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Anti-competitive: Things that Gain from Dispersion

An Application of Synthetic Controls in Ex-Post Merger Analysis

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Abstract

This thesis tests how 16 approved mergers which were subjected to an ex-ante investigation by the Swedish Competition Authority (SCA) between 2000-2015 affected consumer prices through a synthetic control methodology. Ex-post assessments of approved mergers' effect on prices are rare due to a lack of adequate controls for non-merger related price fluctuations. We test whether synthetic controls – built upon comparable HICP price data from 30 EU/EFTA countries - can offer a solution. Comparing (1) the price of Swedish markets with large mergers between 2000-2015 and (2) a synthetic control built on prices in the same markets in 29 other EU/EFTA countries (without mergers). As predicted by the Williamson trade-off, our initial results suggest that some mergers resulted in a consumer price decrease, whilst others resulted in a price increase. However, we then subject our results to multiple robustness checks, which most recorded effects fail to pass. Leading us to conclude that the synthetic control method, absent covariates, is not robust enough to measure the price effects of mergers.

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1 Introduction

In an era of Big Tech and growing market concentrations, antitrust policy appears more relevant than it has been in a century. Just in the past year, highly publicized antitrust cases have been brought against Facebook for acquiring Instagram and WhatsApp, and Google for acquiring dozens of rivals search engines (Federal Trade Commission, 2021). To prevent such market concentrations, policymakers usually rely on merger controls, which grants competition authorities the right to (ex-ante) investigate, and prohibit, mergers that seriously harm competition, and by extension, consumer welfare. However, Facebook's and Google's acquisitions were subjected to such ex-ante merger controls, and were approved. This has lead some to conclude that ex-ante merger controls, which estimate the competition effects of mergers before they occur, are no longer fit for purpose (European Commission, 2021).

Thus, in order to investigate more closely the efficacy of the ex-ante merger control methods employed, governments (and academics) are increasingly turning toward ex-post merger analysis, which estimate the competition effects of mergers that have already occurred. However, this requires methodology which is able adequately to control for non-merger related effects: Did Instagram grow from a 1 Billion USD to a 20 Billion USD juggernaut in 8 years because of its Facebook acquisition? Any ex-post merger control must be able to account for this concern.

This problem is best illustrated in terms of pricing: if a milk company acquires its main competitor, and the price of milk increases by 20% the year after, was it because of the merger? Perhaps, but it may also have been the result of, for instance, changing input costs, a spike in demand, or an exogenous negative supply shock. Hence, whilst ex-post merger analyses appear appealing in theory, they are rarely applied in practice due to the difficult nature of adequately controlling for non-merger related effects (Swedish Competition Authority, 2018; Buehler et al., 2017).

However, in other econometric fields - for want of access to the multiverse - the absence of adequate controls is increasingly being addressed through the application of synthetic control methods (Abadie, 2020;

Abadie et al., 2015; Cunningham, 2018; Powell, 2018). Yet in the field of ex-post merger assessments, they have not been broadly applied, though the EU wants to "explore it" as part of a wider push for ex-post merger controls (European Commission, 2021). Our thesis seeks to contribute to this emerging discussion by building a synthetic control to test how 16 approved mergers subjected to an ex-ante investigation by the Swedish Competition Authority (SCA) between 2000-2015 affected consumer welfare, by looking at price trends in the related markets following the mergers.

Whilst thousands of mergers have been reviewed by the SCA, less than a hundred have required a special ex-ante investigation, and these are the borderline cases where a merger is approved or not. Hence, evaluating special investigation mergers function as an effective proxy for how effective Swedish merger controls are at preventing harmful mergers in general. To the best of our knowledge, no other economic paper has used this legal threshold before to test the effectiveness of Swedish merger controls.

Likewise, the study will be amongst the first to apply a synthetic control approach to ex-post merger analysis of price developments. We use a large novel data set of 86,000 months of prices from 30 EU/EFTA countries in 16 different HICP product markets since 1996, to build our synthetic controls. we use these models to estimate price effects caused by each merger, and review the consumer welfare effects that follow.

Furthermore, we thoroughly test the generated models for robustness, in order to evaluate the reliability of the results, and by extension, the applicability of the synthetic control method going forward in the ex-post merger analysis area of research.

1.1 Research Questions

The study therefore seeks to answer two questions. **(1) have approved mergers previously subjected to a special ex-ante investigation by the SCA between 2000 and 2015 affected consumer welfare?**. And since we rely upon a novel synthetic control methodology to answer this question, we must also secondarily address: **(2) Are synthetic controls robust enough to measure reliably the**

consumer price effects of mergers?

1.2 Outline

To explain our rationale for using synthetic controls on ex-post merger controls, we will begin with a literature review which summarizes the relevant IO-theory, literature on merger controls, and the synthetic control method. We also give an overview of the legal circumstances that enable the analysis.

Next we will describe in more detail the method employed and what each step entails. We give an overview of how the basic synthetic control method works, how the In-Space Placebo method works, how it can be used for inference, and how it can be tested for reliability by excluding ill-fitting placebo models from the analysis. Lastly, we describe two robustness checks: the In-Time Placebo test and the Leave-One-Out test, and how they can be used to evaluate the reliability of the inference generated by the synthetic control base model.

Our initial results indicate that prices modestly increased in most markets following a merger, and decreased in others. This was true in both the base model visual comparison and in the inference test using the In-Space Placebo method. The same tendencies were apparent in the fit-restricted inference test, but the magnitude somewhat decreased. This is surprising since the SCA is not supposed to approve any merger which results in higher consumer prices, and suggests that the ex-ante merger controls imposed by the SCA don't always successfully prevent mergers harmful to consumers. But it fits with the Williamson trade-off hypothesis that the price effects of mergers vary based on the relative size of cost savings and allocative inefficiencies arising from increased market power.

However, we then conduct the In-Time Placebo and Leave-One-Out robustness checks on our results, finding that many markets fail the In-Time Placebo test, and reduced effect sizes are seen in the Leave-One-Out tests. The conclusion to be drawn from this is that price-effects from mergers, contra non-merger price fluctuations, appear to be too small to break through the noise using this synthetic control method.

Consequently, we conclude that we cannot confidently answer research question **(1)**, since the answer to our secondary research question **(2)** is that the synthetic control method used is not robust enough reliably to measure the price effects of mergers. Hence, whilst the need for ex-post merger controls remain, synthetic controls so described may not be adequate. We therefore discuss how the method can be extended in further research, and maybe then be reliable enough to be fit for purpose - namely by including relevant covariate data in the analysis. We also discuss the role of the synthetic control method in light of available alternatives, especially the Difference-in-Differences method, and argue that while imperfect, the synthetic control method shows promise to be an improvement over it in this setting.

We also discuss several limitations and strengths of this study, along with any policy implications to which the study might be of interest. The last section concludes.

2 Background

To justify our synthetic control methodology in an ex-post merger analysis, we briefly review relevant existing literature on merger theory, merger controls, and the synthetic control method. We also give a summary of the legal circumstances around mergers in Sweden that enable this study.

2.1 IO Theory

The effects of different market structures on price and output form the bedrock of most introductory economics courses in the world. Fortunately, for the purposes of this thesis, there is little need to go much further than the content of those introductory courses, hence a brief summary of key points will suffice.

2.1.1 Oligopoly Theory

The first key point is that higher market power is associated with higher consumer prices in the literature (De Loecker & Eeckhout, 2011; Davis & Orhangazi, 2021; Hirsch & Koppenberg, 2020; Khan & Peters, 2015). The Cournot model, under simplified assumptions, explains this as a result of dominant firms being able to put price above marginal cost to maximize their profit function (Motta, 2014).

Most papers which show that increased market power (through mergers) may result in higher consumer prices rely upon structural or reduced-form methods (Motta, 2014). This paper will also rely upon a reduced-form approach, applying the synthetic control as a novel contribution.

2.1.2 The Williamson Trade-off

The second key point is that mergers can have conflicting effects depending upon whether the cost savings created by the merger offset the allocative inefficiency arising from increased market power (Williamson, 1968). This was first noted by Nobel Prize-winning economist Oliver Williamson, thus being commonly known as the Williamson trade-off. Expressed differently, if economies of scale (or other synergy effects) from a merger are great enough, so that they dominate the negative impact of increased market power, market concentrations can actually benefit consumers by lowering prices.

Contrary to common belief, it is arguably the Williamson trade-off, rather than traditional oligopoly theory, which serves as the theoretical basis for merger controls. The relationship between market power and price is not strictly positive; were that the case the case, then no merger should be approved. The reason that we control, rather than prohibit, mergers, is to allow those with sufficiently large relative cost-saving functions to benefit consumers to go ahead, whilst stopping those that are harmful (Bet & Blair, 2019).

Another dimension of the Williamson trade-off is that the synergy-effects from mergers may take longer to manifest (Duso et al., 2007). In theory, the profit-maximising function of raising prices (higher) above marginal cost following a merger would occur immediately, but the integration of factors of production between merging parties (to enable synergy) would take time. Hence, immediately following a merger, the merger may raise prices, until synergy effects manifest and reduce them. Consequently, the immediate competition effect of a merger may not be representative (ibid.).

This paper looks for evidence of both these theoretical hypotheses by looking at the price development of several mergers (16) over a longer time frame (5 years) than most other studies. Hence, if most mergers resulted in comparative price-decreases, which grow over time following the merger, it is evidence for the synergy-effects dominating the allocative inefficiencies-effects in the Williamson trade-off, in these cases.

2.1.3 Consumer Welfare Theory

The third key point is that consumer welfare has prevalently been modelled using a utility function which is strictly increasing in consumption, with a strictly decreasing marginal utility of consumption (Gorman, 1957). Some functional forms which satisfy these conditions are log utility and root utility functions.

Furthermore, when consumer welfare is considered in a cross-temporal framework, time preferences are modelled using a discount factor that compounds multiplicatively over time (Mas-Colell et al., 1995. pp. 733-736), so called net present value models. This is especially useful where welfare effects are either shown to be, or hypothesised to be heterogenous over time (see e.g. the Williamson trade-off), since it allows a vector of results to be summarised with a single measure that is arguably more true to reality than a simple average of outcomes. Hence this paper will utilise such a model as a measure of consumer welfare from changing consumer prices, instead of looking at a snapshot of price effects in a time period some time after the merger. This will allow us to answer the first research question more closely, especially in light of the mechanisms of the Williamson trade-off.

2.2 Merger Controls

Given the theoretical harm that may arise from mergers, governments have regulated their use through merger controls (Affeldt et al., 2021; Kwoka, 2013; Lin & Zhao, 2012). Merger controls can be defined as the dual-right of public authorities to (1) review large mergers, and, if deemed (seriously) harmful to competition, (2) prohibit these mergers from occurring (Lutzker, 2002).

Today most economies take for granted that merger controls exist, but they are recent inventions:

In 1989, only 8 jurisdictions had merger controls, but by 2009, 115 did (Swedish Competition Authority, 2012). Amongst them are Sweden and the EU, which collectively review, and approve, all large mergers in Sweden (Gustafsson & Westin, 2010; Horn & Stennek, 2007).

2.2.1 Legal Theory of Merger Controls

Legally, mergers are justified on the basis that dominant companies may raise prices for consumers, and stifle innovation, hence laws that prohibit mergers and acquisitions that reduce competition may be warranted (Duso et al., 2011). Whilst technical details vary, all merger controls rest upon this fundamental legal/economic principle: the creation of dominant market power reduces effective competition, which in turn harms consumer welfare (ibid.). However, some scholars have argued that in practice, merger controls often harm consumer welfare and stifle innovation (Geroski, 1998). Either directly by prohibiting efficient mergers, or indirectly by scaring off efficient mergers from being attempted in the first place (Kwoka, 2013).

Prohibiting mergers may also prop up inefficient firms that may otherwise be acquired, which also harms economies of scale and distorts resource allocations. Consequently, as Motta puts it, merger controls are concerned with defending market competition in order to increase welfare, not defending competitors (2014). Hence, in practice, merger controls, to be legal, must strike a balance between potential welfare-gains from stopping a harmful merger and the potential inefficiencies generated by government interventions in market structures (Gustafsson & Westin, 2010; Kwoka, 2013).

2.2.2 Merger Controls in Sweden

Swedish firms are prohibited from abusing a dominant market position by national law (2 Ch. 7 § konkurrenslagen 2008:579) and, due to the doctrine of direct effect, EU-law (Article 102 of the Treaty of the Functioning of the European Union). Legal praxis establishes a "dominant market position" to refer to any firm with a market share in excess of 40%, but with the reservation that this varies from market to market (Gustafsson & Westin, 2010).

Based on this, Konkurrenslagen (2008:579) also grants the Swedish Competition Authority some merger control powers. Firstly, any merger where the parties have a revenue in excess of a billion SEK annually, or at-least two have an annual Swedish revenue in excess of 200 million SEK, must be reported to the Swedish Competition Authority for review (4 Ch. 6§). Additionally, in cases where parties of the merger have stake across Swedish borders or where the merger otherwise risks affecting market conditions in other EU states, the case is deferred to the EU Commission for review.

Second, Konkurrenslagen (4 Ch. 1§ 2008:579) grants the Swedish Competition Authority "the right to prohibit mergers if it *seriously* harms any market competition in the country as a whole or part of it" (translated from Swedish, emphasis added). Particular attention is paid to whether the merger would result in a dominant position. That the term "seriously" is used is crucial, as it entails that mergers which only “marginally” affect competition would be allowed (Gustafsson & Westin, 2010). This is due to the trade-off with excessive government interventions in market dynamics discussed above (Gustafsson & Westin, 2010).

2.3 Merger Evaluation Methods

Merger controls, by definition, must evaluate the competition effects of mergers (Motta, 2004). In general, these evaluations are categorized as being either *ex-ante*, where the competition effects of mergers are modelled before they occur, or *ex-post*, where the competition effects of mergers are evaluated after they occurred (ibid.). In either case, evaluations can be primarily driven by theoretical considerations, be qualitative in nature, or lean more toward being quantitative, most commonly using time-series methodology or Difference-in-Differences models (Competition & Markets Authoirty, 2012).

2.3.1 Ex-Ante Merger Analysis

Virtually all competition authorities have implemented some form of procedure for estimating the competition effects of mergers ex-ante (Kwoka, 2012; Lin & Zhao, 2012). The rationale being that harmful mergers can be stopped before they occur if competition effects can be accurately estimated beforehand (Competition & Markets Authoirty, 2012).

In the case of Sweden, there is no standardized ex-ante model (Swedish Competition Authority, 2012). Instead, mergers are evaluated on an ad hoc-basis, usually containing some form of estimate for market share, since a “dominant market position” is defined by legal praxis as ≈40% of market share (ibid.). Competitors of the merging parties are also often interviewed to garner their view on the merger (ibid.). An overall assessment of these considerations is then made on whether a merger should go ahead.

Two principal issues with solely relying upon ex-ante merger controls can be identified in the literature. Firstly, multiple studies have shown that they can be inaccurate, often failing to predict actual competition effects (Polemis, 2016; Motta, 2004; Gorecki, 2014). This is understandable since, as with any ex-ante model, there will always be some epistemic uncertainty (Motta, 2004). Second, relying upon ex-ante merger evaluations alone results in unaccountability (Swedish Competition Authority, 2012). If no merger is evaluated ex post facto, there is no way to test whether the ex-ante models are accurate. This is why, over the past decade, there has been a growing push to complement ex-ante merger modelling with ex-post merger evaluations (ibid.).

2.3.2 Ex-Post Merger Analysis

Ex-post merger evaluations instead seek to determine competition effects of mergers after they have occurred (Motta, 2004). However, compared to ex-ante merger assessments, these are exceedingly rare. The Swedish Competition Authority, for example, has only conducted an ex-post merger assessment on 0.13% of approved mergers. Meaning that the competition effects of 99.87% of approved mergers are never evaluated (Swedish Competition Authority, 2012).

Three reasons are commonly presented for why. Firstly, competition authorities have little incentive to conduct ex-post merger assessments, since it critically evaluates how good the authorities are at stopping harmful mergers in the first place (Lorenz, 2013). Second, it is very resource-intensive, since information needs to be collected from (often unwilling) merging parties who have nothing to gain from cooperation (Kwoka, 2012). Finally, it is exceedingly difficult to find an appropriate control for price developments in markets (Competition & Markets Authority, 2012).

This thesis seeks to address the latter two issues by testing whether synthetic control estimators could be used to evaluate the price development of approved mergers ex-post.

2.4 Synthetic Controls

The synthetic control method was first introduced by Abadie & Gardeazabal (2003) as a solution for case studies on large units where a suitable control unit is difficult to find, in effect acting as an extension of the Difference-in-Differences methodology so common in the field of empirical economics today. By synthetically constructing a hypothetical control unit from a pool of donor units using a data-driven approach, a control unit with more similar properties than any of the donor units, individually, can be attained. Since its introduction it has seen wide usage, especially in studies on large policy interventions such as the reunification of West and East Germany (Abadie et al., 2015), the Brexit Referendum (Born et al., 2017) and the 2006 Massachusetts Health Care Reform (Powell, 2018). In a paper by Athey & Imbens (2017), it is described as "arguably the most important innovation in the policy evaluation literature in the last 15 years". Additionally, lately the field of Industrial Organisation has also seen some application of the method (see, e.g., Gugler & Szucs, 2016; Kolck & Ulrick, 2021). The starting point for this thesis is that synthetic control methods were designed to address the type of issue that ex-post merger analysis struggle with, namely the lack of a suitable control unit (for non-merger related price fluctuations), yet it has still barely ever been used in the field.

2.4.1 Theoretical Overview

Synthetic controls is a data-driven method which relies on minimizing a distance function consisting of an outcome variable and a vector of covariates relevant to the outcome variable over a defined time period preceding an event of interest (treatment). By assigning relative relevance to each variable in the loss function, a linear combination of donor units can be constructed by minimising it over the donor unit weights, which are equal to or greater than zero and sum to 1. The weights vector give rise to a synthetic unit whose outcome data can be extended to the post-treatment period and compared to the real data for the treated unit. This difference becomes the estimator of the synthetic control model (Abadie & Gardeazabal, 2003; Abadie et al., 2010; Abadie et al., 2015; Abadie, 2020).

As can be seen most saliently in Abadie et al. (2015), this approach constructs a control unit which is similar to the treated unit across all dimensions chosen by the researcher, along with the outcome variable. The argument is then that the synthetic control unit, by virtue of being similar to the treated unit in the period preceding treatment, approximates what the treatment unit would have looked like in the post-treatment period in the absence of treatment. As noted by Cunningham (2021, p. 291) and shown by Abadie & Gardeazabal (2003) however, under the condition that the number of pre-treatment periods in the data is large and the fit of the outcome variable in the pre-treatment period is good, bias of the estimator can be greatly reduced even when relevant covariates remain unobserved. Indeed, as the number of pre-treatment periods approaches infinity, the bias bound of the estimator approaches zero (ibid.).

In order to test the reliability of the estimator locally, there are a number of robustness checks to employ. First, the In-Time Placebo test constructs a synthetic model from the same data with the time of treatment artificially moved back in time. The resulting model can then be compared to the original synthetic control unit, both in terms of time of divergence (if there was one originally) and in terms of change in trends in the post-treatment period (Abadie, 2020). For instance, if an In-Time Placebo unit estimates a treatment effect in the time period between the placebo treatment and the true treatment, there is cause to suspect that the original synthetic control unit is unreliable, and the estimate invalid. As a corollary, if the In-Time Placebo unit tracks well in the whole pre-treatment period but shows a different trend in the post-treatment period to that of the original synthetic control unit, there is cause to suspect that the estimate may not be reliable.

Secondly, another tool proposed by Abadie (2020) is the Leave-One-Out Test, whereby an alternative synthetic control unit is constructed after omitting units to the donor pool. Usually, the top contributor to the original synthetic control unit is chosen to be omitted. The Leave-One-Out model can then be compared to the original in two dimensions: if the pre-treatment fit and the post-treatment trend are affected meaningfully. A robust synthetic control model should ideally be able to withstand the restriction without any great compromise to either. A failure of the test may suggest that the original estimate is unreliable owing to idiosyncratic characteristics of the omitted unit.

While a naïve approach to inference would be to satisfy oneself with a close pre-treatment fit and passed robustness tests, then taking the estimates as causal, there are several extensions in the synthetic control literature that suggest more extensive methods. One method employs so-called In-Space Placebo tests, where the treatment is artificially reassigned to untreated units normally belonging to the donor pool, one by one. These In-Space Placebo units are subsequently pooled into an outcome distribution that by definition is comprised of factors other than the treatment effect, such as noise and external shocks. Under the supposition that the true treated unit is also subject to these other factors, statistical tests can be employed that evaluate the probability that the treated unit belongs to the In-Space Placebo distribution and subsequently whether the estimated treatment effect is not equal to zero (ibid.).

One problem with this approach is that it does not take into account the reliability of the In-Space Placebo units. One suggested solution to this issue is to construct pre- and post-treatment Root Mean Squared Prediction Errors (RMSPE) in the outcome variable for each unit and compare the ratio of these measures between the In-Space Placebo distribution and the treated unit (ibid.). By evaluating where the treated unit's ratio falls compared to the In-Space Placebo distribution, a probability of the measured treatment effect not being equal to zero may be attained (ibid.). Another method is to employ a restricted sample in the inference, where a limit is set on how well an In-Space Placebo unit must fit in the pre-treatment period in order to belong to the distribution which is then compared to the treated unit's outcome (Abadie et al., 2015; Abadie, 2020). By restricting the pre-treatment RMSPE to a factor of that of the treated unit, In-Space Placebo units are excluded by the argument that a bad fit in the pre-treatment period lowers the confidence that the computed outcomes in the post-treatment period are reliable. What remains of the In-Space Placebo distribution should according to this logic be a more reliable comparison group than the full sample, despite the compromise in number of observations.

2.4.2 Applications to Industrial Organisation

While the synthetic control method has seen most application in the public policy area of economics, there are numerous exceptions and a few examples of usage in the field of Industrial Organisation, including the Mergers & Acquisitions literature. Koch & Ulrick (2021) conduct a case study on the orthopaedic physician market in Berks County, Pennsylvania, evaluating price trends following a merger. Though the main method employed is a Difference-in-Differences, they also include a synthetic control specification, both of which show significant and substantial price increases following the market concentration. Another paper by Gugler & Szücs (2016) uses a synthetic control model on 183 merger cases in markets with already high market concentrations to evaluate outcomes related to anti-competitive behaviour in competitor firms to those included in the mergers. They find that, on average, there was a significant positive effect on profit to investment ratio, but that the effect was heterogeneous with respect to specific characteristics of the market in which the merger took place. Lastly, Hosken et al. (2018) conducted an ex-post merger analysis on the retail grocery sector in the United States, looking at 14 specific cases using Difference-in-Differences, propensity score weights, and synthetic controls. They find heterogeneous effects, with a relationship between market concentration and price changes; more highly concentrated markets were more likely to suffer price increases following a merger, and the corollary was found as well.

Our paper contributes to the existing literature by applying a synthetic control method directly to test consumer price changes in markets with large mergers, building upon these past studies which focus on wider market concentrations and other competition effects.

3 Method

Following Abadie (2020), we apply the synthetic control method on 16 different merger cases on direct-to-consumer product markets, looking at price trends as the outcome variable. For each case, we use data over 5 years before and after the time of the merger. The timeframe is kept constant across the donor pool in order to increase the match over unobserved characteristics that may be constant across units but vary over time. Additionally, this is the standard in the synthetic control literature (Abadie, 2020). In order to minimize the variation in unobserved characteristics and to increase internal validity through the HICP system, the donor pool consists solely of EEA member states.

In an ideal world, it would have been desirable to look at substitutable markets, where a merger was considered, but not allowed. Looking at substitutable markets in Sweden where a merger was prevented would be impossible since, of the less than a dozen mergers the Swedish Competition Authority has stopped since 1993, none where substituable to the cases we looked at. Similarly, we viewed it as a very low chance of finding a case of a merger in the same market, at the same point in time, in another country which is otherwise comparable - but was not granted. Moreover, reading all litigation related to mergers (in foreign languages) from foreign competition authorities, to identify any such (almost) merger, would be logistically difficult, and most likely not bear any fruit in the end anyway.

As we will discuss later, this is partly captured by the inclusion of covariates on market characteristics, but it is worth pointing out what the "ideal" counterfactual would entail in theory.

Due to data limitations the analysis does not use covariates, rather the models are optimised over the outcome variable in the pre-treatment period alone. We produce results for each of the 16 cases and continue the analysis by running inference tests.

3.1 Modelling Consumer Welfare

The Industrial Organisation literature is clear on the point that price effects following mergers and acquisitions are both positive and negative (Williamson, 1968). Furthermore, these effects develop differently over time (Bet & Blair, 2019). Because of this, we choose not to look at price differences in a set time period following each merger, but rather at aggregates of the outcome, in two forms. First and most straightforwardly, we look at average excess prices, the difference between price in the synthetic model for Sweden and the actual data for Sweden, over the entire 60 month period following the treatment. Secondly, to capture more closely what the first research question specifies, we use a net present value (NPV) model for consumer welfare. The rationale behind this is that derivatives of common utility functions are for all intents and purposes linear in the local domain that a price shift from any of these mergers could cause for a consumer, given that a change in price for a good also changes consumption of alternative goods, all else being equal. We may thus for simplicity's sake assume that the utility function is linear and inversely proportional to the change in price. Furthermore, we follow common practice in the IO literature and use a quarterly discount rate of 10%[1] (Hopkinson, 2016). This gives us the model:

$$NPV = \sum_{t=1}^{T} (1-r)^{t-1} ExcessPrice_t$$

where $t = 1$ denotes the first post-treatment period and r the monthly discount rate.

3.2 In-Space Placebo Tests

Our principal inference methodology makes use of In-Space Placebo tests, as proposed by Abadie et al. (2015) and Abadie (2020). We reassign treatment status to all countries, one at a time, in each market and generate outcomes. That is to say, we construct a synthetic model for Austria in the beef market, despite no treatment (merger) taking place, out of the same pool of donor countries. This procedure is subsequently repeated for all available donor countries, and the results are compiled. This compilation of placebo outcomes can then be treated as a sample distribution to compare with the outcome for Sweden's model (within each market), as we know this placebo distribution not to contain treatment effects (no mergers occurred in these countries). We do this both using average excess prices and the net present value measure described above. Within each market, we run a t-test for both measures and evaluate the results using graphics.

[1] While this can seem quite steep, as the interest rate is often used as standard in other cases, we decided to follow the literature. In the end, choice of discount rate is always somewhat arbitrary. Worth noting is also that, since the interest rate is so low, the average excess price specification yields results very similar to those of an NVP model using the interest rate as discount factor.

To ameliorate issues with ill-fitting In-Space Placebo tests, we re-run the analysis with a restricted sample, in which only observations with a RMSPE less than or equal to twice that of Sweden's are kept in the comparison distribution. Results from both approaches are reported.

One should note that this approach is not one proposed by the creators of synthetic controls, as Abadie (2020) proposes a permutation methodology, whereby the MSPE in pre- and post treatment in each in-space placebo model is ranked within each market. The result for Sweden is then placed in this ranking, and the p-value is defined as the percentile of its position in the list. So for instance, if a market has 20 placebo units, and Sweden shows the 2nd most extreme result, the p-value is 10%. We choose not to use this, primarily because of our highly limited sample size, but it should be mentioned as an alternative for further research.

3.3 Robustness Checks

In order to investigate whether the outcomes generated are conditional on the specifics of the input data or if they remain similar when some of these are altered, we employ two types of robustness tests: the In-Time Placebo test and the Leave-One-Out test.

3.3.1 In-Time Placebo Test

The In-Time Placebo test is done on the same data over the same time period as the base synthetic models, but the pre-treatment period is moved back 12 months. We then evaluate the changes in the resulting placebo models, paying special attention to changes in estimated treatment effects and the fit in the time period between the placebo treatment and the true treatment.

3.3.2 Leave-One-Out Test

The Leave-One-Out test consists of restricting the donor pool for each market's synthetic model of Sweden to include all countries except the country that contributes the most in the base case. The synthetic model is then generated anew without the omitted unit. By looking at pre-treatment fit and effect size estimates between the two synthetic units, we get a sense of the robustness of the results generated by the In-Space Placebo inference tests.

4 Data

4.1 Merger Data from Competition Authority

Any proposed merger in Sweden where the parties collectively generate annual revenues in excess of a billion SEK, or two parties generate 200 million SEK each, must be reported to the Swedish Competition Authority for formal approval (4 Ch. 6§ konkurrenslagen). The Swedish Competition Authority hosts a database with all its decisions. In total, 2959 mergers have been sent to the Swedish Competition Authority for formal approval. The vast majority (96%) of these requests have been approved without any special ex-ante evaluation of the effects the merger would have on competition, the rationale being that the post-merger market share of the parties would be too small feasibly to have a meaningful effect on competition. However, 86 merger requests entailed a sufficiently large joint market share to be considered potentially harmful for competition. In these cases, the competition authority is obliged to conduct an ex-ante evaluation – usually consisting of an ad hoc combination of SSNIP-tests, market share analyses, and interviews – before making a decision about whether the merger should go ahead. These ex-ante evaluations are known as "special investigations" and the Swedish Competition Authority are obliged by law (4 kap 11 §) to conduct them if it suspects a merger will have a significant negative effect on competition.

If an ex-ante evaluation concludes that a merger is likely to have a significantly negative effect on competition, the Swedish Competition Authority is required to stop it. However, only 2 mergers have been stopped in Swedish history, with another 6 being willingly withdrawn by the parties preceding the authority's decision. In other words, out of the 2959 merger requests received by the Swedish Competition Authority, only 86 have been considered potentially harmful enough to require an ex-ante evaluation, and only 8 of these evaluations have resulted in a merger being formally stopped.

The approved merger requests which required a special investigation are the best candidates to apply our ex-post merger analysis for multiple reasons. Firstly, because these cases are, by the Swedish Competition Authority's own account, the approved mergers which poised the largest competition risk. Since mergers prevented by the Swedish Competition Authority require an ex-ante evaluation, they represent the cut-off between those mergers which were approved and those that were not.

Second, the only point of having ex-post merger controls is if ex-ante controls routinely fail accurately to identify negative competition effects arising from mergers. By looking at merger requests which went through an ex-ante evaluation, the thesis can review how effective these evaluations were in preventing mergers with a negative competition effect. The purpose of the Swedish Competition Authority's ex-ante evaluations is to prevent all mergers with a significantly harmful effect on competition. Hence, if an ex-post merger analysis indicates that prices rise in markets following an approved merger, it provides evidence that these ex-ante evaluations are too permissive in allowing mergers which ultimately harm consumers, thereby putting into the question the methodology of the ex-ante analyses conducted.

Finally, there is a practical benefit to restricting our ex-post analysis to those mergers which went through an ex-ante evaluation. Namely, as part of its ex-ante evaluations, the competition authority explicitly identifies which markets are the most susceptible to a negative effect on price following the merger. This allows us to identify which markets to look at when evaluating the ex-post effect of the merger.

We went through the 2959 merger decisions made by the Swedish Competition Authority, identified those which had an ex-ante investigation, and read through each to identify the markets expected to be affected by the merger. We also removed markets with mergers approved post-2015, pre-2000, and which lacked an appropiate HICP classification, due to data limitations. This left us with 22 ex-ante evaluations in 19 markets. After some additional necessary exclusions (discussed below), we were left with 16 cases in 16 markets (for details see table 1).

4.2 EU/EFTA Data on Corresponding Prices

This thesis exclusively relies on the monthly Harmonized Index of Consumer Prices (HICP), specifically the underlying product-data used to build the HICP, to determine and control for the price development in our 16 markets prior to, and following, the approved merger. The HICP is the EU's consumer price index, used to determine inflation by the European Central Bank and other EU institutions. It is collected and presented in a harmonized way by the statistical authorities and/or central banks of EU member states, EFTA members, and a selection of candidate countries. In order to determine inflation, like any other Consumer Price Index, the HICP relies upon data on the monthly price development of almost 500 different consumer markets in each member state, ranging from beef to personal vehicle repair. It is this underlying product-data, which feeds into the final HICP, which we use. Notably however, the data is not complete. The combinations of market and country for which there are data is highly variable, and as a consequence the donor pool for each market varies between 5 and 22 (see table 2).

Using HICP metadata carries several advantages compared to alternative data sources. Firstly, the price data has been collected, in a harmonized way, in dozens of European countries. This is essential to create a robust synthetic control for price developments in the ex-post merger analysis, since the price data from multiple countries are necessary to create a synthetic control unit close enough to Sweden pre-merger. There is simply no other source of consumer price data which is harmonized across as many countries. Second, the price data has been collected on a monthly basis by trustworthy public authorities across Europe since 1996. Without paying exuberant sums to a market research company, it is difficult to find market price data that is as granular and longitudinal.

However, the HICP also has some limitations. Firstly, the HICP is "only" built upon the monthly prices of 500 different consumer markets, which meant that we had to exclude markets with mergers whose monthly price was not collected by the HICP, which in our case was only the market for over-the-counter paracethamol. Second, whilst the main components of the HICP are readily available on Eurostat, some of its metadata had to be requested by Eurostat and member states' governmental agencies. Yet this

Market	HICP	Case Nr.	Merger Date	Parties
Beef	1121	540/2015	2015-09-10	Dalsjöfors Kött Holding / KLS Ugglarps
Beer	213	615/2000	2000-12-12	Carlsberg AS / Pripps Ringnes AB / Orkla AB
Books Hardback	95	452/2012	2012-12-10	KF Media AB / Natur & Kultur / Killbergs Bokhandel AB / Akademibokhandelsgruppen AB / Bokia AB
Books Paperback	951	370/2012	2012-09-19	Bonnierforlagen AB / Pocket Shop AB
Computer Hardware	9131	270/2013	2013-08-02	Komplett AS / Webbhallen Sverige AB
Confectionery	1184	841/2011	2012-02-03	Cloetta AB / Leaf Holland B.V
Flour	1112	694/2000	2001-01-08	Cerealia / Kvarn AB
Food Oil	1154	674/2005	2006-09-16	Melker Schorling AB / Karlshamns & Aarhus United
Frozen Bread	1113	186/2015	2015-04-15	Lantmännen / Lion/Visor Lux 1 Sárl
Hard Cheese	1145	747/2014	2015-03-11	Arla Foods AB / Atria Sverige AB
Meat Preparations	1128	282/2006	2006-05-30	Swedish Meats / HB Slakteri
Milk	1141	445/2011	2011-10-24	Arla Foods AB / Milko
Office Equipment	5119	42/2011	2011-02-18	Office Depot Sweden AB / Frans Svanström & Co AB
Personal Vehicle Repair	723	694/2000	2004-06-14	Bilia Personbilar AB / Eneqvist Bil AB
Spirits	211	161/2010	2010-03-29	Altia / Pernod Ricard S.A
Toys	9312	389/2009	2009-09-07	Distribution Nordic AS / Leksam AB

Table 1: Merger Case Overview

Country	**Beef**	**Beer**	**Books Hardback**	**Books Paperback**	**Computer Hardware**	**Confectionery**	**Flour**	**Food Oil**	**Frozen Bread**	**Hard Cheese**	**Meat Preparations**	**Milk**	**Office Equipment**	**Personal Vehicle Repair**	**Spirits**	**Toys**	**Total**
Sweden	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	16
Austria	YES			YES		YES	YES	YES	YES	YES			YES	YES	YES	YES	11
Belgium		YES		YES			YES	YES		YES	YES			YES	YES	YES	9
Bulgaria															YES		1
Croatia								YES						YES			2
Cyprus	YES			YES		YES		YES		YES				YES			6
Czech Republic	YES	YES															2
Denmark	YES	YES				YES	YES	YES		YES	YES			YES	YES		9
Finland	YES	YES	YES	YES		YES	YES	YES		YES		YES		YES	YES	YES	12
France	YES	YES	YES		YES	YES	YES	YES		YES	YES	YES		YES			11
Germany		YES		YES	YES	YES		YES	YES	YES	YES					YES	9
Greece		YES				YES		YES	YES		YES			YES	YES		7
Hungary			YES												YES		2
Iceland	YES	YES	YES	YES		YES				YES	YES	YES		YES	YES		10
Ireland	YES	YES		YES		YES	YES	YES	YES	YES	YES	YES	YES		YES		12
Italy	YES	YES				YES				YES	YES	YES	YES		YES	YES	9
Latvia	YES	YES		YES				YES			YES	YES		YES	YES		8
Lithuania	YES			YES		YES		YES		YES		YES		YES	YES		8
Luxembourg	YES	YES		YES				YES			YES	YES		YES	YES		8
Malta	YES	YES	YES	YES		YES		YES		YES		YES		YES	YES		10
Netherlands	YES	YES	YES	YES	YES		YES	YES		YES	YES			YES	YES		11
Norway		YES		YES	YES	YES	YES			YES	YES	YES	YES		YES		10
Poland	YES	YES		YES		YES		YES		YES		YES	YES	YES	YES		10
Portugal	YES	YES	YES	YES		YES				YES	YES	YES	YES	YES	YES		11
Romania	YES			YES													2
Slovakia				YES		YES					YES	YES		YES	YES		6
Slovenia	YES	YES		YES													3
Spain		YES	YES	YES		YES	YES	YES	YES	YES	YES			YES	YES	YES	12
Switzerland		YES							YES		YES				YES		4
United Kingdom	YES	YES		YES	YES	YES			YES	YES	YES	YES		YES	YES		11
N	20	22	9	21	6	19	10	18	8	19	18	15	7	20	23	7	

Table 2: Data Availability

underlying data contained a few errors, namely missing months and wrongly labeled observations. The errors noticed were investigated, and corrected, by the data providers. No further error in the data was noticed, but we cannot be sure that all minor errors have been identified.

4.3 Covariates

Though the synthetic control method is proposed to construct its control unit by matching both the pre-treatment values of the outcome variable and any covariates either thought to or found to affect the outcome variable, this study exclusively uses the former. The main reason for this is due to data constraints. Covariates which may be expected to affect prices would include market conditions such as demand estimations for each country/market over time, production costs, company specific characteristics of large actors, subsidies, and other country idiosyncrasies that may be relevant. What these have in common is that they are all very resource-demanding or outright impossible to collect. Some would take a huge amount of time, others would require legal means available only to authorities to extract from companies to recover. With this in mind, it was clear from the outset that the analysis would contain a large amount of unobserved covariates in any case, so we decided to focus the analysis purely on price trends.

4.4 Data Cleaning

In total we collected comparable monthly price data - stretching back to January 1996 - in a total of 30 EU/EFTA countries in 19 different markets. This resulted in 133 000 unique data observations. We then took a few steps to clean up said data, which reduced our dataset. Most comprehensively, we wanted to eliminate all countries in our synthetic controls which had a merger around the time (+/- 5 years) that a merger occurred in Sweden in the same market. Recall that the point of the synthetic control is to measure non-merger related price fluctuations. But if mergers occurred in the countries constituting our synthetic control, then the controls would also be affected by mergers. To identify which countries may have had mergers in the same market, we went through the European Commission merger case database and national competition authorities' documentation. Whilst these sources do not report on all mergers that occur, they do report on those large enough to likely affect competition and thus prices. As expected, we identified a host of mergers in countries which had to be eliminated, shrinking our initial data set of 133 000 months of price data to 91 000.

Second, Turkey has experienced very high levels of inflation since it started recording the HICP. For example, whilst the market price of spirits in Sweden has increased by roughly 16% in a decade, it has increased by roughly 700% in Turkey. Including extreme outliers in the donor pool risks distorting the synthetic control unit for Sweden, and further, it would almost certainly distort the results of the In-Space Placebo tests, which is why we decided to remove Turkey from the analysis entirely. We believe this can also be justified on the grounds that Turkey is not an EU country, and comparatively speaking very different from Sweden in unobserved variables as well. However, this shrunk our data set further to roughly 86 000 points of price data.

Third, the HICP data was indexed to June 2015 as a base month (=100) in most markets, but this varied between countries and markets. We harmonised the base-month to when the merger in Sweden was approved in all markets, e.g. the merger between Arla and Milko occurred in October 2011, hence the base-month for price developments in the milk market was set to October 2011=100. This allows us to better visually compare how the HICP price in markets has changed prior to and after said merger.

As noted above, the HICP does not contain data on over-the-counter paracethamol, one of the markets identified to be potentially of interest by the Swedish Competition Authority. While Swedish data on this market was found to be available through E-hälsomyndigheten, they require payment for the data and so we decided not to pursue collecting it. Furthermore, two markets (broadband & pharmacy products) were subject to more than one merger in the time period of the data, which makes analysing the effects of each very difficult. Because of this, we decided to omit these two markets from the final analysis as well. This left us with 16 merger cases over 16 different product markets in total.

5 Results

5.1 Basic Synthetic Control

The base case (figure 1) shows the synthetic model for Sweden along with the real price data for Sweden. Looking at the graphs, we see that there is large variation in how well the pre-treatment periods fit for the different markets, which is also reflected in the RMSPE measure in table 3, where the compositions for each model can also be seen. At first glance, 11 of 16 cases visually suggest a palpable effect on price in the post-treatment period, most of them showing a relative increase. This analysis is, however, greatly insufficient, and not much can be taken from these results alone. We therefore conduct several robustness checks as well as more sophisticated tests for inference.

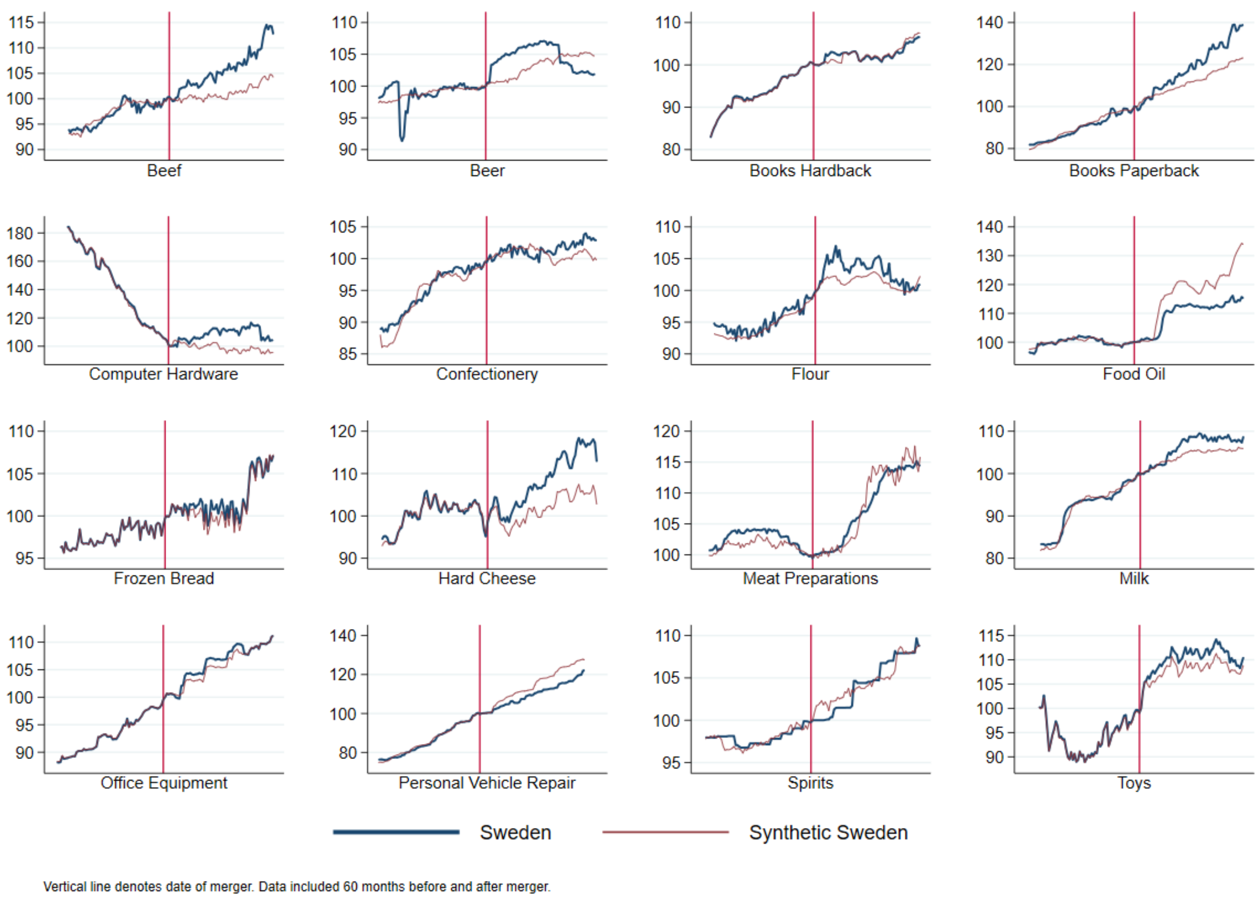


Figure 1: Synthetic Control Base Model

Country	Beef	Beer	Books Hardback	Books Paperback	Computer Hardware	Confectionery	Flour	Food Oil	Frozen Bread	Hard Cheese	Meat Preparations	Milk	Office Equipment	Personal Vehicle Repair	Spirits	Toys
Austria	0.014			0		0	0	0	0.511	0			0	0	0	0
Belgium		0.136		0.144			0	0		0	0			0	0	0
Bulgaria															0	
Croatia								0						0		
Cyprus	0			0		0		0		0				0.007		
Czech Republic	0	0														
Denmark	0.118	0				0	0	0.179		0.959	0			0	0	
Finland	0	0	0.748	0		0	0.939	0		0		0		0	0	0
France	0	0	0		0.392	0.605	0	0.004		0	0	0		0		
Germany		0.724		0	0	0		0.167	0.082	0	0					0
Greece		0				0.019		0	0.238		0			0.148	0.072	
Hungary			0.169												0	
Iceland	0.107	0.01	0	0.098		0.247				0	0	0.122		0	0	
Ireland	0.22	0		0		0.091	0	0	0.132	0	0	0	0.14		0	
Italy	0	0				0				0	0	0.388	0.117		0	1
Latvia	0	0		0				0.038			0	0		0.197	0	
Lithuania	0			0		0		0		0		0		0	0	
Luxembourg	0.218	0		0				0			0	0		0	0.224	
Malta	0.12	0	0.083	0		0		0		0		0.22		0	0.099	
Netherlands	0	0	0	0	0		0	0.38		0.021	0			0.14	0.231	
Norway		0		0	0.381	0.036	0			0.02	0	0.156	0		0	
Poland	0.203	0		0		0.001		0.233		0		0	0.232	0	0.374	
Portugal	0	0	0	0.387		0				0	0.138	0.114	0.512	0.508	0	
Romania	0			0.121												
Slovakia				0		0					0	0		0	0	
Slovenia	0	0		0												
Spain		0	0	0		0	0.061	0	0.022	0	0			0	0	0
Switzerland		0							0.015		0.01				0	
United Kingdom	0	0.13		0.25	0.227	0			0	0	0.852	0		0	0	
RMSPE	0.914	1.902	0.206	1.363	0.936	1.334	0.981	0.681	0.0308	0.637	1.464	0.979	0.116	0.781	0.624	0.283

Table 3: Synthetic Sweden Country Composition

5.2 In-Time Placebo Tests

The first test of robustness we conduct is the In-Time Placebo Test. As can be seen in figure 2, there is a large variation in the results. Several markets seem to be robust to the In-Time Placebo tests, while others clearly fail it, and yet some are edge-cases. For the rest of the analysis, we keep these results in mind, but it is also important to note that the large amount of failed tests also reduce the validity of the models that pass the test, as the same method is employed in all cases, meaning that the passed tests may be false negatives.

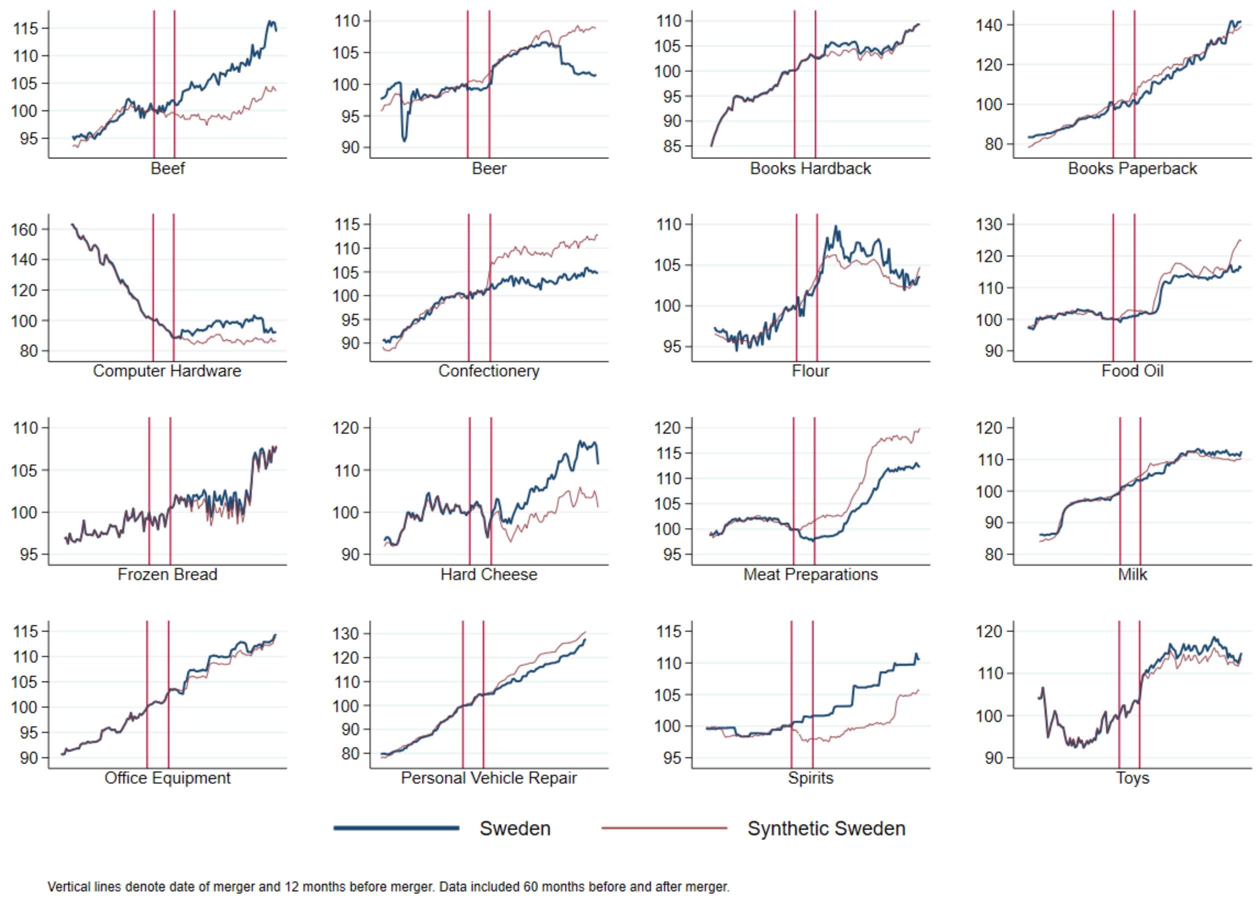
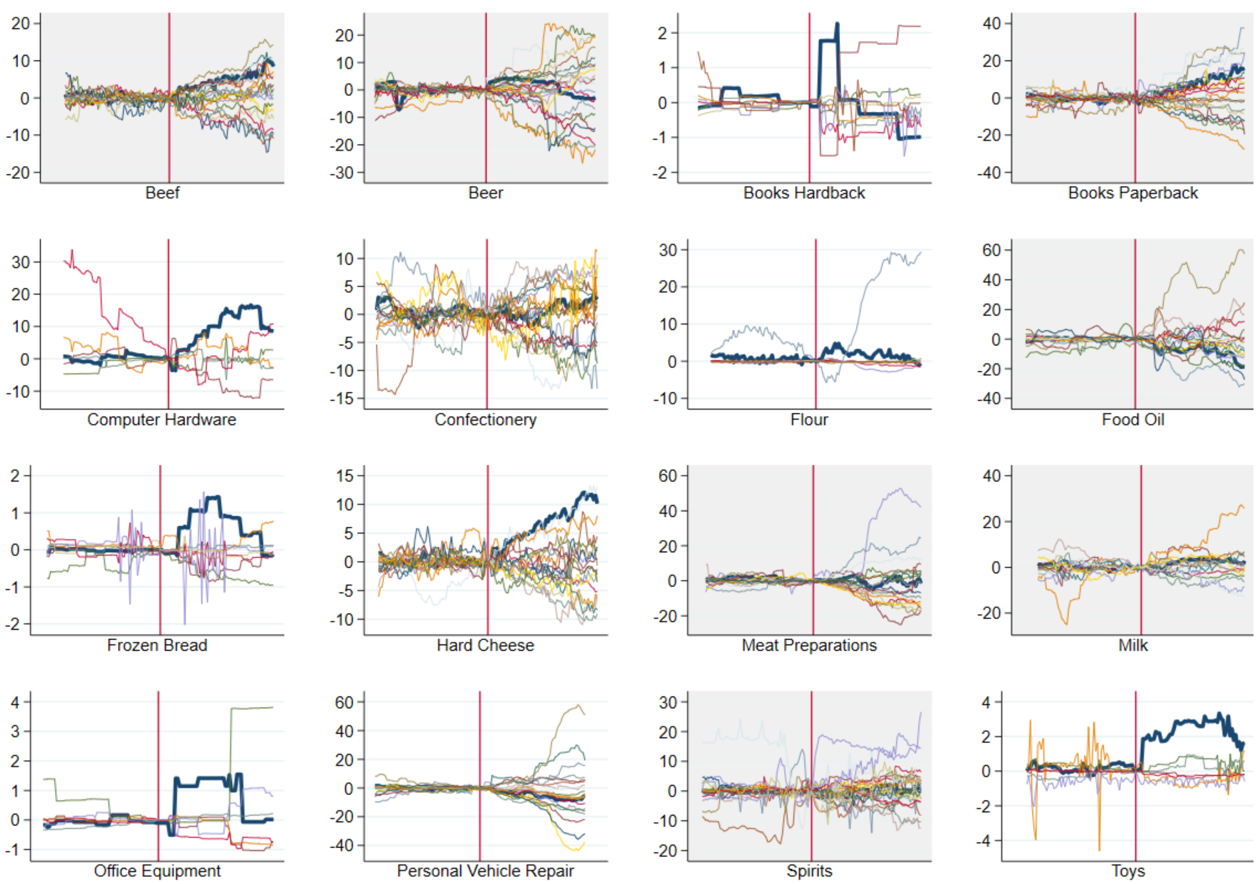


Figure 2: In-Time Placebo Tests

5.3 In-Space Placebo Tests

For inference, we conduct an In-Space Placebo test for all countries in all markets, all of which are visualised in figure 3 (pay close attention to the y-axes, as the scale varies widely). While some patterns can be gleaned from the graphs, the compiled results are presented in tables 4 and 5, where the results for Sweden are compared to the In-Space Placebo distribution using a t-test, both measured as net present value and average excess price. For the full sample, the null is rejected in 11 cases using the NPV model, and in 7 cases using the average excess price model. Out of the 11 significant results in the NPV model, 10 show a decrease in consumer welfare, and out of the 7 significant results in the average excess price model, 6 show a price increase. For a more thorough overview of the compositions of the In-Space Placebo distributions, see tables 8 & 9 in the appendix.



Thick line represents Sweden. Vertical line denotes date of merger. Data included 60 months before and after merger. Shaded background denotes failed In-Time Placebo test.

Figure 3: In-Space Placebo Tests

It is also worth noting that the In-Space Placebo distributions all have a mean value very close to 0, only a single market/model combination having a distribution mean over 1 standard deviation away from 0. As we expect all variation from 0 to be noise, i.e. not either positive or negative, a mean value of the distributions around 0 indicates that this is indeed the case. In other words, had we seen a tendency away from 0 in the placebo distributions, there would have been cause for concern that this test was biased.

5.4 RMSPE-Restricted Inference

As evidenced from the pre-treatment periods in figure 3, there are a lot of In-Space Placebo models which do not fit the data well in the pre-treatment period. These models can reduce the validity of the inference if included in the analysis, and as previously discussed, one way of dealing with this is to exclude them from the analysis, at the cost of reducing the the number of observations. In figure 4, all models with a pre-treatment RMSPE greater than twice that of the Swedish model have been excluded. Because of the variation in fit of the Swedish models, the impact of this exclusion is varied: for some markets very few have been omitted, and in the extreme case, frozen bread, no placebo units remain. The results in the "Low RMSPE" columns of tables 4 & 5 follow the same exclusion criteria.

Notably, this analysis shows that slightly fewer significant effects are estimated, reducing the number in the NPV model to 9 and keeping it constant at 7 in the average excess price model. These results along with the full sample In-Space Placebo results should be taken with a grain of salt, however, which will be elaborated upon more thoroughly in the discussion.

5.5 Leave-One-Out Tests

Finally, in order to test the robustness of the inference quantitatively, we conduct a Leave-One-Out test on the 4 markets that show the strongest estimated treatment effect (Beef, Paperback Books, Hard Cheese and Toys). The composition of these models compared to the base case can be viewed in table 6. As figure 5 shows, all Leave-One-Out models still estimate a discernible treatment effect, but the magnitude is visually reduced in 3 out of 4 cases. Looking at the numbers in the table 7, this is in line with the graphical analysis,

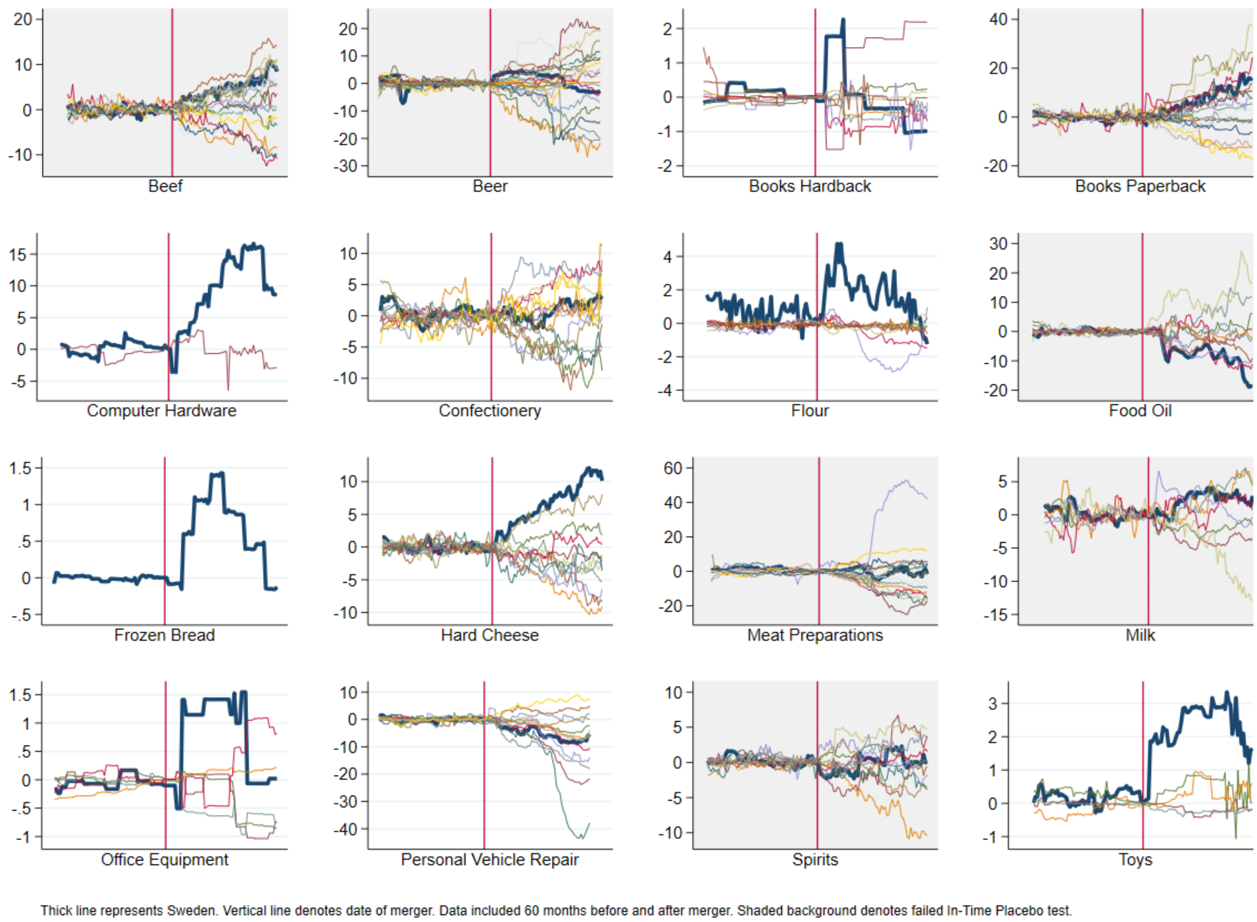


Figure 4: Fit-Constrained In-Space Placebo Tests

	NPV (10% Quarterly Discount)		Avg. Excess Price	
Market	**Full Sample**	**Low RMSPE**	**Full Sample**	**Low RMSPE**
Beef				
Synthetic Sweden	-87.257		4.721	
Sample Mean (SD)	.337 (74.224)	-9.749 (75.776)	.183 (4.012)	.768 (4.023)
t-score	-5.144***	-3.962***	4.931***	3.806**
N	19	15	19	15
Beer				
Synthetic Sweden	-61.445		1.345	
Sample Mean (SD)	5.801 (129.566)	11.954 (130.421)	-.218 (7.198)	-.541 (7.031)
t-score	-2.378*	-2.453*	0.995	1.169
N	21	19	21	19
Books Hardback				
Synthetic Sweden	-8.900		-0.025	
Sample Mean (SD)	2.702 (8.349)	2.702 (8.349)	-.055 (.534)	-.055 (.534)
t-score	-3.931**	-3.931**	0.157	0.157
N	8	8	8	8
Books Paperback				
Synthetic Sweden	-103.488		6.96	
Sample Mean (SD)	-14.175 (134.737)	-29.953 (121.930)	1.088 (8.112)	1.952 (7.160)
t-score	-2.964**	-2.412*	3.237**	2.798**
N	20	16	20	16
Computer Hardware				
Synthetic Sweden	-169.701		9.455	
Sample Mean (SD)	11.852 (46.141)	-12.856 (N/A)	-.619 (2.963)	-.145 (N/A)
t-score	-8.798***	N/A	7.604***	N/A
N	5	1	5	1
Confectionery				
Synthetic Sweden	-6.97		0.76	
Sample Mean (SD)	5.841 (80.284)	7.102 (80.548)	-.301 (3.942)	-.339 (3.929)
t-score	-0.677	-0.63	1.142	1.009
N	18	13	18	13
Flour				
Synthetic Sweden	-51.713		1.702	
Sample Mean (SD)	-16.354 (67.614)	6.128 (5.123)	1.333 (5.118)	-.369 (.380)
t-score	-1.569	-31.934***	0.216	15.409***
N	9	8	9	8
Food Oil				
Synthetic Sweden	129.629		-7.344	
Sample Mean (SD)	.519 (216.607)	-3.222 (99.484)	-.107 (11.148)	.0150 (5.313)
t-score	2.458*	4.006**	-2.677*	-4.155**
N	17	9	17	9

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Sweden vs. Placebo Distributions (1/2)

	NPV (10% Quarterly Discount)		Avg. Excess Price	
Market	**Full Sample**	**Low RMSPE**	**Full Sample**	**Low RMSPE**
Frozen Bread				
Synthetic Sweden	-14.0387		0.593	
Sample Mean (SD)	3.246 (5.540)	N/A	-.148 (.278)	N/A
t-score	-8.255***	N/A	7.051***	N/A
N	7	0	7	0
Hard Cheese				
Synthetic Sweden	-121.85		6.604	
Sample Mean (SD)	13.824 (57.654)	31.351 (51.303)	-.753 (3.142)	-1.530 (2.804)
t-score	-9.984***	-9.904***	9.936***	9.622***
N	18	11	18	11
Meat Preparations				
Synthetic Sweden	1.737		-0.386	
Sample Mean (SD)	12.101 (138.448)	25.105 (130.871)	-.576 (8.924)	-1.341 (8.734)
t-score	-0.309	-0.692	0.088	0.423
N	17	15	17	15
Milk				
Synthetic Sweden	-37.093		2.077	
Sample Mean (SD)	-11.749 (87.131)	6.641 (54.038)	.635 (4.638)	-.347 (2.868)
t-score	-1.088	-2.141	1.164	2.237
N	14	7	14	7
Office Equipment				
Synthetic Sweden	-18.092		0.715	
Sample Mean (SD)	-.053 (9.004)	3.040 (5.443)	.095 (.718)	-.171 (.341)
t-score	-4.907**	-8.682***	2.114	5.803**
N	6	5	6	5
Personal Vehicle Repair				
Synthetic Sweden	88.967		-4.886	
Sample Mean (SD)	21.718 (134.335)	52.772 (91.554)	-1.496 (9.106)	-3.762 (5.811)
t-score	2.182*	1.425	-1.623	-0.698
N	19	13	19	13
Spirits				
Synthetic Sweden	18.507		-0.213	
Sample Mean (SD)	-8.545 (105.311)	3.620 (44.977)	.533 (4.512)	-.231 (2.298)
t-score	1.205	1.098	-0.775	0.026
N	22	11	22	11
Toys				
Synthetic Sweden	-52.145		2.277	
Sample Mean (SD)	3.312 (9.255)	-1.448 (6.938)	-.122 (.411)	.082 (.333)
t-score	-14.677***	-14.615***	14.294***	13.172***
N	6	4	6	4

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Sweden vs. Placebo Distributions (2/2)

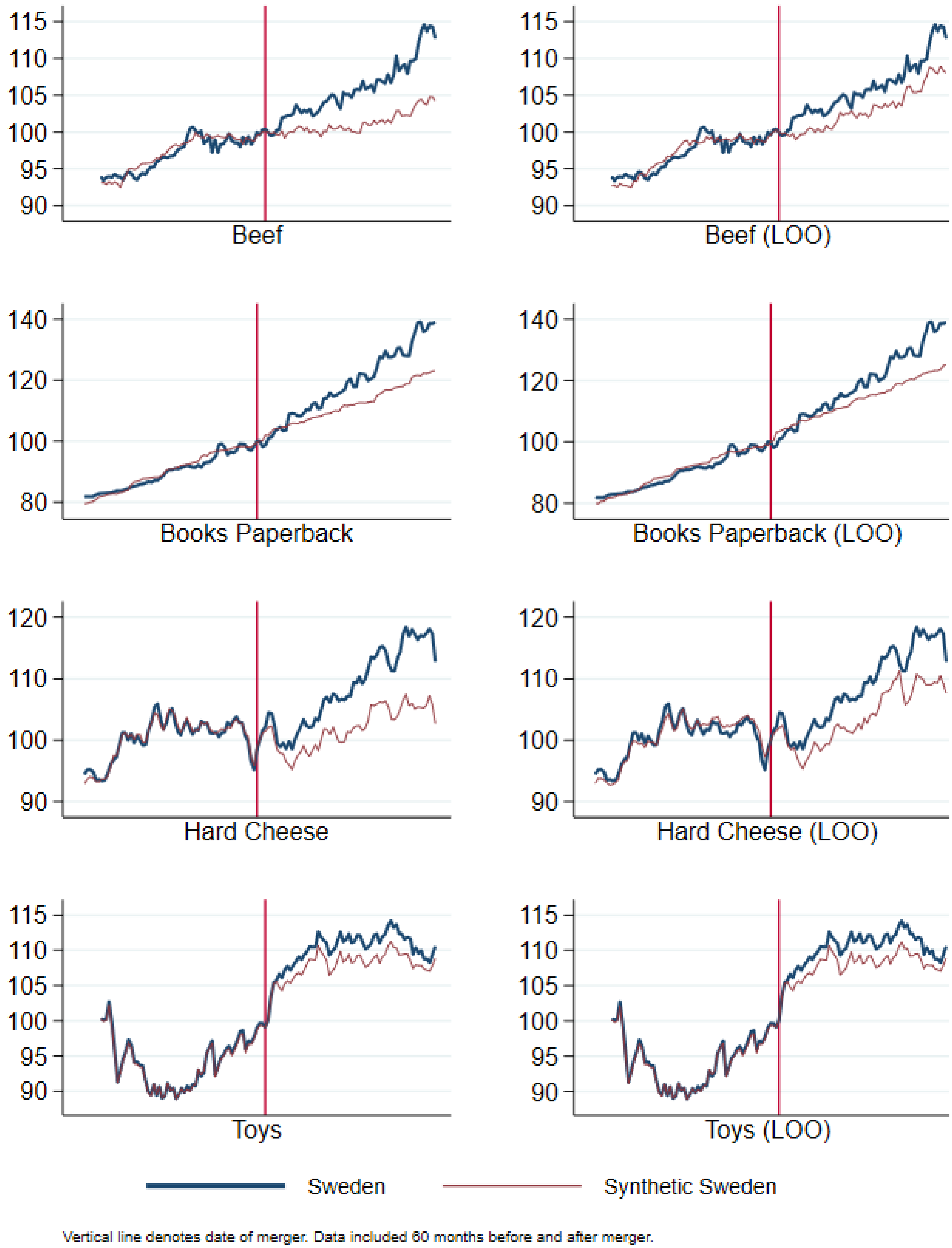


Figure 5: Leave-One-Out Tests

Country	Beef (LOO)	Beef	Books Paperback (LOO)	Books Paperback	Hard Cheese (LOO)	Hard Cheese	Toys (LOO)	Toys
Austria	0	0.014	0	0	0	0	0.866	0
Belgium			0.383	0.144	0	0	0	0
Bulgaria								
Croatia								
Cyprus	0	0	0	0	0	0		
Czech Republic	0	0						
Denmark	0.326	0.118			**L/O**	0.959		
Finland	0.075	0	0	0	0	0	0.04	0
France	0	0			0.083	0		
Germany			0	0	0.634	0	0.022	0
Greece								
Hungary								
Iceland	0	0.107	0.152	0.098	0	0		
Ireland	**L/O**	0.22	0	0	0	0		
Italy	0	0			0	0	**L/O**	1
Latvia	0	0	0	0				
Lithuania	0	0	0	0	0	0		
Luxembourg	0.417	0.218	0.101	0				
Malta	0	0.12	0	0	0	0		
Netherlands	0	0	0	0	0	0.021		
Norway			0	0	0.283	0.02		
Poland	0.162	0.203	0.058	0	0	0		
Portugal	0	0	**L/O**	0.387	0	0		
Romania	0	0	0.064	0.121				
Slovakia			0	0				
Slovenia	0.02	0	0	0				
Spain			0.213	0	0	0	0.072	0
Switzerland								
United Kingdom	0	0	0.029	0.25	0	0		
RMSPE	0.971	0.914	1.340	1.363	1.047	0.637	0.299	0.283

Table 6: Leave-One-Out Model Compositions

	NPV (10% Quarterly Discount)				Avg. Excess Price			
	Full Sample		Low RMSPE		Full Sample		Low RMSPE	
Market	**Full**	**LOO**	**Full**	**LOO**	**Full**	**LOO**	**Full**	**LOO**
Beef								
Synthetic Sweden	-87.258	-61.232	-87.258	-61.232	4.721	2.884	4.721	2.884
Sample Mean (SD)	.337 (74.224)	.337 (74.224)	-9.749 (75.776)	-9.749 (75.776)	.183 (4.012)	.183 (4.012)	.768 (4.022)	.768 (4.023)
t-score	-5.144***	-3.616**	-3.962***	-2.631*	4.931***	2.935**	3.806**	2.037
N	19	19	15	15	19	19	15	15
Books Paperback								
Synthetic Sweden	-103.488	-64.964	-103.488	-64.964	6.96	5.217	6.96	5.217
Sample Mean (SD)	-14.175 (134.737)	-14.175 (134.737)	-29.953 (121.930)	-29.952 (121.930)	1.088 (8.113)	1.088 (8.113)	1.952 (7.160)	1.952 (7.160)
t-score	-2.964**	-1.686	-2.412*	-1.149	3.237**	2.276*	2.798**	1.824
N	20	20	16	16	20	20	16	16
Hard Cheese								
Synthetic Sweden	-121.85	-94.944	-121.85	-94.944	6.604	4.829	6.604	4.829
Sample Mean (SD)	13.824 (57.654)	13.824 (57.654)	31.351 (51.302)	31.351 (51.303)	-.753 (3.142)	-.753(3.142)	-1.530 (2.804)	-1.530 (2.804)
t-score	-9.984***	-8.004***	-9.904***	-8.165***	9.936***	7.539***	9.622***	7.522***
N	18	18	11	11	18	18	11	11
Toys								
Synthetic Sweden	-52.145	-52.744	-52.144	-52.744	2.277	2.306	2.277	2.306
Sample Mean (SD)	3.312 (9.255)	3.312 (9.255)	-1.448 (6.938)	-1.448 (6.938)	-.122 (.411)	-.122 (.411)	.082 (.333)	.082 (.333)
t-score	-14.677***	-14.836***	-14.615***	-14.788***	14.294***	14.470***	13.172***	13.349***
N	6	6	4	4	6	6	4	4

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Leave-One-Out Robustness Tests

where the Toys market is largely unaffected, but effect size and significance is reduced for the other three.

6 Discussion

6.1 Data Limitations

While this thesis includes an analysis of inference, there is reason to be wary to take the estimates too seriously. First and foremost, there are limitations in the data in several dimensions which might be detrimental. Even in the best case in this paper, there is an N of 23, including the treated unit and in the worst case it is as low as 6. While the t-test does account for small sample sizes, low sample size inherently reduces the reliability of the estimates. This is because the assumptions required for the t-test to work cannot be validated unless the sample size is sufficiently large: whether the sample contains outliers, and whether the sample is normally distributed. Furthermore, the synthetic control method relies on a donor pool of a certain size, even though the model usually ends up using only a subset; a small donor pool size puts limitations on how good a synthetic model can be constructed.

The other threat to inference posed by the data is the absence of covariates used in the analysis. Though it is possible to construct a well-fitting model using only outcome variable data (as we've shown), matching a synthetic control across several relevant dimensions lends credence to the assertion that the synthetic control acts as a reliable counterfactual unit. While the problem with low sample size is hard to solve because of limitations in how prices are measured the world over (the HICP is only harmonised in a subset of around 30 countries), including covariates is probably the most important extension to implement, for synthetic controls to be a truly viable method for evaluation purposes in the work of competition agencies. As explained in the data section, this was regrettably not a viable option for us, but a governmental authority would plausibly both have the resources and the legal tools to do so.

Beyond measurable covariates, there is another important dimension of unobserved effects to take into account: idiosyncratic shocks. As noted by Abadie (2020), in order for synthetic controls to work well, treatment effects have to be large relative to noise in the data. As most saliently seen in the average excess price analysis in tables 4 & 5, even in the more extreme cases the measured treatment effect is moderate (below 10 index points in all cases). Over a 5 year evaluation period, it is not unreasonable to assume that external shocks could account for price drifts of the same magnitude, which would pose the question: are we measuring the treatment effect or are we measuring a shock? The Leave-One-Out test is meant to evaluate this concern on the donor-side, and making a more elaborate analysis of this kind could be one extension to ameliorate issues with small effect sizes. The problem remains, however, if the shock occurs in the treated unit (Sweden), in which case a more qualitative approach of validation would be recommended.

Another consideration we discuss with respect to data is the small sample size of cases in the analysis, which is only 16. Though this is a product of some selection and exclusion criteria necessary to conduct a more comparable analysis, it would have been preferable to work with a larger sample size, in which case perhaps a more salient pattern could be discerned. This issue is, however, difficult to solve - impossible even - without relaxing the selection criteria that we used to choose cases in the first place, while keeping the analysis to Swedish merger cases. Perhaps in other markets in the EU where the competition authority works slightly differently, a larger sample size could be collected, which could be considered as an extension of this line of research.

When it comes to the source of the data, the main benefit of using price-data from the HICP is that it enabled the study to build a synthetic model, based on price data from dozens of countries which have been collected in a homogeneous fashion. However, the trade-off is that the HICP at times uses broader market-categories than those identified by the Swedish Competition Authorities as most susceptible to disruptions following a merger. Furthermore, some mergers were expected to affect a particular geographical area of Sweden especially, but HICP data is only collected on a national level. The fact that the HICP may measure larger markets than the mergers affect, means the price effect of the merger will have a smaller aggregate impact. Hence, if more granular market-prices were enabled (both in the product category and geographical dimensions), price effects measured might be larger.

However, since the main purpose of the thesis is to test the validity of synthetic control models for the purposes of ex-post merger evaluations, rather than evaluate the conduct of the Swedish Competition Authority, it was considered more important to have a sample of price-data from multiple countries collected in the same way.

6.2 Limitations for Inference

A few final considerations ought to be accounted for when discussing inference. First, spillover effects are highly likely not to be a threat to identification in this case, because of the legal framework used by the Swedish Competition Authority. As stated previously, the agency has an obligation to defer cases to the EU competition authority in cases where spillover effects are suspected, which means that the sample is already selected for cases where this is unlikely to be of concern. It is of course possible that the judgment not to defer is mistaken in some cases, in which case there could be a problem, but investigating this would be a whole thesis on its own.

Anticipation effects are of more concern, as planned mergers are made known at the latest as soon as the merger application is filed by the parties to the Swedish Competition Authority, as a consequence of the principle of public access to official records in Sweden. Given that the process of approval is not instant, and that nearly all merger applications are approved, it is not an unreasonable assumption that the market would react to this. What makes this issue less dire, however, is that the merging parties are prohibited to act as one entity until the merger is carried out (this would amount to cartel behaviour), so any effects we see would most likely come from the competition adjusting their strategy before the merger is approved.

The risk of such anticipation behavior cannot be discounted, and may be a threat to identification. However, it is possible to investigate this using statistics, by making a secondary analysis where the treatment date is assigned to the time of the merger announcement, rather than to the time of the approval. However, this approach has its limitations as well, since it assumes that market parties is made aware at the time of announcement, which cannot be verified. It may well be the case that word gets out before a public announcement is made. Nevertheless, given less constraints of space and time, this could have been a worthwhile extension to explore.

We might also note that the consumer welfare model clearly affects the estimated results, as evidenced by the discrepancy in number of significant treatment effects between the average excess price and NPV models. While it is generally a good idea to present both, there remains an issue concerning the choice of discount rate in the NPV model. Since the choice may affect what results are generated, this is an opportunity to skew the analysis towards desired results. We decided to deal with this problem by using a standard value found in the adjacent literature. This we believe keeps the analysis honest, but there is no model which is "true" a priori, and since the outcome is affected by the choice of model, this needs to be kept in mind when analysing the results

Lastly, while not directly implying problems in the statistical analysis, it is important to note closely the language in the mandate of the Swedish Competition Authority in the law. As described in section 2.2, the agency only has a right to intervene in cases where mergers could "seriously harm" market competition. As a consequence, we may expect that some level of price effects would be acceptable when considering a merger case, and the size of this effect is to a large extent discretionary to the agency. This means that a positive and significant result does not imply a mistake on the part of the competition authority, since this could have been taken into account and deemed acceptable ex-ante. Therefore, the results can only be considered properly with the knowledge of what is deemed acceptable - something of which only the competition authority has.

6.3 Synthetic Control vs. Other Methods

While there are some problems with applying the synthetic control method on ex-post merger analysis, it is important to view this in the light of available alternatives. Most saliently, the Difference-in-Differences method is often otherwise used, and the synthetic control method has some obvious advantages over it. In a pure numerical sense, a synthetic control can by definition be made to fit the main identifying assumption of Difference-in-Differences (parallel trends) better than a chosen comparison unit, if that comparison unit is part of the donor pool in the synthetic control unit. Therefore, an argument would have to be made on qualitative grounds as to why a chosen comparison unit would be more suitable than a synthetically constructed one. Commonly, this is done with reference to similarity in other relevant characteristics - something that can often be measured and be taken into account by the synthetic control method. Consequently, a Difference-in-Differences model is only preferred over a synthetic control model if a strong case can be made for the importance of matching characteristics which cannot be measured. While this paper does not make use of covariates, it is by no means impossible to implement. Furthermore, most covariates that saliently would be of interest to the outcome variable in this kind of analysis (price), would be possible to quantify and measure. In conclusion, despite limitations, there is a strong case for why the synthetic control method may be preferable to Difference-in-Differences in ex-post merger analysis.

The other alternative would be to conduct ex-post merger analyses by comparing ex-ante analyses, where applicable, with outcome data. This method would not quite capture the treatment effect of the merger directly, however, but rather the discrepancy between the predicted treatment effect and the outcome. In other words, it could be used as a tool to evaluate the validity of the ex-ante analysis specification, and to see whether there is a bias in one direction or the other - but not to measure the market response to the merger, which a synthetic control could under ideal conditions. Therefore synthetic control is not so much an alternative to this approach, but rather a complement - and there is value to be had from both types of analysis.

6.4 Policy Implications

While the main aim of this study is to investigate the value of synthetic controls as a method in ex-post merger analysis, we should mention what can be taken from the results as such as well. As discussed at length, there are a number of factors that put into question the reliability of the estimated effects that we found, despite many rejected null hypotheses. With this in mind, along with the moderate effects estimated, there is a suggestion that the Swedish Competition Authority is not too permissive in its merger approvals - especially considering that the cases studied are selected ex-ante to be cases at risk to affect markets negatively.

Additionally, it is notable that many of the robustness checks that the models were subjected to affected the outcomes more than ideally desirable. It is also true that the results were mixed, indicating that with a more sophisticated specification, the approach could be salvageable. More specifically, we believe that it would be worth investing in conducting this type of analysis with the inclusion of covariate data - something which was not feasible for us. As noted above, it is highly likely that this approach would yield more reliable results than a Difference-in-Differences in almost all cases arguing just from first principles, and we think that the outcome of this study supports that view.

While the base models and robustness tests show mixed results and only slight promise, the In-Space Placebo inference method seems to show more. From the method used in this paper, the main issues in this regard are the low number of donor countries in most markets, and the high standard deviation in the placebo distributions. While the data availability is largely inherently constrained and has to be taken as it is, the analysis can potentially be very valuable in the cases where the N is on the larger side. Additionally, similarly to concerns raised about the synthetic model of the treated unit, it is likely that the noise measured in the In-Space Placebo models would be reduced in an analysis including covariates in the models. With these two considerations in mind, we believe that it could be worthwhile investing in assessing this hypothesis, by collecting and estimating covariate data to see if the robustness can be improved, and the estimated treatment effects less noisy.

This study looks exclusively at merger cases in Sweden over the time period 2000-2015. Not only that, but it heavily relies on the particularities of the legal framework in this area in Sweden. While the justification for antitrust regulation is the same everywhere, the particularities may very well vary. For instance, the ex-ante merger analyses may use vastly differing methodologies in different jurisdictions, and the cutoff values for what is deemed acceptable may vary just as much. Consequently, the external validity of the results in this paper is very constrained. While the methods we employ can just as well be used for other (at least EU) countries, the results we generate could under no circumstances be used to say anything about the efficacy of merger controls in general, only about the efficacy of merger controls in Sweden.

7 Conclusion

The relevance of antitrust legislation is at an all-time high, yet the mechanisms in place to put boundaries on anti-competitive behaviour often leaves a lot to be desired. With the benefit of hindsight, there are a handful of cases which seem outrageous to have been approved, such as Facebook's acquisition of Instagram in 2012. This is an extreme example of what is likely a more widespread phenomenon, suggesting that the ex-ante investigations that take place before M&A cases are approved may not be completely fit for purpose.

In order to evaluate the procedures employed, and to improve upon then, ex-post merger analyses are done in some cases, but the reliability of these are not great owing to the absence of a suitable counterfactual comparison unit. Historically, the Difference-in-Differences approach has been widely applied. This paper expands on the field of ex-post merger analysis by applying the synthetic control method on the problem.

By looking at HICP price data in 30 countries in 16 different markets, we estimate the consumer welfare effects of 16 different mergers in Sweden. Results were mixed, showing a slight leaning toward mergers causing a price increase in the affected markets, with a few showing the opposite effect, and many showing no significant effect. We also test the reliability and robustness of the models, and find that the tests employed fail in more cases than ideal. We hypothesise that this is due to two main reasons: a low number of observations in many markets, and the lack of covariates used in the analysis.

We therefore do not recommend that anything be decided on the basis of our findings, but we nevertheless see some promise in the synthetic control method for application in ex-post merger analysis. While the problem of low N is often very difficult to solve, the problem of covariates could be a feasible extension to implement by an actor with the required resources and legal tools. If such an implementation proved more reliable and robust, it would surely be an invaluable tool to apply for antitrust in the future.

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Appendix

Country	Beef	Beer	Books Hardback	Books Paperback	Computer Hardware	Confectionery	Flour	Food Oil	Frozen Bread	Hard Cheese	Meat Preparations	Milk	Office Equipment	Personal Vehicle Repair	Spirits	Toys
Sweden	-87.3	-61.4	-8.9	-103.5	-169.7	-7	-51.7	129.6	-14	-121.9	1.7	-37.1	-18.1	89	18.5	-52.1
Austria	-13.1			22		-5.1	1.4	-4.2	9.6	24.3			5.1	156.7	61.7	3.3
Belgium		-16.3		-5.3			2.4	-0.8		-20	86			46.1	37.3	-9.4
Bulgaria															-31.5	
Croatia								-7						42.7		
Cyprus	-8.8			237.6		111.4		51.1		1.7				185.7		
Czech Republic	-67.9	237.7														
Denmark	-16.7	289.9				-26.8	3.2	-55.2		95.3	-19.2			59.2	87.1	
Finland	106	54.4	-12.2	47.4		-99.3	4.7	50.9		10.3		-117.7		65	-20.4	10.4
France	6.8	-8.1	-1.6		68.8	59.6	6.9	110.2		18.4	125.9	70.1		32		
Germany		37		-32.1	32.5	-78.1		-128.2	11.7	81.4	68.1					-5
Greece		-102.3				100		358.3	-3.3		143.9			-60.4	-386.3	
Hungary			7.2												2.8	
Iceland	65.2	-146	7.4	-190.1		-62.4				-9	-330	-200.9		-63.6	147.7	
Ireland	79.1	62.7		57.9		-0.1	17.3	11.4	-1.6	25.3	123.6	124	-15.5		41.9	
Italy	-17.8	-89.9				-75.1				61.8	-33.9	48.7	2.9		2.6	5.3
Latvia	-85.9	-10.6		-38.5				-674.5			-236	80.9		-212.9	18.5	
Lithuania	-142.9			-174.4		102.7		-230.1		-19.8		-35.6		-264	90.1	
Luxembourg	-59.9	46.5		-156.9				90.6			-5	-18.9		-13.6	-38.8	
Malta	-8.4	9.7	16.4	-202.9		-145		16.4		83.7		-14.1		219.4	-52.1	
Netherlands	-63.5	-223.2	4.2	16.9	-52.2		8.9	134		55	146.3			7.4	12.6	
Norway		140.5		-58.4	-12.9	50.4	4.2			-118.3	49.2	8.4	-2.6		-78.1	
Poland	112.9	52.3		-273.8		124.1		71.8		2		-67.9	11.1	205.8	30.8	
Portugal	-78.9	-175.3	-0.7	157.8		31.8				-36.4	163.4	-30.4	-1.2	-99.2	-10.3	
Romania	31.6			103.2												
Slovakia				150.1		75.5					-147.8	-75.4		160.4	41.3	
Slovenia	50.7	-157.6		83.4												
Spain		-3.9	0.9	47.8		-22.9	-196.2	213.9	3	75.5	38.8			49.1	-3.6	15.3
Switzerland		-6.5							2.6		65.1				-159.4	
United Kingdom	117.9	131		-74.9	23	-35.7			0.7	-82.4	-32.7	64.1		-103.1	18	

Table 8: Appendix A - Net Present Values for Placebo Models (10% Quarterly Discount)

Country	Beef	Beer	Books Hardback	Books Paperback	Computer Hardware	Confectionery	Flour	Food Oil	Frozen Bread	Hard Cheese	Meat Preparations	Milk	Office Equipment	Personal Vehicle Repair	Spirits	Toys
Sweden	4.72	1.35	-0.03	6.96	9.46	0.76	1.7	-7.34	0.59	6.6	-0.39	2.08	0.72	-4.89	-0.21	2.28
Austria	0.97			-1.03		0.58	-0.17	-0.6	-0.41	-1.04			-0.39	-10.35	-1.82	-0.17
Belgium		1.1		-0.28			-0.08	0.59		1.53	-8.59			-2.5	0.17	0.44
Bulgaria															1.2	
Croatia								-0.29						-2.9		
Cyprus	0.79			-13.78		-5.78		-2.64		-1.38				-9.97		
CzechRepublic	3.11	-11.07														
Denmark	1.07	-15.09				1.08	-0.16	4.73		-5.45	1.43			-4.18	-5.29	
Finland	-5.23	-6.52	1.09	-5.34		4.61	-0.21	-4.11		-1.41		4.87		-5.58	2.61	-0.45
France	-0.23	0.25	0.16		-5.59	-3.49	-0.6	-6.23		-0.17	-6.96	-3.35		-2.64		
Germany		-1.27		2.23	-0.5	4.48		6.71	-0.61	-4.3	-4.52					0.29
Greece		7.56				-3.74		-18.22	0.18		-8.35			2.92	14.54	
Hungary			-0.37												-0.15	
Iceland	-3.5	7.06	-0.37	9.81		2.94				0.73	23.75	12.15		5.46	-5.6	
Ireland	-5.01	-3.96		-3.6		-2.16	-1.22	-1.69	0.08	-1.64	-7.91	-5.51	1.42		-1.31	
Italy	0.88	5.28				3.7				-3.55	2.43	-1.94	-0.29		0.26	-0.23
Latvia	5.11	2.42		3.26				34.49			13.27	-4.79		13.1	-0.64	
Lithuania	7.8			13.11		-5.39		11.26		1.27		2.29		20.69	-5.65	
Luxembourg	3.46	-0.6		8.06				-4.42			0.75	2.2		0.76	0.73	
Malta	-0.2	0.28	-0.7	14.21		6.51		-0.8		-5.17		0.99		-16.28	1.77	
Netherlands	4.3	8.55	-0.22	-1.18	2.1		-0.3	-7.22		-2.71	-9.01			-0.44	-0.44	
Norway		-8.82		4.48	-0.15	0.26	-0.2			6.41	-1.93	0.43	0.13		3.79	
Poland	-6.4	-5.08		15.69		-6.67		-1.97		0.09		1.91	-0.55	-14.51	-1.66	
Portugal	4.26	10.81	0.02	-8.27		-2.57				1.31	-8.99	1.18	0.24	4.83	1.32	
Romania	-1.67			-6.29												
Slovakia				-8.86		-2.91					8.34	2.99		-9.08	-1.8	
Slovenia	-0.28	10.77		-2.05												
Spain		1.93	-0.05	-3.19		1.07	14.95	-11.42	-0.15	-2.67	-2.75			-3.09	-1.12	-0.61
Switzerland		0.26							-0.07		-2.95				9.4	
United Kingdom	-5.76	-8.46		4.79	1.04	2.05			-0.06	4.58	2.18	-4.53		5.34	1.4	

Table 9: Appendix B - Average Excess Prices for Placebo Models