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Ex-post Analysis of the TeliaSonera-Chess 2005 Merger

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Abstract
We provide an ex-post analysis of the 2005 TeliaSonera-Chess merger in the Norwegian mobile telecommunication market. Applying a difference-in-difference approach and a synthetic control group method we find little evidence of price increase in the Norwegian mobile telecom market after the merger. Possible explanations for these findings include that Chess was a small player in the market without its own physical network, with a market share of 8% at the time of the merger and no spectrum license, and that by taking Chess on board moving it from Telenor's network, TeliaSonera could better exploit potential economies of scale related to the operation of its physical network and spectrum.

Keywords Telecommunications · Merger control · MNO · MVNO · Ex-post studies

JEL Classification D49 · K29 · L50 · L96

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1 Introduction

The European mobile telecom market has experienced a large wave of mergers over the past two decades. Many of these mergers were cleared unconditionally by the competition authorities in charge, some of them were cleared with commitments by the merging parties and a few were blocked or withdrawn before a likely prohibition decision.[1] These merger decisions were based on an ex-ante analysis of the possible future impact of the proposed transaction on the market. Ex-post analyses can complement such ex-ante analyses by identifying the actual impact of merger transactions. While ex-post analyses cannot be used to change the decision of competition authorities, such analyses of merger decisions have the potential to single out competition authority decisions that turned out to be incorrect ex-post and identify market conditions that lead to such ex-post incorrect decisions. In this paper, we undertake an ex-post analysis of the impact of the TeliaSonera-Chess merger that was approved unconditionally by the Norwegian Competition Authority (NCA) on 31 October 2005.

We contribute to the growing body of ex-post analyses of mergers in various industries, including the telecom industry.[2] A key feature of the merger that we evaluate is that it took place between two firms both of which had a significant market share, but only one of which (TeliaSonera) owned its (physical) network and had a spectrum license, the other (Chess) being a so-called virtual operator. What is more, the merger lead to Chess moving from the physical network of the largest firm, Telenor, to the network of TeliaSonera. Such "asymmetric" telecom mergers warrant study as the virtual operators are thought to be a key structural remedy for the otherwise high entry costs into mobile telephony, and thereby an important source of potential as well as actual competition in the industry. To the best of our knowledge, this is the first ex-post analysis of a merger between a mobile network operator and a mobile virtual network operator.

The mobile telecom industry has a number of particular features that make an ex-post merger assessment particularly challenging. For example, the industry is characterised by complex tariff offers and non-linear prices. Furthermore, when the merger involves service providers with individual spectrum licenses, the impact of a merger may be longer-lasting than in other industries. The analysis is further complicated by the fact that consumers typically do not purchase individual mobile services. Instead, they purchase a bundle of such services including mobile calls, both to other mobiles (on-net and off-net) and to landline phones, SMS messages and data transfer. We follow the relevant literature to address this complexity and compute a unique one-dimensional compound price index for mobile services consumed

[1] The European Commission blocked the H3G/Telefónica merger in the UK—on 28 May 2020 the General Court overturned the decision. The TeliaSonera-Telenor merger in Denmark was withdrawn in the expectation of a prohibition decision by the European Commission.

[2] See Mariuzzo (2016), Ashenfelter and Hosken (2010) and Kwoka (2013) for an ex-post assessment of mergers, both in Europe and in the US.

by consumers with a certain usage profile.[3] Such a price index aggregates prices of various types of calls and SMSs—the price of data transfer is left out because of the low volume of such services in the examined time period.

We focus on the impact on mobile telecom prices of the TeliaSonera-Chess merger in Norway. This requires comparing the prices that emerged after the merger with the counterfactual prices that would have developed in Norway if there was no merger. We resort to a difference-in-difference (DiD) analysis using, in line with the existing literature, constructed compound price indices.[4] In particular, we compare the evolution of these price indices in Norway with the evolution of the corresponding price indices in a set of benchmark countries where no merger occurred at the time. This way we could identify the impact of the merger that is different from other changes that one could observe in other countries too where no merger occurred. Besides the standard DiD analysis, we also use the synthetic control method which aggregates price evolutions in individual countries in order to construct one single benchmark price evolution pattern.

We find no evidence of a price increase in Norway following the approval of the merger: In individual specifications, the treatment effect estimates vary but were mostly negative in absolute value. When significant, the point estimates were negative in all cases. Potential explanations for our results include: (1) the merger only involved a transfer of ownership at the retail level (MVNO) but not at the wholesale level (MNO owning spectrum) and competition at the retail level of mobile services tended to be stronger than at the wholesale level, (2) Telenor, the market leader, lost Chess, its retail client, and might have wanted to win back its customers by offering lower prices, (3) the now larger TeliaSonera could better explore economies of scale, increasing the competitive pressure on Telenor and (4) free capacity available for the players at the time supported price competition of homogenous product.

1.1 Connection to Existing Literature

Our research is connected to three strands of literature: (1) the ex-post analysis of mergers in the mobile telecom services industry, (2) the application of DiD analysis to ex-post policy evaluation and (3) the application of the synthetic control analysis to ex-post policy evaluation.

The ex-post analysis literature looking at mergers in the mobile telecom industry mergers has grown significantly in the past few years. Affeldt and Nitsche (2014), Aguzzoni et al. (2018), BEREC (2018), Frontier Economics (2015) and RTR (2016) all provide ex-post assessments of mergers in the mobile communication industry. Closest to our research is Aguzzoni et al. (2018) that develops an ex-post assessment of two mobile mergers (Austria and the Netherlands) that were very close in time to the TeliaSonera-Chess merger. In contrast to the TeliaSonera-Chess merger, these mergers were between firms that owned their physical networks and had

[3] See Aguzzoni et al. (2018), BEREC (2018) and Genakos et al. (2018).

[4] Again see Aguzzoni et al. (2018), BEREC (2018) and Genakos et al. (2018).

spectrum licenses and the authors found a positive impact on prices only for the Dutch T-Mobile/Orange merger and no price impact for the Austrian T-Mobile/tele.ring merger.[5] Another important recent paper is Genakos et al. (2018) who study the effects of consolidation in the telecommunications industry using OECD level data and find that consolidation leads to higher prices, but also higher investment. However, that paper looks at a much longer time period (2002–2014) and takes into account the impact of a sequence of mergers rather than singling out one single merger. Both papers use the same Teligen data as the present study. Finally, Csorba and Papai (2013) provide a joint ex-post assessment of telecom mergers in 27 European countries in the period of 2003–2010. The results from these papers point in different directions: in some cases, mobile mergers are found to increase prices, in others there is not statistical relationship, or even a negative relationship between concentration and prices.

There is also a growing literature on DiD being applied to ex-post policy analysis, including mergers in various industries. While DiD is used to assess the impact of telecom mergers by Aguzzoni et al. (2018), BEREC (2018), Csorba and Papai (2013), Genakos et al. (2018) and RTR (2016), it is also used to evaluate mergers in other industries. For example, Ashenfelter and Hosken (2010), look at the effects of five selected consumer product mergers on prices, Miller and Weinberg (2017) evaluate the impact of a merger in the beer industry, Hosken et al. (2018) look at the grocery market and Allain et al. (2017) focus on food prices. In addition to mergers, the DiD framework is often used in policy evaluations, such as estimating the effect of redistributive policies on investment in schooling,[6] the effect of training programs on earnings and the effect of divorce laws on divorce rates[7] to name a few.[8] The main reason for this widespread use is that DiD mimics an experimental research design by comparing the outcomes of two different groups (a treatment and a control group) in a natural experiment.[9]

Finally, our paper also contributes to the growing literature studying the impact of policies by using synthetic control analysis. More specifically, the synthetic control framework is used in case studies to evaluate the effect of an event, and it allows researchers to select groups for comparison in systematic manner.[10] This method was first developed by Abadie and Gardeazabal (2003) and has since been applied to several different areas of study. Examples of this include evaluating the economic cost of conflict (Abadie and Gardeazabal 2003), the effect of California's Tobacco Control Program (Abadie et al. (2010), the economic impact of the 1990 German reunification on West Germany (Abadie et al. 2015). Closer to our focus, the

[5] Aguzzoni et al. (2018) is based on a comprehensive ex-post merger assessment developed by the same authors for the European Commission. See European Commission (2016).

[6] See Abramitzky and Lavy (2014).

[7] See Wolfers (2006).

[8] See Ashenfelter (1978) and Ashenfelter and Card (1985).

[9] See e.g. Ashenfelter (1978), Ashenfelter and Card (1985), Wooldridge (2003) and Imbens and Wooldridge (2009) for a more detailed discussion on the DiD methodology.

[10] See Abadie et al. (2015).

BEREC (2018) and RTR (2016) studies cited above are examples of applying the method to mergers in the mobile telecom industry.

The remaining sections of the paper are structured as follows. Section 2 provides background to the study. Sections 3 and 4 introduce the data and the empirical approach. Section 5 presents the results of the econometric analysis and Sect. 6 concludes. Supporting material and a number of robustness checks are relegated into the "Appendix".

2 Institutional Background

2.1 The Norwegian Mobile Communication Market

The TeliaSonera/Chess merger in the mobile communication industry was investigated by the NCA in the Summer and Autumn of 2005.[11] At that time, the largest player in the market was Telenor, accounting for 56% of subscribers, followed by NetCom (TeliaSonera) with 27%, Sense (Chess) with 8% and Tele2 with 5% of subscribers.[12]

Only Telenor and NetCom had their own national-level physical mobile network, the former serving approximately 70% of subscribers and the latter 30%.[13],[14] Furthermore, only these two players had their own spectrum licenses at least since 2001, in the 900 MHz range. This range was the only relevant range for the period, before the appearance of smartphones and the explosion of mobile data traffic.[15] This means that the merger changed the market share of subscribers for TeliaSonera, but it did not change who owned of the mobile spectrum.

Another feature of the mobile communication market is the significant market power enjoyed by some players in the market of mobile call termination. This triggered the regulation of mobile termination rates (MTRs) for physical network owner Telenor and NetCom as they were found to have significant market power on mobile call termination. These regulated MTRs reflected individual service operators' costs and could therefore be set at different levels for operators from the same country. In case of Norway, Netcom was allowed to charge higher MTRs than Telenor up till July 2008.[16]

[11] On 6th July 2005, TeliaSonera announced that it was in exclusive negotiations to acquire Chess/Sense in Norway. The acquisition was announced on the 29th August, and the Norwegian Competition Authority approved the deal on the 31st October (see https://www.teliacompany.com/en/news/press-releases/2005/11/teliasonera-closes-deal-and-acquires-chesssense/).

[12] The remaining 4% included some small local players such as Teletopia in the Oslo area. See Norwegian Competition Authority (2005) Konkurransetilsynet 2005.1115 Clearence decision TeliaSonera-Vollvik Gruppen.

[13] Teletopia had an own network in the Oslo area but accounted for only a small share of the market.

[14] See Norwegian Communication Authority (2005), pp. 19–23.

[15] Spectrum became scarce only in the 1800 MHz range after the take-off of the smartphones.

[16] See Norwegian Communication Authority (2005, 2007).

3 The Data

Our analysis is based on mobile communication service price (tariff) and usage data from Norway and a number of benchmark countries, i.e. European countries with no merger at the time. In case of mobile telecom services, consumers can pay for services either in advance (pre-paid services) or in a subscription system at the end of the month (post-paid services). Pre-paid services typically include an upfront fixed payment, which can then be used to consume various services, each of them with a separate price. Post-paid services typically have a monthly fee component that includes a pre-defined quantity bundle (monthly quota) of various types of calls and SMSs, as well as individual service prices for usage exceeding the monthly quota. We use all these service pricing and usage features to develop three different compound price indices for each country, i.e. one index for each of the three different usage profiles.

Our main data source for mobile tariff data in a dataset by Teligen ("Teligen data"), a provider of telecom market data covering the OECD countries. In addition, we collected usage and mobile termination rates data from national regulators and obtained data on various control variables (GDP per capita), exchange rates and consumer prices from Eurostat.

3.1 Tariff Data

Tariff data was obtained from Teligen, the only data provider to have quarterly tariff data for the time period around the merger; such data was not available from national regulators for a time period in such a distant past.

The Teligen data includes quarterly data on retail mobile service tariffs offered by the two largest mobile service operators to their customers in each OECD country – the two largest operators do not need to stay the same over the examined period. For Norway, the Teligen data includes prices for Telenor and TeliaSonera but not Chess. However, this is not an effective limitation for our analysis because the impact on prices of the merger can be identified from the evolution of TeliaSonera's prices. Ultimately, as per the prior literature, the focus is on the evolution of main pricing reference points affecting the Norwegian market and for this the Teligen data is a very close proxy.

Each service provider offers multiple tariff bundles. The data covers the period from 2003Q4 to 2007Q3, i.e., covering two full years before and after the date of the merger clearing decision by the Norwegian Competition Authority on 31 October 2005.[17] In our estimation, 2005Q4 is part of the post-merger period. As a robustness test, we leave this quarter out of the sample and find that this does not change the conclusion of our results.

For each tariff bundle, the Teligen tariff data includes data on:

[17] Technically, the data covers 25 months prior to the merger approval decision and 23 months following the merger approval decision. Our robustness analysis indicates that this is not a true problem.

- connection fee,
- monthly fixed fee,
- monthly allowance of national landline, on-net and off-net calls and SMSs included in the monthly fixed fee,
- fees for national landline, on-net and off-net calls and SMSs made in addition to the monthly allowance (out-of-bundle call fees),
- monthly national landline, on-net and off-net calls and SMSs usage—this is the same across countries.

The connection fee is a one-off fee paid by a customer when joining a bundle offered by an operator. As operators often ask for a loyalty period of 2 years (and this was even more true during the observation period), i.e., 24 months, the connection fee is divided by 24 to obtain its monthly value. Fees for out-of-bundle national landline, on-net and off-net calls are often split into peak time, off-peak time and weekend fees.[18] Fees are expressed both in national currencies as well as in USD, with the conversion being made by the same period's exchange rate. The data for each country includes both pre-paid and post-paid tariffs. Handset subsidies, offered as part of a tariff bundle, are not separately identified in the dataset.

In our analysis we exclude packages that can be identified as targeting business users because such users typically receive large discounts and, therefore, such tariffs are less representative.[19],[20]

3.2 Usage Data

Usage data were collected from national regulators. For Belgium and the UK where usage data were not available from the national regulator we relied on usage data presented in Aguzzoni et al. (2018), a study covering the same countries over the same period, but looking at two other mergers.[21] These usage data include monthly average usage of calls made by mobile phone number owners, broken up into minutes to national fixed line, on-net and off-net, and monthly average number of SMSs sent. The usage data do not include international calls, calls to voicemail, MMS and data transfer services—data services usage was very low in the examined period. The usage data used in our analysis are presented in the Table 1.[22]

[18] The earlier periods also split the allowance on peak, off-peak and weekend. But the allowance in the earlier periods only refers to calls, while it can also refer to SMSs in the later periods. Also, the call setup charge is one variable in the earlier period and can depend on national line, on-net and off-net (though they're often the same) in the later periods.

[19] Such data accounts for less than 10% of all observations.

[20] These choices are consistent with the choices made, for example, in Aguzzoni et al. (2018).

[21] See Table 2 in Aguzzoni et al. (2018).

[22] Whenever possible, we present the average usage data for 2005 (the year of the merger). In the cases where we don't have numbers for 2005, we use the average numbers for 2004 if possible (this is the case for the SMS numbers in PT, and the usage data in Norway). For BE, UK and the HU SMS, this data was not available either. In those cases, we relied on the usage data presented in Aguzzoni et al. (2018), which covers the average from 2006.

Furthermore, in order to cover a wider range of tariffs and usage patterns, we follow the approach used by Aguzzoni et al. (2018) and define a low and a high usage pattern in addition to the base usage numbers—to construct the low (high) usage basket we multiply the numbers above with a factor that is less (greater) than 1.[23],[24]

3.3 The Mobile Service Compound Price Index

We follow the literature and compress the various types of price information associated to each bundle into a one-dimensional price index.[25] In particular, for each bundle, we calculate the monthly expenditure associated with the three pre-defined (low, medium and high) country-specific usage patterns.

The incorporation of usage data into the compound price index is needed to get a cleaner estimate of actual prices that users face in the mobile telecom industry as they often consume more than what is covered by the monthly allowances determined by their chosen tariff package.[26]

As a certain bundle typically involves a monthly fixed fee, including allowances for a certain number of call minutes and SMSs, the price index assigned to a particular bundle and usage pattern is defined as the sum of:

- the connection fee broken down to monthly level,
- the monthly fixed fee, and
- the value of calls and SMSs initiated in excess of what is included in the monthly allowance covered by the monthly fixed fee.

In order to make the price indices and their evolution in time across the various countries directly comparable, and to take into account the potential incentives of mobile operators to raise their prices following the merger, we introduce the following adjustments for the calculated price indices[27]:

- deduct value-added taxes (VAT) as they changed in some countries over the examined time window,
- convert figures expressed in national currency into EUR-figures by using the average nominal exchange rate over the period investigated. This choice limits the impact of fluctuations of the exchange rate on the countries' price time series,
- control for inflation by using harmonised indices of consumer prices (HICP) to construct real prices.

[23] See Table 3 in Aguzzoni et al. (2018).

[24] These factors are taken from the OECD's study on mobile tariffs—see OECD (2006), p. 6.

[25] See Aguzzoni et al. (2018), BEREC (2018) and Genakos et al. (2018).

[26] It is important to note, however, that this risks in part to make price information endogenous with demand/consumption—a challenge not solved in this field of literature and thus a limitation also in our research.

[27] These adjustments are in line with the methodology presented in Aguzzoni et al. (2018).

Table 1 Usage data

Type of USAGE	BE	CH	DE	FR	HU	IT	NO	PT	UK
Min to fixed	12.00	22.66	13.96	34.80	7.22	14.22	30.41	5.65	31.00
Min on-net	51.00	21.93	15.85	70.38	47.62	48.37	41.83	54.00	30.00
Min off-net	22.00	13.02	9.88	32.80	27.65	18.41	27.88	15.77	25.00
SMS	60.00	42.04	21.34	31.71	16.00	32.56	67.20	31.71	55.00

Data from national regulators and Aguzzoni et al. (2018)

Table 2 Weighting of usage bundles

Basket	Minutes factor	SMS Factor
Low	0.46	0.66
Medium	1	1
High	2.15	1.1

Data from Aguzzoni et al. (2018)

To account for the fact that a certain usage pattern cannot be associated exclusively to individual tariffs, we take the four cheapest tariffs per operator for each usage pattern, i.e., we calculate four price indices for each operator corresponding to the four lowest expenditure levels for each usage pattern (low, medium, high) for each operator's available tariffs. This choice takes into account heterogeneity across the users for each average usage basket and it also reflects that users may not know their behaviour in advance exactly and may not always be fully rational. Finally, this choice also averages out potential measurement errors of tariffs.[28]

As a next step, for each usage pattern (low, medium and high) we calculate the average price index for each operator in each country by taking the average of the individual euro-level prices computed for each of the four cheapest tariffs. Figure 1 below illustrates the evolution of the average prices of the high usage pattern consumer group for TeliaSonera and Telenor in Norway.

We note that the prices fell before and after the merger for both companies, albeit Telenor's price drop is slightly more pronounced. The similarly looking low and medium usage development are shown below (see Figs. 2, 3).

We used these company level prices to calculate the country level price index for each usage pattern by taking the average of the two price indices computed for the operators for which tariff data is available in the Teligen data.[29] Figure 4 illustrates the price index for medium usage pattern for Norway and for the selected set of

[28] The same choice was made, for example, in Aguzzoni et al. (2018). We carry out a robustness check of this in the appendix, where we construct the price index using only the two cheapest tariffs for each provider.

[29] For a more detailed description of how we construct the price index, please refer to Sect. 8.1 (in the Appendix).

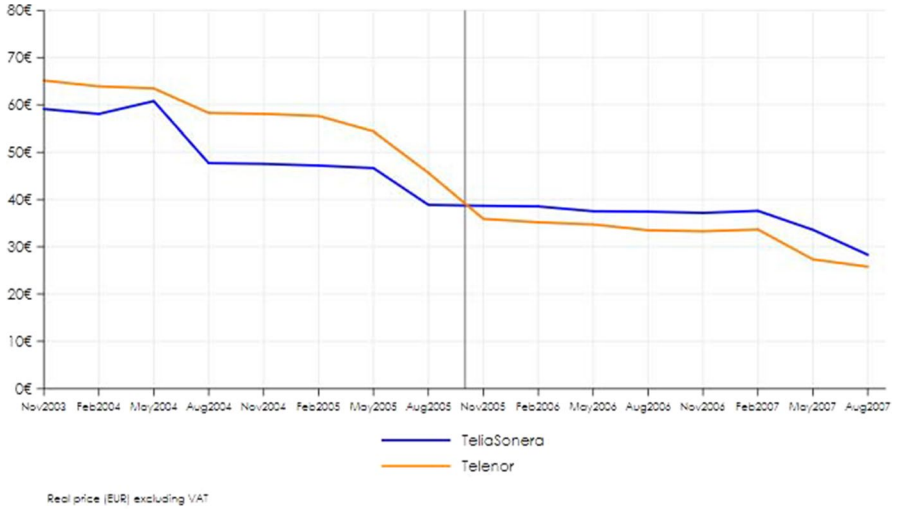

Fig. 1 Price development in Norway for high usage. Note: Real price excluding VAT. Data from Teligen

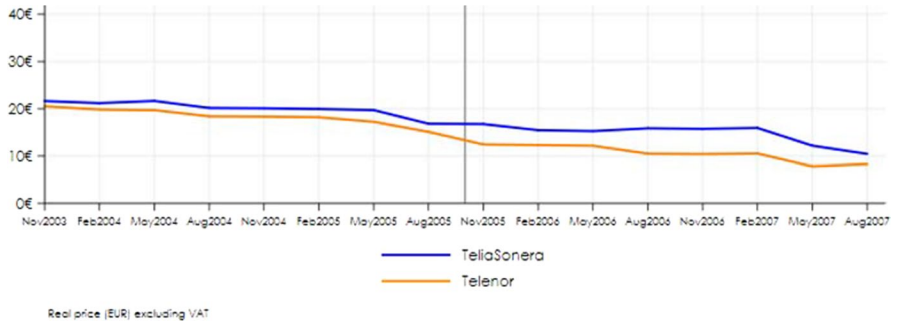

Fig. 2 Price development in Norway for low usage. Note: Real price excluding VAT. Data from Teligen

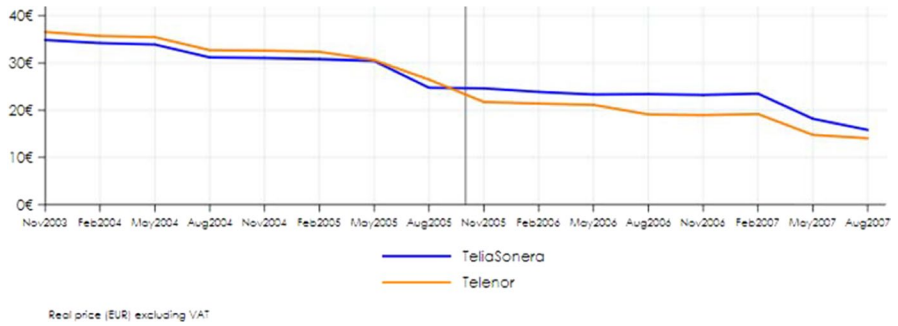

Fig. 3 Price development in Norway for medium usage. Note: Real price excluding VAT. Data from Teligen

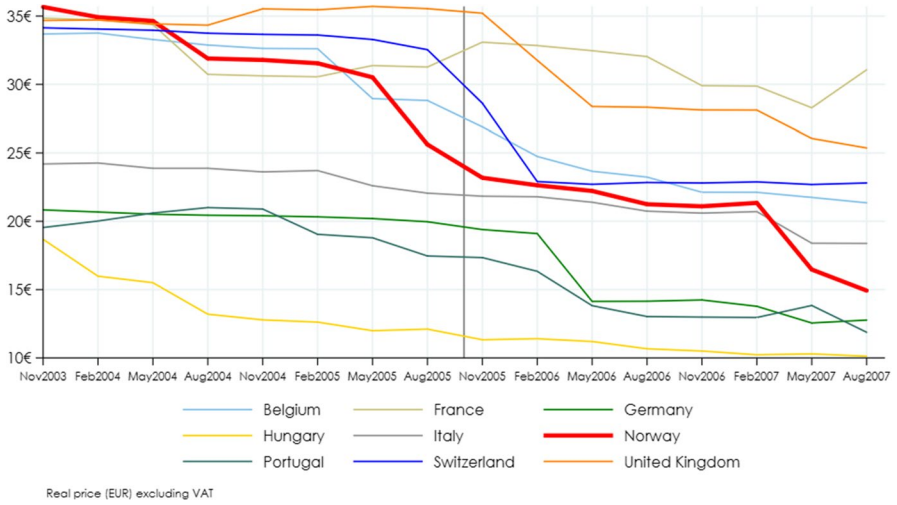

Fig. 4 Price index for medium usage. Note: Real price excluding VAT. Data from Teligen

benchmark countries for the period of 2003Q4–2007Q3, the vertical line indicating the time of the merger clearance decision by the NCA.

The evolution of price indices in this figure is very similar to the price indices for the same period in Aguzzoni et al. (2018) and Genakos et al. (2018).[30] The figure provides a few interesting insights on the evolution of telecom "prices" in a selection of EEA countries. In particular, most countries experience a decline in prices during the investigated 4-years long period. Furthermore, a few countries, notably Norway, Switzerland, the UK and, to a smaller extent, Germany, experience a sharp drop in their prices around the time of the merger approval.[31]

Furthermore, the curves in the figure seem to suggest that Norwegian prices do not show an upward turn after the merger—not even when compared to the price evolution in other countries, e.g. Switzerland or the United Kingdom. In fact, the Norwegian prices show the biggest decline (both in absolute and relative terms) between the two end-points of the examined time period.

3.4 Mobile Termination Rates Data

Mobile termination rates (MTR) data were collected from national regulators. It is important to point out that these MTRs were typically set by the regulator as part of finding some individual operators to have significant market power in their call termination services offered to other operators and the national landline service provider. As these call termination rates were also linked to individual operators' costs

[30] See Fig. 1 in Aguzzoni et al. (2018). The differences are coming from the difference in the time period and the usage data.

[31] We have not been able to identify any event or force driving these price drops.

they often differ across operators from the same country. Furthermore, they can also change in time, depending on the operator's position in the call termination segment and its costs. Finally, these MTRs can be viewed as mostly exogenously set by the regulator even if they are linked to costs.

3.5 Other Data

As one could think of MTRs as factors influencing the supply of services, we add the growth rate of GDP per capita as a proxy for changes in demand. We use the growth rate of the GDP per capita as it is a stationary economic measure, whereas the GDP per capita is a non-stationary measure.

3.6 Descriptive Analysis

Table 3 shows the summary statistics for price indices for the three chosen usage patterns for Norway and our benchmark countries before and after the merger. For each period we have 64 observations for Norway: 8 quarters (2 years) and 4 prices per quarter for each of the 2 largest mobile operators. For each period we have 512 observation for our control group: 64 observations per country (as for Norway), with 8 control countries.

The pre-merger prices are higher in Norway than in the control countries for all baskets (low, medium and high usage), but the MTR is lower. Prices post-merger are essentially identical between Norway and the control group. These observations, in line with Fig. 1, suggest that the merger may have decreased prices in Norway. MTRs are similarly essentially identical post-merger, suggesting that the merger may have led to an increase in MTRs in Norway: the raw DiD estimate is $(0.112 - 0.122) - (0.117 - 0.158) = 0.031$.

4 The Empirical Methodology

4.1 Setup of Methodology

In order to identify the economic impact of the merger between TeliaSonera and Chess one would need to compare the observed prices of mobile services ("prices") after the merger with the prices that one could have observed over the same period if there was no merger, i.e. one would need to compare post-merger observed prices with the counterfactual prices of the period following the completion of the merger.

However, due to the inherent difficulty of reproducing counterfactual prices of mobile services with sufficient precision, the economic impact of the merger can be assessed by comparing prices in Norway with prices in a number of benchmark countries in a time window that includes the merger.

To minimize the distortions from such a cross-country comparison, these benchmark countries should be chosen in such a way that the structure and dynamics of their mobile services industry is sufficiently similar to the structure of the mobile

Table 3 Descriptive analysis

	Norway			Control		
	N	Mean	SD	N	Mean	SD
Pre-merger						
Price, low basket	64	19.286	2.691	512	16.043	6.810
Price, medium basket	64	32.074	4.511	512	26.350	8.029
Price, high basket	64	54.531	10.528	512	43.075	11.283
MTR	64	0.122	0.004	512	0.158	0.037
GDP per capita growth	64	0.031	0.046	512	0.012	0.058
Post-merger						
Price, low basket	64	12.636	4.030	512	12.809	6.207
Price, medium basket	64	20.392	5.538	512	20.892	7.649
Price, high basket	64	34.265	7.202	512	34.292	11.180
MTR	64	0.112	0.004	512	0.117	0.027
GDP per capita growth	64	0.016	0.035	512	0.012	0.055

Descriptive statistics, split by treatment (Norway) versus control (remaining 8 countries) and pre- versus post-merger

services industry in Norway, absent the merger. For example, these benchmark countries cannot have mergers or large-scale entry and exit events in the chosen time window as such events would lead to a price change in the benchmark country that would make the comparison less valid. For example, a large-scale exit from the market in one of the benchmark countries would fail to reveal any post-merger price increase in Norway if there was no control for that exit. It is safer not to include such markets in the set of benchmark countries.

In our choice of benchmark countries we focused on European countries (so that the regulatory regime and country characteristics were as similar as possible to Norway) and our choices was also affected by the availability of price, usage and MTR data. We therefore identified Belgium, France, Germany, Hungary, Italy, Portugal, Switzerland and the UK as suitable benchmark countries for our exercise. The three main Nordic countries (Sweden, Denmark and Finland), who would have been the natural benchmark candidates, were left out because we identified mergers in these countries falling in our examined time period.[32] Due to the national scope of these benchmark markets, one could expect no impact of a merger in one country on the market structure and prices in other EU countries.

Looking at the prices in benchmark countries helps to control for cross-country changes in the evolution of prices. Therefore, the evolution pattern of prices in these countries after the time of the merger in Norway can be considered a good proxy for the Norwegian counterfactual prices, i.e., the prices that would have evolved in

[32] This is in contrast with Aguzzoni et al. (2018) who included these countries in their set of benchmark countries. Furthermore, the Czech Republic was left out because of the lack of mobile termination rate data.

Norway absent the merger. When focusing on the evolution of prices in the chosen countries we use a compound price index for three usage groups rather than individual service prices (see Sect. 0).

4.2 The Econometric Approach

We developed our DiD analysis for a time window encompassing 2 years before and two years after the date of the merger clearance decision, 2005 October 31. This choice of the time window has two motivations. First, in a fast-changing industry such as the telecom industry, a longer time window would risk including market environments that are too dissimilar to the one at the time of the merger approval. Second, this choice is in line with many previous studies.[33]

Besides the standard DiD,[34] we apply the synthetic control method version, where price evolutions in individual countries are aggregated to construct one single price evolution pattern.

4.2.1 The DiD Specification

The DiD approach compares the difference in prices after and before the merger in the treated country, Norway, with the difference in prices after and before the merger in the benchmark countries (the control group). This relies on the assumption that the prices in the control group countries post-merger will be good indicators of the counterfactual price evolving in Norway if there was no merger.[35]

However, as the examined countries are somewhat heterogenous from the point of view of their price evolution, one would need to include some additional variables in the regression, e.g. growth of GDP per capita and marginal termination rates (MTRs),[36] to control for some of the observed differences.[37]

The DiD model that we take as our starting point takes the following form:

$$\log(p_{i,j,t}) = \alpha + \gamma_s D^s_{i.t} + \gamma_m D^m_{i.t} + \Sigma_t \tau_t + \Sigma_i S_i + \delta_1 GDP\ growth_{it} + \delta_2 \log(MTR_{it}) + \varepsilon_{ijt} \quad (1)$$

where subscript i denotes country, j a usage bundle (low, medium, high) and t a specific quarter. The dummy variables $D^s_{i.t}$ and $D^m_{i.t}$ take the value of one for Norway for quarters in the first and second year after the merger respectively and zero otherwise, and thus their coefficients measure the short-term (1st year) and medium-term (2nd year) impact of the merger. Finally, $\Sigma_t \tau_t$ is a series of time fixed effects. These take account of market changes that affect countries in the same way (e.g.

[33] See, for example, Aguzzoni et al. (2018) and BEREC (2018).

[34] See e.g. Ashenfelter and Hosken (2010).

[35] See e.g. Wooldridge (2003) and Imbens and Wooldridge (2009) for a more detailed discussion on the DiD methodology.

[36] We use the MTR as a control for supply side factors. These rates are mostly exogenous, as they are exogenously set by the regulator (despite being linked to costs).

[37] GDP growth is used as a proxy for demand side factors, whereas MTR is used as a proxy for supply side factors affecting price evolution.

technological change) over time. Similarly, $\Sigma_i S_i$ is a series of country-fixed effects. ϵ_{ijt} is an independently and identically distributed random term. We label this model as the "*Base specification*" model.

To control for the possibility of a first-order autoregressive (AR(1)) error term, we estimate the parameters of the above regression using two different assumptions about the standard errors. First, we use a cluster-robust estimator with clustering at the country level but assume that there is no autocorrelation.[38] Second, we allow for first order autocorrelation, but at the cost of not being able to cluster the standard errors.[39]

We note that the DiD model implicitly assumes the countries follow a common trend in the pre-merger period. We therefore test for this hypothesis of common trends by following the method in Abramitzky and Lavy (2014). We investigate the period before the merger (2003Q4–2005Q3) using a linear time trend. We add an interaction term between the time variable and the treated country (Norway). This interaction term is our variable of interest: if the coefficient is statistically insignificant, our common trends hypothesis cannot be rejected, and we believe our DiD model to be correctly specified. A statistically significant coefficient means that our common trend hypothesis fails (and our DiD model would thus be incorrectly specified). We carry out the analysis using country-fixed effects.

We do not reject the common trend hypothesis and therefore argue that our DiD model is correctly specified. This is in contrast with e.g. the results from Aguzzoni et al. (2018), where the common trend test in several specifications fails. In these cases, they use an alternative specification which doesn't rely on common pre-merger trends, but instead includes country-specific trends in the model. The issue with this specification (as the authors highlight) is that it assumes the treated country's pre-merger trend would remain unchanged in the absence of a merger. This is a strong assumption (especially if trends converge in the long run), which could induce large biases. We therefore refrain from looking at this model, given that our DiD model does not fail the test for common pre-merger trends.

Finally, we note that Norway experienced a price drop just before the approval of the merger and that some other countries (Switzerland, the UK and Germany) experience a similar drop just after the merger. To filter out the impact of these price drops occurring in time periods close to each other but with some happening before the merger and some happening after the merger, we re-ran our estimations on the same two models but by leaving out the three quarters (2005Q3–2006Q1) when these price decreases happened in the various countries.

[38] Brewer et al. (2018) show that tests of the correct size can be obtained with, for example, Stata's cluster-robust estimates in many DiD settings. However, the small number of clusters (available countries) is likely to result in a downward bias of standard errors, overstating the statistical significance of the results, see Wooldridge (2003).

[39] Not accounting for autocorrelation can lead to underestimating the standard errors, overestimating the statistical significance of the results. See Bertrand et al. (2004).

4.2.2 The Synthetic Control Method

The synthetic control framework is used in case studies to evaluate the effect of an event.[40] In our case, the event is the TeliaSonera-Chess merger.[41] The synthetic control method uses prices in the countries in the control group to build a single unique counterfactual price. This counterfactual price estimates what the prices in Norway would have looked like absent the merger. It is a weighted average of the prices in the control group countries, where the weights are chosen in such a way that the constructed weighted average prices pre-merger best approximate the treated country's (Norway's) prices pre-merger. These weights are computed by an algorithm that minimises the distance between the realisations of pre-merger prices in the treated country (Norway) and the (weighted) average of pre-merger prices in the control group countries. This difference is formally called the root mean squared prediction error (RMSPE). Subsequently, the counterfactual price is constructed such that the same weighted average of the prices in the control group is extended to cover the post-merger period. The impact of the merger is then identified by comparing the constructed post-merger counterfactual price with the post-merger prices in the treated country (Norway). Figure 5 illustrates the price index for Norway along the price index for the constructed synthetic control for the medium usage bundle.

Visually, there seems there seems to be little evidence for the merger in Norway having affected prices adversely. If anything, prices in Norway seem to have dropped faster than the synthetic Norway from February 2007 and onwards.

To investigate the statistical significance of the estimated impact of the merger, we carry out a so-called "in-space" placebo test where we substitute the treated country, Norway, with each of the eight other benchmark countries, as if they had experienced a merger (leaving out Norway from the sample of benchmark countries).[42] For each test we store the RMSPE for both the pre-merger and post-merger period. We then compute the ratio of post-merger to pre-merger RMSPE. If Norway's prices have changed significantly following the merger, we would expect Norway's RMSPE ratio to be higher than all the placebo RMSPE ratios.[43]

As for the DiD estimation, the price drop in Norway just before the merger followed by similar price drops in Switzerland, the UK and Germany after the merger, can be an issue in correctly identifying the impact of the merger. The elimination of price points from the period 2005Q3–2006Q1 can help address the issue. In this case the synthetic control group price will be constructed by relying on control group countries' price point up until 2005Q2 (inclusive). Accordingly, the comparison of the counterfactual price and the Norwegian post-merger price will be done for the period of 2006Q2–2007Q3.

[40] See Abadie and Gardeazabal (2003), Abadie et al. (2010), Abadie et al. (2015).

[41] Both RTR (2016) and BEREC (2018) also use the synthetic control method to evaluate the ex-post effect of mobile mergers.

[42] This method has been proposed by, for example, Abadie et al. (2010) and Abadie et al. (2015).

[43] This follows the method proposed by Abadie et al. (2015).

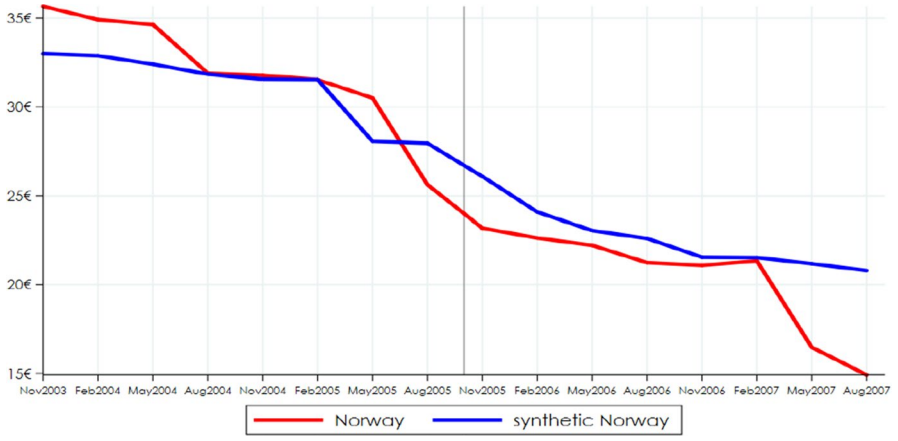

Fig. 5 Synthetic price index for medium usage. Note: VAT excluded. Teligen data

5 Results

Our results section is split into five subsections: First, we discuss the common trends test and its implications for our results. Second, we go through our initial findings for the DiD regressions. We discuss the results both for the standard fixed effects model and for our specification with an AR(1) error term. Third, we go through our initial findings for our synthetic control. Fourth, we carry out sensitivity analysis of our DiD regressions and the synthetic control group approach by excluding the period just around the merger from the analysis. We do this to make sure we correctly identify the impact of the merger. The descriptive statistics of Norway and the control group countries and some further robustness checks can be found in the "Appendix".

5.1 The Common Trends Test

We discuss the results of the common trends test and the validity of our specified model. Our results show that when testing for the entire pre-merger period (2003Q4–2005Q3), we reject the hypothesis of common trends for two out of three usage bundles, see Table 4.

However, we also earlier noted that Norway experienced a price drop just before the approval of the merger (and that some other countries experience a similar drop just after the merger). As TeliaSonera already announced that it was in exclusive negotiations to acquire Chess/Sense in Norway on 6th July 2005,[44] this price drop may in fact have been an anticipation for the merger being approved (e.g. as it did not involve change in control at the wholesale level). We

[44] See https://www.teliacompany.com/en/news/press-releases/2005/11/teliasonera-closes-deal-and-acquires-chesssense.

Table 4 Results of common trends test for the entire pre-merger period

Usage bundle	Low	Medium	High
Conclusion of test	Pass	Fail	Fail
P value of test	0.058	0.026	0.007

For the linear trend, we "pass" the common trend test if we cannot reject the coefficient of the interaction between Norway and the linear trend at 5% level

Table 5 Results of common trends test for the pre-merger period, excluding the last quarter

Usage bundle	Low	Medium	High
Conclusion of test	Pass	Pass	Pass
P value of test	0.434	0.337	0.076

For the linear trend, we "pass" the common trend test if we cannot reject the coefficient of the interaction between Norway and the linear trend at 5% level

therefore also conduct the common trends test for the pre-merger period excluding the last pre-merger quarter, namely 2003Q4–2005Q2, see Table 5. Here, the common trends test passes for all three usage bundles.

We therefore reasonably believe that the common trends assumption is satisfied in our analysis. Although it fails in when including the entire pre-merger period, we argue that it's most economically sound to exclude the quarter right before the merger from the test for common trends.

5.2 DiD Estimation

5.2.1 The Standard Fixed Effects Method

Table 6 presents our main results using the standard fixed effects method. Columns (1)–(3) present the results for the Base specification for the low, medium and high usage bundles respectively. Similarly, columns (4)–(6) present the results when allowing for the AR(1) error term for the low, medium and high usage bundles respectively.

As shown in the table, the Base specification indicates a significant price drop after the merger. This effect is present both in the short and the medium term. The estimated short-term impact varies between − 14% (low usage bundle) and − 18% (high usage bundle). For the estimated medium-term impacts, the ordering of the estimated effects is reversed, with the largest effect in absolute value (− 24%) obtained for the low usage bundle and the smallest (− 16%) for the high usage bundle. The results from the AR(1) specification look similar, albeit the coefficients are not statistically significant. This is both because the point estimates decrease somewhat in absolute value, and because the estimated standard errors increase.

Table 6 Standard fixed effects model

Specification	Base specification			AR(1) error term		
Usage bundle	(1)	(2)	(3)	(4)	(5)	(6)
	Low	Medium	High	Low	Medium	High
Short-term effect	−0.137**	−0.159***	−0.180***	−0.083	−0.088	−0.097
	(0.050)	(0.042)	(0.040)	(0.065)	(0.060)	(0.064)
Medium-term effect	−0.244***	−0.233***	−0.157**	−0.132	−0.115	−0.096
	(0.071)	(0.061)	(0.057)	(0.086)	(0.080)	(0.086)
GDP growth	−0.075	−0.122	−0.224	0.039	0.029	0.017
	(0.122)	(0.145)	(0.179)	(0.084)	(0.076)	(0.078)
Log MTR	−0.016	−0.078	−0.226	0.036	0.028	0.015
	(0.203)	(0.271)	(0.311)	(0.097)	(0.089)	(0.093)
Constant	2.366***	2.713***	2.870***	2.427***	2.875***	3.256***
	(0.485)	(0.611)	(0.692)	(0.062)	(0.052)	(0.049)
Observations	144	144	144	135	135	135
R-squared	0.775	0.814	0.775	0.399	0.437	0.38
AR(1) error term	No	No	No	Yes	Yes	Yes
Coefficient of AR(1) error term				0.745	0.770	0.798
P value				0.000	0.000	0.001

Dependent variable: log prices; standard errors in parentheses (clustered at country level); time fixed effects and country-fixed effects; cheapest four tariffs; significance level: *10%, **5%, ***1%. The AR(1) error term is statistically significant if the test that the correlation between past error terms is zero is rejected at 5% level

Table 7 Synthetic control estimation

Usage bundle	Period	Effect	Rank	Control countries
Low	Short	−0.104	5/9	Belgium, France, Hungary
	Medium	−0.191	3/9	
Medium	Short	−0.070	7/9	Belgium, Hungary
	Medium	−0.154	5/9	
High	Short	0.011	9/9	Switzerland, Hungary
	Medium	−0.063	8/9	

A rank of 1/9 implies that Norway has seen the largest difference between the post- and pre-merger RMSPE, thus indicating that the effect is statistically significant. The control countries are the countries chosen by Stata to construct the synthetic Norway which estimates how the prices in Norway would have evolved in the absence of the merger

5.2.2 The Synthetic Control Method

To complement our DiD analysis, we also use the synthetic control method. Table 7 shows that prices in Norway have dropped following the merger in all but one (short-term for high usage bundle) case, and even there the point estimate is low at 0.01. As the rankings show, these findings do not appear to have any statistical significance—in fact, Norway is consistently one of the countries with the smallest post- to pre-merger RMSPE ratios compared to the control countries.[45]

5.3 Robustness Analysis

In this section, we report in detail the results of the robustness test where we drop the quarters close to the merger. We then report more briefly on other robustness analyses, the detailed results of which can be found in the "Appendix".

5.3.1 Dropping Quarters Close to Merger

To investigate the robustness of our results, especially to the price drops by four countries around the time of the merger approval and Norway being the first one experiencing this price drop right before the merger approval, we exclude the period from 2005Q3–2006Q1 and re-run our regressions and synthetic controls.

[45] We use the "synth" function in Stata to estimate the synthetic control. This doesn't produce standard errors. As discussed earlier, we therefore conduct an "in space" placebo test, where we let the other countries be the merged country. The statistical significance is investigated by comparing the ratio of pre- to post-merger RMSPE for the actual synthetic control effect with the placebo tests. We would expect Norway's RMSPE ratio to be largest if Norway's prices have changed significantly after the merger (and therefore have a rank of 1).

Table 8 The standard fixed effects model with 3 missing quarters

Specification	Base specification			AR(1) error term		
Usage bundle	(1)	(2)	(3)	(4)	(5)	(6)
	Low	Medium	High	Low	Medium	High
Short-term effect	−0.122	−0.148*	−0.159**	−0.08	−0.096	−0.091
	(0.059)	(0.046)	(0.045)	(0.099)	(0.095)	(0.100)
Medium-term effect	−0.252*	−0.260**	−0.186*	−0.128	−0.097	−0.081
	(0.076)	(0.066)	(0.066)	(0.109)	(0.107)	(0.113)
Observations	117	117	117	108	108	108
R-squared	0.833	0.842	0.805	0.865	0.839	0.849
AR(1) error term	No	No	No	Yes	Yes	Yes
Coefficient of AR(1) error term				0.762	0.854	0.867
P value				0.001	0.009	0.145

Dependent variable: log prices; standard errors in parentheses (clustered at country level); time fixed effects and country-fixed effects; cheapest four tariffs; significance level: *10%, **5%, ***1%; 2005Q3–2006Q1 excluded from regression. The AR(1) error term is statistically significant if the test that the correlation between past error terms is zero is rejected at 5% level; 2005Q3–2006Q1 excluded from regression

Table 9 The synthetic control estimation with missing 3 quarters

Usage bundle	Period	Effect	Rank	Control countries
Low	Short	−0.136	2/9	Belgium, France, Hungary, United Kingdom
	Medium	−0.255	1/9	
Medium	Short	−0.270	2/9	Belgium, France, United Kingdom
	Medium	−0.371	1/9	
High	Short	0.015	9/9	Switzerland, Hungary
	Medium	−0.096	8/9	

A rank of 1/9 implies that Norway has seen the largest difference between the post- and pre-merger RMSPE, thus indicating that the effect is statistically significant; 2005Q3–2006Q1 excluded from the synthetic control estimation (treatment time is assumed to be *starting at* 2006Q2)

5.3.2 The Standard Fixed Effects Method

Excluding the middle three quarters from the regression does not significantly change the conclusion of the Base specification. The treatment effect estimates in Table 8 are somewhat weaker statistically than those in Table 6, and smaller in absolute value, but point to the same direction: the merger at the very least did not increase prices.

Again, the specification with AR(1) error term produce negative, but statistically insignificant point estimates. In contrast to the earlier results, we now find no evidence of autocorrelation in the error terms for the high usage bundles. Taken together, these results support the earlier findings that at the very least, the merger did not increase prices, and possibly decreased at least some of them.

We thus find that our results are robust when controlling for the quarters immediately around the merger.

5.3.3 The Synthetic Control Method

The sensitivity analysis for the synthetic control tell a similar story to those from the DiD regressions. As can be seen from Table 9, excluding the middle three quarters from the analysis puts Norway with the highest medium-term rank for both the low and medium usage bundles, where the effect of the merger in both cases is a fall in prices. This finding supports the view that we should treat the period just around the time of the merger with care. The sensitivity analysis for the synthetic control thus implies that the merger caused statistically significant lower prices in Norway.

5.3.4 Further Robustness Analyses

A question that is relevant not only regarding our analyses but pertains also to the existing literature on telecom mergers is whether the results are biased due to the construction of price indices. As a robustness test, we therefore also used prepaid tariffs as our dependent variable (see Table 15). We do not reject the Null of

a common linear pre-trend. The effect of the merger is less statistically significant compared to our base model. With the synthetic control method, we again find negative effects, with Norway ranking first or second for the low and medium usage prepaid tariffs, but eighth or ninth for the high usage, suggesting that the negative treatment effect is significant for the first two, but not the last usage group.

As a further robustness test, we use only the two cheapest tariffs of each provider when calculating the price indices (see Tables 21, 22, 23, 24). Again, we generally reject the Null of common pre-trends (in all but one case). Again the results suggest a negative or no impact of the merger on prices. With the synthetic control method, all point estimates are negative, but the ranks point to statistical insignificance.

6 Conclusions

We studied the impact of the TeliaSonera-Chess merger on mobile prices in Norway. Compared to the existing literature, this merger stands out in that only one of the parties has a physical network and a spectrum license and the other party changed from one physical network to another; such mergers are however quite frequent in the mobile phone market and therefore relevant from a competition policy point of view. We find no evidence of the merger increasing prices: most of our treatment effect estimates suggest that if the merger had any effect on prices, it decreased them. The results of the analyses where we use the synthetic control group method give further support for the conclusion that prices did not rise after the merger.

We have identified four main explanations for why we don't observe any price increases after the merger: *First,* the merger was between a mobile network operator (TeliaSonera) and a mobile virtual network operator (Chess). The merger thus affected the competition in the retail market, i.e. the consumers, rather than in the upstream market, i.e. the mobile spectrum. Telecom regulation suggests that there is more competition at the retail level than at the upstream (infrastructure) level.[46] This could help explain why we have found no anticompetitive effects of the merger in our analysis.

Second, the merger can have made the competition between the two largest operators more equal. Before the merger, Telenor was the biggest operator on the market by far, and Chess operated on its network. After the merger, TeliaSonera's market share increased from 27 to 35% (while Telenor lost roughly 400,000 subscribers from their network). Telenor could have been incentivised to lower their prices following the merger to win some of their subscribers back. This may thus have strengthened the competition between the two largest operators following the merger. At the same time, TeliaSonera was still a significantly smaller competitor than Telenor, which potentially lowered the risk of coordinated effects between the two operators. The disentangled price development in Norway (see Fig. 1) supports

[46] See for example Gurpegui (2017) and, more generally, the European Commission's Telecom Regulation at https://ec.europa.eu/competition/sectors/telecommunications/overview_en.html.

this: both main operators in Norway lowered their prices following the merger, indicating that price-competition was not adversely affected by merger.

Third, the merger may have resulted in efficiency gains. The mobile market is characterised by high fixed costs and low marginal costs. This means that mobile network operators have a strong interest in increased network traffic. In its clearance decision of the merger, the NCA noted that acquisition-specific efficiency gains could strengthen TeliaSonera's ability and incentives to compete.[47] Our findings are consistent with TeliaSonera experiencing such efficiency gains after the merger that were, in turn, passed on to consumers.

Fourth, the NCA decision indicates that there was spare capacity in the market and that the competition therefore is in prices for what are relatively homogenous products.[48] Together with efficiency gains for TeliaSonera, this could also point towards increased competition and lower prices for consumers.

Our results thus suggest that the removal of a virtual mobile phone operator did not impede competition in the case of Norway, suggesting that the impact of such firms on competition may rely as much on the threat of entry as it relies on their impact on actual competition.

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Appendix

In this "Appendix", we provide a detailed description of how construct the price index, the country weights for the synthetic control estimation, as well as the results from the following refinements and robustness checks:

- Leaving out the quarter of the merger from the regression analysis;
- Only looking at pre-paid tariffs. Handset subsidies are not included in the Teligen data, but they may affect the prices we observe. Pre-paid tariffs are less affected by these subsidies, and only looking at this subsample thus controls for this potential issue;
- Only looking at the two cheapest tariffs.

[47] Norwegian Competition Authority (2005), "TeliaSonera's acquisition of the Vollvik Group – Clearance Decision", available in Norwegian at https://konkurransetilsynet.no/wp-content/uploads/2018/08/a2005-36_teliasonera-vollvik.pdf.

[48] Norwegian Competition Authority (2005), "TeliaSonera's acquisition of the Vollvik Group – Clearance Decision", available in Norwegian at https://konkurransetilsynet.no/wp-content/uploads/2018/08/a2005-36_teliasonera-vollvik.pdf.

Constructing the Price Index

We use the Teligen data to construct our price index. For each country, we match our usage data with the Teligen data. Then, we multiply these usage numbers with the weighting of the usage bundles from the OECD's study on mobile tariffs (see Table 2), such that the usage varies between the different bundles.[49] To calculate the monthly tariff, we need to know the monthly fixed fee and the cost of any out-of-bundle usage as indicated in the box below.

Box 1: Total monthly tariff

The total price paid per month equals the fixed monthly fee plus any out-of-bundle usage:

$price_{i,t} = fixedfee_{i,t} + \left(usage_i - includedinbundle_{i,t}\right) * \left(cost_{i,t}\right)$

Where

$price_{i,t}$ is the total cost of bundle i at time t

$fixedfee_{i,t}$ is the total fixed fee paid for bundle i at time t

$usage_i$ is our usage vector (calls min to fix, on-net, off-net, SMSs) figure for bundle i

$includedinbundle_{i,t}$ is the usage vector included in bundle i at time t

$cost_{i,t}$ is the cost of out-of-bundle usage vector for bundle i at time t

Note: Illustration, Source: Copenhagen Economics

For the **fixed fee**, we assume that the consumer pays the connection fee once every 2 years (the typical duration the package binds the consumer, especially around that time).[50] Thus, we calculate the monthly fixed fee as the sum of the monthly fixed fees and 1/24 of the connection fee.

To calculate the **cost of out-of-bundle usage** we use the Teligen data's information on how many minutes of national fixed line, on-net and off-net calls are included in each bundle and whether the bundle includes calls during peak-time, evenings or weekends. For each type of call, we thus know the minutes of peak, evening and weekend out-of-bundle usage. We assume that calls last for 2 min on average and that they are split 50/50 between peak and off-peak (evenings and weekends) time, as also done by Aguzzoni et al. (2018).[51] This is relevant when we calculate the out-of-bundle usage, as the price of a call depends on (1) it's length of the call and (2) any one-off charges associated with setting up the call. Furthermore, we use the Teligen data's information on how many SMSs are included in the bundle. We use this and the respective call and SMS prices to calculate the total out-of-bundle cost.[52]

[49] See OECD (2006), p. 6.

[50] This follows the method of Aguzzoni et al. (2018).

[51] See Aguzzoni et al. (2018) p. 70.

[52] In some cases, there is also a minimum monthly usage fee. If the calculated out-of-bundle usage cost is smaller than the minimum fee, we replace the calculated cost with the minimum fee.

Then, for the monthly tariffs to be comparable across countries, we

- convert non-euro currencies to euro, using the average nominal exchange rate over the period (2003Q4 to 2007Q3). We use the average nominal exchange rate to limit exchange-rate fluctuations affecting the relative prices;
- convert nominal prices to real prices, using the HICP with base year 2005—this enables us to investigate the real development of prices rather than the development of inflation;
- exclude VAT from the price—VAT did not remain constant for all countries across the period investigated,[53] which would lead to comparisons not being made across a constant basis.

Once we have real monthly tariffs that are comparable across countries, we can construct the price index. We do this through the following three steps:

First, we exclude all packages that we managed to identify as business packages. This follows the method of Aguzzoni et al. (2018), as such consumers are more likely to obtain large discounts on their packages, which makes the prices less representative of the average consumers. In our data this accounts for less than 10% of all observations.

Second, since a certain usage pattern cannot be explained exclusively by one individual tariff, we take the four cheapest real prices per operator for each basket (low, medium and high) each quarter. This choice both mimics heterogeneity across the consumers for each basket and reflects that consumers may not be fully rational or know their exact behaviour in advance. Finally, this choice also averages out potential measurement errors of tariffs.[54]

Third, for each basket and quarter we calculate the price index for each provider by taking the average of the four cheapest prices for that provider. To calculate the country level price index, we take the average of the two providers' price indexes for each country, basket and quarter. This leaves us with a price index consisting of one price per basket per country per quarter.

Country Weights for Synthetic Control Estimation

See Tables 10 and 11.

Some Further Robustness Checks

In this section we report our findings from the further robustness checks. Briefly summarised, our further robustness checks show conclusions that resemble our original findings. As our initial findings do not change we remain confident with our original results.

[53] In Germany, for example, VAT increased from 16 to 19% in 2007Q1.

[54] The same choice was made by Aguzzoni et al. (2018).

Table 10 Country weights for the base synthetic control

Low basket		Medium basket		High basket	
Country	Weight	Country	Weight	Country	Weight
Belgium	0.567	Belgium	0.965	Hungary	0.12
France	0.358	Hungary	0.035	Switzerland	0.88
Hungary	0.075				

Weights of the synthetic control for the base investigation of Norway

Table 11 Country weights for the robustness of the synthetic control

Low basket		Medium basket		High basket	
Country	Weight	Country	Weight	Country	Weight
Belgium	0.446	Belgium	0.356	Hungary	0.074
France	0.463	France	0.557	Switzerland	0.926
Hungary	0.072	United Kingdom	0.087		
United Kingdom	0.018				

Weights of the synthetic control for the robustness investigation of Norway

Excluding Only the Quarter of the Merger from the Regression

We exclude only the quarter of the merger from the analysis to see if our base and robustness results are sensitive to the number of quarters excluded. We note that when we just exclude the quarter of the merger, the common trends test is the same as in Tables 4 and 5. We thus still believe our standard fixed effects model to be correctly specified. Excluding the quarter of the merger does not change the results, see Table 12: the conclusion of the Base specification remains the same, but less statistically significant (compared to the standard model. But it is more statistically significant than the robustness check). The specification with AR(1) error term remain statistically insignificant.

Only Prepaid Tariffs

As for our Base specification, we test for common trends, both for the entire pre-merger period (Table 13) and the pre-merger period excluding the last quarter (Table 14). We note that the common trends test passes in both cases, and we thus argue that our DiD is also correctly specified when looking only at prepaid tariffs.

Table 12 The standard fixed effects model, excluding the quarter of the merger

Specification	Base specification			AR(1) error term		
Usage bundle	(1)	(2)	(3)	(4)	(5)	(6)
	Low	Medium	High	Low	Medium	High
Short-term effect	−0.123*	−0.137**	−0.150**	−0.075	−0.051	−0.05
	(0.055)	(0.047)	(0.046)	(0.082)	(0.079)	(0.084)
Medium-term effect	−0.240**	−0.229***	−0.152**	−0.12	−0.066	−0.047
	(0.073)	(0.064)	(0.063)	(0.096)	(0.094)	(0.101)
Observations	135	135	135	126	126	126
R-squared	0.792	0.819	0.781	0.62	0.643	0.66
AR(1) error term	No	No	No	Yes	Yes	Yes
Coefficient of AR(1) error term				0.759	0.809	0.819
P-value				0.000	0.001	0.002

Dependent variable: log prices; standard errors in parentheses (clustered at country level); time fixed effects and country-fixed effects; cheapest four tariffs; significance level: *10%, **5%, ***1%; 2005Q4 is excluded from the regression. The AR(1) error term is statistically significant if the test that the correlation between past error terms is zero is rejected at 5% level; 2005Q4 is excluded from the regression

Table 13 Results of common trends test for the entire pre-merger period, only prepaid tariffs

Usage bundle	Low	Medium	High
Conclusion of test	Pass	Pass	Pass
P-value of test	0.995	0.545	0.419

For the linear trend, we "pass" the common trend test if we cannot reject the coefficient of the interaction between Norway and the linear trend at 5% level. Only prepaid tariffs

Table 14 Results of common trends test for the pre-merger period, excluding the last quarter, only prepaid tariffs

Usage bundle	Low	Medium	High
Conclusion of test	Pass	Pass	Pass
P-value of test	0.812	0.663	0.547

For the linear trend, we "pass" the common trend test if we cannot reject the coefficient of the interaction between Norway and the linear trend at 5% level. Only prepaid tariffs

Table 15 shows our results when running our specifications on a price index constructed with only prepaid tariffs. We see that the conclusion is similar to our initial findings, albeit less statistically significant: the Base specification implies lower prices after the merger, however, this is only statistically significant at the 5% level for the low usage bundle in the medium run. The AR(1) error term specification remains statistically insignificant.

Table 16 reports our prepaid only synthetic control group findings. The implied effect of the merger is still a fall in prices. The effect seems somewhat statistically significant when comparing to the placebo tests on the benchmark countries for the low and medium usage bundles.

Table 17 shows the robustness of our prepaid only estimation. We see that both the Base and AR(1) error term specifications are statistically insignificant. The robustness of the synthetic control group using only prepaid tariffs remains the same as our initial findings (see Tables 7, 9): it implies lower prices in Norway after the merger, but these results do not seem statistically significant when comparing to the placebo tests on the benchmark countries (Table 18).

Only Two Cheapest Tariffs

As for our Base specification, we test for common trends, both for the entire pre-merger period (Table 19) and the pre-merger period excluding the last quarter

Table 15 The standard fixed effects model, only prepaid tariffs

Specification	Base specification			AR(1) error term		
Usage bundle	(1)	(2)	(3)	(4)	(5)	(6)
	Low	Medium	High	Low	Medium	High
Short-term effect	0.038	0.037	0.032	−0.029	−0.023	−0.022
	(0.036)	(0.039)	(0.047)	(0.075)	(0.086)	(0.096)
Medium-term effect	−0.201**	−0.157*	−0.171*	−0.149	−0.08	−0.069
	(0.069)	(0.076)	(0.089)	(0.096)	(0.116)	(0.128)
Observations	144	144	144	135	135	135
R-squared	0.596	0.576	0.572	0.238	0.221	0.226
AR(1) error term	No	No	No	Yes	Yes	Yes
Coefficient of AR(1) error term				0.705	0.793	0.795
P-value				0.004	0.010	0.025

Dependent variable: log prices; standard errors in parentheses (clustered at country level); time fixed effects and country-fixed effects; cheapest four tariffs; significance level: *10%, **5%, ***1%; using only prepaid tariffs. The AR(1) error term is statistically significant if the test that the correlation between past error terms is zero is rejected at 5% level

Table 16 Only prepaid tariffs: synthetic control

Usage bundle	Period	Effect	Rank	Control countries
Low	Short	−0.116	2/9	France, Hungary, United Kingdom
	Medium	−0.381	1/9	
Medium	Short	−0.115	2/9	France, United Kingdom
	Medium	−0.364	1/9	
High	Short	−0.142	9/9	France, United Kingdom
	Medium	−0.424	8/9	

A rank of 1/9 implies that Norway has seen the largest difference between the post- and pre-merger RMSPE, thus indicating that the effect is statistically significant; 2005Q3–2006Q1; only prepaid packages

(Table 20). We note that the common trends test fails for the high usage bundles, also when excluding the quarter just before the merger. We thus argue that our DiD is correctly specified only for the low and medium usage bundles when looking only at the two cheapest tariffs. The conclusions for the high usage bundles should be made with caution.

Table 21 reports our results when using only the two cheapest tariffs for each provider when calculating the price index. Our conclusion for the Base specification remains the same but becomes more statistically significant, and the short-term effect is now a statistically significant price decrease rather than being statistically insignificant. Our AR(1) error term specification now also displays a statistically significant decrease in prices following the merger, compared to being statistically insignificant in our initial version.

Table 22 shows our synthetic control using only the two cheapest tariffs. Here, the conclusion matches our initial findings: the implied effect of the merger is lower prices in Norway, however, this does not seem statistically significant when comparing with the placebo tests for the benchmark countries.

Table 23 shows our results from our robustness checks using only the two cheapest tariffs. Our Base results' conclusions remain the same as our original findings, showing statistically significant price decreases in Norway following the merger. The AR(1) error term results remain statistically insignificant (but negative). We

Table 17 The standard fixed effects model, robustness of prepaid only

Specification	Base specification			AR(1) error term		
Usage bundle	(1)	(2)	(3)	(4)	(5)	(6)
	Low	Medium	High	Low	Medium	High
Short-term effect	0.041	0.051	0.05	0.022	0.043	0.05
	(0.051)	(0.048)	(0.055)	(0.124)	(0.155)	(0.171)
Medium-term effect	−0.176	−0.139	−0.157	−0.074	0.016	0.033
	(0.080)	(0.093)	(0.106)	(0.136)	(0.174)	(0.192)
Observations	117	117	117	108	108	108
R-squared	0.628	0.595	0.59	0.817	0.727	0.765
AR(1) error term	No	No	No	Yes	Yes	Yes
Coefficient of AR(1) error term				0.746	0.846	0.846
P-value				0.019	0.051	0.193

Dependent variable: log prices; standard errors in parentheses (clustered at country level); time fixed effects and country-fixed effects; cheapest four tariffs; significance level: *10%, **5%, ***1%; using only prepaid tariffs; 2005Q3–2006Q1 excluded from the estimation. The AR(1) error term is statistically significant if the test that the correlation between past error terms is zero is rejected at 5% level

Table 18 Robustness of only prepaid tariffs: synthetic control estimation

Usage bundle	Period	Effect	Rank	Control countries
Low	Short	−0.153	5/9	Belgium, France, Hungary, United Kingdom
	Medium	−0.400	3/9	
Medium	Short	−0.030	7/9	Belgium, France, United Kingdom
	Medium	−0.305	3/9	
High	Short	−0.071	4/9	Belgium, France, Switzerland, United Kingdom
	Medium	−0.356	2/9	

A rank of 1/9 implies that Norway has seen the largest difference between the post- and pre-merger RMSPE, thus indicating that the effect is statistically significant; 2005Q3–2006Q1 excluded from the synthetic control estimation (treatment time is assumed to be *starting at* 2006Q2); only prepaid packages

Table 19 Results of common trends test for the entire pre-merger period, only two cheapest tariffs

Usage bundle	Low	Medium	High
Conclusion of test	Fail	Fail	Fail
P-value of test	0.005	0.001	0.002

For the linear trend, we "pass" the common trend test if we cannot reject the coefficient of the interaction between Norway and the linear trend at 5% level. Only two cheapest tariffs

Table 20 Results of common trends test for the pre-merger period, excluding the last quarter, only two cheapest tariffs

Usage bundle	Low	Medium	High
Conclusion of test	Pass	Pass	Fail
P-value of test	0.264	0.067	0.037

For the linear trend, we "pass" the common trend test if we cannot reject the coefficient of the interaction between Norway and the linear trend at 5% level. Only two cheapest tariffs

also note that the AR(1) error term is not statistically significant in this case for the medium and high usage bundles.

The robustness check of the synthetic control using only the two cheapest tariffs (see Table 24) is similar to what we've previously found: prices fell in Norway following the merger. For the medium and high usage, this effect is statistically

Table 21 The standard fixed effects model, two cheapest tariffs

Specification	Base specification			AR(1) error term		
Usage bundle	(1)	(2)	(3)	(4)	(5)	(6)
	Low	Medium	High	Low	Medium	High
Short-term effect	−0.267***	−0.249***	−0.209***	−0.152**	−0.122*	−0.1
	(0.040)	(0.044)	(0.039)	(0.072)	(0.073)	(0.079)
Medium-term effect	−0.266***	−0.271***	−0.202**	−0.186**	−0.162*	−0.116
	(0.064)	(0.063)	(0.061)	(0.091)	(0.094)	(0.104)
Observations	144	144	144	135	135	135
R-squared	0.798	0.819	0.74	0.455	0.455	0.36
AR(1) error term	No	No	No	Yes	Yes	Yes
Coefficient of AR(1) error term				0.688	0.720	0.752
P-value				0.000	0.001	0.025

Dependent variable: log prices; standard errors in parentheses (clustered at country level); time fixed effects and country-fixed effects; significance level: *10%, **5%, ***1%; using only the two cheapest tariffs. The AR(1) error term is statistically significant if the test that the correlation between past error terms is zero is rejected at 5% level

Table 22 Two cheapest tariffs: synthetic control

Usage bundle	Period	Effect	Rank	Control countries
Low	Short	−0.39	4/9	France, Hungary, Portugal
	Medium	−0.42	4/9	
Medium	Short	−0.18	7/9	Belgium, Hungary
	Medium	−0.24	6/9	
High	Short	−0.03	9/9	Switzerland, Hungary
	Medium	−0.13	7/9	

A rank of 1/9 implies that Norway has seen the largest difference between the post- and pre-merger RMSPE, thus indicating that the effect is statistically significant

insignificant, however, when comparing to the placebo tests using the benchmark countries. For the low usage bundle, the effect does seem to be statistically significant.

Table 23 The standard fixed effects model, robustness of two cheapest tariffs

Specification	Base specification			AR(1) error term		
Usage bundle	(1)	(2)	(3)	(4)	(5)	(6)
	Low	Medium	High	Low	Medium	High
Short-term effect	−0.242**	−0.238**	−0.192**	−0.193	−0.193	−0.137
	(0.051)	(0.052)	(0.047)	(0.097)	(0.108)	(0.120)
Medium-term effect	−0.289**	−0.311**	−0.238*	−0.230*	−0.205	−0.14
	(0.061)	(0.065)	(0.072)	(0.107)	(0.120)	(0.134)
Observations	117	117	117	108	108	108
R-squared	0.861	0.857	0.774	0.853	0.831	0.855
AR(1) error term	No	No	No	Yes	Yes	Yes
Coefficient of AR(1) error term				0.746	0.846	0.846
P-value				0.019	0.051	0.193

Dependent variable: log prices; standard errors in parentheses (clustered at country level); time fixed effects and country-fixed effects; significance level: *10%, **5%, ***1%; using only the two cheapest tariffs; 2005Q3–2006Q1 excluded from the estimation. The AR(1) error term is statistically significant if the test that the correlation between past error terms is zero is rejected at 5% level

Table 24 Robustness of two cheapest tariffs: synthetic control estimation

Usage bundle	Period	Effect	Rank	Control countries
Low	Short	−0.420	1/9	Belgium, France, Hungary
	Medium	−0.449	1/9	
Medium	Short	−0.326	3/9	Belgium, France, Hungary
	Medium	−0.385	2/9	
High	Short	−0.021	9/9	Switzerland, Hungary
	Medium	−0.145	6/9	

A rank of 1/9 implies that Norway has seen the largest difference between the post- and pre-merger RMSPE, thus indicating that the effect is statistically significant; 2005Q3–2006Q1 excluded from the synthetic control estimation (treatment time is assumed to be *starting at* 2006Q2)

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