Post-merger price variation matters, so why do merger retrospectives ignore it?

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CCP Working Paper 16-5v2
(updated November 2017)

Abstract

The price effect of past mergers has been extensively researched over the last two decades. The overwhelming majority of these studies pool post-merger data to estimate the average price effect of a merger. Merger guidelines agree that mergers should be approved if market dynamics, such as entry, eliminate negative welfare effects. However, pooled post-merger data ignore key information about price dynamics and are unable to identify if post-merger prices eventually revert to pre-merger levels or drift away. We provide evidence, from a meta-analysis of previous works, and a set of Monte Carlo experiments, on how serious this problem is. Finally, we show the conditions where pooled data studies make an erroneous conclusion about the merger.

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Post-merger price dynamics matter, so why do merger retrospectives ignore them?*

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November 24, 2017

Abstract

The price effect of past mergers has been extensively researched over the last two decades. The overwhelming majority of these studies pool post-merger data to estimate the average price effect of a merger. Merger guidelines agree that mergers should be approved if market dynamics, such as entry, eliminate negative welfare effects. However, pooled post-merger data ignore key information about price dynamics and are unable to identify if post-merger prices eventually revert to pre-merger levels or drift away. We provide evidence, from a meta-analysis of previous works, and a set of Monte Carlo experiments, on how serious this problem is. Finally, we show the conditions where pooled data studies make an erroneous conclusion about the merger.

Keywords: mergers, merger retrospectives, meta study, Monte Carlo simulation
JEL Classification codes: C51, K21, L49

1 Introduction

The price effect of past mergers has received ample attention since the early 1990s. The recent upsurge in the number of such studies generated a considerable body of evidence of the price effect of mergers. Kwoka (2013, 2014) identified more than 60 US studies that looked at the price effect of mergers. Mariuzzo et al. (2016) reviewed another 20 similar European studies.

Methodologically, these studies are rather homogeneous, using standard difference in-differences type models; varying mainly in the way the counterfactual is constructed.

The objective of our paper is to focus on a specific empirical aspect of these studies, the handling of post-merger time-periods. We argue that in most of the previous works the wrong research design is being applied, inasmuch as these studies estimate average post-intervention price-effects, overlooking post-merger price dynamics. In general, in many causal inference studies the post-intervention dynamics of the outcome variable would not matter. However, there are equally many situations where evidence on this variation is exactly what the researcher should be aiming to get. Estimating the price effect of mergers is one of these.[1]

Estimating the over-time average price effect of mergers is counter-intuitive for two main reasons. First, merger guidelines agree that no intervention is needed if the post-merger price increase disappears within a reasonable amount of time, for example because of new entry. The US Horizontal Merger Guidelines say: "*The Agency will consider entry to be timely so long as it would deter or counteract the competitive effects of concern within the two year period and subsequently.*"[2] Similarly, the European Commission's guidelines on the Merger Regulation state: "*The Commission examines whether entry would be sufficiently swift and sustained to deter or defeat the exercise of market power. What constitutes an appropriate time period depends on the characteristics and dynamics of the market, as well as on the specific capabilities of potential entrants. However, entry is normally only considered timely if it occurs within two years.*"[3] Of course for entry to occur, a potential entrant would have to find such entry profitable, and the extent of entry would have to be such that prices would revert to (or remain at) pre-merger levels. Retrospective studies could verify whether these conditions are fulfilled after a merger. However, entry and its effects are a gradual process (prices might take some time to drop or to increase), which requires looking at the dynamics of post-merger prices in order to identify whether pre-merger expectations about entry–and the effect of entry–were correct.

Second, merger retrospectives are typically conducted to inform policy-makers whether a merger decision was correct or not. For this reason the conclusion on how prices change post-merger is crucial. Estimating the average post-merger price-effect requires a differ-

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[1] The importance of controlling for dynamics in difference-in-differences studies, is not new to the economics literature. A good example of the bias that can be generated by using a misspecified model is given by Wolfers's 2006 study of the causal effect of the implementation of unilateral divorce laws in the US on divorce rates.

[2] U.S. Department of Justice & Federal Trade Commission, Horizontal Merger Guidelines (2010), available at `https://www.justice.gov/atr/horizontal-merger-guidelines-08192010`, accessed 25/09/2017.

[3] Guidelines on the assessment of horizontal mergers under the Council Regulation on the control of concentrations between undertakings (2004/C 31/03), available at `http://eur-lex.europa.eu/LexUriServ/LexUriServ.do?uri=OJ:C:2004:031:0005:0018:en:PDF`, accessed 25/09/2107.

ent empirical model than estimating annual effects. Is it possible - and this is the main focus of our paper - that the two empirical models return different evidence on the price effects of the merger? We show in this paper that the likelihood of making the wrong conclusion is very high if the wrong empirical model is used.

There is surprisingly little empirical work on the dynamics of how markets evolve post-merger, and whether the antitrust agency's expectations at the time of the merger came true (for example, did entry happen as expected and did it reduce prices as predicted?) A notable exception is a set of annually conducted studies by the UK Competition Commission, which collected qualitative evidence on how market characteristics (other than just price) evolved post-merger.[4] Regarding specific mergers, Winston et al. (2011) look at the long-run effect of two railroad mergers. They estimate how prices evolved (year-by-year) post-merger and find that despite short-term price increases the mergers had no effect in the long-run on prices and welfare. Looking at mergers in the US market for bank deposits Focarelli and Panetta (2003) arrive at similar conclusions: mergers create higher prices in the more immediate aftermath of the merger, but this effect later disappears.[5]

The purpose of this paper is not to find consensus on how quickly prices might revert to pre-merger levels (if they ever do), rather than to highlight the importance of acquiring evidence on post-merger price dynamics. We argue, that this evidence should be easily generated from the data already available to authors of most ex-post merger studies, and it was simply a matter of preference, rather than data availability that led to the wrong model-choice (estimates on average price effect).

We contribute to the rich body of merger retrospective studies in an important way, by pointing out how inappropriately designed empirical models will mask important information on the price effect of mergers. First we formalise the problem and argue that pooling together all post-merger time periods will provide price-effect estimates that are the average of the per period (annual) effects with a standard error that is lower than the standard errors of the annual price effects (at least under the assumption of non-positive correlation). This difference in standard errors has an important role to play in our main argument. Next, we look at how previous studies would have led to different conclusions had they used the wrong model. We employ a meta analysis with alternative weights to highlight the difference between the two types of empirical models.

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[4]See for example here: http://webarchive.nationalarchives.gov.uk/20140402154651/http://www.competition-commission.org.uk/assets/competitioncommission/docs/pdf/non-inquiry/our_role/evaluation/ex_post_evaluation_of_mergers

[5]Of course when the time period post merger is too long there is more of a risk of picking confounding effects. However, this is an issue shared both by pooled and unpooled models. In light of this in our analysis we concentrate on a medium-run period of time.

Finally, we present a set of Monte Carlo simulations and calibrations to illustrate the problem. Our results show that models with pooled time periods (estimating an average post-merger price increase) are much more likely to conclude that the merger increased prices, even if the price-increase disappeared within reasonable time. Similarly, using models with pooled time periods, a conclusion that the merger did not increase prices is pretty likely, even when it actually did within a reasonable time.

The paper is structured the following way. First, we describe how previous papers have handled post-merger price dynamics and show that this has been typically ignored. We then provide a discussion of the difference between the pooled and unpooled time period models. This is followed by a set of simulations and calibrations to demonstrate the magnitude of the problem, and we conclude with policy recommendations.

2 Post-merger price-dynamics in previous merger retrospectives

We surveyed a large body of retrospective merger studies reviewed by Kwoka (2013) and Mariuzzo et al. (2016).[6] We focus on one aspect of these studies: the econometric specification for estimating post-merger price effects. The main selection conditions for our sample are: that ex-post percentage price-effect estimates are provided in the study, that their standard errors can be recovered, and that the time span of the data is identifiable. All the studies in our sample used reduced form causal inference models (typically difference-in-differences).

Our sample contains price-effect estimates of 68 mergers (discussed in 39 papers, which are either published or in circulation as working paper). Of the 68 mergers, 52 are US mergers, and 16 EU.[7] Some of the retrospective merger papers report estimates for the effect of the given merger on a range of different product and geographical markets. This gives us a sample of 626 market-level price-effect estimates.[8]

We distinguish between two types of studies. The first, and by far the most common, treats the post-merger period as one, and pools all post-merger price observations, and thus estimates the average effect of the merger over the post-merger period. We call this set of studies "*pooled studies*". The second group controls for price observations year by year, and estimates annual post-merger price changes ("*unpooled studies*").

More than 80% of the 68 merger studies have pooled the post-merger time periods

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[6]The full list of these studies is provided in the Appendix.

[7]Kwoka (2013, 2014) and Mariuzzo et al. (2016) provide more information about these mergers.

[8]We provide descriptive information on these estimates in the Appendix.

and estimated an average price effect for the entire post-merger period. Similarly most of the product/geographic market-level price effects (558 out of 626) are estimated by pooling the post-merger period data. The remaining 68 estimates are from unpooled studies. Less than 20% of the merger retrospectives (10 merger studies published in 5 papers) estimate the post-merger price effects year-by-year. More detailed description of the data is given in the Appendix.

Another striking figure in the data is that more than 80% of these merger studies have estimated the average price effect within less than 2 years of the merger. There are two problems with this. First, estimating an over-time average completely masks the dynamics of post-merger prices, and as such is inadequate for evaluating the effectiveness of merger control. Second, it does not allow for a full assessment of how prices evolve over time. Both the EU and the US guidelines emphasise that a merger may not be considered problematic if appropriate entry happens within 2 years of the merger, therefore knowing how prices changed within two years cannot fully answer the question whether a merger control was effective.

Table 1 shows the number of mergers evaluated in retrospective studies published in the most frequent outlets (two or more mergers covered). The American Economic Review (including Papers and Proceedings) published the largest number of merger studies, and the Journal of Law and Economics has the highest propensity of studies using an unpooled model.

Table 1: Number and fraction of pooled and unpooled studies by journals

Journal	#	# of pooled	# of unpooled	% of pooled	% of unpooled
AM ECON REV	8	6	2	0.75	0.25
GAO	7	7	0	1.00	0.00
J INDUS ECON	6	5	1	0.83	0.17
J L AND ECON	6	4	2	0.67	0.33
INT'L J ECON BUS	6	6	0	1.00	0.00
WP	6	6	0	1.00	0.00
FTC	3	2	1	0.67	0.33
AM ECON J-ECON POL	3	3	0	1.00	0.00
AM ECON J-ECON POL	3	3	0	1.00	0.00
EUROPEAN COMMISSION	2	2	0	1.00	0.00
J TRANSP ECON POLICY	2	2	0	1.00	0.00
MANAG DECIS ECON	2	2	0	1.00	0.00

An interesting question - although somewhat tangential to our main story - is whether the choice of the model (pooled or unpooled) is random with respect to the type of mergers. As a simple test, we compare the distribution of price effect estimates for

the two groups. A comparison at the level of estimates (626 estimates), using a two-sample Kolmogorov-Smirnov (KS) equality-of-distributions test, provides evidence that the two distributions differ. However, when we collapse the samples to merger-level (68 estimates) and repeat the two-sample KS test we find that the null hypothesis of equality-of distributions is not rejected. The focus of this paper is on the dynamics of price changes and not on their general distribution, however the evidence that the pooled and unpooled distributions do not differ at merger level is a sign that the mergers in the two groups are crudely comparable. Again, this is not required for our main point about using the right estimation model, but it helps illustrate our point.

2.1 Pooled and unpooled post-merger price effect estimates

To summarise the price-effect estimates we adopt a meta-analysis approach. For each retrospective merger study we take note of the estimated difference-in-differences (DiD) coefficients of percentage price changes and their corresponding standard errors. The rule that we follow in recording the relevant estimates is the following. If the retrospective study presented more than one regression for each market (often included in the study as part of robustness checks), our strategy was to include the average DiD estimates (and average standard error estimates) over these regressions. When the standard error was not reported in the merger study we proxied it from any useful information available. We often used the stars on the significance of the coefficients to retrieve the standard errors.[9]

Table 2 shows the result of our meta-analysis, when the estimates are collapsed by mergers (and not by product/market).[10] The pooled column shows the price effects estimated using models that pool data for up to 2 years after the merger. The unpooled column also shows estimated effects within two years from the merger but only for studies where the first, and the second year effects were separately estimated. The first couple of rows displays the unweighted combined price effects of mergers. The second section describes the combined estimates where we use the t-value of the estimated percentage price effect as weights. The idea is that observations with larger t-values should weigh more in combining the results from different merger studies.[11] The third

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[9]For example, if the DiD coefficient was significant at 1% (typically indicated with three stars), we generated the proxied standard error as ratio between the estimate and critical value 2.575. When it was significant at 5% we used as denominator the mid-point between 1.96 and 2.575, and when the significance was at 10% the division was by the mid-point between 1.645 and 1.96. Finally, in case of non-significance the value chosen as denominator was the mid-point between 0.8225 (itself a mid-point between 0 and 1.645) and 1.645.

[10]The market level summary of the meta study is given in the Appendix.

[11]This is very similar to inverse variance weights.

section contains the meta price effect where we weight each estimate by the number of citations the publication received up to that day (as shown on Google Scholar on 20 September 2017). Finally, in the fourth section, we conduct a similar exercise and obtained from ReserchGate the impact factor of the publications for the most recent year, 2015/16. For manuscripts and working papers, for which no such information was available, we associate a minimal weight value of 0.3.

Table 2: Meta analysis results - price effects within two years of the mergers

Pooled				Unpooled			
Period	Mean	SE	Count	Period	Mean	SE	Count
		Unweighted					
				First year	5.388**	2.63	10
≤ 2 years	3.240***	0.648	52	Second year	-1.04	2	10
		Weighted by t-stat					
				First year	5.28***	2.04	10
≤ 2 years	5.044***	0.269	52	Second year	-0.782	1.554	10
		Weighted by journal impact factor					
				First year	5.989***	2.17	10
≤ 2 years	4.607***	1.355	52	Second year	-0.826	1.43	10
		Weighted by journal citations					
				First year	3.301	2.069	10
≤ 2 years	5.149**	2.047	52	Second year	-1.708	1.64	10

The results from our meta-analysis reveal an interesting pattern. When the unpooled model was used (i.e. post-merger annual estimates are provided) the post-merger price increase, on average, disappeared by the second year following the merger. When the pooled model was used, the estimates give an average effect over the two years following the merger. This is in line with what we will show formally later - the pooled model delivers the arithmetic mean of the estimates from the unpooled model. If we accept that the selection of the model (pooled or unpooled) is independent of the merger, this would suggest that had the 10 studies that used the unpooled model relied on the pooled model, the estimates would have been similar to the estimates found in the pooled model.

More importantly, this suggests that on average, using the pooled model, as it has

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been done dominantly in previous literature, is more likely to wrongly conclude that the merger increased prices, even if the price increase disappeared by the end of the second post-merger year. This matters because the policy interpretation of the two findings are contrastingly different. A merger with a price increase that disappears within 2 years would not be intervened, but a merger that leads to a price increase over the first two years would.

Overall, one of the main reasons for conducting merger retrospectives is to improve our understanding of the effectiveness of merger control (Kwoka (2013)). On this ground, one would expect the merger retrospective to provide an idea of whether the antitrust authority made the right decision. For example, if prices increased after the merger it might be interpreted as an error by the antitrust authority. On the other hand, if the estimates show that post-merger prices converge to zero within a reasonable length of time, then the antitrust authority did not make an error even if the initial price effect was significantly positive. Estimating the post-merger over-time average does not allow us to analyse price dynamics. Instead, it could wrongly conclude that merger control was too lax – even in cases where post-merger prices gradually did revert to pre-merger levels. In the following section we formulate the difference between estimating average or annual post-merger price effects.

3   A conceptual framework

Let p_{mt} denote the price of a unit (product or firm) in market m at time t. Indicate with W_m the treatment dummy (firm/product involved in the merger). Represent with d_k a dummy variable, which takes value 1 the kth period after the merger, and zero otherwise. By adding the post treatment dummy variables we obtain a pooled version, denoted as $D_K \equiv \sum_{k=1}^{K} d_k$. For simplicity, assume that there is only one period pre-merger, but this assumption can easily be relaxed. Furthermore, indicate with $\tau = 1, ..., T$ fragments of time within a period k, for example τ could measure days and k years–when we have daily price data but are estimating yearly price effects post treatment–. Looking at yearly effects for K periods following the merger we can express the linear econometric equation for prices as:

$$p_{mt} = \beta_0 + \beta_1 W_m + \sum_{k=1}^{K} (\gamma_k d_k + \delta_k d_k W_m) + \alpha_m + \lambda_\tau + \varepsilon_{mt},$$

$$m = \{1, 2, \cdots, M\}; \ t = \{T, \cdots, -1, 0, 1, \cdots, T\},$$

(1a)

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where δ_k are the difference-in-differences (DiD) estimators for each year after the merger, α_m is a market-specific fixed effect, and λ_τ a set of controls that capture possible seasonality in the data. We indicate with $t = 0$ the time when the treatment takes place. Assuming that the dataset is balanced, the total time observations are $T \times (K + 1)$. Equation (1a) refers to the unpooled model.

As we discussed earlier, what is typically estimated in most merger retrospective studies is a slightly different model, with time period post-merger dummies replaced by a pooled period post-merger dummy:

$$p_{mt} = \beta_0 + \beta_1 W_m + \Gamma_2 D_2 + \Delta_2 D_2 W_m + \alpha_m + \lambda_\tau + \varepsilon_{mt}, \tag{1b}$$

where the DiD estimator for a pooled period of length 2 is denoted with Δ_2. Equation (1b) can be seen as a restricted version of Equation (1a) once we impose $\gamma_1 = \gamma_2 = \Gamma_2$, along with the further restriction $\delta_1 = \delta_2 = \Delta_2$, but as we will show later, even in this case standard errors may differ. Equation (1b) captures the pooled model.

If we consider an analysis restricted to only two years after the merger (and one year before the merger), and use the unpooled model, we have that the DiD coefficient for the second year from the above equation when separate time period dummies are included can be written as:

$$
\begin{aligned}
\delta_2 \quad = \quad & [E\left(p_{mt} \mid W_m = 1, d_2 = 1\right) - E\left(p_{mt} \mid W_m = 0, d_2 = 1\right)] - \\
& - [E\left(p_{mt} \mid W_m = 1, d_2 = 0\right) - E\left(p_{mt} \mid W_m = 0, d_2 = 0\right)].
\end{aligned} \tag{2a}
$$

Looking at two periods after the merger the pooled model DiD estimator is:

$$
\begin{aligned}
\Delta_2 \quad = \quad & (E\left[p_{mt} \mid W_m = 1, (d_1 + d_2) = 1\right] - E\left[p_{mt} \mid W_m = 0, (d_1 + d_2) = 1\right]) - \\
& - (E\left[p_{mt} \mid W_m = 1, (d_1 + d_2) = 0\right] - E\left[p_{mt} \mid W_m = 0, (d_1 + d_2) = 0\right]).
\end{aligned} \tag{2b}
$$

What are the implications, for the ex-post merger analysis, of using a period-by-period dummy (unpooled) version rather a pooled period dummy sample?

Continuing with the simplifying example where we focus only on two periods following the merger, assume that there is a price increase in the first period, i.e. $\delta_1 > 0$, but then prices revert to pre-merger levels in the second period, that is, $\delta_2 = 0$. This is an example of a merger that would be considered unharmful because the price effect would have vanished after one year, perhaps because of entry. Suppose we have estimated the parameters $\widehat{\Delta}_2$, $\widehat{\delta}_1$, and $\widehat{\delta}_2$ using the two different specifications. It can be shown that the restricted estimator $\widehat{\Delta}_2$ is equal to the average of the yearly time period

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parameters, $(\widehat{\delta}_1 + \widehat{\delta}_2)/2$. Then, assuming central limit theorem holds, the time period estimators for a sample of size $N = T \times (K + 1) \times M$ have the following probability distributions, $\widehat{\delta}_1 \sim N\left(\delta_1, \frac{\sigma_{\delta_1}^2}{N}\right)$, $\widehat{\delta}_2 \sim N\left(\delta_2, \frac{\sigma_{\delta_2}^2}{N}\right)$, leading to $\widehat{\Delta}_2 \sim N\left(\frac{\delta_1+\delta_2}{2}, \frac{\sigma_{\Delta_2}^2}{N}\right)$, with $\sigma_{\Delta_2}^2 = \frac{1}{4}\left(\sigma_{\delta_1}^2 + \sigma_{\delta_2}^2 + 2\sigma_{\delta_1\delta_2}\right)$.

Using the pooled model, a merger retrospective would conclude that a merger was harmful and that intervention was insufficient, if $\Delta_2 > 0$. Using the unpooled model one can identify the dynamics of post-merger prices, and would only conclude that the merger was harmful if prices are still above the pre-merger level by the second period, i.e. if $\delta_2 > 0$. Both cases can be tested empirically. Given the asymptotic normality of the parameters, which follows the central limit theorem, the probability values are:

$$\Pr\left(Z_{unpooled} > \frac{\widehat{\delta}_2}{\widehat{\sigma}_{\delta_2}/\sqrt{N}}\right), \quad \Pr\left(Z_{pooled} > \frac{\left(\widehat{\delta}_1 + \widehat{\delta}_2\right)/2}{\widehat{\sigma}_{\Delta_2}/\sqrt{N}}\right). \tag{3}$$

Assume a simple case, where in both periods we have a small, 1 percent price increase ($\widehat{\delta}_1 = 0.01$, $\widehat{\delta}_2 = 0.01$), and standard deviations $\widehat{\sigma}_{\delta_1} = \widehat{\sigma}_{\delta_2} = 0.1$. Assume also that our sample size is $N = 200$. Under the assumption of independence between $\widehat{\delta}_1$ and $\widehat{\delta}_2$, $\sigma_{\delta_1\delta_2} = 0$, we would conclude that the merger increases prices under the pooled, but not the unpooled model:

$$2 \times \Pr\left(Z_{unpooled} > \frac{0.01}{\frac{0.1}{\sqrt{200}}}\right) = 0.159 > 0.05, \quad 2 \times \Pr\left(Z_{pooled} > \frac{0.01}{\frac{0.07}{\sqrt{200}}}\right) = 0.043 < 0.05. \tag{4}$$

The purpose of this simple, back of the envelope example, is to highlight the importance of carefully choosing which estimation model to use. To further demonstrate how much it matters, we looked at all the studies in our sample, where the unpooled model was used to ex-post identify the effect of mergers. Using the above method we then re-estimated these merger effects but now using the pooled model. The purpose of this exercise is to demonstrate the prevalence of making an erroneous conclusion on the evaluated merger, where the researcher (wrongly) chooses the pooled model.

For each merger published in each paper we report in Table 3 the effect estimated in the original paper for the first and the second year following the merger, together with their standard errors (columns δ_1 and δ_2). As mentioned earlier where more than one model was estimated for each merger (different treatment and control groups, or different specifications) we report an average (unweighted) effect and standard error.[12]

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[12]As the purpose of this exercise is to demonstrate our analytical point, this should not affect our key

The table also shows what the price effect and its standard error would have been had the authors of the respective studies (wrongly) used a pooled model (column Δ_2). Not having access to the original data, to calibrate the 'pooled' standard errors, we rely on the assumption that the annual estimates are independent (their covariance is zero).[13]

The table shows (dark grey) that in four out of the ten studies, the final conclusion of the study would have been different under the pooled model. For example, Winston et al (2011) estimated that post-merger prices would have reverted to pre-merger levels by the second year after the merger. Had the authors used the pooled model (column Δ_2), they would have concluded that the merger led to a price-increase. In Kemp et al. (2012) and in Hosken et al. (2011) the authors estimated that after an initial price drop after the merger, prices increased back to pre-merger levels in the second post-merger year. Had they used a pooled model, they would have concluded that both mergers would have reduced post-merger prices. We also highlight (in lighter grey) the studies, where using the pooled model would have produced estimates with higher statistical power (become more significant). For example Schumann et al (2011) estimated a drop in post-merger prices both in the first and the second year after the merger. This was significant at 95%. Had the authors used a pooled model, their results would have shown significance at 99%.

The main message of this simple exercise was to highlight that using the wrong model (pooled instead of unpooled) would have led to the wrong conclusion about the merger in 40% of the cases. As another demonstration of the gravity of this problem, on the following pages we provide a generalisation of this argument via Monte Carlo simulations.

4   Simulations

In the following we simulate a set of cases, where we know how prices evolve post-merger. The objective of this exercise is to gauge how likely it is that using the pooled model leads to the wrong conclusion on the effect of the merger. We consider the following hypothetical cases. (1) Where following an initial post-merger price increase, prices revert to pre-merger levels. (2) Where post-merger prices do not increase immediately after the merger but they do within a reasonable amount of time. (3) Where the merger

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findings (i.e. we could equally report a full list of all estimates reported in each study to demonstrate the same point).

[13]We have also re-estimated these studies under the assumption of unit correlation and got qualitatively similar results.

Table 3: Estimates using the unpooled model and their calibrated pooled counterparts

Study	Firm1	Firm2	Journal	N		$\hat{\delta}_1$	$\hat{\delta}_2$	$\hat{\Delta}_2$
Hosken Silvia Taylor (2011)	UDS	Tosco	AM ECON REV (PAPERS PROC)	903	estimate s.e. p-val	-1.440*** 0.548 0.009	0.180 0.553 0.745	-0.630* 0.379 0.097
Hosken Silvia Taylor (2011)	Tosco	Unocal	AM ECON REV (PAPERS PROC)	918	estimate s.e. p-val	-3.868*** 0.613 0.000	-2.948*** 0.688 0.000	-3.408*** 0.460 0.000
Kemp, Kersten and Severijnen (2012)	Ziekenhuis Hilversum	Ziekenhuis Gooi-Noord	De Economist	404	estimate s.e. p-val	2.000** 0.777 0.011	5.260** 2.043 0.011	3.630*** 1.093 0.001
Kemp, Kersten and Severijnen (2012)	Erasmus MC Ziekenhuis	Havenziekenhuis Rotterdam	De Economist	404	estimate s.e. p-val	-1.610** 0.633 0.012	-0.735 0.670 0.274	-1.173** 0.461 0.012
Kemp, Kersten and Severijnen (2012)	Medisch Centrum Alkmaar	Gemini Ziekenhuis	De Economist	404	estimate s.e. p-val	1.310 3.025 0.665	17.780** 6.905 0.011	9.545** 3.769 0.012
Kemp, Kersten and Severijnen (2012)	St. Lucas Ziekenhuis	Delfzicht Ziekenhuis	De Economist	404	estimate s.e. p-val	3.535** 1.373 0.011	5.630** 2.186 0.011	4.583*** 1.291 0.000
Schumann et al. (1992)	Lone Star Industries	Kaiser Cement	FTC	48	estimate s.e. p-val	-28.000** 12.580 0.036	-26.000** 11.681 0.036	-27.000*** 8.583 0.005
Taylor & Hosken (2007)	Marathon	Ashland Petroleum	J INDUS ECON	620	estimate s.e. p-val	-0.045 0.835 0.957	0.430 1.465 0.769	0.193 0.843 0.820
Winston et al. (2011)	Burlington Northern	Santa Fe	J L AND ECON	6804	estimate s.e. p-val	53.000** 22.000 0.016	-1.000 13.000 0.939	26.000** 12.777 0.042
Winston et al. (2011)	Union Pacific	Southern Pacific	J L AND ECON	6804	estimate s.e. p-val	29.000*** 6.000 0.000	-9.000 6.000 0.134	10.000** 4.243 0.018

is followed by an immediate price drop, which then disappears. (4) And finally, where the post-merger price change is constant over multiple periods.

4.1 Simulation framework explained

First we simulate the price data. We associate a unitary average value to prices, as if prices were normalised to the average price. In this way we can think of proportional changes, avoiding using logs. Then we draw a vector of iid values for the error term from a normal distribution with mean 0 and a value of standard deviation that results in proportional changes in price. For example a standard deviation of 0.2 would mean that around 68% of the simulated price data would be less than 20% away from its mean (allowing at most a 40% price change within the analysed period), and there would be 27% chance that prices diverge by 20-40% from the mean (allowing at most a 80% price change within the analysed period). As we are talking about a relatively short time period (two years) we think this is a reasonable working assumption, it is less likely - although still possible under the assumption of sd=0.2 - that prices would increase or drop by a larger extent within such a short time period. For this reason we use 0.2 as our starting point but we provide the simulation results for other values in Figure 4 in the Appendix. The price data is generated using the unpooled model specification displayed in Equation (1a). As our interest is on the DiD coefficients δ_1 and δ_2, we set all other coefficients to zero.

We maintain the simplifying assumption that there are $K = 2$ post-merger periods (and one pre-merger period), and that each period equals a calendar year. This works for expositional purposes but also in more than 2/3 of the merger retrospective studies in our sample, only two post-merger periods were looked at. Needless to say, the simulations can easily be generalised to any $K > 2$. We also assume that there are T time series observations in each period. For example, if we have daily price data, then $T = 365$. Finally we assume that there are M markets analysed. This is the cross-sectional dimension of our data.

Next, we use the simulated price vector to estimate the parameters of the model by OLS for both the unpooled, Equation (1a), and the pooled models, Equation (1b). We record the estimated unpooled coefficient $\widehat{\delta}_2$ and pooled coefficient $\widehat{\Delta}_2$ and test if they are different from zero using a t-test (at a significance level of 5%).[14] We record the

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[14]As implied earlier, we focus on the second period coefficient as for the purposes of merger analysis, this has more importance. A merger with an immediate price increase that disappears by the second period should not warrant intervention. Similarly, a merger with no price increase in the first but a price increase in the second period should be considered harmful.

results of the t-tests. We then repeat this 1000 times. Finally we compare the proportion of cases where a statistically significant price change is estimated for the pooled and the unpooled models.

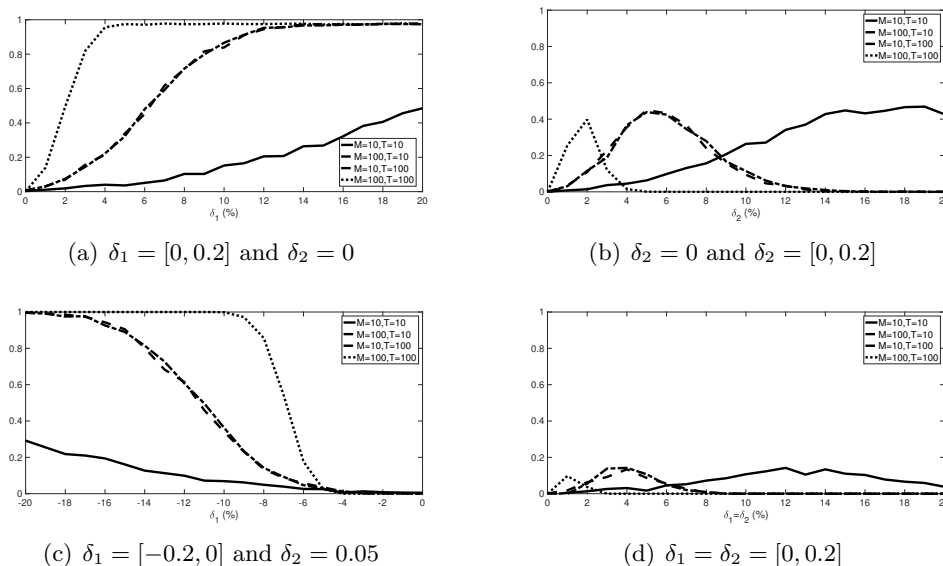### 4.1.1   Wrongly concluding that a merger should have been intervened

We first view the case of a merger that should be considered unharmful because the second post-merger period price is the same as the pre-merger price. In generating the price data therefore we ensure that $\delta_2 = 0$ and allow δ_1 to take on any value between zero and a 20% price increase, i.e. $\delta_1 = [0, 0.2]$. Because $\Delta_2 = (\delta_1 + \delta_2)/2$, it is clear that the pooled model estimates a price increase every time $\delta_1 > 0$, and could lead one to conclude that there was insufficient intervention by the antitrust agency. Using the unpooled model we would correctly estimate that $\delta_2 = 0$, i.e. there was no need for (further) intervention.[15] Moreover, the standard error of $\widehat{\Delta}_2$ will be different from the standard errors of $\widehat{\delta}_1$ and $\widehat{\delta}_2$ (simply because the dummy variable D_2 pools together the observations where $d_1 = 1$, or $d_2 = 1$). Our simulations show how much this difference in the mean of $\widehat{\delta}_1$ leads to the wrong conclusion about the merger.

Figure 1(a) displays the relative inaccuracy of the pooled model in comparison to the unpooled model (the probability of wrongly concluding that the merger intervention was insufficient, i.e. prices increased, in the pooled model minus the same probability in the unpooled model). The four lines represent four specific types of data: T refers to the number of time-series observations, and M refers to the number of cross-sections (markets). The data is time series dominated where $T = 100$, and $M = 10$, and cross-section dominated where $M = 100$ and $T = 10$.

We can see that as δ_1 increases, the probability of incorrectly concluding that the merger was harmful (i.e. the intervention was insufficient) increases, and this is more pronounced when we have a larger sample. This is as expected, with small samples, the estimated coefficients have a higher standard error and so they are less likely to be significantly different from zero in both samples. As the sample size increases the $\widehat{\Delta}_2$ standard errors decrease and the hypothesis tests become more likely to be statistically significant - i.e. in this case predicting that the merger increased prices despite the fact that this price increase disappeared by the second period. The level of the probability of an erroneous conclusion is striking. When a large sample of data is available (such is the case in the analyses of petroleum mergers), even a 4% increase in the first year after the merger is enough for the pooled model to conclude (at 95% significance) that the

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[15]Although even in this case it would (approximately) incorrectly estimate a price-increase in 2.5% of the cases (as we are using a two-tail 95% significance test).

Figure 1: Probability of drawing wrong conclusions on the merger when using the pooled sample

(a) $\delta_1 = [0, 0.2]$ and $\delta_2 = 0$

(b) $\delta_2 = 0$ and $\delta_2 = [0, 0.2]$

(c) $\delta_1 = [-0.2, 0]$ and $\delta_2 = 0.05$

(d) $\delta_1 = \delta_2 = [0, 0.2]$

merger increased prices despite the fact that prices reverted to pre-merger level in the second year. Put differently, in the previous studies where the pooled model was used and it estimated that the merger increased prices, there was a realistic chance that this price increase disappeared as early as the second year after the merger.

4.1.2 Wrongly concluding that the merger did not need intervention

Now consider the opposite scenario, where the merger increased prices, but only with a short delay (i.e., no price increase in the first year, and a price increase in the second year following the merger, $\delta_1 = 0$, and $\delta_2 > 0$). Estimating $\widehat{\Delta}_2$ would always give a positive coefficient, therefore it would appear that even the pooled model would be able to detect that the merger would have needed intervention (because it increased prices). Figure 1(b) shows that this is not always the case. In fact, it is highly possible that the pooled model would estimate that $\widehat{\Delta}_2$ is not significantly different from zero and conclude, wrongly, that the merger did not increase prices. Relative to the unpooled model this could happen in up to 40% of the cases. For example, even if one has rich time-series and cross-sectional data, the pooled sample would be unable to identify mergers where prices increased by up to 3% in the second year after the merger. With smaller samples

the problem would be even more pronounced. For example, with monthly data over 10 cross-sections (a very common data endowment) the pooled model would be wrongly concluding that a merger did not increase prices even when the second year price increase was 10%, and actually in that case the larger is the second period price increase, the more pronounced is the wrong decision.

4.1.3 Wrongly concluding that the merger reduced prices

There is a large number of merger retrospectives that conclude that the merger resulted in a price reduction. To illustrate the weakness of using the pooled model in these cases, we present a scenario where the merger increased prices in the second post-merger year by 5%. In any interpretation these mergers should be considered harmful and the conclusion would be that (more) intervention would have been warranted. However, when using the pooled model, the pooled dummy coefficient $(\widehat{\Delta}_2)$ will be negative if there was a sufficiently large price-drop in the first period. The expected value of $\widehat{\Delta}_2$ is zero where there was a price drop of 5% in the first period, therefore one would expect that the pooled model will only lead to a wrong conclusion if $\delta_1 < -5$.

Figure 1(c) shows that - as expected - if the first period price drop is sufficiently large then the pooled dummy will always wrongly conclude that the merger reduced prices. With a large sample (daily prices over 100 cross-sections) this will almost always be the case.

4.1.4 Mergers with constant post-merger price effect

In this final scenario we look at how the pooled and unpooled samples perform where the price effect of the merger is constant $(\delta_1 = \delta_2)$. On the face of it, it would appear that there should not be any difference between the two estimates, because $\Delta_2 = \delta_1 = \delta_2$, but again the difference in standard errors will have a role to play. Figure 1(d) shows the relative probability of finding a price increase is significant when post-merger prices are constant, using a pooled model. Put differently, this is the probability that the unpooled sample concludes that the merger did not increase prices even though the pooled model concludes that it did. The intuition is simple, the pooled dummy estimated coefficient $\widehat{\Delta}_2$ has a lower standard error where $\delta_1 = \delta_2$, and is therefore more likely to estimate that the price increase is significantly different from zero. This relative weakness of the unpooled model is rather small, but, when one deals with small samples, it could be around 10 percent.

The main message of this final point is important for research design. We would argue

that the researcher should run the unpooled model and estimate yearly price effects, but if these are of similar magnitude (and non-significant) then it is good strategy to also run the pooled model (or at least conduct an F-test for the joint significance of the yearly dummies). If the pooled model returns significant price effects or if the yearly dummies are jointly significant, then one could conclude that although the annual price effects are not individually significant, the overall post-merger effect is.

5   Conclusions

Merger retrospectives are typically conducted to inform policy-makers about the fitness of merger control for filtering out and remedying price increasing mergers. For this reason the conclusion on whether a merger increased prices is crucial. We showed above that that the probability of making the wrong conclusion is very high if the wrong empirical specification is used.

If the purpose of the merger retrospective is to evaluate the effectiveness of merger control, then one will want to know not the post-merger average price-change but how prices developed after the merger. If the antitrust agency predicted that the merger would lead to a temporary price hike, but prices would eventually (within a reasonable length of time) revert to pre-merger levels, then it would probably refrain from intervention. Therefore the retrospective study should not look at how prices change on average, rather at whether (and how quickly) they revert to pre-merger levels.

We find that more than 85% (58 mergers) of previous studies estimate post-merger average price effects (pooled sample), and yearly effects were estimated for only 10 mergers (unpooled sample). We argue that this is a mistaken approach as it masks information on post-merger price dynamics which would be crucial for the assessment of the merger. By running a set of Monte Carlo simulations we show that estimating the mean post-merger price effect might lead to erroneous conclusions on the effect of the merger. Our simulations demonstrate that potentially all studies (using the pooled sample) that concluded that the merger led to a price increase, could have been wrong, and that in actuality the price-increase disappeared within a reasonable time. This is more likely where the studies had large samples (for example daily price data over large cross-sections). Similarly, up to half of the studies that concluded - based on the pooled sample - that the merger did not increase prices might have been wrong, and in actuality after a short period of unchanged prices, prices went up. Finally, studies that estimate that the merger reduced prices can potentially all be wrong in their conclusion on the merger if the merger was eventually followed by a price increase, but this was preceded

by a sufficiently large price drop.

In our view, where data was available to estimate the pooled sample, it should be equally available to estimate yearly effects and thus gather evidence on how prices evolve post-merger. Not only could this more accurately inform the researcher whether the antitrust authority's intervention was appropriate, but accumulating a large mass of these studies could tell us more about the dynamics of post-merger market self-correction.

Estimating the unpooled sample does a lot more than just showing whether the merger significantly increased prices in the K-th period. It gives the researcher highly valuable information on the dynamics of post-merger prices. This is important not only because one cares about whether prices eventually revert to pre-merger levels, but it also allows the identification of whether prices remain stable or unstable over time. These two outcomes can have very different welfare implications and would lead to different policy conclusions. The literature on the welfare implication of price stability can offer us more general insights, starting from simplistic models of Waugh (1944), and followed by the highly influential work of Newbery and Stiglitz (1979) and Stiglitz (1981). Growing out of this tradition we can find a rich body of works that look at the effect of price stability making realistic assumptions about consumer utility functions and introducing risk-attitude (Turnovsky et al. (1980)). These models show that depending on consumers' risk-averseness, stable prices may mean lower or higher associated consumer surplus than volatile prices. The bottom line however is that the welfare implications of stable and volatile post-merger prices are likely to be different, therefore it matters which empirical model is used for estimating the changes in these prices.[16]

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[16]We provide a simple back-of-the-envelope demonstration in the final section of the Appendix.

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A Appendix

A.1 Papers used to create sample

A.1.1 EU sample

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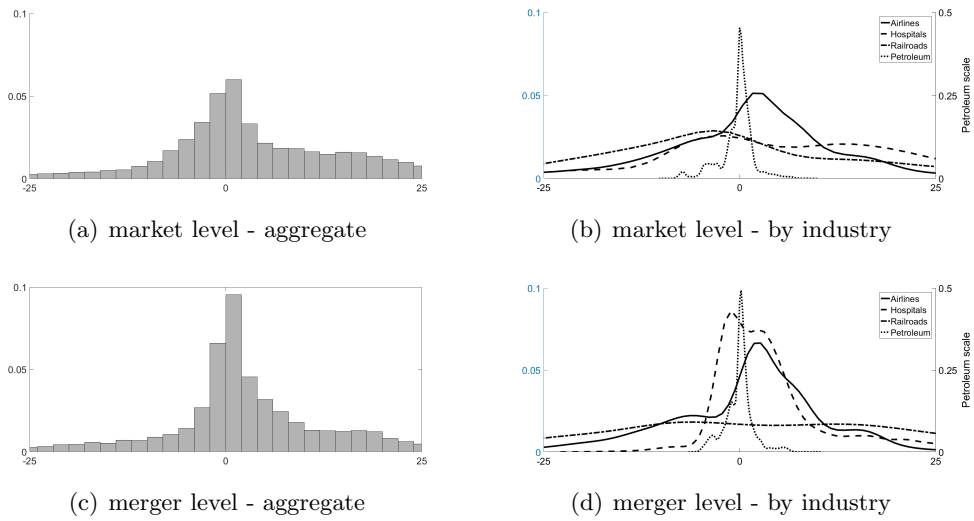
A.2 Descriptive statistics

Although not directly relevant for the main argument in this paper, we can reveal how the estimates in our meta-analysis vary across industries. Table 4 shows the industry average price effects (weighted by the inverse t-stat). Figure 2 shows the empirical distribution of these estimates for the industries with the largest number of estimates.

Airlines, hospitals, and petroleum are the most researched areas, which is probably due to data availability. From these estimates only, it appears that petroleum mergers on average do not increase prices (the density curves in Figure 2 also show that the estimates are very concentrated around zero). Rail mergers are typically not anti-competitive. On the other hand airline mergers seem to be price-increasing. Finally, the bimodal density of hospital estimates suggests two groups of hospital mergers, one clearly price-reducing, and another clearly price-increasing.

Table 4: Market-, and merger-level estimates by industry

Industry	Market level estimates			Merger level estimates		
	N	es	se	N	es	se
Airlines	28	1.936	0.378	14	2.405	0.215
Books	11	0.022	0.156	2	1.028	0.584
Brewing	7	-1.915	0.457	1	-1.557	1.157
Car Parking	32	2.581	0.382	1	1.866	1.938
Cement	2	-27	8.583	1	-27	12.130
Computer games	2	-19.087	3.713	1	-19.083	5.251
Corrugating medium	2	0.950	4.727	1	0.950	6.672
Diazo microfilm	18	11.100	1.445	1	11.100	5.394
Energy	1	-6	1.255	1	-6	1.255
Health care	1	14.683	5.548	1	14.683	5.548
Home appliances	16	0.617	0.492	1	-0.175	1.750
Hospitals	338	11.628	0.526	10	6.822	2.301
Law journal	11	13.267	1.155	4	11.069	1.502
Petroleum	54	-2.039	0.097	17	-0.361	0.218
Pharmaceuticals	9	33.271	0.299	1	18.489	0.979
Rail	30	0.951	1.584	3	1.111	4.662
Ready-to-eat cereal	11	9.346	0.564	1	5.164	1.455
Scientific journal	3	5.544	1.619	1	4.333	2.723
Spirits	28	0.155	0.158	1	-0.679	0.889
Supermarkets	1	0.004	0.028	1	0.004	0.028
Telecoms	2	1.000	3.307	2	1.000	3.307
Titanium dioxide	1	28	12.219	1	28	12.219
Vesticular microfilm	18	22.839	2.329	1	22.839	8.869
Total	626	11.164	0.255	68	4.189	0.320

Figure 2: Empirical distribution of price effect estimates for market and merger level data

(a) market level - aggregate

(b) market level - by industry

(c) merger level - aggregate

(d) merger level - by industry

A.3 Additional figures

### A.3.1 The effect of serial correlation and clustering

In the main text we have assumed that there is no serial correlation in our simulated price data. We now relax on that assumption. Bertrand et al. (2004) show that as a result of serial correlation OLS standard errors badly underestimate the standard deviation of the estimators. This means, that in the case of positive serial correlation difference-in-difference studies (using OLS) would falsely estimate significant intervention effects. For this reason we look at how having serially correlated price data affects the above findings.

We define the error term as an AR(1) model with serial-correlation parameter ρ set to 0.8. We first estimate the pooled and the unpooled samples by OLS and then we cluster the standard deviation of the estimated coefficients and re-estimate both models.[17] We confine the simulations to two cases, first, where the cross-sections are large and time period is small, and vice versa. In each case we plot the difference in the probability of estimating a significant price increase in the pooled sample, relative to the unpooled sample. Figure 3 plots how much more likely it is for the pooled sample to estimate a significant price increase than the unpooled sample.

The figure shows that our initial findings (Figure 1(a)) still hold even with serially correlated data. In fact we find that serial correlation (or the clustering of standard errors) does not noticeably affect the difference between the pooled and the unpooled samples. We repeated these experiments for the other scenarios and found the same result.

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[17]The cluster-robust formula for the estimated variance-covariance of the OLS estimator is: $\widehat{V}\left(\widehat{\boldsymbol{\theta}}\right) = (\mathbf{X}'\mathbf{X})^{-1}\left(\sum_{m=1}^{M}\mathbf{X}_m'\widehat{\mathbf{u}}_m\widehat{\mathbf{u}}_m'\mathbf{X}_m\right)(\mathbf{X}'\mathbf{X})^{-1}$, with $\boldsymbol{\theta}$ including all coefficients.

Figure 3: Type I error: Price change in period 1 and no price change in period 2 (clustered standard errors)
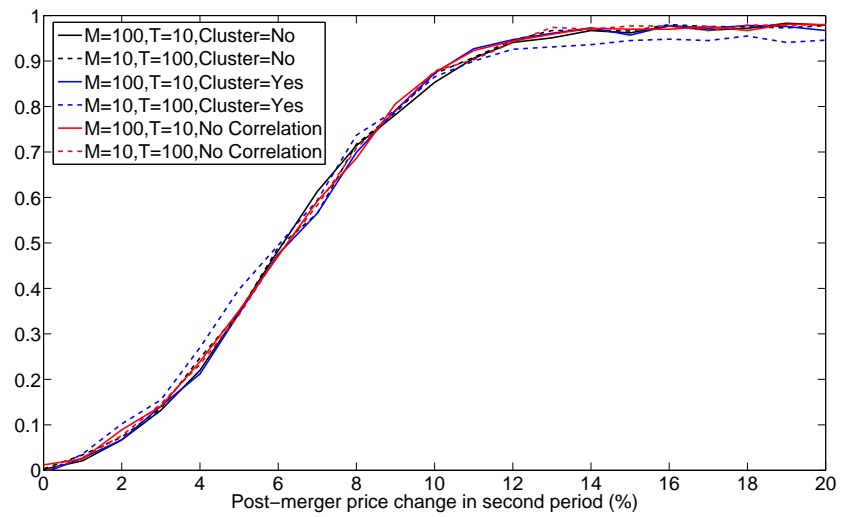
A.3.2 Robustness checks simulations

Figure 4: Assuming different levels of standard deviation for the simulated model

(a) $\delta_1 = [0, 0.2]$ and $\delta_2 = 0$, sd=0.1

(b) $\delta_1 = [0, 0.2]$ and $\delta_2 = 0$, sd=0.4

(c) $\delta_2 = 0$ and $\delta_2 = [0, 0.2]$, sd=0.1

(d) $\delta_2 = 0$ and $\delta_2 = [0, 0.2]$, sd=0.4

(e) $\delta_1 = [-0.2, 0]$ and $\delta_2 = 0.05$, sd=0.1

(f) $\delta_1 = [-0.2, 0]$ and $\delta_2 = 0.05$, sd=0.4

(g) $\delta_1 = \delta_2 = [0, 0.2]$, sd=0.1

(h) $\delta_1 = \delta_2 = [0, 0.2]$ sd=0.4