies in our sample. The only exceptions are *Scottish & Newcastle*/*Courage*, *Bass*/*Carlsberg Tetley*, which point out the weaknesses of studies that do not evaluate the effect of merger under alternative assumptions on modelling competition in the market.[55] It appears prudent practice to demonstrate how different assumptions on the mode of competition affect the final results.

- *Assumptions on marginal costs*: Two studies (*Scottish & Newcastle*/*Courage*, *Bass*/*Carlsberg Tetley*) point out the importance of providing results under alternative scenarios of marginal cost (for example because of the expected levels of merger efficiencies). Acknowledgment of cost efficiency is provided in *GSK*/*Astra Zeneca*, *Cerealia*/*Schulstad* (though not implemented in the latter case) and *Volvo*/*Scania*.

- *Correct functional form*: An additional assumption is that the chosen functional form is the correct one. As pointed out in Nevo and Whinston (2010), the functional form should not be the driver of the results. Identification is the priority. The researcher needs to spend time ensuring that the model is identified and the instruments are valid for the case to be studied.

- *Demand parameters do not change with the merger*: Counterfactuals rely on the assumption that consumer behaviour does not vary after the merger. There are many reasons why this may not be the case. For example, a merger might create a new brand, and demand for this brand might be completely different from the pre-merger demand for the individual brands of the merging firms.

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identifying restrictions are valid the statistic of interest is asymptotically distributed as a Chi-square variable with degrees of freedom given by the difference between the number of instruments and the number of endogenous variables.

[53] Another potentially endogenous variable is within-segment market shares. Instruments for within segment market shares are typically the sum or average of product characteristics within the segment and/or the sum of products in the segment.

[54] Ackerberg, Crawford and Hahn (2011) have shown that by using proper instruments it is possible to estimate unbiased demand coefficients in the presence of endogenous product characteristics.

[55] Running the estimations under alternative models of competition was one of the contributions to the economic literature provided by Nevo (2001) and Bresnahan (1987). An alternative approach is undertaken in Brito et al. (2013), where the demand is estimated using a continuous demand method, which we do not cover in this report.

- *Entry and exit are hard to be anticipated and modelled*. They should be incorporated in the counterfactual, as to provide information on additional scenarios. This is a problem for MS using ex-ante data. When using ex-post data information on entry/exit can be plugged into the simulations.
- *Structural models rely on strong assumptions of rationality*. If consumers or firms are not fully rational, this can lead to biased estimated coefficients, of the type discussed above.

3.2.4 Data used in MS

Typically DiD is more conservative on data requirement. Often panel data with information on prices, quantities and local market characteristics - when the analysis is run at the local market level – is sufficient, but it is becoming an increasing practice to include scanner data or similar data with product characteristics, and to use the additional information as controls in the empirical analysis aimed at identifying the control group. The data used to run a DiD estimation are more and more those that one would need for a MS analysis.[56] The data employed for MS is also becoming richer. For example, the *Morrisons*/*Safeway* study combines detailed individual survey data on individual consumption with more classical product-level data. Such data requirement is of course typically linked to rather time consuming analyses.

The dependent variable for demand estimation

In this subsection we describe the construction of the dependent variable to estimate the demand equation in our MS sample of studies. The dependent variable is typically derived from the market shares of relevant products and the consumption choice outside of the relevant market.

The *Rio Tinto*/*North*, *CVRD*/*Caemi Lundmark* studies measure market share in terms of production of each mine in the iron ore industry. The methodology that the authors adopt, proportionally-calibrated almost ideal demand system does not require an outside option. *Morrisons*/*Safeway* defines the demand as units of groceries by household type and region, and the outside option as products purchased on trips to non-grocery shops. Whereas *Cerealia*/*Schulstad* compute the market share as the amount of fresh bread sold by one of the selected brands, they do not discuss how the outside option is constructed. Mergers in the beer market are analysed in *Carlsberg*/*Pripps*, *Scottish & Newcastle*/*Courage*, and *Bass*/*Carlsberg Tetley*. *Carlsberg*/*Pripps* calculate the market share based on litres of beers sold by one of the top 8 firms in the market and the outside option is defined as wine, alcohol consumed in restaurants and bars and low-alcohol beer sold in supermarkets. *Scottish & Newcastle*/*Courage*, and *Bass*/*Carlsberg Tetley* compute the market share from the beer sold by a brand in a type of establishment and region in the UK and derives the outside good from other alcoholic beverages. By studying a merger in the pharmaceutical industry (*GSK*/*Astra Zeneca*) the authors define a product to be a brand-form-package-dose (defined in three ways) and given the product definition compute the market shares using values rather than volumes. They define the outside option to be 50 per cent of the market in the unit demand model and have the same value of the inside market in the constant expenditure model. In *Volvo*/*Scania* the market share is measured by sales of heavy trucks and the outside good is captured by medium-duty trucks, second hand trucks, and other modes of transportation.

3.2.5 Robustness checks and sensitivity analysis in MS models

Robustness checks and sensitivity analysis were discussed in some of our sample studies. *Scottish & Newcastle*/*Courage*, and *Bass*/*Carlsberg Tetley* provide sensitivity analysis by inferring marginal costs under different equilibrium behaviour: perfect competition,

[56] With the exception of distributions of individual characteristics for random coefficients logit models (rarely used though).

Bertrand differentiated products and joint profit maximization. As a robustness check, *Carlsberg*/*Pripps* complement their simulations with a parametric bootstrap. *GSK*/*Astra Zeneca* perform a sensitivity analysis estimating the model under three different measures of dose and by setting alternative factors for the outside option. Alternative factors for the outside option are also the core of the sensitivity analysis undertaken in *Volvo*/*Scania*. Finally, *Rio Tinto*/*North*, and *CVRD*/*Caemi Lundmark* provide sensitivity analysis based on different levels of elasticities.

3.3 Choosing between DiD and MS

There is still an ongoing debate in the empirical industrial economics literature on the relative merits of DiD and MS in the context of policy evaluation. Angrist and Pischke (2010) have openly provoked and criticized the use of structural econometrics (Merger Simulations) to evaluate mergers and have suggested DiD as preferable methodology, at least for retrospective analysis of past merger decisions. Their criticism is against the complexity of the MS method to evaluate the effect of mergers on prices. The authors feel that the structure imposed by the econometric modelling leads to restrictions on substitution patterns. Moreover, they argue that the validity of the instruments is often questionable and warn about the restrictive way in which simulations are carried out, typically by reducing the number of competitors, which the authors claim to be only one of the various means by which mergers can affect pricing.

Liran and Levin (2010) and Nevo and Whinston (2010) presented counter-arguments for the MS methodology, while acknowledging that there may be market conditions where the chosen instruments for dealing with the price-endogeneity may be invalid.

The above discussion listed the main assumptions required for DiD and MS methods. In general, because of its simplicity and overall good performance (in terms of unbiased estimates), DiD is often preferred over MS on post-merger evaluations. However, there are three situations where MS shows its strengths against DiD.

1. The DiD method always compares the observed state of the world (factual) with a counterfactual world where the merger did not happen. Because of this, it is more limited in its ability to estimate if there was an unnecessary intervention by the CA (Type I error) as it would require estimating a world with the merger but without the intervention. This may be possible in some cases, for example, in the UK mergers do not require pre-notification and thus can temporarily exist before a remedy is imposed. This temporary period could be used as a control for the merger without remedy. Nevertheless, this is a special case that does not apply to the large majority of mergers. Therefore in these cases the MS method might be preferred for detecting Type I errors, because it allows comparisons across different counterfactuals (no merger, unconditional merger, remedied merger, alternative remedy). Section 4 discusses this in more detail.
2. Another comparative weakness of DiD methods is the inability of the causal approach to evaluate change in welfare, which is often important for merger decisions. For this reason, MS is preferred if a welfare analysis is required.
3. Finally DiD may suffer of the contagious/umbrella effect. The Control group responds to the merger, making it hard to identify the causal effect of the merger on the outcome. Merger simulations use hypothetical equilibria of the Treatment and the Control group (e.g. merger against the no-merger world, or a remedy against an alternative remedy world). Because of this, the Control is never affected by the Treatment. For this reason – if done correctly – merger simulations are likely to perform better if the Control group in DiD suffers from spill-over effects.

3.3.1 Estimating with both DiD and MS

There are studies that use both methodologies for *ex-post* analysis. This is an ideal situation because the efficacy of the two methods can be compared. There are two mergers

(*GSK*/*AstraZeneca* and *Carlsberg*/*Pripps*) in our sample which were analysed by both methods. Both studies use competitors' prices as a Control for difference-in-differences. The *GSK*/*AstraZeneca* study uses a nested logit demand model for their simulation and the *Carlsberg*/*Pripps* study uses a random-coefficients logit demand model.

The *GSK*/*AstraZeneca* study found very large price effects with both methodologies, estimating a 42% increase with DiD and a 34% increase with their simulation. The authors take the first figure as authoritative and question why their simulation model underestimated the true price increase. They find that both merged firms raised prices by a similar proportion, whereas their simulation assumed that each merged firm would independently set prices on their products. They believe that unpredicted changes in market conditions were responsible for the rest of the difference in price change since many outsiders lost market share after the merger.

In the *Carlsberg*/*Pripps* study the authors set themselves the aim of assessing the success of the divestments required as a remedy to the merger. Their simulation of the remedied merger estimates a price increase of 4.2% for the merged parties. The DiD analysis finds that the remedied merger caused a fall in prices of 2%. They conclude that their simulation and the real price effects of the remedies are very dependent on the purchaser of the divested products. The fall in prices observed was largely caused by the fact that the divested products become a strong competitor to Pripps.

The general question is of course what to do in general if the two estimates are so different. As a first step it should always be the authors of the study to discuss possible causes of the different estimates by examining possible sources of bias in the execution of both methods. If no sources of bias can be established then one way to proceed would be to use the direct price estimates of the DiD to establish the price effect of the merger and the simulation as the source of ordering the counterfactuals. For example in the *Carlsberg*/*Pripps* case the DiD estimated a 2 per cent price drop as a result of the remedy. The merger simulation estimated that the imposed remedy produced the smallest possible price (i.e. the alternative remedy or no remedy would not have been so efficient). Therefore from this evidence the CA made no error in its judgement and the only way of maintaining the pre-merger price level would have been through blocking the merger – which would have seemed far too interventionist for a 2 per cent price rise.

Finally, a remark on the relative power of the two methods: the DiD estimate is more likely to suffer from spill-over effects (which would produce estimates that are lower than the real merger effect). This is not true for Merger Simulations, where each counterfactual scenario is 'artificially created' and as such are free of spill-over effects. For this reason it may appear tempting to interpret the DiD estimate as the lower bound and the MS estimate as the upper bound merger effect. We would caution against such simplification, mainly because both DiD and MS estimates are susceptible to other biases. For example the Control market may not be similar enough to the Treatment market and as a result the effect of a merger might have been over-estimated. Similarly, in MS it is possible that an incorrect choice of the model of competition in the market biased the results, which under/overestimate the effect of the merger.

Summary remark 8: *If possible, the ex-post assessment of merger decisions should use both Difference-in-Differences and Merger Simulation to learn from the comparison between the two methods.*

Table 9 documents a statistical comparison between DiD and MS methodologies for some key variables in our sample. The main message is that – apart from the year of the decision – the two procedures do not display a statistical difference in other relevant variables. We highlight that the percentage price change estimated by the two methods is not statistically different, thus suggesting that DiD and MS have a similar average performance. However the result has to be taken with a grain of salt given the small sample.

Table 9: Sub-sample means for mergers estimated by DiD v MS

		DiD		MS	
		N	mean	N	Mean
Price change		16	2.33	7	6.94
Decision year		18	2005***	9	2001***
Number of pre-merger firms		16	7.22	9	7.56
HHI	Pre-merger	8	1720	6	1935
	Post-merger	8	2230	6	2502
Market share	Firm 1	12	0.17	8	0.23
	Firm 2	12	0.08*	8	0.13*

*,**,*** implies t-test significance in the difference between means at 90%, 95% and 99% respectively.

4 How to evaluate ex-post assessments

This section starts from the premise that the ex-post assessment of the impact of a merger decision has been conducted, unbiased estimates have been provided (or at least the research fully identifies and acknowledges the bias) and the results are waiting to be interpreted. A surprisingly large number of retrospective merger studies circumvent the interpretation of price-change estimates in light of the CA's decision. We argue that this exercise should ideally be conducted as part of the ex-post merger study, which is also best placed to examine whether the CA made an error and what might have been the source of the error.

This section has three main parts. First, we set out the criteria for which we define a potential error. Then we combine the findings of the merger studies with the CA's decision to find the cases where such an error can be identified. Through this exercise we provide a framework for evaluating the CA's decision with regards to the result of an ex post study. Finally, we examine the circumstances in which a potential error cannot be attributed as an error by the CA.

It is important to make some clarification regarding our terminology in this section. We look at one component of welfare, price, and identify cases where the CA's decision was unable to eliminate a price increase (or to avoid an unnecessary intervention). As a shorthand terminology, for the purposes of this report, we refer to these cases as 'potential errors'. We use the word 'potential' to acknowledge that there are numerous situations where the CA made no error and yet the merger decision was followed by a price increase. Analysing these situations is an important part of this report, which is further discussed in Section 4.2. Against this backbone we first discuss the cases where there was a potential error (Section 4.1) and then examine whether this was indeed an error by the CA or caused by other factors (Section 4.2).

4.1 Does the ex-post assessment reveal a potential error in the CAs decision?

The rationale for this exercise is simple: an ex-post DiD or MS estimate that shows prices increased post-merger does not necessarily mean that the CA had made an error. To be able to assess whether there was an error one has interpret the findings of the study in light of the CAs decision.

In establishing the type of the potential error we rely on the traditional terminology of statistical hypothesis-testing and its dichotomy of errors: Type I and Type II. In technical terms Type I errors refer to an incorrect rejection of the null hypothesis and Type II errors refer to a failure to reject the null. This terminology is often used in merger analysis. Merger control is an ex-ante analysis, therefore every decision carries some level of uncertainty and possibility of error. In the context of mergers, insufficient intervention into a problematic merger indicates a Type II error. Wrongly intervening in a non-problematic merger signifies a Type I error.

To identify the potential errors it helps if we break down the CAs merger decision into two parts:

(1) Decision on whether the merger raises competition concerns: the authority can have two possible decisions: (a) to unconditionally approve, (b) to intervene (remedy or block). For example, if the merger turns out (ex-post) to have increased prices then a decision to intervene would have been the right option.
(2) Decision on the extent of intervention. If intervention happens it can be either through remedies or the blocking of the merger. For example, if a merger is blocked, although it could have been remedied, then we have a Type I error. If, however a merger is remedied when it should have been blocked, we have a Type II error.

Using the retrospective study together with the CA's decision can inform us of the potential errors in the merger decision. Because of the difference in the way the factual and the counterfactual are interpreted, the following discussion addresses DiD and MS separately.

4.1.1 Identifying a potential error using DiD methods

Table 14 summarises how the findings of a retrospective merger assessment, using DiD method, could be used together with the decision of the CA to assess the merger decision. The first column of the table describes the CA's decision. This can be thought of as the factual world (what actually happened). The second column presents the findings of the DiD study, which is the price-change that was estimated for the factual scenario. In DiD the counterfactual is always the no-merger world, therefore the findings reported in Column 2 are the estimated difference between the factual price and the no-merger price.

Combining information from Columns 1 (Decision) and 2 (Finding), Column 3 (Assessment of the decision to intervene) identifies whether – focusing purely on price effects – there was a potential error in the CA's decision. Column 4 (Assessment of the intervention) presents the (potential) errors made in the scope of the intervention. Columns 5 and 6 contain the number and the names of cases for each category. Below, we provide a more detailed explanation by each type of CA decision.

The first set of rows contains cases where the CA's decision was to ***unconditionally approve*** a merger. Put differently, the DiD estimate shows the price difference between the world with the unconditionally approved merger and the no-merger world. In this case a price increase post-merger would indicate a Type II error – the CA should have intervened but it did not. If prices do not increase then the CA made no error. In these cases – as there was no intervention by the CA – the decision on the scope of intervention cannot be interpreted. Table 14 shows that in the case of unconditional approval, DiD estimates can fully identify the type of the potential error. Note also that by definition, the CA cannot make a Type I error (making an unnecessary intervention) in unconditionally approved mergers.

The next set of rows displays cases where the CA ***conditionally approved (remedied)*** the merger. Here, the DID estimate is the difference in the price under the remedied merger and the price in the no-merger world. This reveals one relative weakness of the DiD method: it typically does not offer a comparison of the remedied and the un-remedied merger worlds (there might be exceptions but only in very specific cases, as explained in Section 3.3). If a counterfactual of a merger without remedy does not exist, the only situation where the retrospective study and the CA's decision can fully identify the potential error is where prices increase post-merger. In this case the CA was right in intervening – as the prices have increased even after a remedy – but the price increase implies that the scope of the intervention was deficient (Type II error).

A price drop or no price change after a remedied merger would imply that there was no Type II error (the intervention was not deficient). However, without knowing what would have happened under an un-remedied merger it is impossible to decide if there was a Type I error. It is possible that the merger would have led to a price drop even without the remedy – in which case it was an error to intervene (Type I error). It is also possible that the un-remedied merger would have pushed up prices and the remedy eliminated this threat (No error). In these cases we cannot identify whether the CA made an error in the design of the remedy.

The final set of rows indicates ***blocked*** mergers. As blocking a merger is the strongest possible intervention, we can, by definition, exclude Type II errors in these decisions (a Type II error refers to insufficient intervention). However blocking a merger may have caused a Type I error if the anti-competitive effect could have been eliminated by a remedy or if there would not have been any anticompetitive effects. In these cases, without a Control group (we do not observe the world where the merger was allowed), DiD methods have more limited use.

Table 14 reveals that the DiD method can fully identify the potential errors in unconditionally approved mergers. As pointed out earlier, all but 4 of the DiD studies looked at unconditional approvals. This is compatible with our argument above, that it is harder to find a counterfactual of the merger without the remedy and thus fully identify errors made in remedied mergers. Assuming all DiD retrospective studies in our sample produce unbiased estimates, we identified a potential decision error in 6 out of the 14 unconditionally approved cases – the CA unconditionally approved a merger that should have been intervened. In the remaining 8 cases the DiD study did not find an error. We could not establish using DiD methods if there was a Type I error in the analysed sample. Again, we have to remind the reader that 'error' is our shorthand terminology for discrepancy between the CA's decision and the findings of the merger retrospective. Whether this implies an error on the CA's part is discussed in Section 4.2.

Table 14: Identifying potential errors using DiD

Decision	Finding	Assessment of the decision to intervene	Assessment of the intervention	Number of cases	Name of the case
Approve	Price increase	Type II error	–	6	Ziekenhuis Hilversum + Gooi-Noord (2005, Netherlands) Medisch Centrum Alkmaar + Gemini Ziekenhuis (2007, NL) St. Lucas Ziekenhuis + Delfzichtziekenhuis (2008, NL) Agip-Esso (2007, EU) Lukoil-Jet (2007, EU) GSK-Astra (2009, Sweden)
	Price drop	No error	–	3	Game + Gamestation (2008, UK) Easyjet + Go Fly (2002, EU) Ryanair + Buzz (2003, EU)
	No price change	No error	–	5	Erasmus MC ziekenhuis + Havenziekenhuis Rotterdam (2005 NL) Ziekenhuis Walcheren + Oosterscheldeziekenhuizen (2005, NL) Ziekenhuis Bethesda + Scheperziekenhuis (2009, NL) Waterstones + Ottokars (2006, UK) DISA + Shell (2004, Spain)
Remedy	Price increase	No error	Type II error	2	Vinci + GTM (2001) Shell + Rontec (2012)
	Price drop	?	?	1	Carlsberg + Pripps (2001, Sweden)
	No price change	?	?	1	Carrefour + Promodes (2000, France)
Block	Price increase	?	?		
	Price drop	?	?		
	No price change	?	?		

4.1.2 Identifying a potential error using merger simulations

Table 15 summarises our guidance on how to use the results of a Merger Simulation together with the CA's decision to identify the potential errors. Whilst DiD was mainly limited to the no-merger counterfactual, with merger simulations any counterfactual scenario can be simulated. This allows identification of both types of errors in all cases – provided that the right simulations are conducted.

Most simulations look at four different scenarios and we find that it is sufficient to allow a full assessment of the quality of the merger decision. These four scenarios are:

(1) Merger with no remedies, denote the no remedy price outcome by *M*
(2) Where the merger does not take place, denote the no-merger price outcome by *N*,
(3) Merger with a remedy imposed by the CA (if there was a remedy), denote the post-remedy price outcome by *R*, and
(4) Merger with the best of any other (alternative) remedies, its price outcome denoted by *AR*.

Simulating the prices in these 4 scenarios, ordering them, and identifying which scenario matches the CA's decision can help us identify potential errors. For example if the decision was to remedy a merger, and the simulations show that $N = \boldsymbol{R} < M$ (i.e. the remedied price restores the no merger equilibrium, whereas the unconditionally approved merger would have resulted in higher prices) it would suggest that the CA was right to intervene and the design of the remedy was correct. However, if it turns out that $\boldsymbol{R} > N$ (i.e. the price after the remedy is higher than the price under no merger) then the scope of the intervention was insufficient, implying a Type II error.

Column 1 in Table 15 shows the CAs decision (approve, remedy or block). Column 2 displays the outcome of the simulation by ordering the simulated price effects for each scenario (Column 3 gives a short explanation of these scenarios). We highlighted with **bold** the factual (the scenario that actually happened as a result of the CA's decision). In determining the potential error we can generate the following simple rules of thumb:

- The CA made a Type II error if it unconditionally approved the merger and the merger led to a price increase ($\boldsymbol{M} > N$).
- The CA was always right to intervene if the merger would have led to a price increase ($M > \mathbf{R} \geq N$).
- The CA made a Type II error in defining the scope of the intervention if the remedy is not sufficient to eliminate all price increases ($\boldsymbol{R} > N$).
- A Type I error is identified in any case where there was intervention (remedy or block) although the unconditionally approved merger price would not been higher than the price under no merger ($\boldsymbol{M} \leq N$).
- A Type I error is identified if the merger was blocked and a remedy would have been sufficient ($M > R = \mathbf{N}$).
- The CA was right to block the merger if even the best possible remedy would not have eliminated the price increase ($R, AR > \boldsymbol{N}$).

Although DiD performed equally well when identifying errors in unconditionally approved mergers, for all other scenarios (all intervened mergers) merger simulation is better suited at identifying Type II errors. MS methods are also capable to identify Type I errors, but it requires that merger efficiencies are plugged into the simulations – otherwise simulations will always predict a price rise following the merger. Despite the fact that most simulations incorporated efficiencies, we could not find any potential Type I errors in EU merger decision.

Assuming that all simulations in our sample provide unbiased estimates, we find potential errors in 6 out of 9 cases. In two of these cases the potential error was in the decision not to intervene and in the other two it was on the extent of the intervention. To summarise

the findings from DiD and MS studies, in over half of the cases we identified a potential error (14/25 cases).

Summary remark 9: *Because of its relative simplicity the Difference-in-Differences method is typically preferred to Merger Simulations for analysing approved mergers with no intervention. On the other hand, Difference-in-Differences methods are less suitable for detecting if the competition authority made an unnecessary intervention (Type I error). Ex-post Merger Simulations are capable of fully identifying decision errors in the merger intervention (remedy or block) provided that efficiency gains are incorporated in the simulations. Merger simulations are also able to estimate the welfare impact of mergers.*

Table 15 : Identifying potential errors using MS

Decision	Finding	Meaning of finding	Assessment of the decision to intervene	Assessment of the intervention	Studies
Approve	**M** > N	Approved harmful merger	Type II error	–	GSK + Astra Zeneca (2009, Sweden) Cerealia + Schulstad (2003, Sweden) Rio Tinto + North Ltd. (2000, EU)
	M ≤ N	Approved efficient merger	No error	–	
Remedy	M > AR > **R** = N	Best remedy applied	No error	No error	Courage + Scottish&Newcastle (1995, UK)
	M > AR > **R** > N	Best remedy applied but merger still harmful	No error	Type II error	Carlsberg + Pripps (2001, Sweden) CVRD + Caemi (2001, EU) Morrisons + Safeway (2004, UK)
	M > **R** > AR > N	Suboptimal remedy applied but merger harmful even with best remedy	No error	Type II error	
	M > **R** > AR = N	Suboptimal remedy applied when best remedy would have prevented harm	No error	Type I error	
	R > M > AR > N **R** > AR > M > N AR >**R**> M > N	Badly designed remedy increased the anti-competitive effects	No error	Type II error	
	in all remedied cases where M ≤ N	Intervention in efficient merger	Type I error	Type I error	
	M > AR = **N**	Harmful merger blocked although remedy would have been possible	No error	Type I error	
Block	M > AR > **N** AR > M > **N**	Harmful, irremediable merger blocked	No error	No error	Bass + Carlsberg (1997, UK) Volvo + Scania (2000, EU)
	M ≤ **N**	Efficient merger blocked	Type I error	Type I error	

4.2 Is a potential error really an error by the CA?

This section looks at the 14 cases where we established that there is a possibility that the CAs made an error. In our discussion we refer to Werden (2015), who identifies four types of 'error' sources: faulty facts, erroneous economics, bad breaks, and random variation. He argues – and we echo this argument – that the ex-post merger study should try to investigate and establish what caused the error. To some extent, some of the studies in our sample did this. Where this was available, we report these findings; otherwise we try to establish the source of error by using the study and the case report of the CA.

We identify 5 sources that may have led to us identifying a potential error in the shape of post-merger price increases. These are: (1) erroneous analysis, (2) focus on non-price effects, (3) faulty facts, (4) random variation, and (5) small price increase. We would argue that only in the case of erroneous analysis could we unambiguously attribute an error to the CA.

4.2.1 Erroneous analysis

It is possible that the CA had all information at hand that should have enabled it to reasonably predict the price increase, but it made an erroneous assessment leading up to a decision that did not stop the negative effects of the merger. One example could be the inappropriate use of quantitative techniques. An upward-price-pressure type analysis may be a good indicator of the closeness of competition in heterogeneous markets but it may be sensitive to the values of input and can provide a misleading indicator on whether to conduct a detailed investigation or not. Similarly, if simulations are used in the investigation and the wrong assumption is made, then the results will suffer from the resulting biases. Another source of erroneous analysis might come from the inadequate analysis of available evidence, or analysis based on no solid evidence.

A good example is the merger between *GSK* and *Astro Zeneca*. According to the authors of the ex-post study a post-merger price increase was due to errors in the analysis of the CA. The authors – who were involved in the investigation as experts – estimated at the time of the investigation that the merger would significantly increase prices. Yet, the CA decided to unconditionally clear the merger, based on its assessment that sufficient competition from other main market segments would prevent any such price increase. In this case the CA was also optimistic that the coming deregulation of the pharmaceutical industry would encourage new entry and competition, which did not happen.

In the *Ziekenhuis Hilversum/Gooi-Noord* merger it appears that the CA expected consumers to switch, which did not happen. It is not clear from the decision and the study whether this was based on a faulty analysis or a faulty fact. Also, the ex-post study identified some other possible sources of the price increase (the merger happened shortly after market liberalisation so the pre-merger prices had been below average).

4.2.2 Other merger impacts dominate

In most cases the merger has a range of effects on factors other than price. For example a merger might lead to quality improvement, or wider penetration of the relevant products. In these cases even if the CA had perfectly foreseen a small price increase, these could have been tolerated if the CA had predicted that the merger would contribute to other dimensions of consumer welfare.

In the *Agip/Esso* merger prices increased for Esso. The authors of the ex post study argue that this might have been caused by the fact that Esso stations became part of a larger firm with a reputation for higher quality, so the price increase can be attributed to an upwards brand repositioning of the Esso stations. Although this seems like a

credible explanation (which could explain why the CA – the European Commission – allowed the merger despite the potential for a small price increase), it appears that the CA's case report only discusses market shares and concludes that the level of competition in the market should ensure that prices do not increase post-merger.

In the case of *Lukoil/Jet*, only Lukoil's prices increased. The ex-post study claims that this might have been due to the 'considerable expansion' in Lukoil's presence and recognition.[57] However, from the case report it does not seem like that CA used this as an argument for approving a potentially price-increasing merger.

Another case was the *Lidl/Plus* merger in Romania.[58] This study was not included in our analysis above because its price impact estimates are kept as confidential. Nevertheless, using a DiD method the study concludes that in 67% of the relevant geographical and product markets the merger did not change prices, in 27% of the cases it decreased prices and only in 6% of the cases it increased prices. Therefore, on the whole, the assessment concluded that the CA was correct in unconditionally approving the merger and the positive effects outweighed the negative ones.

4.2.3 Faulty evidence

'Errors' might have been caused by relying on faulty facts, which led to inaccurate forecasts. The ex post study in these cases should investigate any potentially faulty information gathered during the investigation. One possible source of faulty information may come from surveys of industry representatives, which could have painted a misleading picture of entry conditions based on which the CA predicted that entry would dampen any post-merger price increase and refrained from intervention. If there was no entry post-merger and prices did increase then this faulty fact would have led to the Type II error of not intervening. Whether relying on faulty facts should be recognised as an 'error' by the CA is a difficult question. From our conversations with officials of CAs we found that opinions differ on this question, some seeing faulty facts as a mistake of the CA while others considering that the reliability of information used by CAs may depend on the rate and quality of responses to their requests for information and therefore there are cases where verification of the evidence becomes practically impossible.

Without detailed information on the merger investigations we cannot identify which cases were based on faulty facts. The CA would be in the best position to go back to its decisions every time a post-merger price increase was estimated, and look at the evidence it used to identify the ones that may have led to inadequate intervention.

4.2.4 Random variation

We need to make in important caveat that applies to all ex-post merger evaluations. A small post-merger price increase/drop may not imply a CA error. At the time of the decision post-merger prices are random variables, making precise price-effect predictions impossible. The importance of this point is also highlighted by Carlton (2009) and Werden (2015).

To give some illustration, say that a merger is approved without intervention and an ex post evaluation finds a positive price increase. Does this mean that the CA made a Type II error? During the merger investigation the CA looks at the expected price change of

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[57] There is an interesting debate in the mergers world about whether a repositioning that is due to new ownership is necessarily a SLC/SIEC, if it would have occurred even if that new owner was not a competitor (but, for example, had different branding). In this case, one could potentially observe a post-merger price rise without this implying a SLC/SIEC or a Type II error.

[58] http://www.consiliulconcurentei.ro/uploads/docs/items/id9142/raport.pdf

a merger and unconditionally approves mergers if the anticipated price change, $\Delta P \leq x$, where x is the specific value of ΔP, above which the CA intervenes a merger; in the case of an unbiased CA, $x = 0$.

Say that $F(\Delta P)$ is the probability distribution of ΔP: if the merger is allowed even though $E(\Delta P) > 0$ then the CA makes a Type II error – allows bad mergers. If the merger is intervened even though $E(\Delta P) < 0$ the CA makes a Type I error – intervenes where it should not have. If $E(\Delta P) = 0$ then the CA would (rightly) allow the merger to go ahead unconditionally. But even in this case there is a $1 - F(x)$ chance that the merger leads to a price increase.

To be able to decide whether the CA was right or wrong one would need to look at the distribution of ΔP that the CA observed during its investigation and the value x, and not just the actual ΔP that the ex-post evaluation estimated. To give a numerical example: if x is the median of ΔP, then $1 - F(x) = 0.5$.

If there are two possible price effects: -5 and $+5$ then $E(\Delta P) = 0.5 \times (-5) + 0.5 \times (+5) = 0$, therefore the CA is rightly deciding to allow the merger without remedy. But even a merger like this, with a zero predicted price increase, will turn out, for random reasons, to raise the price about half the time.[59]

The above simple thought experiment shows that there is always the possibility of a random error in the CAs decision. It is needless to add that the issue of random variation is a largely abstract one in most assessments. If the ex-post study could observe the distribution of expected post-merger prices at the time of the decision then one would be able to assess whether the actual price-effect fell within the realm of random variation. Of course, we cannot think of a scenario where this would be possible and thus, unsurprisingly, we found no considerations of random variation in any of our sample studies.

In terms of how to deal with random variation, probably the best advice comes from Werden (2015), who argues that this error is likely to only lead to small post-merger price changes. If – pre-merger – the CA had accounted for all available information, and post-merger we eliminated the possibility of all other errors then the remaining (random) variation in the post-merger price distribution (as anticipated pre-merger) must be small.

Summary remark 10: *In more than half of the analysed sample, prices increased after the competition authority's intervention. A post-merger price increase may not imply an erroneous merger decision: (1) if non-price effects dominated price effects and the authority recognised this, (2) if the decision was based on faulty facts, or (3) if the post-merger price increase could have been seen as random variation at the time of the authority's decision.*

4.2.5 Small price increase

As we have shown in Section 2.2, the overwhelming majority of studies estimated a price increase of less than 5 per cent following the merger. Some of this may be due to random variation (see above). It is also possible that a small price increase is not considered a sufficiently significant impediment to competition to justify blocking/intervening a merger. The *Carlsberg/Pripps* study provides evidence that the divestiture was the best possible remedy and although it raised the prices in comparison to the no-merger equilibrium, it was still preferred to blocking the merger. Although no specific explanation is given, this might be due to efficiencies that in the long run would

[59] See also Carlton (2009) for a similar argument.

outweigh the small price increase, or due to the expectation of new entrants who will in the long run reduce the price to the pre-merger level.

Finally, there is another issue that is important to flag up if a study finds no (or little) post-merger price-increase, especially if the market is concentrated and/or there are concerns about collusion. It is a possibility in these cases that the firms have been colluding before the merger and the merger is a response to changes in the dynamics of the collusive process.[60] In this case the merger is less likely to further increase prices even though the market is susceptible to collusion and the merger made the market even more collusive.[61]

4.3 Which cases are more likely to attract error

We now turn to examining the characteristics of the cases where there was a potential error in the CA's decision. Because of our sample size we are unable to provide a regression analysis, therefore the following analysis is limited to a simple pairwise comparison of sub-sample means.

We present our results in two steps. First we break down the sample into 'error' and 'no error' groups, where we consider all potential errors identified in Section 4.1 as error. Table 16 presents the sample means for some case characteristics for these two groups. We could only rely on data that was available from either the ex-post study or the merger decision. A simple t-test was run to test the difference in sample means. Note that we excluded blocked mergers from the comparison of mean price effects.[62]

Table 16: Sample means broken down by CA error

		no error		error	
		N	mean	N	mean
Price effect		9[63]	-3.27***	14	8.24***
Decision year		13	2003**	14	2005**
Number of post-merger firms		10	6.7	11	7.91
HHI	Pre-merger	6	1525	8	2027
	Post-merger	6	2047	8	2571
Combined post-merger market share		13	0.37	11	0.32
Market share	Firm 1	10	0.17	10	0.22
	Firm 2	8	0.11	10	0.09

*,**,*** implies t-test significance in the difference between means at 90%, 95% and 99% respectively.

As a limitation of the applied methods (see above), we were only able to focus on Type II errors. This leads to a straightforward finding: potential errors were identified in all

[60] Davies, Ormosi and Graffenberger (2015) provide evidence that a breakdown in collusion is likely to lead to mergers between previous colluders.

[61] In the *DISA*/*Shell* study the authors point out that this could have played a role in their finding that post-merger prices did not increase.

[62] The reason for this is that here we look at what happened in the market in cases with/without CA error and in blocked mergers the estimated price effect did not take place (we found no error in the decisions to block two mergers). Put differently, in a blocked merger, if the CA decided correctly, then the estimates would show that allowing the merger would have led to a price increase. On the other hand if the CA made an error in a remedied merger then the estimated price effect will also be positive. Therefore a price increase may imply error in one case and no error in the other.

[63] We excluded the two blocked mergers from the price effect statistics as they refer to a hypothetical price effect.

mergers where prices increased post-merger (irrespective of whether the merger was remedied or not). This means that the first line of Table 16 simply breaks down our sample into price increasing and other mergers. Table 17 below shows the same, looking only at the direction of the price change.

Errors were more common in more concentrated industries (this finding was not statistically significant). There seems to be no difference between the two groups in terms of the number of firms. These last two findings together seem to suggest that in the sample more concentrated markets have more (fringe) firms.

Table 17 breaks down the number of cases with/without potential error by the type of the CA's decision. It appears that post-merger prices were just as likely to increase in intervened and non-intervened mergers, and that the choice of control had no effect on identifying a potential error.

Table 17: Number of cases with/without error

		no error	potential error
Sign of price effect	Increase	0	14
	Decrease	9	0
	No change	2	0
Decision	Approve	9	9
	Remedy	2	5
	Block	2	0
Control (DiD)	Competitor	2	1
	Local market[64]	8	7
	with PSM	3	1
	without PSM	5	6

In the second step we re-define 'error' based on Section 4.2 – i.e. assuming that not intervening a merger that led to a price increase of ≤1% is not an error, either because it is only due to random variation or because it is small enough to be overridden by efficiency gains or market dynamics. For the same reason, we corrected all price changes not larger than 1% to zero.[65]

Table 18 shows the results for the re-defined sample. What appears is that the market structure differences are now significant. This is no surprise. Unconditionally approved mergers make up most of our sample. The decision not to intervene in a merger in a concentrated market is more likely to turn out to be erroneous. When we removed <1% price changes from the 'error' group we redefined this sub-sample to only include cases with larger price-effects (dominantly price-increases). Larger price-increases are connected with higher market concentration, which is very much in line with what theory would suggest.

[64] We broke down the number of cases using local markets as Control into cases where local markets were selected using some formal method (PSM or other iterative techniques) and those where no such methods were used.

[65] The following mergers were involved: *DISA/Shell*, *Scottish & Newcastle/Courage*, *Waterstones/Ottokars*, *Ziekenhuis Walcheren/Oosterscheldeziekenhuizen*, *Agip/Esso*, *Morrisons/Safeway*, *Lukoil/Jet*.

Table 18: Sample means broken down by CA error (<1% price change assumed as zero)

		no error		error	
		N	mean	N	mean
Price effect		12[66]	-2.32***	11	10.33***
Decision year		16	2003	11	2005
Number of post-merger firms		13	7.54	8	7.00
HHI	Pre-merger	9	1407***	5	2540***
	Post-merger	9	1775***	5	3374***
Combined post-merger market share		14	0.31	11	0.38
Market share	Firm 1	13	0.15***	7	0.29***
	Firm 2	13	0.10	7	0.12

*,**,*** implies t-test significance in the difference between means at 90%, 95% and 99% respectively.

The main message of Table 18 is that competition authorities should have to be more careful with concentrated markets. These findings also reinforce that measures of market concentration give a good first approximation to the likely competitive issues a merger would raise. Market structure and the analysis of market concentration are at the heart of the EC Merger Regulations, and are a crucial component of the assessment of mergers.[67]

Summary remark 11: *The competition authority was more likely to have made a decision error in more concentrated markets.*

Of course the above discussion was limited by our access to information on the individual cases. Competition authorities would be in the best position to investigate if and really an error was made in their decision. This may take the form of a two-step procedure, where in the first step, a quantitative ex-post study provides evidence of the effect of the merger, and in the second step a qualitative investigation identifies if the predictions (for example regarding entry, or cost savings) of the CA were correct, or whether a decision error was made in anticipating some of the key effects of the merger. The Commerce Commission in New Zealand, which examines the outcome of specific expectations across numerous decisions to determine if these expectations developed as predicted, has applied an interesting example for the second step.[68]

In the next section we analyse a sample of studies that use non-quantitative methods to analyse the effect of mergers from a longer time-perspective. Although there is no connection between the studies used in Sections 2-4 and the studies in Section 5, we would advise that it would be good practice to connect these two types of analysis. A quantitative estimate of the price effect of a merger could identify cases where an error was potentially made. In a next step the CA could use qualitative methods to further examine what happened in these markets.

[66] We excluded the two blocked mergers from the price effect statistics as they refer to a hypothetical price effect.

[67] Looking at EU merger decisions, both Bergman et al. (2005), and Garrod and Lyons (2015) find that a second phase investigation were more likely in more concentrated markets.

[68] http://www.comcom.govt.nz/dmsdocument/12984

5 Non-quantitative evaluations of the effect of mergers

The preceding part of the study focused solely on quantitative estimates of the price effect of mergers. However, we must also acknowledge those works that use qualitative methods. In the focus of most of these works are the non-price effects of mergers, although some also look at price effects but instead of estimating its magnitude, only try to ascertain its sign. These qualitative studies do not provide statistically robust estimates of the mergers' impact. Nevertheless, they contribute significantly to our understanding of the effectiveness of merger control systems by looking at factors that are much more difficult to quantify, such as entry and exit conditions, buyer power, innovation, service quality, and so on. The purpose of this section is to systematically review these studies and provide a summary of the findings. We believe that this is the first attempt to offer a systematic review of relevant works.

The most prolific publishers of these studies are competition authorities. The academic literature of individual mergers using ex-post data on non-price effects is limited: we found three academic papers that fit our search criteria. Competition authorities produced nine of our sample studies, examining 48 mergers. The majority of these studies were prepared by the UK Competition Commission (in partnership with the Office of Fair Trading and the Department for Trade and Industry), with multiple-merger studies published in 2003, 2005, 2006, 2008, 2009 and 2010. The Dutch and Swedish competition authorities also have conducted qualitative retrospectives of multiple mergers, and the EC commissioned one such study.

In total, 42 of the 50 mergers in the sample of qualitative studies are mergers decided upon by UK competition authorities and the assessment of their effects is restricted to the UK. The industries in which the studied mergers happened are diverse; no particular industry is studied more than twice in the sample. The earliest merger in the sample was in 1996 and the most recent in 2010. The oldest study in the sample was published in 2001 and the most recent in 2014. The mean time gap between a merger and the retrospective study of that merger is 5.2 years. Table 32 in the Appendix contains a list of the sample studies.

A limitation of summarising the results of these studies is that there is a bias in whether a given piece of information is reported by a study. When an event such as the entry of a new firm into the market or a further merger occurs, it is likely to be reported by a retrospective study. However, when a particular type of activity is not mentioned by a retrospective it does not necessarily mean that it did not occur - the study may simply not have examined it. For example, for 12 mergers the studies report that further mergers followed them, but there is no specific mentioning that the other mergers were not. This bias is felt and is noted throughout the analysis of the findings below. It means that the ratio of studies, which reported certain merger effects (such as entry and exit) may not be taken as indicative of the proportion of mergers, which led to these outcomes.

5.1 Methodologies used in sample studies

As the main title of the section suggests, a common denominator of the studies in this section is that they rely on non-quantitative information and research methods. Background information from industry publications, market research firms and similar sources were used in almost all of the studies as the main source of simple data such as market shares, entry and exit. The competition authorities' reports frequently make heavy use of their own experiences in the market, post-decision, particularly when describing the process of implementing remedies.

5.1.1 Interviews

Interviews with market participants are another popular methodology, especially in the studies commissioned by CAs. The effect of 16 mergers was evaluated this way. The subjects of the interviews were merged firms, competing firms and customers of the merged firms. Regulators and suppliers were also interviewed in some cases. Interviews are used to learn about industry facts that are not available elsewhere (for example, approximate market shares in specific product markets), to find out about the perceived importance of various developments in industries, to discover information, which is only known to market participants (such as the quality of service), and to learn the overall opinion of market participants of the merger and of the competition authority's decision.

The UK Competition Commission's 2005 report provides a detailed description of the approach taken in selecting cases and interview subjects. They avoided cases, which were both too far in the past or too recent. In cases that were closed more than 8 years prior to the study it was difficult to find interviewees who were sufficiently knowledgeable about the merger and because the procedures of the competition authorities had changed so much in that time that it was felt that useful guidance could not be gained. Cases more recent than 3 years prior to the study were also excluded as being too recent to judge properly whether a significant reduction in competition had occurred. Cases in markets subject to an ongoing competition inquiry were excluded, as were cases where the consultants conducting the report felt they had a conflict of interest. This type of exclusion is unavoidable in research performed by competition authorities and consultancy firms, which work on competition cases.

Interview-based studies frequently conclude by giving interviewees the opportunity to express their overall opinion on whether the merger had a positive or negative effect on the market. Merging parties and customers have clear interests in the merger, and the interests of the customers of the merging firms align with the priorities of the competition authority. Frequently other groups with an interest in the merger are interviewed. These groups include competitors of the merging parties, firms in related industries and industry regulators. It may seem that the customers of the merging firms are the ideal source of information and views on the effects of a merger since the competition authority is concerned with the welfare consumers. However, final consumers may be private individuals from whom informed opinions and detailed information are difficult to gather. Where the customers of the merging firms are businesses the process is simpler, but the interests of intermediate firms may not align with those of final consumers. Customers of the merging firms may be able to pass higher prices through to their own customers.

The US Department of Justice and Federal Trade Commission 2010 Horizontal Merger Guidelines discuss the value of information provided to merger inquiries by third parties. Rival firms may gain from an anticompetitive merger, at the expense of customers, so their views must be treated with caution. However where exclusionary conduct is feared by competition authorities, rival firms will wish to prevent this. Firms offering products complementary to those offered by the merging firms will have similar interests to customers.

The merged party was contacted in every case. Other market participants were chosen largely because they had made representations to the competition authority during the merger review process, though others, such as recent entrants, were also contacted if they were likely to provide useful insights. The response rate to interview requests was 44% and each of the ten cases studied had between 4 and 7 interviewees.

Interviews were conducted primarily by telephone, and also face-to-face where possible and lasted between 20 and 45 minutes. The approach taken was to allow interviewees to focus on the market developments they felt to be important. As such, the interviews took the form of a structured discussion rather than a fixed list of questions.

Interviewees were also contacted after the main interviews in order to fill gaps in knowledge. The authors of the study felt that these choices greatly improved the quality of the research.

The 2008 UK Competition Commission report also uses interviews with market participants and follows a methodology very similar to that outlined above. In addition to the detailed telephone interviews with selected market participants, a wider group of market participants was surveyed with a questionnaire. The primary aim of the questionnaire was to gain facts about the market rather than opinions on the merger decision. A survey was also used to gauge the opinions of customers in one of the merger cases.

5.1.2 Quantitative techniques

Where the constraints of data prevent a full econometric analysis, useful findings may still be gained from simple uses of quantitative data. Chakreborty et al (2014) examines closely the trends in retail prices of groceries in the UK supermarkets sector, and finds – from simple trends – that averages prices fell following the merger. Intriguingly, they are also able to make a strong argument that the 2004 merger of Morrisons and Safeway led to a large change in the mode of conduct of supermarkets. They show that the market is characterised by asymmetric price leadership, where market-wide price increases tend to be initiated by a different firm to market-wide price decreases. Similarly, Leheyda et al (2003) examines closely the levels of R&D spending by EU pharmaceuticals companies, relying on simple descriptive statistics and qualitative information.

Some of the CA conducted reports also used simple quantitative techniques such as simple ('back of the envelope') simulations. These simulations are not reported in the level of detail that we discussed in the previous part of this study. Frequently, price elasticity of demand is assumed and diversion ratios are calculated on this basis. Different specifications where firms are or are not capacity constrained may be estimated.

The 2008 UK Competition Commission report uses such techniques as a major part of its assessment of the price effects of mergers. The study notes that the four mergers (out of eight studied) for which such simulations were conducted were cases where the products were relatively homogeneous, with strong capacity constraints and the decision focused on unilateral effects from the merger, making the simulations coherent. The report argues that the simulations are useful guide to discussion since they provide estimates of the possible magnitude of price changes from the mergers.

We found some other simple quantitative methods used in the relevant studies. For example the 2006 UK Competition Commission report conducts a simple statistical test to compare the change in price of the one geographical area, which was feared to have a significant lessening in competition at the time of the merger with a single comparable area with no such concerns.

In the following discussion we look at the main categories of merger effects that our sample studies looked at. First, we review our sample to the extent they report on how prices developed after the merger. This is followed by a discussion of merger effects on market structure (market shares, concentration, market size, rivalry, entry, exit). We then review the dynamic effects of mergers (innovation, capacity/investment, further mergers), before concluding with a set of miscellaneous other effects (buyer power, consumer welfare, import, service quality, third party opinion).

5.2 Post-merger prices

Some of the qualitative studies in our sample also looked at price effects but in a less rigorous way than the studies, which used difference-in-differences or merger simulation techniques. Where data on prices was available, studies frequently attempted to distinguish between the direction of price changes, which could be attributed to the merger and those, which were due to other factors. However, without the use of rigorous techniques (such as difference-in-differences) such judgements are highly subjective. In the studies, which used interviews, the effect of the merger on prices was established by relying on the judgement of market participants, most frequently customers.

Price effects were reported in 33 out of 50 merger cases. Out of these, 18 cases saw an increase in prices, 9 cases a decrease, and in 6 cases stable prices. It appears that the CA was more likely to intervene in cases that were later followed up by a price increase (the CA intervened in 11/19 mergers where prices increased, and 6/15 mergers where they did not increase). These figures also mean that in 8 cases the CA understood correctly that an intervention was needed (i.e. required a remedy), yet it made a Type II error in judging the extent of the intervention. From the perspective of prices, only 5/13 remedies were effective (i.e. prices did not increase). In 3 cases prices increased even after a blocking decision.

Table 19: Non-quantitative assessments of price effects

	CA's decision			
Price effect	Approve	Remedy	Block	Total
Increase	7	8	3	18
Decrease	5	2	2	9
No change	3	3	0	6
Total	15	13	5	33

In theory a merger, which causes no efficiency gains will always raise prices. Spector (2003) shows that this holds even when entry is possible after the merger - though the price effect will be more severe without entry. However, mergers are unlikely to be privately profitable for the firms involved if they do not lead to efficiencies, unless an extremely high increase in concentration also results. In the BUPA/CHS (private medical insurance) merger, increased technology levels in medicine led to higher costs for service providers and so higher insurance premiums. Parallel National Health Service reforms led to lower prices for services from nationalised hospitals in the NHS's internal marketplace, forcing private firms competing for public healthcare work to also lower their prices.

Information on market shares of merging firms was only given in 23 cases. In cases where the information is available, the average joint market share was similar in price increase and non-increase cases (45% and 41.55% respectively).

In the *Pirelli/BICC* study (see box on page 53 for details) the authors conducted both an event study and a series of interviews. Interview subjects believed that the merger had no negative effects on the market price (respondents reported a "moderate" decrease in prices). Market share tables indicated a relatively stable market share of the merged firm over time. Price decreases were caused by an increase in rivalry, a decrease in the quantity of power cables demanded, and a decrease in production costs. The study concluded that the merger was motivated primarily by efficiency and DG Comp made the correct decision.

Pirelli/BICC

The decision to approve the merger between Pirelli and BICC in the market for power cables was taken by the European Commission in 2000. An event study, using data on the stock prices of the firms involved, combined with a survey of the main market participants was undertaken by LEAR Group. DG Comp investigated theories of harm including joint and individual dominance in the two relevant markets for power cables it identified.

The event study found that the merger had been pro-competitive. The stock prices of customers increased, reflecting their lower costs, and the stock prices of competitors fell, indicating that the market had become more competitive. Interviewees indicated that the merged firm was considerably more efficient that the unmerged firm and market data indicate a fall in prices. The study concluded that DG Comp had been right to reject concerns of joint and individual dominance and recognise the efficiencies arising from the merger.

An interesting picture appears if we break down the 'estimated' post-merger price change into two groups, where the analysis was done within 5 years from the merger and where the assessment was done later. 5 years is chosen as the point for comparison since it is the median duration between the merger and the study in the sample of non-quantitative post-merger studies (the same breakdown will be used throughout this section).

Table 20: Post-merger price change by the timing of the study

	Decrease	No change	Increase	Total
< 5 years from merger	1	4	9	14
≥ 5 years from merger	9	2	6	17
Total	10	6	15	31

It appears that 9/10 cases where the estimated post-merger price change was negative came from studies that were done at least 5 years after the merger. This echoes what we have stated in Section 2.4.2: studies that are taken from a sufficient distance after the merger (i.e. allow time for markets to self-correct), are more likely to find lower price-effects.

5.2.1 Consumer welfare

Closely related to the price-effect of the merger is the change in consumer welfare. Measuring consumer welfare is not simple (even if one only focuses on the price dimension). Ex post merger simulations could be used for this purpose as proposed in Section 3. In our sample of studies welfare evaluations were simple, back-of-the-envelope calculations, assuming that consumer welfare is simply a product of the change in market price and the change in total output resulting from the merger. This measure of course reflects a simple tautological relationship with price (higher price – lower welfare, etc).[69]

More specifically, the 2006 UK Competition Commission study attempted to estimate a monetary value for the change in consumer welfare caused by the five mergers it studied. It used several techniques for this: reporting estimates from documents provided by the parties, doing a very simple difference-in-differences test, and applying

[69] However, this does not entirely capture the overall welfare of consumers. A merger may lead to higher prices but allow the merged firm to provide a higher quality product, thus increasing consumer welfare despite first appearances.

a simple potential price change to known sales quantities. This 2006 CC study found 4 mergers that estimated to decrease consumer welfare. The calibrated damages varied between £13.6m per year and £0.3m per year. This study is relatively brief, giving few exact details of the techniques used so its conclusions should be evaluated with care.

Consumer welfare changes were 'calculated' in 6 cases in our sample. 5 mergers were said to have been followed by a decrease in consumer welfare (all of these mergers were reported to have increased prices) and one merger an increase (in this case prices were reported to have dropped post-merger).

5.3 Market structure

Theoretical IO literature in the footsteps of Farrell and Shapiro (1990) agrees that in the absence of efficiency gains, mergers would almost invariably harm consumers.[70] There is a wide array of works on the characteristics that contribute to this consumer harm, the most important of which is a changing market structure that gives way to market power.

Table 21: Qualitative evidence on the effect of mergers on market structure

	CA's decision			
	Approve	Remedy	Block	Total
Share of merged firm				
Increase	2	3	1	6
Decrease	8	1	2	11
No change	1	2	0	3
Total	11	6	3	20
Concentration				
Increase	7	2	1	10
Decrease	1	0	1	2
No change	5	1	1	7
Total	13	3	3	19
Market Size				
Increase	3	0	3	6
Decrease	5	1	1	7
No change	2	1	0	3
Total	10	2	4	16
Rivalry				
Low	4	0	1	5
High	4	2	1	7
Total	8	2	2	12

Table 21 summarises the market structure effects that our sample studies looked at. Most of the studies discussed post-merger market structure developments, focusing on characteristics such as the market share of the merged firm, entry, exit, and/or market concentration. We present these separately.

[70] For this reason it is somewhat surprising that these ex post studies did not report on merger-induced efficiency gains.

5.3.1 Market share of the merged firm

There is mixed evidence[71] on whether the merged firm's market share is likely to be more or less than the joint market shares of the unmerged firms once the market begins moving towards its new equilibrium. Mueller (1985) found that in US manufacturing industries, the market shares of merged firms fell sharply, to the benefit of the other firms in the industry. In their study of the UK financial mutual sector Haynes and Thompson (2005) found that merged firms do substantially increase their market shares following mergers. This difference may be because building societies offer homogeneous products whereas manufacturing firms offer differentiated products. The strong price competition building societies engage in allows even a small efficiency gain from a merger to lead to a large competitive advantage in attracting customers. Intuitively, if the merger makes the merged entity more efficient then it can lead to an increase in their market share (if they reduce prices).

The market share of the merged firm was mentioned in 20 case studies. In 11 of these the share decreased, in 6 it increased and in 3 cases it was stable. It appears that from the perspective of market shares, the CA was more likely (9/11 times) to approve mergers that did not lead to market share increases.

In *Compass/Rail Gourmet* (on-train catering service) the parties' joint market share increased due to high entry barriers. However, competition from other markets (in-station catering) was found to exert sufficient competitive pressure on the merging firms. In the other case *CHC/Helicopter* market shares of the merging firms increased in the first years following the merger (reported in a 2003 study), but later this turned around and they lost market share (reported in a 2005 study).

In 7 out of the 10 cases in which the merged firm's share decreased and price information was also available, prices also decreased. For the cases where data is available, the mean combined market shares of the merging firms at the point of the merger in cases where the merged firms' market share increased was 57% and where the merged firm's market share decreased the mean was 45%.

Finally, an interesting finding (shown in Table 22) is that almost all (10/11) market share decreases were identified by studies that were conducted at least 5 years after the merger. Put differently, in 4/7 studies done within 5 years from the merger, market shares increased post-merger. In 10/13 studies conducted at least 5 years after the merger, market shares decreased. The most obvious interpretation would be that if allowed enough time, initial increases in the market share of the merged firms will drop.

Table 22: The effect of the merger on market shares (by the timing of the study)

	Share of merged firm			
Timing of study	Decrease	No change	Increase	Total
< 5 years from merger	1	2	4	7
≥ 5 years from merger	10	1	2	13
Total	11	3	6	20

5.3.2 Concentration

In the short run, mergers increase market concentration. There is some empirical evidence that in the longer run this effect might disappear, or concentration would even

[71] See Ghosh (2004) for increasing joint market share and Mueller (1985) for decreasing joint market share following mergers.

decrease – depending on how concentrated the market was at the time of the merger.[72] Once the other firms in the market have reacted to the merger and entry or exit has taken place, the longer-run impact of the merger on concentration may be observed. In a theoretical setting Gowrisankaran and Holmes (2004) find that, within the context of their model, markets tend towards their previous structure following mergers.

It is important to put forward that most changes to market concentration that were detected by these ex-post studies were due to either new entry into the market or by subsequent acquisitions by the merging parties or their rivals.

The change in market concentration following the merger was mentioned in 19 cases. In 10 cases concentration increased following the merger and in 7 cases concentration remained the same.

Any analysis has to be careful when finding an increase in concentration following a merger, which could have been caused by events independent of the CAs merger decision (for example unforeseen subsequent mergers). The important thing here is to identify if the concentration increase was caused by a factor that the CA was aware of during the decision process. There were two such cases in our sample of studies. In one of these (*Boots*/*Unichem*) the retail pharmacies market became more concentrated mainly as a result of acquisitions made by two rivals Lloyds and Cooperative pharmacy. In *William Hill*/*Stanley* following the merger, the 3 largest operators in the supply of betting services were involved in 8 further acquisitions, which made the market more concentrated.[73] There was also one case (*BUPA*/*CHG – private medical insurance*)[74] where concentration increased despite the blocking of the merger – this was in the medical insurance sector, where there was one entry, one exit and an increase in concentration (gains in market share by largest firms) post-merger.

There was one approved merger where post-merger concentration decreased (*Klaus J Jacobs*/*SCIA* in industrial chocolate), which was due to new entry. In another case a blocked merger was followed by decreased concentration (*BUPA*/*CHG – private medical services*). Both mergers, where concentration decreased after the merger, were studied at least 5 years following the merger, unsurprisingly, as in these cases more time was allowed for entry to happen.

Table 23: The effect of the merger on concentration (by the timing of the study)

	Share of merged firm			
Timing of study	Decrease	No change	Increase	Total
< 5 years from merger	0	4	4	8
≥ 5 years from merger	2	2	7	11
Total	2	6	11	19

5.3.3 Market size

The question of whether the size of the relevant market was affected by a merger is largely contingent on how prices develop after the merger. If prices increase post-merger, that would likely to lead to a drop in market size – as customers would switch to the nearest substitutes (or not buy at all). Therefore an increase in market size may imply lower prices and/or improvements in other dimensions that attracts more demand. On the other hand, changes in market size are frequently caused by changes

[72] Adams, Johnson and Pilloff (2009)

[73] Three of these were assessed by the OFT (1 remedied, 2 unconditionally approved).

[74] Concentration increased in the insurance market, and decreased in the medical services market.

in demand for the product in question, which are exogenous to the merger and the merger decision.

16 out of 50 cases looked at market size effects. Out of these, 6 mergers led to increased market size, and 7 to a drop in market size. For the 13 cases where we had post-merger price and market size observations at the same time we could not conclude that the two were correlated in any way. Similarly, we found that the timing of the study did not affect whether increasing or decreasing market size was reported by the study.

In the *BUPA*/*VHG* merger (which was blocked by the Competition Commission) in private medical services (PMS) in the UK, the study concluded that the size of the market increased post-merger. The primary force behind the expansion of the market in PMS was the NHS Plan, which led to expansion in the overall size of the PMS market and further increased the attractiveness of new entry. The study also noted that entry was not anticipated at the time of the merger, and it might have overturned any negative effect of a merger, had it been approved.

There was one case where the market size decreased following a blocked merger (*Littlewoods*/*Freemans*).[75] The merger was blocked by competition authorities in the UK market for agency mail-order products. The market was sharply declining in size when the merger was proposed and it continued to do so after the blocking of the merger. A merger between two of the other firms in the market followed, but the industry was still characterised by low innovation by all firms, falling sales and exiting firms.

The UK Competition Commission blocked the *Knauf*/*Superglass* merger, which would have been a 3-2 merger in the loft-roll market, 4-3 in the blowing wool market and not very significant (post-merger share < 20%) in the slab market. Foreign import was not suitable for some sections of UK market; also foreign producers were operating at capacity. Post-merger, there were no substantial changes in concentration but large capacity increases, increasing the size of the market and a strong new entrant (foreign producer).

5.3.4 Rivalry (coordinated behaviour)

In the ex-post studies in our sample rivalry was typically interpreted as the intensity of competition between specific firms in the market. This largely captures concerns over coordinated behaviour among firms. Studies, which noted decreases in rivalry based this on price leadership behaviour, as noted in the UK supermarkets sector following the merger of Morrisons and Safeway, and lower levels of switching by consumers, as in the UK personal current accounts market following the blocked merger of Lloyds TSB and Abbey National.

Compte, Jenny and Rey (2002) performed a detailed analysis with the aim of producing a set of guidelines for assessing the probability of tacit coordination resulting from mergers. They found that the reduction in the number of firms from a merger always results in a greater risk of coordination, however this may be counteracted if the market structure becomes more asymmetrical after the merger, despite higher concentration. Greater asymmetry makes coordination between firms less likely, especially if the firms face severe capacity constraints.

Rivalry may or may not be correlated with market structure measures like concentration (in fact in the 7 studies where both rivalry and concentration were looked at we found no correlation between the two).

Rivalry was reported in 12 cases - high in 7 cases and low in 5 cases. Where price and rivalry information were both available we found that when rivalry was high, prices were

[75] UK Competition Commission - *Comments by academic economists on past reports*, 2003

more likely to decrease (5/7 cases). We also found that all the studies (7/7 cases) that reported high rivalry were conducted at least 5 years after the merger.

Optiroc Group AB/Stråbruken AB

Optiroc Group AB and Stråbruken AB, two Swedish producers of roofing materials, merged in July 1998, unopposed by the Swedish Competition Authority. The Swedish Competition authority conducted an ex-post analysis of the merger and the decision using evidence considered during a judicial review of the decision and interviews with market participants.

The approval of the merger was based on a judgement that the Swedish market was highly open to imports of roofing materials from neighbouring countries. In fact, imports did not increase as much as the CA had expected because of the unwillingness of consumers to switch to non-Swedish products. Concentration in the market increased greatly and the degree of competitive pressure between the firms in the industry was decreased, leading to a decrease in rivalry. Ultimately, a new entrant prevented prices from rising as a result of the merger. Also, increased efficiencies promised by the merging firms were found to have failed to materialise. Consumers do not seem to have been harmed substantially by the merger, but the merged firm has gained dominance and the competition authority was judged to have made a number of errors in its approval of the merger.

There were 4 unconditionally approved mergers, which were followed by a reported drop in the level of rivalry. In *CHC/HSG* (a merger between providers of helicopter transport service to oil and gas installations on the UK continental shelf, see box on p.65) the ex-post study reported that rivalry between Brintel (acquiring firm) and Bristow (rival) was less intense than it had been prior to the merger, and that prices and quality were less competitive as a result. In *Compass/Rail Gourmet* (a merger between suppliers of on-train catering services) rivalry dropped mainly because of the difficulty for new providers to enter the market. However, at the same time significant rivalry evolved from in-station services, which helped mitigate the adverse effects from the drop in rivalry in the on-train catering service. In the *Cowie/British Bus* merger (market for bus services in London) rivalry dropped, mainly as a result of the further consolidation of the market. Finally, in *Optiroc/Strabruken* (in the markets for roofing materials in Sweden) rivalry decreased as a result of an increase in Optiroc's market power (see box on p.58). The CA expected factors such as international competition and synergies to mitigate these effects, but they did not happen.

Summary remark 12: *Looking at how market structure changed post-merger may provide useful information for assessing the competition authority's decision. Developments in the joint market share of the merging firms, the level of rivalry, the level of concentration, and the size of the market are all informative for this purpose. We found that there is a non-trivial number of cases where the merger was followed by higher concentration, less rivalry, or larger market power of the merging firms. Time also seems to play an important role; studies that are conducted more than 5 years after the merger were less likely to find similar concerns.*

5.4 Dynamic effects

Almost all the effects that we look at under Section 5 are dynamic to some extent. For example, we could see how market structure changes may be different depending on how long after the merger we decide to take a snapshot of the market. Nevertheless, in this section we focus on those effects that are typically categorised by merger literature as dynamic: innovation, investment, entry, exit, and further mergers. These effects are summarised in Table 24.

Table 24: Qualitative evidence on dynamic effects of mergers

	CA's decision			
	Approve	Remedy	Block	Total
Innovation				
Increase	5	4	3	12
Decrease	0	0	1	1
No change	0	1	0	1
Total	5	5	4	14
Capacity investment				
Yes	3	0	2	5
No	5	1	0	6
Total	8	1	2	11
Entry				
Yes	9	3	3	15
No	2	3	1	6
Total	11	6	4	21
Exit				
Yes	2	2	1	5
No	2	0	0	2
Total	4	2	1	7
Further mergers				
Yes	4	5	3	12
No	0	0	0	0
Total	4	5	3	12

5.4.1 Innovation

Evolving from the cradle of Schumpeter's (1942) book, IO literature in general agrees that there is at least a significant number of instances in which higher concentration is conducive to innovation.[76] Aghion et al. (2005) formalised this relationship in a seminal paper that found that there is an inverted-U shaped relationship between the level of competition and innovation. Because there is no one-size-fits-all explanation to how increased/reduced competition affects innovation, it is down to the CA to judge in each case whether the dynamic innovation effects outweigh the short run consumer welfare loss from increased market power.

Morgan (2001) compares US and EU treatment of research and development in merger control in the pharmaceuticals industry. In the mergers considered, US authorities were willing to consider concerns in downstream markets, which did not yet exist on the basis of the directions of research of the merging firms. EU authorities judged the best solution to firms' dominance in innovation was to ensure healthy competition in the downstream product market, as opposed to attempting to impose structural remedies on the research and development market. With regards to the treatment of efficiencies in merger control, Ormosi (2012) and Veugelers (2012) point out that the EC's scepticism of merger efficiencies leads to a lack of incentives for merging firms to argue that

[76] For an overview of this topic see Katz and Shelanski (2007).

beneficial dynamic effects will result from mergers. This creates a deficiency in the quantity of discussion of dynamic effects from mergers in Europe. For this reason it would seem obvious to look at how the mergers affected innovation. Yet, we found only a small number of ex-post studies that actually did this.

Innovation was defined in different ways by various studies. Regarding the *Pfizer/Pharmacia* pharmaceutical merger Leyheyda et al. (2003) used data on the levels of R&D spending by the firms in the industry in order to establish that there was no change after the merger. In 5 cases the innovation represented firms' focus shifting to higher-value and more technologically advanced products in the market, and offering alternative purchasing options to consumers. The launching of new product varieties is also seen as indicating innovation on the part of firms, as in the *HP/Heinz* food merger (see in more detail under p.61). Despite the lack of an econometric analysis, the conclusion that there was no significant change in the levels of R&D spending following the merger appears robust.

In ex-post studies conducted by the CAs, there appears to be an obvious bias regarding the case selection. CAs only investigated cases where innovation had already been anticipated to play a role post-merger. Therefore not surprisingly, in 12/14 cases where innovation was mentioned in the ex-post study, it was found to have increased. 5 of these were unconditionally approved, which seems to imply that the CA was right in non-intervening. There were 3 cases where increases in efficiency followed the blocking of a merger. In *BUPA/CHG* (hospital mergers) the study attributes most of this effect to the changes in the regulatory environment and the increase in the private provision of treatments. In *Lloyds/Abbey National* (provision of financial products for personal customers) the study suggests that the blocking decision was what made innovation more of a reality in the market. It is difficult to judge without further information whether allowing the merger would have led to more or less innovation due to the impossibility of examining the counterfactual cases where the mergers were permitted.

Of the cases where innovation increased, there was only 1 where at the same time concentration dropped. When data was available on both entry and innovation, 6 out of the 7 cases with entry also saw higher innovations. Regarding the time-dimension of the study, for identifying innovation effects it did not seem to make any difference whether the study was conducted early or late after the merger.

5.4.2 Capacity/Investment

In industries where firms face capacity constraints, investment in additional capacity is necessary for increased output. Where a merger is anticompetitive, industry output is expected to decrease for all firms, leading to a sharp decrease in investment for all firms. If the merger increased the efficiency of the merging firms then an increase in capacity investment for the merging firms is to be expected. Increases in investment for both merger insiders and outsiders may indicate that the conduct of all firms in the market has become more intensely competitive

Pesendorfer (1998) examines multiple mergers in an industry where firms face capacity constraints. He is able to estimate how the cost efficiency of firms changed after the merger in addition to observing the investment decisions of the merged and unmerged firms. He finds that mergers, which lead to large efficiency gains are indeed followed by large capacity investments.

Investment in capacity was only mentioned in 11 cases. 5 mergers were said to have been followed by significant investment or increase in capacity and 6 mergers were said to have been followed by a decrease in investment or shrinking capacity. In two of the cases where investment did increase following the merger, the new capacity investment came from new entrants. In one of these, *Knauf/Superglass*, the lack of spare capacity in the market was one of the reason the CA blocked the merger. Following the decision,

capacity investments increased in the market. In *BUPA/CHG* (hospital mergers) the increase in capacity investment was partly due to the changes in the regulatory environment and the increase in the private provision of treatments.

The problem of there being large amounts of excess capacity left from the merger was noted in one case: *Cott Beverages/Macaw Holdings* (market for generic soft drinks in the UK). The study investigated concerns that coordination would result from the presence of excess capacity, but firms were instead in the process of diversifying into higher-value products, such as speciality and branded soft drinks.

5.4.3 Entry

In theoretical literature post-merger entry is often connected to how efficiencies develop after the merger. Werden and Froeb (1998) examine the entry inducing effect of horizontal mergers and find that entry depends on the efficiency effect of the merger: entry is not expected unless the merger generates significant efficiencies. Assuming a free-entry equilibrium before and after the merger, Cabral (2003) predicts the opposite: cost savings reduce the likelihood of entry. The studies in our sample do not seem to discuss entry and efficiencies together.

Information on entry is extremely vulnerable to the bias described in the introduction of this section – i.e. in cases where there was no entry the absence is not always mentioned. In 15 cases it was noted that a new firm, or multiple new firms, entered the market following the merger and in 7 cases that no entry had occurred after the merger.

Unconditional approval often relies on the expectation that any post-merger price increase will be eliminated by the entry of potential rivals.[77] From the 11 unconditionally approved mergers 9 were followed by entry. Where there was no entry, the CA intervened in 4/6 cases.

HP/Heinz

The 2005 merger between food producers HP and Heinz was referred to the UK Competition Commission for a detailed investigation and was ultimately approved unconditionally. Five different product markets were identified by the CC. All of the product markets were highly concentrated pre-merger but the merging firms did not overall in their dominance of individual markets. HP was dominant in the Brown Sauce and Barbeque Sauce markets, and Heinz in the Ketchup, Baked Beans and Tinned Pasta markets. Only in the Barbeque Sauce market was there any substantial overall at all. Price was judged to be the primary mode of competition and the primary concern for the CC. The key theory of harm examined by the CC was that of the abuse of product portfolio power through bundling or tying. Several products, which the merged firm would control are all essential for a retail food shop or a food service firm to stock. The concern was that such market power could be used to lever market power across markets in order to raise prices.

Post-merger, entry did occur by branded firms in the affected markets, for example by Branston in the Baked Bean and Tinned Pasta markets and Reggae Reggae Sauce in the Barbeque Sauce market. These entries have decreased the merged firm's market share from where it was immediately after the merger. Innovation was strong after the merger, with new variants of products and new packaging types. Retail and wholesale prices rose but this was largely as a result of increased costs. Overall, the CC was judged by the study to have made a good decision in approving the merger. Significant lessening of competition did not result in any of the five markets. However, some of their assumptions about entry were flawed. Branson's low market share entry into the Baked Bean market shows that branded entry into the market is not as simple as predicted by the CC and Heinz did entry the barbeque sauce market, contrary to the CC's expectations.

[77] An example of the theoretical backing of this idea: Davidson et al. (2007).

In 2 cases entry did not happen in unconditionally approved mergers. In one of them, *Cargill/Cerestar* (glucose syrup production) the CA did not anticipate entry, instead the merger was approved as rivalry was high (and was expected to be high) and there was no suspicion of coordinated effects (the ex-post study did not identify any coordinated effects). In *Impress/Alcan* (a merger in can ends, tinplate containers, and food cans) the CA did not expect entry to happen. Instead there were other characteristics of the market, which ensured that there was no SLC.

In *Knauf/Superglass*, *EWS/Marcoft*, *Cott/Macaw*, *BUPA/CHG*, and partially in *Heinz/HP* entry happened although the CA did not anticipate it. This means that there might have been Type I errors in the intervention, as some concerns could have been eliminated by subsequent entry.

In the *GHC/Helicopter*, *Klaus J Jacobs/SCIA*, *Nutricia/Milupa*, *Pfizer/Pharmacia*, *Cowie/British Bus*, *DS Smith/Linpack*, *Case VI*[78] in the Dutch study the CA correctly predicted that barriers to entry were low and entry would happen and this was a deciding factor in the decision to unconditionally approve the merger

In *Draeger/Hill Rom* and *Stericycle/Sterile Technologies Group* one reason for a remedy was high entry barriers. The ex-post studies confirmed that post-remedy there was entry, implying that the remedy was (at least partially) effective.

Table 25: The relationship between entry and market shares

	Market shares			
Entry	Decrease	No change	Increase	Total
Yes	2	0	1	3
No	7	2	1	10
Total	9	2	2	13

In 7 out of the 10 mergers that were followed by entry and market share data was available, the market share of the merged firm decreased. When contrasting with price, in 7/10 cases where prices increased post-merger, entry also happened, which seems to support the intuition that higher prices attract more entry. We also found that entry was more likely in studies that were conducted at least 5 years following the merger (implying possibly that new entry may take time to happen).

5.4.4 Exit

Competing firms exiting a market as a result of a merger may seem unequivocally negative. A reduction in the number of firms in the market represents a further increase in the concentration of the market, which had not been anticipated by the competition authority when they made their decision on the merger. However, if the merger increased the efficiency of the merged firms then it is expected that inefficient firms will be forced to leave the market as a result of the lower prevailing prices. Exit might happen by completely withdrawing from the given market but it can also happen through merger. Filson and Songsamphant (2005) point out that in declining markets mergers are a substitute for exit for inefficient firms (this relates to the following section).

Exit of firms from the market was reported in only 7 cases, 5 times to report an exit and 2 times to report the lack of an exit. In *Cargill/Cerestar* (glucose syrup production) the reason the study looked specifically at exit was because the UK Competition

[78] Note that the Dutch ex-post evaluation did not identify the cases by the names of the merging parties.

Commission approved the merger, as rivalry was high despite the high entry barriers. Because of the high entry barriers it was crucial to assess whether exit (which would have decreased rivalry) happened post-merger.

The increased concentration caused by a merger is less likely theoretically to cause exit than entry so this may account for the gap in reporting between exit and entry. We also found that exit was more likely in studies that were conducted at least 5 years following the merger.

Reports specifically note that particular types of firms exited as a result of the merger. In the baby milk sector, the merger between *Nutricia* and *Milupa* was followed by the exit of supermarket own-brand suppliers due to the high brand loyalty of consumers. In the industrial chocolate sector, the merger between *Klaus J Jacobs* and *SCIA* caused firms which were vertically integrated (both selling chocolate on the open market and using it for their own manufacturing) to cease selling on the open market because buyers were not willing to be reliant on their competitors in the downstream market.

5.4.5 Further mergers

Market structure changes caused by mergers can make further mergers in the same industry profitable. If a merger increased the efficiency of the merged firm then competitors in the market may need to merge in order to achieve similar efficiencies and remain competitive. This is also true when the merged firm can acquire a portfolio of products, which its customers prefer to buy together rather than from multiple suppliers, forcing other suppliers to offer similar portfolios of products.

The phenomenon of merger "waves" has long been noted empirically. Qiu and Zhou (2008) identify the possibility of merger waves solving the problem, which faces merging firms, that non-merging firms will "free ride" by benefiting from the higher prices caused by the merger while taking market share from the merged firm. If a large proportion of the firms in an industry merge then this strategic problem for the firms is ameliorated, though to the detriment of consumer welfare.

Because of the ambiguous interpretation of further mergers, it would be desirable to investigate these effects together with other effects, such as price changes.

12 mergers were mentioned as being followed by further mergers between firms in the same market. No mergers were specifically mentioned as not being followed by mergers – which is likely to be reporting bias (no reporting is more likely to mean no further mergers). In the cases where further mergers were mentioned, eight markets experienced only one further merger (including one where the merger was noted as purely vertical in nature, and another where the merged firm acquired another competitor). Two markets experienced two mergers, and one market (*William Hill*/*Stanley*) eight mergers following the merger being studied.

In all three of the cases where a merger followed prohibition by the competition authority (*BUPA*/*CHG*, *Littlewoods*/*Freemans*, *Lloyds*/*Abbey National*), the merger involved the attempted acquiree being acquired by a different firm. In two of these cases the firms were acquired by foreign firms (*BUPA*/*CHG* and *Lloyds*/*Abbey National*) as a means of entering the domestic market, and in the remaining case the firm was acquired by a smaller firm, leading to a smaller increase in concentration than if the initial merger had been approved.

Summary remark 13: *Very few studies looked at how dynamic effects develop post-merger. This is somewhat surprising because these dynamic effects are typically the most debated part of merger decisions, and therefore it would be useful to improve our knowledge on how these effects unfold after the merger. Most of what we know is on market entry, in which case the sample of studies suggests that in general CAs do a*

good job in predicting where entry can potentially eliminate short-term competition concerns in the market.

5.5 Other effects

In the final section we gathered information on some of the other effects that the studies in our sample looked at. The most widely analysed of these is buyer power. The effect of mergers on import is to some extent related to our market concentration discussion, as the typical concern is that constraints on import reduce competition. A few studies also looked at service quality.

Table 26: Qualitative evidence on other effects of mergers

		CA's decision		
		Approve	Remedy	Block
Buyer power	High	7	2	0
	Low	5	0	0
Imports	Increase	4	0	1
	Decrease	2	0	0
	No change	2	0	0
Service Quality	Increase	5	3	0
	Decrease	1	0	1
	No change	3	2	0

5.5.1 Buyer power

Downstream buyer power is often seen as being capable of countervailing increased upstream market power caused by a merger. Scott, Hviid and Lyons (2006) note that this is particularly true of cases where coordination, tacit or explicit, among firms is feared. Customers with large amounts of buyer power are able to change the design of the market, for example from an open market to a secret auction, to counteract collusion. Care should be taken that the market is not already distorted by downstream market power, however. Pass-through of higher prices to final consumers in these cases may be very high. For this reason it is useful to look at how the buyer power of downstream firms develops after the merger.

One of the cases where downstream buyer power played an important role was the *Klaus J Jacobs/SCIA* (industrial chocolate) merger. Here, customers had a very strong preference to have multiple suppliers of industrial chocolate in order to maintain a reliable supply in the face of potential refusal to supply by vertically-integrated manufacturers which compete with them in downstream markets. Following the merger to near-monopoly, customers immediately sponsored the entry of a new firm managed by the former management of the acquired firm. This new entrant invested heavily, and successfully acquired a large market share, thus entirely countering the increased market power of the merged firm.

Where governments are involved or are major customers, buyer power becomes even more central. The UK Competition Commission blocked a merger between *BUPA* and *CHG* in the private medical services market because of the increase in concentration it would cause. Following reform to NHS procurement procedures shortly after the blocking of the merger, seven new firms entered the market and the market became extremely competitive. The ex-post study points out that the CC could not have relied on the reforms producing such an effect given the uncertainty associated with them, but the

addition of an extremely power buyer to the market changed its structure completely. Buyer power was also extensively discussed in the *CHC/HSG* merger study (see below).

CHC/HSG (aka, Brintel/Bond)

CHC and HSG were owners of the second and third largest firms in the market for helicopter transport to offshore oil and gas rigs in the North Sea from the UK. The market became a duopoly after the UK Competition Commission approved it unconditionally. The CC made this decision on the basis of the market being receptive to entry and the high degree of market power possessed by the customers (which included large oil and gas producers). The key question in this case is whether harm from coordination would result from the duopoly in the market. This possibility was dismissed by the CC, partly because of the countervailing buyer power.

Following the merger, the merged firm substantially increased its market share, to over 63% of the market. While the market was shrinking when the merged occurred, it has since recovered and was growing quickly at the time of the study. One author of the study criticised the lack of a formal quantitative analysis of the merger by the CC.

5.5.2 Imports

Where domestic competitors in the relevant industry are lacking, imported products can provide a crucial competitive constraint on the merged firm to prevent harm to consumers. The reactions of customers and foreign firms after a merger are completely unpredictable. Foreign firms may find it difficult to enter the domestic market and customers may be unwilling to switch from a domestic to a foreign supplier. Although the sample is very small, it appears that studies taken at least 5 years after the merger were more likely to have found increased competitive pressure from imports (4/5 cases), which would imply that the reaction of foreign producers to a change in a domestic market may take years to manifest.

In the Swedish Competition Authority's study of *Optiroc Group AB/Stråbruken AB* imports did not increase as much as the CA had expected, leading to an increase in concentration and a fall in the level of rivalry between the firms. A new entrant prevented prices from rising above the pre-merger level but the willingness of customers to switch to imported products was lower than expected. Customers were also unwilling to switch to imported aluminium cans after the merger between *Impress Group* and *Alcan Packaging Sutton* in the UK[79]. Cans represent a small proportion of the costs of producers of canned goods, but are essential. Firms were unwilling to risk switching to a possibly unreliable new supplier for the sake of a saving in cost.

5.5.3 Service quality

The effect of an increase or a decrease in competition on the quality of the service offered is ambiguous and depends on whether firms compete on prices or some other characteristic of their products. Competition may force firms to offer a higher-quality product in order to attract and keep customers, or it may force them to decrease the quality of their product in order to cut prices. Retrospectives, especially interview-based studies, focus on the quality of the service offered to customers rather than the quality of the product offered. Poor service quality was seen by interview subjects as a definite indication that customers are in a weak position and are unable to switch suppliers.

In all studies that were conducted at least 5 years after the merger, an increase in service quality was reported. In the *Heinz/HP Foods* (food condiments) an unconditionally approved merger was followed by a drop in service quality as perceived

[79] UK Competition Commission - *Review of merger decisions under the Enterprise Act 2002*, 2009

by consumers. This was due to merger-induced rationalisation of production (the closure of a table sauce plant in Aston and a shift in production overseas). The quality of the service was restored later on.

5.6 Concluding thoughts on qualitative studies

There are some limitations to the analysis above. Firstly, it is based on a small sample that is mainly restricted to UK case studies. Second, the sample studies are dominantly qualitative assessments and as such are more difficult to review at an aggregate level. This review required us to categorise the findings and focus on simple comparable characteristics. Naturally, a lot of detail in the individual assessment is lost this way, therefore we advise the reader to use our systematic review together with the individual case studies. The tables in the Appendix should provide help in finding these studies.

Despite its limitations, this analysis still offers us with some useful insights. Firstly, it helps identify cases where the CA might have gone wrong with its decision. For example, mergers followed by a drop in rivalry and an increase in price would be obvious candidates for further assessment of the CA's decisions. Second, it highlights that the some of the merger effects and market self-correction (for example through entry) might take take time to unfold.

It seems clear that qualitative ex-post merger evaluations on the non-price effect of mergers are a useful exercise. However, to make it suitable for an aggregate assessment of the quality of CA decision-making (to identify potential systematic errors), it would be useful if the studies focused on the same set of relevant factors. We identified some of these and gathered them in three groups: the effect of the merger on market structure (market shares, concentration, rivalry, and market size), dynamic effects (innovation, investment, entry-exit, further mergers), and other effects (buyer power, imports and service quality).

6 Conclusion

This report provided a detailed review of ex-post studies on the merger decisions of EU competition authorities. Studies on the price and non-price effects of mergers were included in the review. One of our objectives was to replicate Kwoka's (2013) study on US merger retrospectives with EU merger cases. We concluded that in the sample of EU studies, mergers typically only increased prices by a small amount. Such price increases happened mainly in unconditionally approved mergers but to a small extent also in cases where the merger had been remedied. We also discussed the potential causes of the relative lack of merger retrospectives in Europe and concluded that this may be due to a deficit in appropriate incentives for the competition authority to carry out or to commission such studies.

An important contribution of this report is its overview of the methodologies used in merger retrospectives. We provide a detailed discussion of the assumptions needed for both Difference-in-Differences and Merger Simulation methods, the potential biases that may arise from violating these assumptions, and how the studies in our sample handled these methodological challenges. One key finding of this discussion is that any merger retrospective has to be taken sufficiently long after the merger was consummated, in order to allow time for all of its effects to unfold and the market to 'self-correct' these effects (if such self-correction takes place). Our methodological discussion, together with a technical appendix of the relevant methods, can be used as a reference guide for further merger retrospectives.

The report does not stop at summarising the individual price-effect estimates of the sample studies. We look at these estimates in combination with the merger decision to be able to identify cases where there was potentially an error in the merger decision. First, this required an assessment of whether the merger decision anticipated a price increase and whether there was indeed a price increase. Second, we tried to categorise these potentially erroneous decisions into four groups: (1) error in the analysis, (2) faulty evidence, (3) random variation, and (4) small price increase, and argue that we can only talk about a genuine error in the decision in the case of (1). The overwhelming majority of our sample studies do not seem to fall in this category. Nevertheless, the purpose of this exercise was not to identify where the authority made an error, rather to provide a framework, against which the authority (together with the authors of the ex-post study) can assess their decisions, identify errors, and – if necessary – change policies to eliminate future errors.

Finally, the report provides a review of qualitative studies that look at the non-price impact of mergers. We have a very imbalanced set of studies, with the UK providing almost all the relevant works. Nevertheless, the sample size is sufficiently large to merit aggregate assessment. We grouped the merger effects that were studied into three categories: effects on market structure (concentration, market shares, etc.), dynamic effects (innovation, further mergers, etc.), and other effects (buyer power, service quality, etc.). Our conclusion is that conducting simple studies on the non-price impact of the merger can provide an invaluable source of information and authorities should be given the right incentives to engage in such exercises on a regular basis.

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Appendix

A1. Tables

A1.1. Price-effect studies

Table 27: List of price-effect studies

Merger	Study	Jurisdiction	Year of decision	Industry
Agip/Esso	Csorba, Koltay and Farkas (2011) "Separating the ex post effects of mergers An analysis of structural changes on the Hungarian retail gasoline market"	EU	2007	G47.3.0 - Retail sale of automotive fuel in specialised stores
Bass/Carlsberg–Tetley	Slade, Margaret E. "Merger simulations of unilateral effects: what can we learn from the UK Brewing Industry?" (2006).	UK	1997	C11.0.5 - Manufacture of beer
Carlsberg/Pripps	Romahn, & Friberg (2014) "Divestiture Requirements as a Tool for Competition Policy: A Case from the Swedish Beer Market"	SV	2001	C11.0.5 - Manufacture of beer
Carlsberg/Pripps	Romahn & Friberg (2014) "Divestiture Requirements as a Tool for Competition Policy: A Case from the Swedish Beer Market" Working paper	SV	2001	C11.0.5 - Manufacture of beer
Carrefour/Promodes	Allain, et al. *The impact of retail mergers on food prices: evidence from France*. No. 181943. 2013.	FR	2000	G47.1.1 - Retail sale in non-specialised stores with food, beverages or tobacco predominating
Cerealia/Schulstad	Nilsson & Strand (2005). "On Simulation and Reality: a Swedish example". In P.A.G. van Bergeijk & E. Kloosterhuis (Eds.), *Modelling European mergers: theory, competition policy and case studies* (pp. 184-189)	SV	2003	C10.7.1 - Manufacture of bread; manufacture of fresh pastry goods and cakes

CVRD/Caemi	Lundmark and Wårell (2008) "Horizontal mergers in the iron ore industry—An application of PCAIDS." *Resources Policy*	EU	2001	B7.1.0 - Mining of iron ores
DISA/Shell	Perdiguero and González (2012)"Mergers and difference-in-difference estimator: why firms do not increase prices?" *IREA–Working Papers*	ES	2004	G47.3.0 - Retail sale of automotive fuel in specialised stores
Easyjet/Go Fly	Dobson and Piga (2013) "The impact of mergers on fares structure: Evidence from European low-cost airlines" *Economic Inquiry*	EU	2002	H51.1.0 - Passenger air transport
Erasmus MC ziekenhuis/Havenziekenhuis Rotterdam	Kemp, Kersten, and Severijnen. (2012) "Price effects of Dutch hospital mergers: an ex-post assessment of hip surgery." *De Economist*.	NL	2005	Q86.1.0 - Hospital activities
Game/Gamestation	Aguzzoni, Argentesi, Buccirossi, Ciari, Duso, Tognoni, & Vitale, (2013). They played the merger game: A retrospective analysis in the UK videogames market.	UK	2008	G47.6.5 - Retail sale of games and toys in specialised stores **or** G47.4.1 - Retail sale of computers, peripheral units and software in specialised stores
GlaxoSmithKline/AstraZeneca Tica	Björnerstedt, & Verboven, (2013). "Does merger simulation work? Evidence from the Swedish analgesics market" Working paper, Katholieke Universiteit Leuven	SW	2009	C21.1.0 - Manufacture of basic pharmaceutical products
GlaxoSmithKline/AstraZeneca Tica	Björnerstedt, J., & Verboven, F. (2013). *Does merger simulation work? Evidence from the Swedish analgesics market.* Working paper, Katholieke Universiteit Leuven	SV	2009	C21.1.0 - Manufacture of basic pharmaceutical products
Lukoil/Jet	Csorba, Koltay and Farkas (2011) "Separating the ex post effects of mergers An analysis of structural changes on the Hungarian retail gasoline market"	EU	2007	G47.3.0 - Retail sale of automotive fuel in specialised stores
Medisch Centrum Alkmaar/Gemini Ziekenhuis	Kemp, Kersten, and Severijnen. (2012) "Price effects of Dutch hospital mergers: an ex-post assessment of hip surgery." *De Economist*.	NL	2007	Q86.1.0 - Hospital activities
Morrisons/Safeway	Skrainka (2012) "The Geography of Grocery Demand in the UK: An Evaluation of the 2003 Morrisons-Safeway Merger" mimeo, University of Chicago.	UK	2004	G47.1.1 - Retail sale in non-specialised stores with food, beverages or tobacco predominating
Rio Tinto/North	Lundmark and Wårell (2008) "Horizontal mergers in the iron ore industry—An application of PCAIDS." *Resources Policy*	EU	2000	B7.1.0 - Mining of iron ores

Ryanair/Buzz	Dobson and Piga (2013) "The impact of mergers on fares structure: Evidence from European low-cost airlines" *Economic Inquiry*	EU	2003	H51.1.0 - Passenger air transport
Scottish & Newcastle/Courage	Slade (2004) "Mergers, brand competition, and the price of a pint" European Economic Review	UK	1995	C11.0.5 - Manufacture of beer
Shell/Rontec (Total-branded stations)	Office of Fair Trading (2014) – "Shell - Rontec An evaluation of the OFT's conditional clearance of the merger"	UK	2012	G47.3.0 - Retail sale of automotive fuel in specialised stores
St. Lucas Ziekenhuis/Delfzicht ziekenhuis	Kemp, Kersten, and Severijnen. (2012) "Price effects of Dutch hospital mergers: an ex-post assessment of hip surgery." *De Economist*.	NL	2008	Q86.1.0 - Hospital activities
Vinci/GTM	Choné and Linnemer (2012) "A treatment effect method for merger analysis with an application to parking prices in Paris." *The Journal of Industrial Economics*	FR	2001	Car parking
Volvo/Scania	Ivaldi and Verboven (2005) "Quantifying the effects from horizontal mergers in European competition policy" Internation Journal of Industrial Economics	EU	2000	C29.1.0 - Manufacture of motor vehicles
Waterstones/Ottokars	Aguzzoni, Argentesi, Ciari, Duso, & Tognoni, (2013). Ex-post merger evaluation in the UK retail market for books.	UK	2006	G47.6.1 - Retail sale of books in specialised stores
Ziekenhuis Bethesda/Scheperziekenhuis	Kemp, Kersten, and Severijnen. (2012) "Price effects of Dutch hospital mergers: an ex-post assessment of hip surgery." *De Economist*.	NL	2009	Q86.1.0 - Hospital activities
Ziekenhuis Hilversum/Ziekenhuis Gooi-Noord	Kemp, Kersten, and Severijnen. (2012) "Price effects of Dutch hospital mergers: an ex-post assessment of hip surgery." *De Economist*.	NL	2005	Q86.1.0 - Hospital activities
Ziekenhuis Walcheren/Oosterscheldeziekenhuizen	Kemp, Kersten, and Severijnen. (2012) "Price effects of Dutch hospital mergers: an ex-post assessment of hip surgery." *De Economist*.	NL	2009	Q86.1.0 - Hospital activities

Table 28: Market Structure in price-effect studies

Merger	Market share of acquiring firm (%)	Market share of acquired firm (%)	HHI post-merger	Number of firms post-merger
Agip/Esso	9	3	1504	8 + fringe
Bass/Carlsberg–Tetley	23	14	2332	5+fringe
Carlsberg/Pripps	29	17	2500	8
Carlsberg/Pripps	29	17	2500	8
Carrefour/Promodes	20.2	10.3	1534	5+fringe
Cerealia/Schulstad	44	16	3662	3+fringe
CVRD/Caemi	30	10		>10
DISA/Shell	33	18	3084	6
Easyjet/Go Fly				5 low cost, 8 full service
Erasmus MC ziekenhuis/Havenziekenhuis Rotterdam	0.48	0.66		1
Game/Gamestation	29	10.6		
GlaxoSmithKline/AstraZeneca Tica	31.5	10.6	3867	5
GlaxoSmithKline/AstraZeneca Tica	31.5	10.6	3867	5
Lukoil/Jet	4	3	1504	8 + fringe
Medisch Centrum Alkmaar/Gemini Ziekenhuis	1.47	0.8		
Morrisons/Safeway	2.5	5.3	693	15
Rio Tinto/North				>10
Ryanair/Buzz				4 low cost, 8 full service
Scottish & Newcastle/Courage	11	19		6+fringe
Shell/Rontec (Total-branded stations)				
St. Lucas Ziekenhuis/Delfzicht ziekenhuis				
Vinci/GTM	35	20	2975	6+fringe
Volvo/Scania	15	16	1957	6
Waterstones/Ottokars	14.1	7.7	873	5 + fringe

Ziekenhuis Bethesda/Scheperziekenhuis	0.32	1.09		
Ziekenhuis Hilversum/Ziekenhuis Gooi-Noord				9+
Ziekenhuis Walcheren/Oosterscheldeziekenhuizen				

Table 29: Data used in price-effect studies

Merger	Main source	Time span	Number of time periods	Size of cross-section (mean)
Agip/Esso	holtankoljak.hu public price data	2007-2008	96	1303
Bass/Carlsberg–Tetley	Nielson scanner data	1995	2	222
Carlsberg/Pripps	Swedish retail monopoly for alcohol	2000, 2001-2003	34	139
Carlsberg/Pripps	Swedish retail monopoly for alcohol	1996-2003	73	
Carrefour/Promodes	TNS Worldpanel scanner data	1998-2001	8	76860
Cerealia/Schulstad	Scanner data from merging parties	2002	1	28
CVRD/Caemi	Raw Materials Data Group and UNCTAD	1999-2000		
DISA/Shell		2003-2005	28	7
Easyjet/Go Fly	Original data scraped from internet	2002-2005	36	
Erasmus MC ziekenhuis/Havenziekenhuis Rotterdam	NZa - Netherlands Healthcare Authority	2005-2010	6	808
Game/Gamestation	CC investigation	2007 and 2010	2	1791
GlaxoSmithKline/AstraZeneca Tica	Apoteket AB - Swiss state pharmacy retail monopoly	2008-2011	168	43
GlaxoSmithKline/AstraZeneca Tica	Apoteket AB - Swiss state pharmacy retail monopoly	1995-2008	168	43
Lukoil/Jet	holtankoljak.hu public price data	2007-2008	96	1303
Medisch Centrum Alkmaar/Gemini Ziekenhuis	NZa - Netherlands Healthcare Authority	2005-2010	6	808
Morrisons/Safeway	TNS Worldpanel scanner data	2003-2004		9640
Rio Tinto/North	Raw Materials Data Group and UNCTAD	1999-2000		
Ryanair/Buzz	Original data scraped from internet	2002-2005	36	
Scottish & Newcastle/Courage	Nielson scanner data	1995	2	222

Shell/Rontec (Total-branded stations)	Experian Catalist ltd	2011 to 2013	118	12
St. Lucas Ziekenhuis/Delfzicht ziekenhuis	NZa - Netherlands Healthcare Authority	2005-2010	6	808
Vinci/GTM	City of Paris and , 'Le guide des parkings de Paris'	2000-2001	8	379
Volvo/Scania	Survey by authors' Merger Task Force	1998	2	
Waterstones/Ottokars	CC investigation	2004-2007	48	12000
Ziekenhuis Bethesda/Scheperziekenhuis	NZa - Netherlands Healthcare Authority	2005-2010	6	808
Ziekenhuis Hilversum/Ziekenhuis Gooi-Noord	NZa - Netherlands Healthcare Authority	2005-2010	6	808
Ziekenhuis Walcheren/Oosterscheldeziekenhuizen	NZa - Netherlands Healthcare Authority	2005-2010	6	808

Table 30: Estimation methods in price-effect studies

Merger	Methodology	Counterfactual	Robustness checks	Controls used
Agip/Esso	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms	Different merger timings, multiple control groups, multiple model specifications	Station fixed effects, time fixed effects, market structure, costs
Bass/Carlsberg–Tetley	Merger simulation-based, price effects estimated		Multiple model specifications	Product characteristics, cost of retailing, level of market penetration
Carlsberg/Pripps	Difference-in-differences method, price effects estimated	Competitors' prices	Different merger timings, multiple control groups, multiple model specifications	Product fixed effects
Carlsberg/Pripps	Merger simulation-based, price effects estimated		Different merger timings, multiple model specifications	Product characteristics, marketing expenditure
Carrefour/Promodes	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms (alternative PSM-based controls also)	Different merger timings, multiple control groups, multiple model specifications	Store, product and product-time fixed effects
Cerealia/Schulstad	Merger simulation-based, price effects estimated			None
CVRD/Caemi	Merger simulation-based, price effects estimated			
DISA/Shell	Difference-in-differences method, price effects estimated. Also a coordinated effects simulation	Local markets with no merger	Multiple model specifications	Seasonal trend, cost variables, local market structure
Easyjet/Go Fly	Difference-in-differences method, price effects estimated	PSM	Multiple control groups	Flight characteristics, destination city characteristics, number of route competitors

Erasmus MC ziekenhuis/Havenziekenhuis Rotterdam	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms. All other Netherlands hospitals	Alternative control, multiple model specifications	Hospital characteristics, local area characteristic, local market structure, quality
Game/Gamestation	Difference-in-differences method, price effects estimated plus before-and-after comparison	Competitors' prices	Multiple control groups, multiple model specifications	Product characteristics, vertical integration of supply chain
GlaxoSmithKline/AstraZeneca Tica	Difference-in-differences method, price effects estimated	Competitors' prices	Multiple model specifications	Product manufacturer, product type
GlaxoSmithKline/AstraZeneca Tica	Merger simulation-based, price effects estimated		Multiple model specifications	Seasonal trend, product characteristics, marketing expenditure, numbers of sick men and women
Lukoil/Jet	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms	Different merger timings, multiple control groups, multiple model specifications	Station fixed effects, time fixed effects, market structure, costs
Medisch Centrum Alkmaar/Gemini Ziekenhuis	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms. All other Netherlands hospitals	Alternative control, multiple model specifications	Hospital characteristics, local area characteristic, local market structure, quality
Morrisons/Safeway	Merger simulation-based, price effects estimated			Consumer household characteristics, identity of supermarket
Rio Tinto/North	Merger simulation-based, price effects estimated			
Ryanair/Buzz	Difference-in-differences method, price effects estimated	PSM	Multiple control groups	Flight characteristics, destination city characteristics, number of route competitors
Scottish & Newcastle/Courage	Merger simulation-based, price effects estimated		Multiple model specifications	Product characteristics, cost of retailing, level of market penetration

Shell/Rontec (Total-branded stations)	Difference-in-difference, price effects estimated	PSM	Different merger timings, multiple control groups, multiple model specifications	Past prices, wholesale price
St. Lucas Ziekenhuis/Delfzicht ziekenhuis	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms. All other Netherlands hospitals	Alternative control, multiple model specifications	Hospital characteristics, local area characteristic, local market structure, quality
Vinci/GTM	Difference-in-differences method, price effects estimated	An iterative construction of control using local markets	Multiple control groups	Station fixed effects, quarterly trend
Volvo/Scania	Merger simulation-based, price effects estimated		Multiple model specifications	
Waterstones/Ottokars	Difference-in-differences method, price effects estimated	Local markets - PSM. National market - rival firms prices and prices of widely-sold bestsellers	Multiple control groups, multiple model specifications	Seasonal trend, local area characteristics, local market structure, product characteristics, cost variables
Ziekenhuis Bethesda/Scheperziekenhuis	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms. All other Netherlands hospitals	Alternative control, multiple model specifications	Hospital characteristics, local area characteristics, local market structure, quality
Ziekenhuis Hilversum/Ziekenhuis Gooi-Noord	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms. All other Netherlands hospitals	Alternative control, multiple model specifications	Hospital characteristics, local area characteristic, local market structure, quality
Ziekenhuis Walcheren/Oosterscheldeziekenhuizen	Difference-in-differences method, price effects estimated	Local markets with only non-merging firms. All other Netherlands hospitals	Alternative control, multiple model specifications	Hospital characteristics, local area characteristics, local market structure, quality

A1.2. List of non-price studies

Table 31: List of non-price effect studies

Study	Firms	Jurisdiction	Decision Year
Academic studies			
Chakreborty et al. (2014)	Morrisons/Safeway	UK	2004
Coppi and Dobson (2002)	Interbrew/Bass	UK	2001
Leheyda et al. (2003)	Pfizer/Pharmacia	CH	2002
Agency studies			
Competition Commission (2003)	Littlewoods/Freemans	UK	1997
	National Express/Central Trains	UK	1997
	BSkyB/Manchester United	UK	1999
	CHC/Helicopter	UK	2000
Competition Commission (2005)	Nutricia/Milupa	UK	1996
	Nutricia/Milupa	UK	1996
	Klaus J Jacobs/SCIA	UK	1997
	Cowie/British Bus	UK	1997
	Technicolor/Metrocolor	UK	1997
	Universal Foods/Pointing	UK	1999
	CHC/Helicopter	UK	2000
	NTL/Cable and Wireless	UK	2000
	Kodak/Colourcare	UK	2001
	Cargill/Cerestar	UK	2002
	Compass/Rail Gourmet	UK	2002
Competition Commission (2006)	SDEL/Coors	UK	2006
	Morrisons/Somerfield	UK	2005
	Deutsche Borse/LSE	UK	2005
	Euronext/LSE	UK	2005
	Vue/Ster	UK	2006
Competition Commission (2008)	British United Provident Association/Community Hospitals Group	UK	2000
	British United Provident Association/Community Hospitals Group	UK	2000
	Lloyds TSB/Abbey National	UK	2001
	Reed Elsevier/Harcourt General	UK	2001

Competition Commission (2009)	Impress Group/Alcan Packaging Sutton	UK	2005
	Boots/Unichem Alliance	UK	2006
	William Hill/Stanley	UK	2006
	Heinz/HP Foods	UK	2006
	Cott Beverages/Macaw Holdings	UK	2006
	English Welsh and Scottish Railways/Marcroft Engineering	UK	2006
	Knauf Insulation/Superglass Insulation	UK	2004
	DS Smith/LINPAC Containers	UK	2004
Competition Commission (2010)	Alanod/Ano-Coil	UK	2000
	Sibelco/Fife	UK	2001
	Coloplast/SSL	UK	2002
	Centrica/Dynegy	UK	2003
	Draeger/Hill-Rom	UK	2004
	Emap/ABI	UK	2005
	Somerfield/Morrisons	UK	2005
	Stericycle/Sterile Technologies Group	UK	2006
LEAR (2006)	Pirelli/BICC	EU	2000
Netherlands Competition Authority (2004)	Anonymous	NL	Not given
	Anonymous	NL	Not given
	Anonymous	NL	Not given
	Anonymous	NL	Not given
	Anonymous	NL	Not given
Swedish Competition Authority (2001)	Optiroc Group AB/Stråbruken AB	SV	1998

Table 32: Effect on market structure (non-price studies)

Merger	Prices	Share of merged firm	Concentration	Rivalry	Market Size
Alanod/Ano-Coil - Aluminium	No change		Increase		
Anonymous Netherlands Case VI	Increase	Decrease	No change		
Anonymous Netherlands Case VIII	Increase				
Anonymous Netherlands Case X	Increase				
Boots/Unichem Alliance - Healthcare		Increase	Increase		
British United Provident Association/Community Hospitals Group - Private medical services	Decrease	Decrease	Decrease	Increase	Increase
British United Provident Association/Community Hospitals Group - Private medical insurance	Increase	Increase	Increase		Increase
Cargill/Cerestar - Glucose syrups	Increase				
CHC/Helicopter - Helicopter services		Increase			Increase
CHC/Helicopter - Helicopter services	Increase			Decrease	Decrease
Coloplast/SSL - Medical supplies		Increase			
Compass/Rail Gourmet - Train catering		Increase	Increase	Decrease	
Cott Beverages/Macaw Holdings - Beverages	No change	No change		Decrease	
Cowie/British Bus - Bus services		Decrease	Increase	Decrease	Increase
Deutsche Borse/LSE - Finance	Increase				
Draeger/Hill-Rom - Medical supplies	Decrease	Decrease			Decrease
DS Smith/LINPAC Containers - Packaging	Increase		No change		
Emap/ABI - Media	No change		Increase		
English Welsh and Scottish Railways/Marcroft Engineering - Rail Freight Maintainance	Increase		No change		
Euronext/LSE - Finance	Increase				
Heinz/HP Foods - Food	Increase		No change		Increase
Impress Group/Alcan Packaging Sutton - Cans	Increase		No change		

Interbrew/Bass - Brewing	Decrease				
Klaus J Jacobs/SCIA - Industrial chocolate		Decrease	Decrease		
Knauf Insulation/Superglass Insulation - Insulation	Increase				Increase
Kodak/Colourcare - Wholesale photograph development	No change			Decrease	
Littlewoods/Freemans - Agency-model mail-order products					Decrease
Lloyds TSB/Abbey National - Personal current accounts	Decrease	Decrease	No change	Decrease	
Morrisons/Safeway - Supermarkets	Decrease		Increase	Increase	
NTL/Cable and Wireless - Pay TV	Increase	Decrease	Increase		
Nutricia/Milupa - Baby meals	Decrease	Decrease	Increase		
Nutricia/Milupa - Baby milk	Decrease	Decrease	Increase		
Optiroc Group AB/Stråbruken AB - Roofing materials	No change	Decrease	Increase	Decrease	Decrease
Pfizer/Pharmacia - Pharmaceuticals	No change	No change			
Pirelli/BICC - Power cable systems	Decrease	No change		Increase	Decrease
Reed Elsevier/Harcourt General - Publishing	Increase				
SDEL/Coors - Beer dispensing equipment	Increase				
Stericycle/Sterile Technologies Group - Medical services	Increase	No change			No change
Technicolor/Metrocolor - Film processing	Decrease	Decrease	Increase	Increase	No change
Total/ - Petrol					
Universal Foods/Pointing - Synthetic food colourings	Decrease	Decrease		Increase	No change
Vue/Ster - Cinemas	Increase				
William Hill/Stanley - Betting		Increase	Increase		

Table 33: Dynamic effects (non-price studies)

Merger	Innovation	Capacity/Investment	Entry	Exit	Further mergers
Anonymous Netherlands Case IX	Increase				
Anonymous Netherlands Case VI			Yes		
Boots/Unichem Alliance - Healthcare	Increase		No		Yes
British United Provident Association/Community Hospitals Group - Private medical services	Increase	Increase	Yes		Yes
British United Provident Association/Community Hospitals Group - Private medical insurance	Increase	Increase			
Cargill/Cerestar - Glucose syrups		Increase	No	No	
Centrica/Dynegy - Gas storage					
CHC/Helicopter - Helicopter services			Yes		
Coloplast/SSL - Medical supplies					Yes
Cott Beverages/Macaw Holdings - Beverages	Increase	Decrease	Yes	No	
Cowie/British Bus - Bus services			Yes		Yes
Draeger/Hill-Rom - Medical supplies			Yes	Yes	
DS Smith/LINPAC Containers - Packaging		Decrease	Yes		Yes
Emap/ABI - Media			No		
English Welsh and Scottish Railways/Marcroft Engineering - Rail Freight Maintenance	Increase		No		
Heinz/HP Foods - Food	Increase		Yes		
Impress Group/Alcan Packaging Sutton - Cans			No		
Interbrew/Bass - Brewing					Yes
Klaus J Jacobs/SCIA - Industrial chocolate		Increase	Yes	Yes	Yes
Knauf Insulation/Superglass Insulation - Insulation	Increase	Increase	Yes		
Kodak/Colourcare - Wholesale photograph development		Decrease			

Littlewoods/Freemans - Agency-model mail-order products	Decrease				Yes
Lloyds TSB/Abbey National - Personal current accounts	Increase		No		Yes
NTL/Cable and Wireless - Pay TV	Increase			Yes	
Nutricia/Milupa - Baby meals	Increase		Yes		
Nutricia/Milupa - Baby milk			No	Yes	
Optiroc Group AB/Stråbruken AB - Roofing materials	Increase	Increase	Yes		
Pfizer/Pharmacia - Pharmaceuticals	No change	Decrease	Yes		
Pirelli/BICC - Power cable systems		Decrease			
Stericycle/Sterile Technologies Group - Medical services			Yes	Yes	Yes
Universal Foods/Pointing - Synthetic food colourings		Decrease			Yes
William Hill/Stanley - Betting	Increase				Yes

Table 34: Other merger effects (non-price studies)

Merger	Consumer welfare	Buyer power	Imports	Service Quality
Alanod/Ano-Coil - Aluminium		Increase		
Anonymous Netherlands Case IX				Increase
Boots/Unichem Alliance - Healthcare		Decrease		Increase
British United Provident Association/Community Hospitals Group - Private medical services	Increase			
British United Provident Association/Community Hospitals Group - Private medical insurance			Increase	
Cargill/Cerestar - Glucose syrups		Decrease	No change	
CHC/Helicopter - Helicopter services	Decrease		Increase	
Compass/Rail Gourmet - Train catering		Increase		No change
Cott Beverages/Macaw Holdings - Beverages			Decrease	No change
Cowie/British Bus - Bus services		Increase		Increase
Deutsche Borse/LSE - Finance	Decrease			
DS Smith/LINPAC Containers - Packaging			Increase	
English Welsh and Scottish Railways/Marcroft Engineering - Rail Freight Maintenance			Decrease	
Euronext/LSE - Finance	Decrease			
Heinz/HP Foods - Food				Decrease
Impress Group/Alcan Packaging Sutton - Cans		Increase	Decrease	Increase
Interbrew/Bass - Brewing		Increase		
Klaus J Jacobs/SCIA - Industrial chocolate		Decrease	No change	
Knauf Insulation/Superglass Insulation - Insulation				
Kodak/Colourcare - Wholesale photograph development		Increase		Increase
Morrisons/Somerfield - Supermarkets	Decrease			Decrease
NTL/Cable and Wireless - Pay TV		Decrease		Increase
Nutricia/Milupa - Baby meals		Increase		
Nutricia/Milupa - Baby milk		Increase		
Optiroc Group AB/Stråbruken AB - Roofing materials			Increase	No change
Pirelli/BICC - Power cable systems	Increase			
SDEL/Coors - Beer dispensing equipment	Decrease			Decrease
Technicolor/Metrocolor - Film processing		Increase	Increase	
Universal Foods/Pointing - Synthetic food colourings			Increase	
Vue/Ster - Cinemas	Decrease			
William Hill/Stanley - Betting				Increase

A2. Technical Appendix

The following discussion contains more technical explanation to some of the specific issues related to the two evaluation methods, Difference-in-Differences and Merger Simulation. The purpose of this appendix is to supplement the discussion in Section 3 and it is aimed at readers with some background in econometrics.

A2.1. Difference-in-Differences

A2.1.1. Causal analysis

Causal analysis has developed attractive econometric tools that are suitable for policy evaluations.[80] The core of the methodology is identification of causal effects in the presence of non-random assignments of the treatment. The challenging part of the task is to design a proper counterfactual and find proper data on the factual and counterfactual.

In what follows we outline the methodology using the notation provided in Angrist and Pischke (2009), which is now widely used in causal inference literature. Suppose that from a population we observe an *outcome* variable (say price) Y_i and a binary *treatment* variable D_i which can take values 0 or 1. In the context of mergers the subscript i is the identifier of the product or firm (we now refer to product). The outcome variable is typically the price and the treatment variable a dummy variable, which is one for any product involved in the merger. Consider two potential outcomes for the product i, denoted respectively as:

$$Y_i = \begin{Bmatrix} Y_{i1} \; if \; D_i = 1 \\ Y_{i0} \; if \; D_i = 0 \end{Bmatrix} \tag{1}$$

The problem is that each observation i is the realization of only one of the two outcomes, depending on whether the *treatment* (merger) happens $D_i = 1$, or not, $D_i = 0$. We are interested in the *treatment (merger) effect*, which is computed as the difference between the treatment and its absence.

Now assume that the outcome variable Y_i is price for product i and that the producer of i merges with another firm (i.e. the *treatment* is a merger). For product i the effect of the merger captured by the equality $\beta = Y_{i1} - Y_{i0}$. However the identification and the measurement of this effect is logically impossible because we do not observe the same product i both engaging in the merger and not engaging in a merger. Consequently, we cannot infer the effect of the merger because we do not have the counterfactual evidence, i.e. what would have happened with the same product i had it not been involved in a merger.

Therefore, instead of focusing on just the individual (product) we focus on the *average causal effect* for the entire population (large sample), or for sub-groups. The *average treatment effect* (ATE) is expressed in equation form as:

$$E(\Delta Y_i) = E(Y_{i1} - Y_{i0}) = E(Y_{i1}) - E(Y_{i0}) \tag{2}$$

Assuming a *constant* treatment effect, the price for product i can be formulated, disregarding for simplicity additional controls, as the linear regression equation:

[80] See Khandker et al (2010).

$$Y_i = \underbrace{\beta_0}_{E(Y_{i0})} + \underbrace{\beta_1}_{Y_{i1}-Y_{i0}} D_i + \underbrace{\varepsilon_i}_{Y_{i0}-E(Y_{i0})} \tag{3}$$

From which we can get the individual conditional expectations for the merger and non-merger groups respectively:

$$\begin{aligned} E(Y_i|D_i = 1) &= \beta_0 + \beta_1 + E(\varepsilon_i|D_i = 1) \\ E(Y_i|D_i = 0) &= \beta_1 + E(\varepsilon_i|D_i = 0) \end{aligned} \tag{4}$$

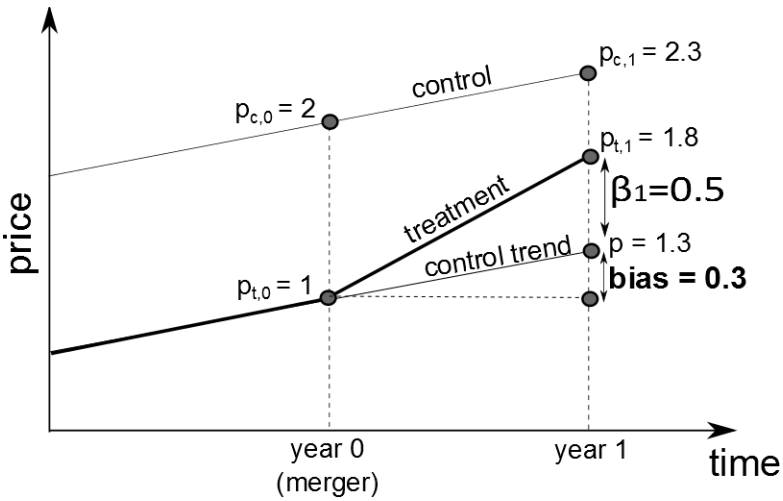
The evaluation of the treatment effect β_1 (key parameter) in Eq.(3) is given in full by the difference between conditional moments:

$$E(Y_i|D_i = 1) - E(Y_i|D_i = 0) = \beta_1 + \underbrace{E(\varepsilon_i|D_i = 1) - E(\varepsilon_i|D_i = 0)}_{bias} \tag{5}$$

Eq.(5) shows that a simple cross-section (merger and no-merger) analysis would lead to a biased estimate if the error term is correlated with the merger decision, i.e. if the merging firms/product are different from the non-merging ones.

Using the Figure 3 and assuming that $D_i = 0$ pre-merger, and $D_i = 1$ post-merger we can quantify the difference in expectations as: $\underbrace{E(Y_i|D_i = 1)}_{1.8} - \underbrace{E(Y_i|D_i = 0)}_{1} = \underbrace{\beta_1}_{0.5} + \underbrace{bias}_{0.3}$.

Figure 3: Bias in the DiD estimator



Put differently, the reason for the bias when it comes to mergers is that merging companies are not chosen at random, but through self-selection, and those firms involved in a merger would do better (or worse) than others regardless of the merger taking place. Typically the difficulty of any causal effects model is to find a comparison group which has the same characteristics as (behaves similarly to) the products/firms selected for the merger.

The causal inference econometric literature has developed techniques that are suitable to cope with or limit the *selection bias* using methods like: Differences-in-Differences, triple differences, instrumental variables, regression discontinuity design, structural modelling. In the next subsection we describe the DiD method, which is the most commonly used method in the merger evaluation literature.

A2.1.2. Specific issues to complement discussion on Difference-in-Differences

Given a period before the merger, t=0, and one period after the merger has taken place, t=1, the DiD can be identified as the difference in expectations:

$$\mathrm{DiD} = E\left(Y_{i1}^{T} - Y_{i0}^{T} \middle| D_{i1} = 1\right) - E\left(Y_{i1}^{C} - Y_{i0}^{C} \middle| D_{i1} = 0\right) \quad (6)$$

Where Y_{i0}^{T} and Y_{i1}^{T} are the prices charged by the merging firms before and after the merger respectively. $D_{i1} = 1$ if the merging firms merged, and $D_{i1} = 0$ is the counterfactual scenario where the merging firms did not merge. Naturally, we do not observe the latter case. Instead we choose the Control so that it is as similar to the Treatment as possible – the only difference being that it did not experience the merger.

The corresponding regression equation to produce a DiD estimator, omitting other controls for notational simplification, is:

$$Y_{it} = \beta_0 + \beta_1 D_{i1} t + \beta_2 D_{i1} + \beta_3 t + \varepsilon_{it} \quad (7)$$

and under regularity conditions the difference-in-differences estimator: $\mathrm{DiD} = \beta_1$. As formally shown in the Technical Appendix, the DiD estimator is an unbiased estimator, under regularity conditions. However the research design relies on some key assumptions, without which DiD estimates are going to be biased.

Testing for parallel trends

To do this, for all pre-merger price data of a merger occurring at time t, we denote with d_m the dummy variable for each period pre-merger. If we have price data for q periods pre-merger then $m = 1, \dots, q$. To test for parallel trends before the merger, we estimate:

$$Y_{it_m} = \beta_0 + \sum_{m=1}^{q} d_{\tau-m}(\gamma_m + \delta_m D_{i1}) + \varepsilon_{it}, \quad t > \tau > q. \quad (8)$$

Where δ_m is the difference between Control and Treatment, therefore we simply need to test if this difference is constant for all $m \in \{1, \dots, q\}$.

Testing for serial correlation

Serial correlation (autocorrelation) is the correlation between members of series of observations ordered in time.[81] Serially correlation is mostly an issue with long time series. In a DiD context this means that the error terms at different time periods are correlated: $E(u_t, u_{t+s}) \neq 0$, where $s \neq 0$.

There are various ways of detecting autocorrelation:

- Graphical method: Although graphical methods do not offer a formal test, they are invaluable in that they offer an easy to understand picture of an often complex problem. Plotting the error terms against time can reveal if there is a patter in the error terms that might suggest that the error terms are not random. To do this, one would need to estimate the DiD model and use the estimated coefficients to predict the error terms.
- Durbin-Watson d test: This is probably the most frequently used test for detecting serial correlations. It provides a simple test that works in most

[81] We can also talk about spatial autocorrelation but for the purposes of this report we limit our discussion to the time-dimension.

circumstances (although cannot be used in autoregressive models – models that include lagged terms). It uses the sum of squared differences in the successive residuals.

$$d = \frac{\sum_{t=2}^{t=n}(\hat{u}_t - \hat{u}_{t-1})^2}{\sum_{t=2}^{t=n} \hat{u}_t{}^2}$$

The mechanism of the test is very simple. In the first step we estimate the DiD model using OLS and predict the residuals and use them to compute d. We then find the critical values (d_L and d_U) from statistical tables. From these we have evidence of autocorrelation if $d < d_L$ or $d > 4 - d_L$.

- Breusch-Godfrey test: To avoid some of the issues with the Durbin-Watson test (e.g. the use of lagged variables). First we estimate the DiD model using OLS and obtain the residuals, $\hat{u}_t$. Then we regress $\hat{u}_t$ on all the regressors in the original DiD model to obtain R^2. If the sample size is large, then $(n-p)\,R^2 \sim \chi_p^2$, where n is the number of observations and p is the order of the autoregressive process. If $(n-p)\,R^2$ exceeds the given critical value (depending on the chosen level of significance) then we can reject the null that there is no autocorrelation.
- Finally, Wooldridge (2002) offers an autocorrelation test for panel data. First we take the first difference of the outcome and the independent variables: $\Delta Y_{it} = Y_{it} - Y_{it-1}$, and $\Delta \mathbf{X}_{it} = \mathbf{X}_{it} - \mathbf{X}_{it-1}$ and regress ΔY_{it} on $\Delta \mathbf{X}_{it}$ to acquire the first-difference (FD) estimator $\widehat{\boldsymbol{\beta}_{\mathbf{FD}}}$ and the residuals $\widehat{\Delta\varepsilon}_{it} = \hat{\varepsilon}_{it} - \hat{\varepsilon}_{i,t-1}$. Then we regress these residuals in an autoregressive model: $\widehat{\Delta\varepsilon}_{it} = \rho\widehat{\Delta\varepsilon}_{i,t-1} + error_{it}$. A finding of ρ being significantly different from zero (i.e. significant serial correlation in the $\widehat{\Delta\varepsilon}_{it}$ warrants computing the robust variance matrix for the FD estimator.

If evidence for autocorrelation has been found then we can proceed in various ways:

- *Aggregating data* (for example from daily to monthly observations) might eliminate non-stationarity and also the serial correlation (if the source of autocorrelation is non-stationarity) however it comes at the expense of losing some information.
- If there is a sufficient number of cross-sections, the time-series dimension could be collapsed to a *before and after stage*.
- *GLS models* could be used (instead of OLS), although Bertrand et al (2004) warn of their ineffectiveness in DiD studies. Simple GLS is typically not feasible because it requires that one knows the variance-covariance matrix of the error term up to a multiplicative scalar. However one can use feasible GLS, as this estimator is consistently equivalent to GLS. In feasible GLS the variance-covariance matrix of the error is replaced with a consistent estimator constructed from a first stage regression that accounts for the serial correlation, as it could be an autoregressive regression of order k. The aim of the first stage regression is to derive the estimator for the variance-covariance matrix of the error term and this estimator is employed in the second stage of the regression.
- *Randomisation inference*: Bertrand et al. (2004) argue that this is the only method that provides reliable results even with small samples. The idea is to run a large number of models on the same data, using various placebo interventions (i.e. re-estimating the model assuming that the merger happened at different times). Then use the acquired effects of these placebo treatments to generate a distribution of effects. Finally, look at where the estimated effect of the true intervention (the merger) lies in this distribution and form a simple test to establish whether this effect is significantly different from the placebo effects.

Propensity score matching

Propensity score matching (PSM) is a statistical method that is used to build a control group based on the probability an individual (a firm) with certain observed characteristics belongs to the treatment group (chooses to merge). The procedure provides an objective way to detect control group members. Firms are matched on the basis of the estimated probability, or propensity score, relative to merging firms. Logit and probit models can be employed to estimate these probabilities. The control variables (observable product characteristics) can enter linearly or nonlinearly (for example polynomials). The validity of PSM depends on two key conditions: (a) conditional independence, which requires the unobserved heterogeneity not to affect participation and (b) that control and treatment groups have a common support or overlap in propensity scores.

PSM is a powerful approach if observed characteristics are the only cause of selection into treatment. This is another way to rephrase condition (a) mentioned above. Whether this is true or not, all depends on the case under investigation, but good news is that there are statistical tests available to assess the degree of selection bias or participation on unobserved characteristics.

A2.2. Merger simulations

The origin of merger simulation methods dates back to the nineties from the early work of Farrell and Shapiro (1990) and further research undertaken by Hausman et al. (1994), and Werden and Froeb (1994). Subsequent findings by Berry (1994), Berry et al. (1995) and Nevo (2001) have sharpened and extended the methodologies by proposing sophisticated discrete choice techniques to analyse demand and supply in differentiated product markets in the presence of observable and unobservable product heterogeneity, and observable and unobservable distributions of individual characteristics. The novelty of their technical extension is that it allows the researcher to estimate richer matrices of own- and cross-price elasticities, which are necessary primitives to evaluate the effect of mergers on competition (typically the price effect), along the lines of the three-stage procedure discussed in Section 3.

Merger simulation methods rely on a theoretical model of oligopolistic industry competition and for that reason are sometimes called merger structural models.[82] As such, they rely on a set of assumptions that are not always easy to verify. These assumptions embrace the demand function, the equilibrium in the market and the functional form of the cost function. Because of this, merger simulations are often subject to criticism on their ability to provide reliable information on merger effects. However, there is growing agreement among practitioners that with proper justification of the assumptions, and with models tailored to the merger situation under study, the outcome of the simulation can gain credibility.

In the main text we have presented merger simulations in three stages. MSs entail that one specifies a structural model with the following features: 1) system of demand equations at the product-level, 2) system of marginal cost function at the product-level, 3) equilibrium assumption. It is common to assume price as the strategic variable and product to be differentiated, but the structural model can be tweaked to allow for advertising or quality as (additional) strategic variables.

[82] Structural models make use of data to identify parameters (primitives) of an underlying economic model, relying on an observed outcome of individual or aggregate choice.

A2.2.1. Deriving the demand function

In the discrete choice literature, which is the literature that we discuss here below, the underlying demand function is derived from an individual's indirect random utility function. For each consumer i interested in the product j clustered in the group g, the indirect utility is linear and of the type:

$$u_{ij} = x_j\beta_i - \alpha_i p_j + \xi_j + \zeta_{ig} + (1-\sigma)\varepsilon_{ij}, \quad j = \{0,1,\cdots,J\} \text{ and } g = \{0,1,\cdots,G\}, \tag{9}$$

where x_j is the vector of observable product characteristics, p_j is the price, ξ_j is the unobserved product heterogeneity, and ε_{ij} is an identically and independently distributed extreme value realisation.[83] For each individual i the random variable ζ_{ig} varies only by group g and is assumed to have probability distribution such that $\zeta_{ig} + (1-\sigma)\varepsilon_{ij}$ is also an extreme value random variable.[84] The parameters β_i and α_i have index i when they are modelled as random coefficients. By parameterising the error term to have an extreme value distribution a random coefficient logit model can be estimated. Next, by imposing restrictions on the parameters $\beta_i = \beta$, $\alpha_i = \alpha$, and $\sigma = 0$ simpler logit demand can be derived. Define $\delta_j \stackrel{\text{def}}{=} x_j\beta - \alpha p_j + \xi_j$ and define $B_g \stackrel{\text{def}}{=} \sum_{j\in J_g} exp(\delta_j)$, and denote individual deviations from the mean utility by μ_{ij}. Depending on the chosen econometric specification the following parametric functional forms can be derived:

a. Logit:

$$s_j = \frac{exp(\delta_j)}{\sum_l exp(\delta_l)} \tag{10}$$

b. Two-level nested logit:

$$s_j = \frac{exp[\delta_j/(1-\sigma)]}{B_g^{\sigma}\sum_g B_g^{1-\sigma}} \tag{11}$$

c. Random coefficient logit model:

$$s_j = \frac{1}{ns}\sum_{i=1}^{ns} s_{ij} = \frac{1}{ns}\sum_{i=1}^{ns}\frac{\exp(\delta_j+\mu_{ij})}{\sum_l exp(\delta_l+\mu_{il})}. \tag{12}$$

Berry (1994) has shown the existence of invertibility between the mean utility part of the random utility and the product market share, proving that the parametric specification (2a and 2b) can be estimated by simple instrumental variables' linear estimators, with dependent variable $lns_j - lns_0$, where s_j and s_0 are the observed market share for product j and product 0, respectively. For the parametric specification (2c) a contraction mapping has to be added to the econometric procedure, along the lines explained in Berry et al (1995). The system of demand equation for the logit and nested logit, for the j-th product in segment g, can be formulated as

$$lns_j - lns_0 = x_j\beta - \alpha p_j + \sigma lns_{j/g} + \xi_j. \tag{13}$$

In the next subsection we present the alternative models that are used in the literature.

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[83] To ease the notation we have omitted the time subscript t. However, a time t and/or area a dimension can be added to the equation.

[84] See Cardell (1997).

A2.2.2. Demand specifications

Alternative econometric specifications have been proposed in the literature to estimate a system of demand for differentiated products (first stage of the aforementioned procedure) and our sample of papers covers almost all of them. Below we outline the competing methods:

- *Logit specification*: This specification is simple to use as it can be estimated by a linear instrumental variable (IV) estimator using instrument(s) for the price endogeneity. The limitation of the method is that it imposes restrictions on the price-elasticity of demand. As pointed out in Slade (2010) all elasticities are proportional to the prices and their level depend on the choice of the outside good. A large outside good implies low elasticities, leading to high mark-up. Furthermore, introduction of new products affect competition with existing products in the same way. This is known as independence of the irrelevant alternative (IIA) property, which is not a desirable property in this context, for it states that the odds (ratio of probabilities) of preferring one product to another is independent of the presence of the introduction of another 'irrelevant' product. In terms of elasticities the property translates into the cross-price elasticities being similarly affected by the introduction of a new product, hence showing the same degree of substitutability even when the new product may be more similar in terms of characteristics to some of the incumbent products. The simplicity of the logit model has been preferred in the ex-post analysis on the merger *Cerealia AB*/*Schulstad*; it has been exploited as benchmark to highlight its limitation in the study based on the mergers *Bass*/*Carlsberg-Tetley* and *Scottish*/*Courage* and in the mergers and acquisitions that occurred in the Portuguese insurance market between 1999 and 2007.
- *Two-level nested logit model*: it allows for partition of products into segments based on how similar products are. It is preferred to the logit model because the IIA property only kicks-in within segments (where products should logically be more similar) and is called independence of irrelevant nested alternatives, IINA. The price elasticity is now enriched by within and between segment effects. New products that can be placed in one of the existing segments will have stronger cross-price effects with products placed in the same segment. Similarly to the logit model it can be estimated by linear IV regression, employing instruments for the endogeneity of price and the within segment market shares. A criticism of the method is that the choice of the segment can be arbitrary and not fully driven by the degree of substitution across products. While the criticism may be valid at times, there is recognition by experts that many markets are naturally segmented and often in front of a merger decision such segmentation is refined by experienced CA economists, leading to a segmentation of the market that is widely accepted. The typical assumption is that the market segments are exogenously imposed. This is a common assumption, but unfortunately seldom is defended and its implications explained.

  Two versions of the two-level nested logit model have been used in the literature:

  - <u>Constant within correlation of tastes</u>, which is the most common used methodology. The derivation is shown in Berry (1994). While this is the most widely accepted methodology in the merger valuation literature, it is used only as benchmark methodology in the research on *Bass*/*Carlsberg-Tetley* and *Scottish*/*Courage* mergers, the Volvo/Scania merger and the mergers and acquisitions that occurred in the Portuguese insurance market between 1999 and 2007.
  - <u>Heterogeneous within correlation of tastes</u> is a less used methodology, for it requires the constrained estimation of various coefficients, along the lines discussed in Goldberg (1995). This is an additional estimation

provided in the same work for the mergers *Bass/Carlsberg-Tetley* and *Scottish/Courage* mergers.

- *Distance metrics*: this approach, put forward by Pinkse, Slade and Brett (1998) and extended by Pinkse and Slade (2004), is nothing more than a general alternative to the nested logit model, as it allows the researcher to experiment with the strength of competition along many competing dimensions. The ultimate goal of using this method is that it leads to richer estimated price elasticities than those estimated by logit or one level nested logit. The methodology is shown to provide the more reliable estimate for the ex-post analysis on the mergers *Bass/Carlsberg-Tetley* and *Scottish/Courage*. A criticism that this methodology incurs is that it gives the researcher discretion on the choice of the relevant observable characteristics to be used to enrich the model, and subsequently it reduces the room for an objective method.

- *Three-level nested logit model*: which in the merger literature takes on two forms:

    - Unit demand: This is an extension of the three-level nested logit model and is suitable for situations where the decision-tree is hierarchical. Its assumptions are similar to those of the two-level nested logit model with the addition of an economic restriction on the inequality between the correlation coefficients at the two-levels of choice. In our sample the method is used in the empirical work on the merger *GSK/Astra Zeneca* and is employed as comparison for alternative methods described underneath.

    - Constant expenditure: This method is rarely used in the literature, but is promoted in the in the study of the merger *GSK/Astra Zeneca* as the preferred method for merger evaluation. The benefit of using this model is that it makes price elasticities quasi-independent of prices.

- The *random coefficient logit model* is the most complicated to estimate of the models presented so far, because of its numerical difficulties and the lack of implementation within a user friendly statistical software. The difficulty of estimation however is balanced by the benefit of having a model that allows for heterogeneity in observable and unobservable individual valuation of product characteristics, able to provide richer and more realistic price elasticities. Also, the model is well suitable to accommodate for individual variability that may differ by location. In our sample the model is adopted in the study on the merger *Carlberg/Pripps* and an extended version of the model is estimated in the merger *Morrisons/Safeway*. This model is subject to the criticism that its estimation procedure is difficult to be understood and the technique may be difficult to be performed under limited time. Hence it is more suitable to retrospective merger valuation than ex-ante analysis. Furthermore, its complexity may be difficult to be explained to lawyers and tribunal members.

- *Random coefficient nested logit model*: This is an extension of the random coefficient logit model and is described in Grigolon and Verboven (2014). It is a new model in circulation and to our knowledge it has not yet been used on EU retrospective studies. The model is certainly appealing because of the quality of the estimation results that it produces, but of course this comes as the price of additional complexity.

In our sample of studies we also have one article that employs an extension of the continuous demand approach almost ideal demand system, called the proportionality-

calibrated almost ideal demand system, to study the mergers *Rio Tinto*/*North*, and *CVRD*/*Caemi*. As we mentioned in the main text, we omit discussing the continuous demand model approach, but interested readers can read the paper by Lundmark and Warell (2008) for a discussion on the relevant literature.

A2.2.3. Inferring the marginal cost

Upon having estimated the demand parameters using one of the demand specifications presented above, the marginal cost can be retrieved by assuming some model of oligopoly competition, and by filling out an ownership matrix for the portfolio of products the firms have control over. Below we describe the procedure.

Consider a market with F multiproduct firms selling J differentiated products. The marginal cost of production for the product j is c_j and the demand function for the same product is denoted by $D_j(p;\theta)$, where p without subscript indicates the J-th vector of prices and $\theta \stackrel{\text{def}}{=} \{\alpha, \beta', \sigma\}$ captures the set of demand coefficients.[85] For each product j that the firm f produces, $j \in J_f$, the firm maximizes the profit:

$$\sum_{l \in \mathcal{J}_f} \max_{p_j} \Pi_l\left(p_j, p_{-j}\right) = \max_{p_j} \sum_{l \in \mathcal{J}_f} [p_l - c_l(w_l;\gamma)] D_l\left(p_j, p_{-j};\theta\right), \tag{14}$$

where p_{-j} is the $J-1$ vector of all prices but the j–th one, and θ and γ are vectors of coefficients that enter the demand and cost side. If we define the j,l element of the partial derivative of the demand matrix, denoted with Δ, as $\Delta_{jl} \stackrel{\text{def}}{=} -\frac{\partial D_l(p_j, p_{-j};\theta)}{\partial p_j}$ and the j,l element of the ownership matrix , indicated with Ω, as $\Omega_{jl} \stackrel{\text{def}}{=} \{0,1\}$ - depending on whether the firm f owns both product j and product l or not,[86] we can express the first order condition of the profit maximization for all firms and products in compact form as the system of J first order conditions

$$p = c(w;\gamma) + (\Delta \times \Omega)^{-1} D(p;\theta), \tag{15}$$

where $.\times$ indicates an element by element matrix multiplication. The second term in the right hand side of the system of J equations displayed in (5) is the vector of mark-ups. Each price is decomposed in the marginal cost and the mark-up. As we can see from the system of equations (5), had we estimated the parameters θ, we could infer the marginal cost and subsequently estimate the parameters γ. It is however more efficient to estimate demand and the pricing equations simultaneously. In the restrictive case of single-product firms, the pricing equation for the j-th product simplifies to the Lerner index

$$\frac{p_j - c_j(w_j;\gamma)}{p_j} = \frac{1}{\eta_j(p;\theta)}, \tag{16}$$

where $\eta_j(p;\theta) \stackrel{\text{def}}{=} -\frac{\partial ln D_l(p_j, p_{-j};\theta)}{\partial ln p_j}$ is defined as the demand price elasticity.

Next, holding the marginal cost and consumer preferences (demand coefficients) fixed, a merger can be evaluated ex-ante. The merger is accounted for in the computation via an adjustment of the ownership matrix. The new ownership matrix will account for the

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[85] The demand function $D_j(p;\theta)$ can be expressed as the interaction between the product market share and the total population in the market $D_j(p;\theta) = s_j(p;\theta)I$.

[86] The ownership matrix does not have to be full. The numbers $\{0, 1\}$ could be replaced by fractions to allow for partial ownership.

change in ownership after the merger. Below we provide a numerical example to explain the procedure.

A2.2.4. Simulating the merger (a hypothetical example)

Consider a market, which initially has three firms, each producing a single differentiated product. The demand functions for the three products are given by

$$\begin{aligned} D_1 &= 1 - 0.5p_1 + 0.1p_2 + 0.1p_3 \\ D_2 &= 1 + 0.1p_1 - 0.5p_2 + 0.1p_3 \\ D_3 &= 1 + 0.1p_1 + 0.1p_2 - 0.5p_3. \end{aligned} \tag{17}$$

Assume the marginal costs to be constant and homogeneous and set to 0.2. The chosen demand functions and marginal costs yield to the system of first order conditions

$$\begin{matrix} p_1 \\ p_2 \\ p_3 \end{matrix} = \begin{matrix} 0.2 \\ 0.2 \\ 0.2 \end{matrix} + \left(\begin{bmatrix} .5 & -.1 & -.1 \\ -.1 & .5 & -.1 \\ -.1 & -.1 & .5 \end{bmatrix} .\times \begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{bmatrix} \right)^{-1} \begin{matrix} D_1 \\ D_2 \\ D_3 \end{matrix}. \tag{18}$$

The solution to the above system of simultaneous equations is $p_1^* = p_2^* = p_3^* = 1.375$ and the optimal quantities are $q_1^* = q_2^* = q_3^* = 0.5875$.

Assume now that a merger occurs between firm 1 and 2. In that case, the new ownership matrix is a matrix containing only ones, except for cells (1,3 and 2,3) and (3,1 and 3,2), which are zeros. Using the updated ownership matrix and maintaining the marginal cost and demand parameters fixed, a new equilibrium price can be computed from the first order condition of profit maximization. The new equilibrium prices can be compared with the old ones and the difference is the valuation of the effect of the merger on prices. The new system of first order conditions that accommodates for the change in ownership is given by

$$\begin{matrix} p_1 \\ p_2 \\ p_3 \end{matrix} = \begin{matrix} 0.2 \\ 0.2 \\ 0.2 \end{matrix} + \left(\begin{bmatrix} .5 & -.1 & -.1 \\ -.1 & .5 & -.1 \\ -.1 & -.1 & .5 \end{bmatrix} .\times \begin{bmatrix} 1 & 1 & 0 \\ 1 & 1 & 0 \\ 0 & 0 & 1 \end{bmatrix} \right)^{-1} \begin{matrix} D_1 \\ D_2 \\ D_3 \end{matrix} \tag{19}$$

Leading to the new equilibrium prices $p_1^{**} = p_2^{**} = 1.5257$ and $p_3^{**} = 1.405$ and quantities $q_1^* = q_2^* = 0.530$ and $q_3^* = 0.603$. As expected in case of no efficiency, prices increase and total output falls. If the merger goes from duopoly to monopoly prices will increase further and total output will fall more.

In this example we have used a linear demand function because it has the nice property of leading to a linear system of simultaneous equations. If the error term and specification were such that the demand function was a logit model, then:

$$D_j = \frac{exp(\delta_j)}{\sum_l exp(\delta_l)}$$

which is a function of all three prices. The only inconvenience with this functional form is that the system of simultaneous equations is no longer linear.

Modifying the marginal costs in order to model cost efficiency can provide robustness checks.

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doi 10.2763/84342