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

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The price-effect of past mergers has been extensively researched over the past two decades. The overwhelming majority of these studies estimate the over-time average price effect of the merger. Merger guidelines agree that mergers should be approved if market dynamics, such as entry, eliminate negative welfare effects. Estimating price averages ignores key information about the post-merger dynamics of prices and is unable to identify if post-merger prices eventually revert to pre-merger levels. We provide evidence from a set of Monte Carlo experiments to show how serious this problem might be. Firstly, potentially all the studies that concluded - estimating post-merger over-time averages - that the merger led to a price increase, could have been wrong, and in fact the merger price increase disappeared within a reasonable time. Similarly, up to half of the studies that concluded that the merger did not increase prices could have been wrong in their conclusion.

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The price-effect of past mergers has been extensively researched over the past two decades. The overwhelming majority of these studies estimate the over-time average price effect of the merger. Merger guidelines agree that mergers should be approved if market dynamics, such as entry, eliminate negative welfare effects. Estimating price averages ignores key information about the post-merger dynamics of prices and is unable to identify if post-merger prices eventually revert to pre-merger levels. We provide evidence from a set of Monte Carlo experiments to show how serious this problem might be. Firstly, potentially all the studies that concluded - estimating post-merger over-time averages - that the merger led to a price increase, could have been wrong, and in fact the merger price increase disappeared within a reasonable time. Similarly, up to half of the studies that concluded that the merger did not increase prices could have been wrong in their conclusion.

Keywords: mergers, merger retrospectives, difference in differences

JEL Classification codes: C51, K21, L49

1 Introduction

The price-effect of past mergers has been extensively researched over the past two decades. The recent upsurge in the number of such studies generated a considerable body of evidence of the price effect of mergers. Kwoka (2013, 2015) identified more than 60 US studies that looked at the price effect of mergers using simple methods of causal inference. Mariuzzo et al. (2016) reviewed another 20 similar European studies.

Methodologically, these studies are rather homogeneous, using a standard difference-in-differences type model, differing mainly in the way the counterfactual is constructed.

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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 an average post-intervention price-effect, overlooking post-merger price dynamics. In general, in many causal inference studies the post-intervention variation 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 those.

Estimating over-time average price effects 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 says: "*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.*"[1] 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.*"[2] 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 were fulfilled after a merger. However, entry and its effects are a gradual process, which would require 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, estimating the average post-merger effect requires a different empirical model than estimating annual effects. It is possible - and this is the main focus of our paper - that the two models return different evidence on the price effect of the merger. 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 changed post-merger is crucial. We show below that that the probability 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

[1] U.S. Department of Justice & Federal Trade Commission, Horizontal Merger Guidelines (2010)

[2] Guidelines on the assessment of horizontal mergers under the Council Regulation on the control of concentrations between undertakings (2004/C 31/03).

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 that collects qualitative evidence on how market characteristics (other than just price) evolve post-merger. Regarding specific mergers, Clifford Winston (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.

Both of the latter two works find that the negative effects of mergers disappear after the merger, although this takes longer than two years. In light of this, it is somewhat puzzling that more than half of all published previous merger retrospectives looked at price effects within only two years of the merger. Nevertheless, the purpose of this paper is not to find a consensus on how quickly prices might revert to pre-merger levels, rather to highlight the importance of acquiring evidence on post-merger price dynamics. We contribute to the rich body of merger retrospectives in an important way, by pointing out how inappropriately designed 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 also different from the standard errors of the annual price effects. Then we provide a set of Monte Carlo simulations to illustrate the problem. These 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 a pooled model is more likely to lead to the conclusion that the merger did not increase prices even though it 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 model and the model that estimates yearly effects. This is followed by a set of simulations to demonstrate the magnitude of the problem, and we conclude with policy implications.

2 How post-merger price-dynamics are handled by previous literature

We surveyed a large body of retrospective merger studies that were reviewed by Kwoka (2013) and Mariuzzo et al. (2016).[3] The main selection condition for our sample was that ex-post price-effect estimates were provided in the study, and the time span of the data was identifiable. All the studies in our sample used reduced form causal inference models (typically difference-in-differences).

Our sample contains price-effect estimates of 67 mergers (discussed in 37 published papers). 47 of these were US mergers, and 20 EU ones.[4] We focused on one aspect of these studies, the model specification for estimating post merger price effects. More specifically, we distinguish between two types of studies. The first, and by far the more favoured one 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. The second group pools price observation year by year, and estimates annual post-merger price changes. The model specifications for these two approaches are given in Section 3.1.

2.1 Average post-merger price effect

55 merger retrospectives (>80% of the sample) estimated the over-time average price change following the merger. More than half of these (31) estimated the average price effect within 2 years of the merger and almost 90% of them looked at the effects within 3 years post-merger. We summarised the headline price-effect estimates from these studies.[5] Table 1 shows the number of observations, the average of the post-merger price effect estimates and the standard deviation of these estimates, broken down by the post-merger time-window that was used in the study. On average the estimated price effect when only data from the year of the merger is used is zero. When the post-merger 2-, and 3-year averages are used, the average of all estimates is positive.[6] There are 7 merger studies that estimate price effect 4-year (or longer) after the merger.

There are two problems with these estimates. First, more than half of them looks at effects within two years after the merger, and therefore it does not allow a full assessment of how prices evolve over time. Both the EU and the US guidelines emphasise that a

[3] A full list of these studies is provided in the Appendix.

[4] Kwoka (2013, 2015) and Mariuzzo and Ormosi (2016) provide more information about these mergers.

[5] Most studies typically provided more than one estimate. To use a single figure we followed the same method as Kwoka (2013).

[6] The average of the 2-, and 3-year estimates are significant.

Table 1: The price-effect of mergers - over time averages

Post-merger time widow	count	mean	sd
1 year	13	0.01	3.79
2 years	18	4.89	8.85
3 years	17	4.41	13.87
4 years	2	18.55	6.01
5 years	1	-6.00	.
6 years	3	5.07	21.76
8 years	1	1.85	.
Total	55	3.84	10.82

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. Second, they ignore over-time price variation that follows mergers. Economic theory, and the relevant merger guidelines agree that it might take time for the price-effects of a merger to fully unfold. Estimating an over-time average completely masks the dynamics of post-merger prices and as such are inadequate for evaluating the effectiveness of merger control.

2.2 Annual post-merger price effect

Less than 20% of the merger retrospectives (12 merger studies in 6 papers) estimated the post-merger price effects year-by-year. For 7 out of these 12 studies price effects were estimated only for the first two years following the merger. All of these studies report price-change estimates for each year following the merger. When treating these annual estimates separately, we can record 42 estimates.

Table 2 shows the averages of the annual estimates. For these mergers, on average, prices tended to be negative from the 4th year following the merger.[7] This appears to imply that the price increasing effect of many mergers only lasts until the end of the 3rd year after the merger - although we do not want to draw a conclusion on that question. What we wanted to highlight was simply that using average post-merger estimates would not have been able to identify this pattern. To show this, Table 3 reports the price-effect estimates averaged in each study over the annual estimates. The

[7] Of course caution is warranted on estimates many years after the merger where confounding effects are more likely to muddle the picture.

Table 2: The price-effect of mergers - yearly estimates

Year after the merger	count	mean	sd
1st year	16	0.94	15.89
2nd year	8	3.77	6.37
3rd year	5	6.98	6.82
4th year	4	-3.56	9.13
5th year	4	-0.38	13.84
6th year	2	22.50	3.54
7th year	1	-12.00	.
8th year	1	-2.00	.
9th year	1	18.00	.
10th year	1	-18.00	.
Total	43	2.22	13.02

pattern that we observed on Table 2 disappears.

Table 3: The price-effect of mergers - yearly estimates averaged over each study

	count	mean	sd
Within 1 year after the merger	4	-4.98	14.29
Within 2 years after the merger	3	0.17	3.63
Within 3 years after the merger	1	9.91	
Within 5 years after the merger	2	1.64	2.30
Within 6 years after the merger	1	10.67	
Within 10 years after the merger	1	2.40	
Total	12	2.22	7.86

In the following section we formulate the difference between estimating average or annual post-merger price effects. We will show that evidence on average price-effects can lead to misguided conclusions on the effect of the merger.

3 Misguided assessment of merger control

One of the main reasons for conducting merger retrospectives is to improve our understanding of the effectiveness of merger control (Kwoka, 2013). For this reason 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. Merger guidelines are clear that a merger should be approved if its initial negative effect (price-increase) is only temporary. Therefore, 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 identify these cases. Instead, it could provide estimates that would suggest that merger control was inefficient - even in cases where post-merger prices gradually revert to pre-merger levels.

3.1 A conceptual framework

Let p_{mt} denote the price of a product or a firm in market m, at time t, and let W_m be the treatment dummy (firms/products involved in the merger). Denote with d_k a dummy that equals 1 in the k^{th} period after the merger, and with $D_K \equiv \sum_{k=1}^{K} d_k$ a pooled dummy set to K periods after the merger. There is one period pre-merger. Furthermore, indicate with $\tau = 1, ..., T$ a unit of time within a period k, for example τ can measure days and k years when we have daily price data but are estimating yearly price effects. Looking at yearly effects for K periods following the merger we can express the linear econometric equation for prices as:

$$\begin{gathered} p_{mt} = \alpha_m + \lambda_t + \beta_0 + \beta_1 W_m + \sum_{k=1}^{K} (\gamma_k d_k + \delta_k d_k W_m) + \varepsilon_{mt}, \\ m = \{1, 2, \cdots, M\},\ t = \{-T_L, \cdots, -1, 0, 1, \cdots T_R\}, \end{gathered} \tag{1}$$

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_t a set of controls that capture possible seasonality in the data. The total time observations are $T \times (K+1) \equiv T_L + T_R + 1$. Below we use the shorthand term '*unpooled model*' to refer to Equation (1).

If we consider an analysis restricted to only two periods after the merger, the DiD coefficient for the second period from the above equation when separate time period dummies are included can be written as:

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

As we showed above, 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 dummy post-merger:

$$p_{mt} = \alpha_m + \lambda_t + \beta_0 + \beta_1 W_m + \Gamma_2 D_2 + \Delta_2 D_2 W_m + \varepsilon_{mt}, \tag{3}$$

where the DiD estimator for a pooled period of length 2 is denoted with Δ_2. Equation (3) can be seen as a restricted version of Equation (2) once we impose $\gamma_1 = \gamma_2 = \Gamma_2$, along with the further restriction $\delta_1 = \delta_2 = \Delta_2$. In this paper we use the shorthand term '*pooled model*' to refer to Equation (3).

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

$$\begin{aligned} \Delta_2 \;=\; & \left(E\left[p_{mt} \mid W_m = 1, (d_1 + d_2) = 1\right] - E\left[p_{mt} \mid W_m = 0, (d_1 + d_2) = 1\right]\right) - \\ & - \left(E\left[p_{mt} \mid W_m = 1, (d_1 + d_2) = 0\right] - E\left[p_{mt} \mid W_m = 0, (d_1 + d_2) = 0\right]\right). \end{aligned} \tag{4}$$

What are the implications, for the ex-post merger analysis, of using period-by-period dummies version a pooled period dummy? Take the example where we are looking at two periods following the merger. Assume that there is a price increase in the first period: $\delta_1 > 0$ but then prices revert to pre-merger levels in the second period: $\delta_2 = 0$, i.e. this is an merger that would be considered unharmful. Suppose we have estimated the parameters $\widehat{\Delta}_2$, $\widehat{\delta}_1$, and $\widehat{\delta}_2$. It can be shown that the restricted estimator $\widehat{\Delta}_2$ is equal to the average of the yearly time period parameters, $(\widehat{\delta}_1 + \widehat{\delta}_2)/2$ (provided that the weak law of large numbers holds).

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, $\widehat{\delta}_1 \sim Normal\left(\delta_1, \frac{\sigma^2_{\delta_1}}{N}\right)$, $\widehat{\delta}_2 \sim Normal\left(\delta_2, \frac{\sigma^2_{\delta_2}}{N}\right)$, leading to $\widehat{\Delta}_2 \sim Normal\left(\frac{\delta_1+\delta_2}{2}, \frac{\sigma^2_{\Delta_2}}{N}\right)$, with $\sigma^2_{\Delta_2} \approx \frac{1}{4}\left(\sigma^2_{\delta_1} + \sigma^2_{\delta_2} + 2\sigma_{\delta_1\delta_2}\right)$. We temporarily assume $\widehat{\delta}_1$ and $\widehat{\delta}_2$ be independent random variables, and therefore $\sigma_{\delta_1\delta_2} = 0$.[8]

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 normality of the parameters,

[8] This simplification induces the variance $\sigma^2_{\Delta_2}$ to be lower than the case of positive correlation (a fairly common scenario).

which follows the assumption of central limit theory, the probability values are:

$$\Pr\left(Z_{pooled} > \frac{\left(\widehat{d_1}+\widehat{d_2}\right)/2}{\widehat{\sigma}_{\Delta_2}/\sqrt{N}}\right) \tag{5a}$$

$$\Pr\left(Z_{unpooled} > \frac{\widehat{d_2}}{\widehat{\sigma}_{\delta_2}/\sqrt{N}}\right). \tag{5b}$$

Assume a simple case, where in both periods we have a small, 1 percent price increase ($\widehat{d_1} = 0.01$, $\widehat{d_2} = 0.01$), and standard deviations $\widehat{\sigma}_{\delta_1} = \widehat{\sigma}_{\delta_2} = 0.1$. Assume also that our sample size is $N = 100$. In this case we would conclude that the merger increased prices under the pooled but not in the unpooled model:

$$\Pr\left(Z_{pooled} > \frac{0.01}{\frac{0.05}{\sqrt{100}}}\right) = 0.022 \tag{6a}$$

$$\Pr\left(Z_{unpooled} > \frac{0.01}{\frac{0.1}{\sqrt{100}}}\right) = 0.159, \tag{6b}$$

This simple back of the envelope example highlights the importance of carefully choosing which estimation model to use. Of course, the more interesting case, and the story we would like to highlight is where $\delta_1 \neq \delta_2$. In this case the difference in standard errors is analytically more involved to show, therefore we turn to a set of Monte Carlo simulations.

3.2 Simulations

In the following we conduct simulations to gauge how likely it is that the pooled model leads to the wrong conclusion on the effect of the merger. Here we consider three main scenarios: (1) where the study concludes that a merger increased prices even though the price increase disappeared within a reasonable time, (2) where the study concludes that a merger did not increase prices even though prices increased within a reasonable amount of time, and (3) where the study concludes that the merger led to a price drop even though it did not.

3.2.1 Simulation framework explained

First we simulate the price data. We associate a unitary value to prices, as if the prices were normalised to 1. 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 $sd = 0.20$ - that prices would increase or drop by a larger an extent within such a short period. For this reason we use 0.20 as our starting point but we provide the simulation results for other values in the Appendix. The price data is generated using the unpooled model in Equation (1). As our interest is on the DiD coefficients δ_1 and δ_2, we set all other coefficients to zero.

We also assume that there are $K = 2$ post-merger periods (and one pre-merger period) and each period equals a calendar year. This works for expositional purposes but also in more than half of the merger retrospective studies in our sample, only two post-merger periods were looked at. Nevertheless, 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/products analysed. This is the cross-section dimension of our data.

Next, we use the simulated price vector to estimate the parameters of the model by OLS for both the unpooled model, Equation (1), and the pooled model, Equation (3). 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 0.95).[9] We record the results of the t-tests. We then repeat this 1000 times. Finally we compare the proportion of cases where a significant price change is estimated for the pooled and the unpooled models.

3.2.2 Wrongly concluding that a merger should have been intervened

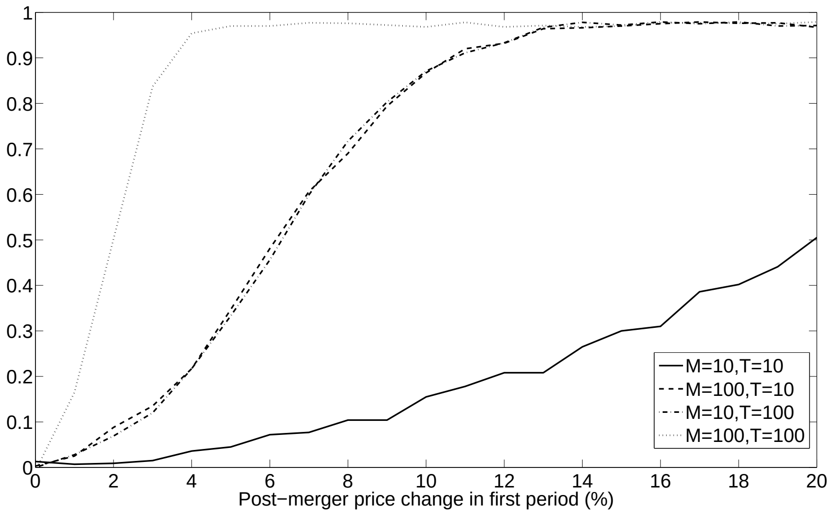
We first consider the case of a merger that should be considered unharmful because the second post-merger period price is the same as the pre-merger price. In generating the price data therefore we ensure that $\delta_2 = 0$ and allow δ_1 to take on any value between zero and a 20% price increase. Because $\widehat{\Delta}_2 = (\widehat{\delta}_1 + \widehat{\delta}_2)/2$, it is clear that the pooled

[9]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.

model estimates a price increase every time $\delta_2 > 0$, and would lead to conclude that there was insufficient intervention by the antitrust agency. Using the unpooled model would correctly estimate that $\delta_2 = 0$, i.e. there was no need for (further) intervention.[10] 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 Δ_2 pools together the observations where $\delta_1 = 1$, or $\delta_2 = 1$). Our simulations show how much this difference in the mean and standard error of $\widehat{\delta}_2$ and $\widehat{\Delta}_2$ leads to the wrong conclusion about the merger.

Figure 1 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 or products). The data is time series dominated where $T = 100$, and $M = 10$, and cross-section dominated where $M = 100$ and $T = 100$.

Figure 1: The probability of wrongly concluding that the merger increased prices when using the pooled model



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

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

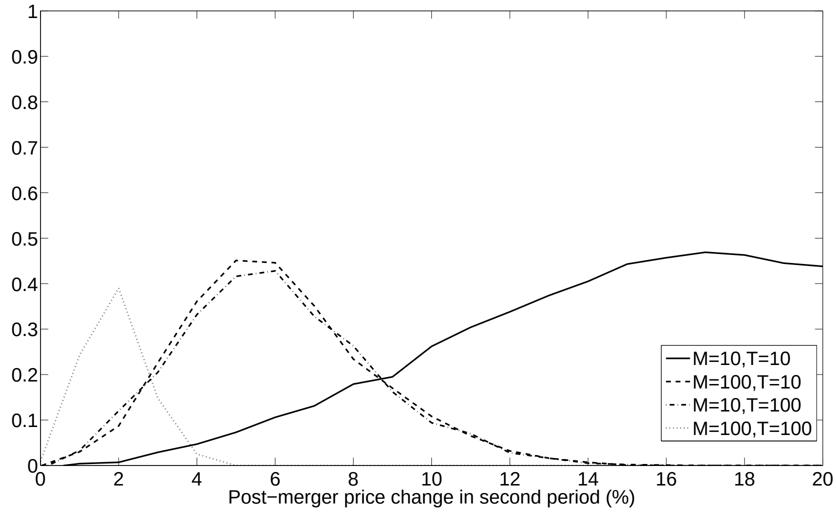
pronounced when we have a larger sample. This is as expected, with small samples, the estimated coefficients have a higher standard error and so they are less likely to be significantly different from zero.[11] As the sample size increases the $\widehat{\Delta}_2$ standard errors decrease and the hypothesis 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 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.

3.2.3 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 2 below 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 50% of the cases. For example even if one has rich time-series and cross-section data, the pooled model would still be unable to identify mergers where prices increased by up to 5% in the second year after the merger. With smaller samples the problem would be even more pronounced. For example, with monthly data over 20 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%.

[11] Figure 6 in the Appendix shows how the empirical t-distributions, which we use for testing whether the price effect is significantly different from zero, differ under the pooled and unpooled model.

Figure 2: The probability of wrongly concluding that the merger did not increase prices when using the pooled model

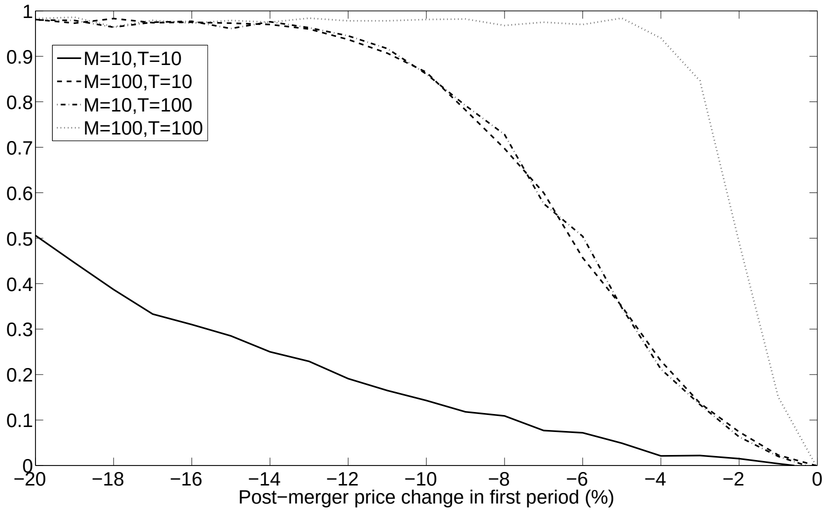


3.2.4 Wrongly concluding that the merger reduced prices

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 3 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 20 cross sections) this will almost always be the case.

Figure 3: The probability of wrongly concluding that the merger reduced prices when using the pooled model

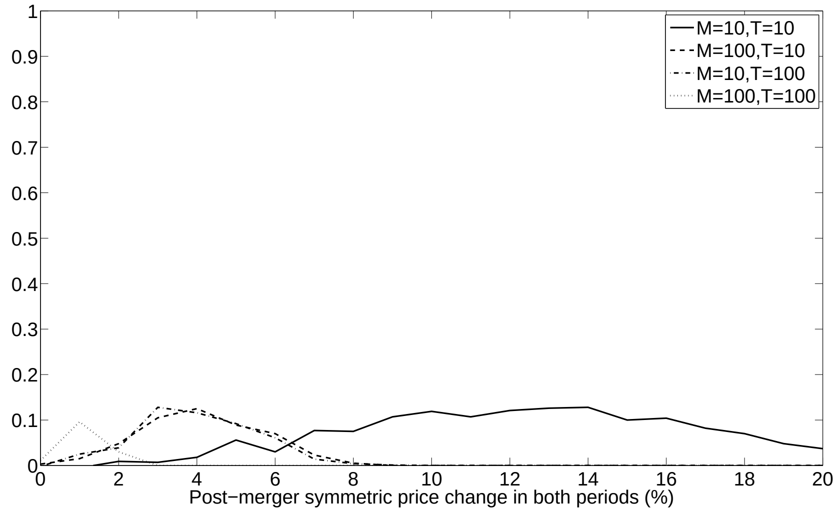


3.2.5 Mergers with constant post-merger price effect

In this final scenario we looked at how the pooled and unpooled models perform where the price effect of the merger was constant ($\delta_1 = \delta_2$). On the face of it, it would appear that there should not be any difference between the two models, because $\Delta_2 = \delta_1 = \delta_2$, but again the difference in standard errors will have a role to play. Figure 4 shows the relative probability of finding a price increase significant when post-merger prices are constant, using a pooled model. Put differently, this is the probability that the unpooled model 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

Figure 4: The relative probability of finding a price increase significant when using the pooled model



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.

3.2.6 The effect of serial correlation and clustering

So far 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.

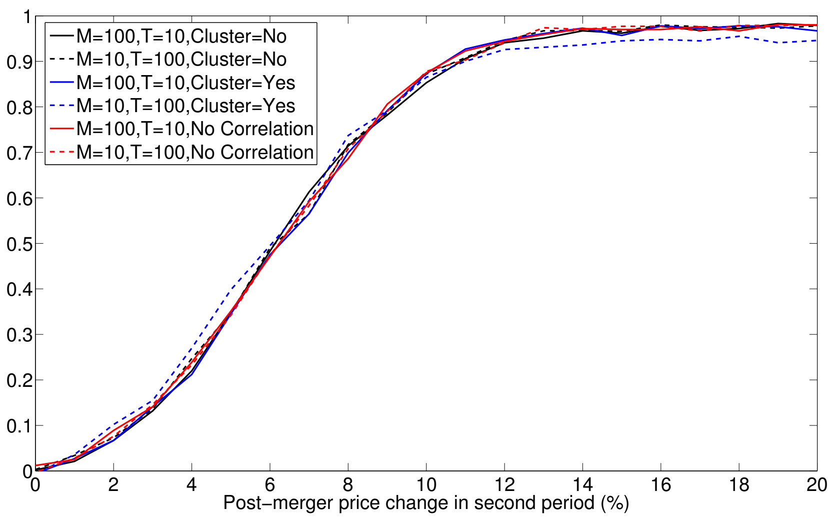
We define the error term as an AR(1) model with serial-correlation parameter ρ set to 0.8. We first estimate the pooled and the unpooled models by OLS and then we cluster the standard deviation of the estimated coefficients and re-estimate both models.[12] We

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

confine the simulations to two cases, first, where the cross-section is 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 model, relative to the unpooled model. Figure 5 plots how much more likely it is for the pooled model to estimate a significant price increase than the unpooled model.

Figure 5 shows that our initial findings (Figure 1) still hold even with serially correlated data. We plot three sets of cases: (1) no serial correlation, (2) serial correlation, and (3) serial correlation with clustered standard errors. We find that serial correlation (or the clustering of standard errors) does not noticeably affect the difference between the pooled and the unpooled models. We repeated these experiments for the other scenarios and found the same result.

Figure 5: The probability of wrongly concluding that the merger increased prices when using the pooled model - with serial correlation in data



4 Conclusion

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 model is used.

However, estimating the unpooled model 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 Stiglitz and Newbery (1979, 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 consumers 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.

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 80% (55 mergers) of the previous studies estimate post-merger average price effects (pooled model), and yearly effects were estimated for only 12 mergers (unpooled model). 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 demonstrated that potentially all studies - using the pooled model - that concluded that the merger led to a price increase, could have been wrong, and the price-increase disappeared within a reasonable time. This is more likely where the studies had large samples (for example daily price data over a large cross section). Similarly, up to half of the studies that concluded - based on the pooled model - that the merger did not increase prices might have been wrong, and 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 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 model, 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.

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B Appendix

B.1 Studies used to create sample

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B.2 Additional figures

Figure 6: Empirical t-distribution assuming no price effect

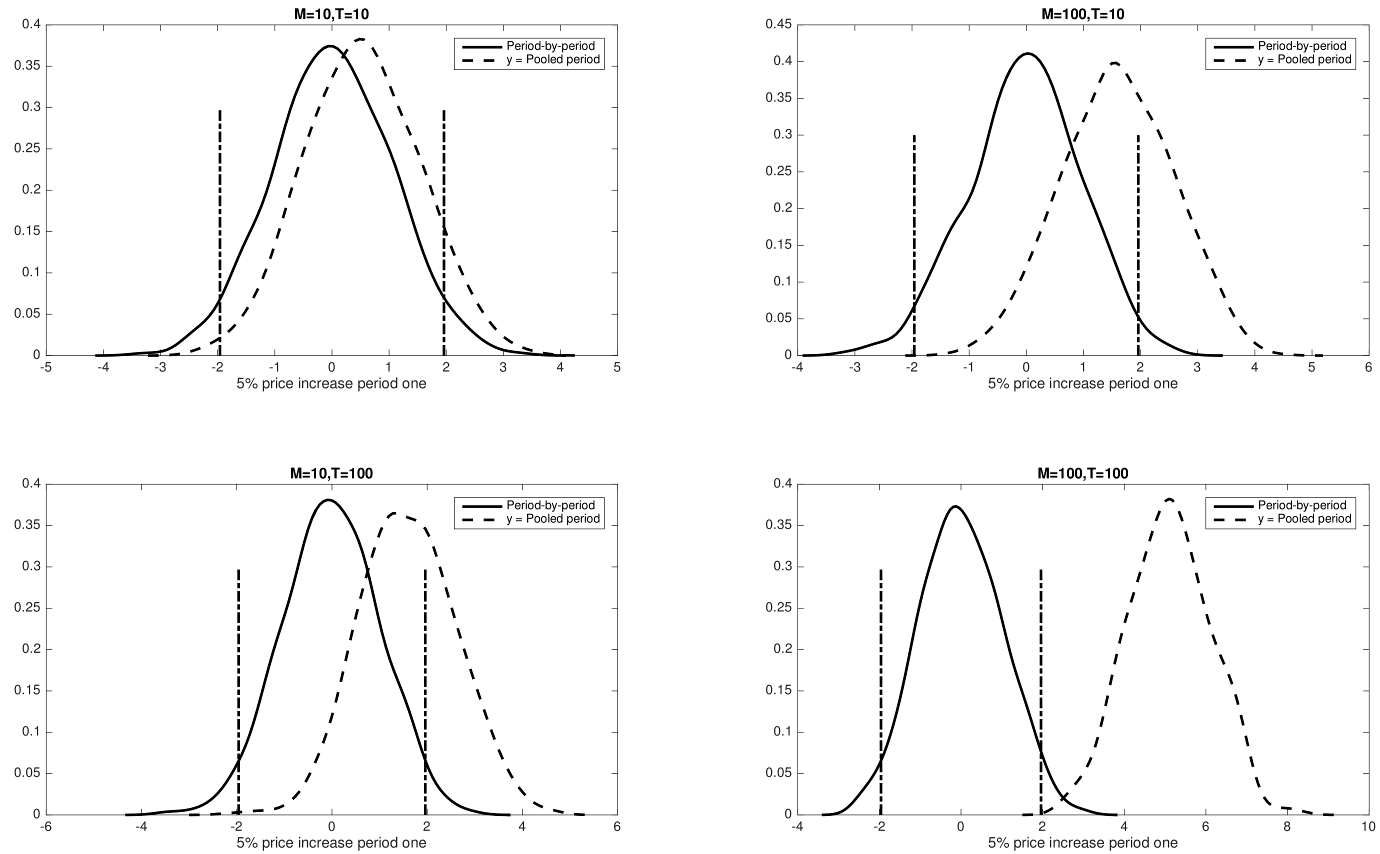
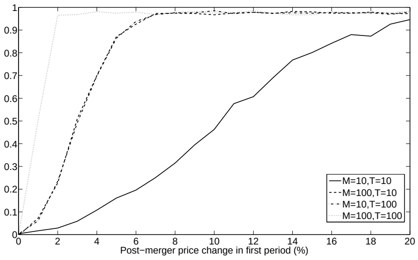
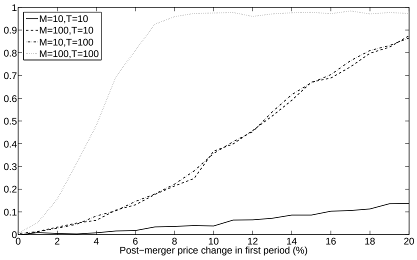


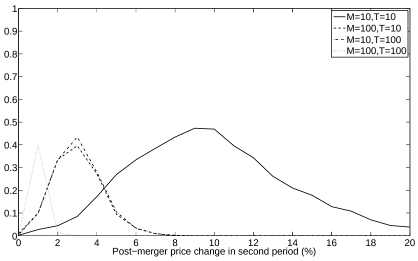
Figure 7: Assuming different levels of standard deviation for the simulated model



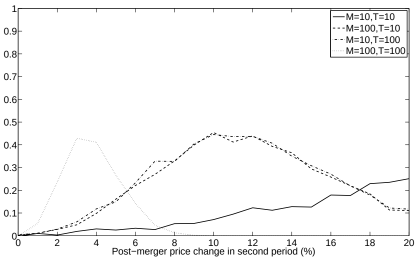
(a) Figure 1: sd=0.1



(b) Figure 1: sd=0.4



(c) Figure 2: sd=0.1



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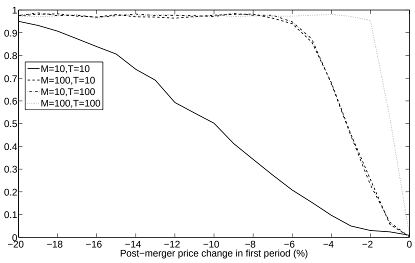
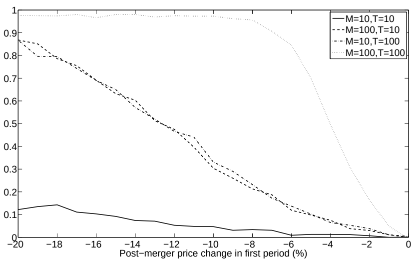


(e) Figure 3: sd=0.1



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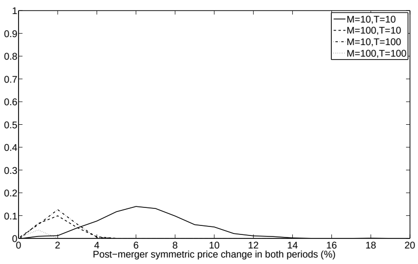
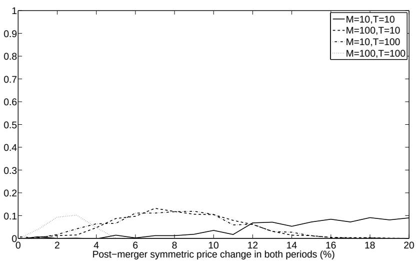


(g) Figure 4: sd=0.1



(h) Figure 4: sd=0.4