Does one more or one less mobile operator affect prices? A comprehensive ex-post evaluation of entries and mergers in European mobile telecommunication markets[1]

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Abstract

This paper estimates the impact of entries and mergers on the price of mobile voice services in a panel database of 27 European Member States between 2003 and 2010. Our difference-in-differences econometric methodology exploits the variance in different structural changes between countries to separate the respective effects. Our results show that the effect of entry crucially depends on the number of active operators and the type of entrant, and not controlling for these differences might lead to misleading conclusions. We find no robust evidence that entry has a price-decreasing effect on markets with originally 2 operators. However, the entry of a 4th operator does have a price-decreasing effect, but with different dynamics concerning the entrant's type. When we separate entry effects for the subsequent years, we show that the significant price-decreasing effects for local operators entering occur only in the first year after entry, while the price-decreasing effects for multinational entries are significantly larger on the long-run. Last, we find no price-increasing effects of 5-to-4 mergers, but a long run price-increasing effect of a 4-to-3 merger.

JEL classifications: L11, L49, L59, L96

Keywords: ex-post evaluation; mobile telecommunications; entry; merger; difference-in-differences estimations

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1. Introduction

In mobile telecommunications markets of European Member States, we see a vast number of entries between 2000 and 2010, and also a large decrease (more than 50%) in the price of voice services. But are these price changes driven by entry or are they due to other influencing factors? Answering this question is of crucial importance for regulatory policies in these markets, because mechanically associating entry with falling prices could lead to excessive policy steps towards incentivizing entry (for example, concerning spectrum allocation, and asymmetric regulation of termination charges favoring new operators), sometimes resulting in non-sustainable market structures. On the other hand, we also observe a tendency of consolidation in the mobile industry in the form of mergers and joint ventures, and so competition policy faces the opposite question whether the decrease in the number of mobile operators would lead to an increase of prices. At the end, all of the European mergers in this period were cleared (although some of them only with remedies), and so we can evaluate whether they have been indeed beneficial or at least neutral to the competitive process.

Our paper aims to evaluate the ex-post effects of entries and mergers on the prices of mobile voice services by using a database of 27 European Member States between 2003 and 2010. Our results show that the effect of entries and mergers crucially depends on the number of active operators and the type of entrant, and not controlling for these differences might lead to misleading conclusions. Our panel database allows us to follow markets affected by changes in market structure, and compare the post-entry (or post-merger) outcomes both to pre-entry outcomes in the same market and to market trends in other markets not affected by entry. As we can find a different set of treatment and control groups for each separate entry or merger effect we want to estimate, almost all of parameters of interest can be identified. This classical difference-in-differences estimation method also minimizes the risk resulting from unobserved heterogeneity between markets.

Although prices are not the only indicators of the intensity of competition and for the welfare of consumers, they are of central importance both from a theoretical and policy point of view. Many regulatory policies are based on a comparison of prices between European countries,[4] but these simple cross-sectional analyses can lead to misleading conclusions because of at least two reasons. First, the price difference between countries of n and $(n + 1)$ active mobile operators can be due to other factors, and the analyst can never be sure of having solved the

[4] See for example the regularly published Progress Report on the Single European Electronic Communications Market by the European Commission and the biannually published OECD Communications Outlooks.

omitted variable bias problem. Second and more importantly, the effect of an additional operator estimated from a cross-sectional comparison cannot be equated with the effect of an actual entry that might have a long-lasting effect on a single market.

Summarizing the experience of entries and mergers between 2003 and 2010 can be also useful because this period coincides with the widespread launch of third generation mobile services and also with the allocation of the corresponding new radio spectrum bands in the early 2000s. As the launch of fourth-generation (4G) mobile services is currently under way, new spectrum needs to be allocated,[5] so a thorough understanding of the ex-post effects would allow more informed future policy decisions, and more realistic expectations about the effect of entry on mobile markets. Therefore, we are testing four hypotheses that can provide empirical evidence to fine-tune traditional rules of thumb on entry and merger issues in mobile telecommunication markets.

First, we examine whether the effect of entry depends on the number of mobile operators originally active on the respective markets. Our results show that only the effect of an operator entering markets with originally 3 or 4 operators turns out to be significant for most specifications.[6] Although this result alone cannot lead to a general conclusion that the optimal number of mobile operators is 3 or 4, we observe that after several entries and mergers, all 27 European Member States but Cyprus ended up with 3 or 4 active operators by 2012.

Second, we test whether the type of entrant matters for entry effects, partly because of their potentially different competitive impact, and also because regulatory policies are sometimes biased towards or against the entry of multinational or local operators. This separation becomes especially important for markets with originally 3 operators, and so our results show that the effectivity of entry depends not only on the initial number of firms but also on the type of the entrant, which should be taken into account both for ex-ante planning and ex-post assessment.

Third, we examine the dynamics of entry effects and therefore separate the effect of entry on the first, second and subsequent years after it occurred. This separation highlights further important differences. Entries on the markets with originally 2 operators are still mostly insignificant, but the point estimates indicate that local 3rd entrants might have a larger beneficial impact. The entry of a 4th local entrant has a price-decreasing effect only in the first year, but no effect afterwards. Conversely, the entry of a multinational firm to the 4th position (these types of entries always correspond to Hutchison) does not affect prices in the

[5] These new spectrum awarding procedures already happened in some European Member States.

[6] We do not include Virtual Mobile Network Operators (MVNOs) when calculating the number of operators.

first year, but then decreases prices (although statistically non-significantly) from the second year onwards. We can however find a statistically significant difference in the price effects brought by the types of entrants: in the first year after entry, local entrants have a more beneficial (negative) effect on prices than multinational firms, and the reverse is true from the third year onwards. These findings indicate that although local entrants might have been associated with more aggressive price strategies, their entry does not lead to a long-run competitive impact on 3-to-4-firm markets.

The table below summarizes the dynamics of average price effects for different entry types based on the estimation results of our final econometric specification.[7] Note that these effects are not cumulative over time and they always refer to the price difference in the specific year between countries affected by a specific type of entry and otherwise similar countries but for that entry (this is done by controlling for other country-specific properties).

Table 1. Estimated average price effects (point estimates) for different entry types

	2-to-3 entry			3-to-4 entry	
	Multinational	Local		Multinational	Local
In 1st year	+8%	-33%		+2%	-31%***
In 2nd year	+28%**	-24%		-20%	-10%
From 3rd year	+3%	-15%		-21%	+26%

Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Fourth, we evaluate the effect of mergers depending on whether they occurred in 5- or 4-player markets. For the only 4-to-3 merger (in Denmark) cleared with remedies, we find no significant price changes in the first and second year after the merger, but a significant price increase (+29%) can be shown from the third year, compared to the trend of similar 4-operator countries. For 5-to-4 mergers, we no price-increasing effects are found after the merger or in the subsequent years. Based on these results, we can also refute the hypothesis that the effects of *x*-to-*y* entries and *y*-to-*x* mergers are similar in magnitude with an opposite sign, which simplified idea is sometimes present in competition policy analyses.

[7] We do not display here the effects of 4-to-5 entries as there are too few of them, but discuss them later.

2. Literature review

Our impact assessment method based on difference-in-differences estimation fits into the broad class of "program evaluation" or "treatment effect" literature, discussed comprehensively by Imbens and Wooldridge (2009). However, while this method is very common in labor economics for example, there are substantially lower number of applications in Industrial Organization.[8]

Recently, ex-post evaluation studies of mergers have become more common, partly because competition authorities felt the need to quantify the welfare benefits of their activity.[9] Most ex-post studies using a treatment effect approach compare local markets in a given country affected by a merger to other non-affected local markets.[10] In the telecommunication industry, however, the formation of treatment and control groups would usually not be possible within a country, so the analysis must be built on inter-country comparisons.

The use of country-level panel data is relatively scarce for studying the change in market structure.[11] Li and Lyons (2012) study the speed of market penetration of mobile telephone services for a large panel of OECD countries in 1991-2006 and find that an increase in the number of mobile operators significantly improves the diffusion rate till the entry of the fifth player (although the entry of the 4th player is shown to be insignificant). The subject of our paper is on mobile pricing and for a different time period, so it is of no surprise that our results provide different indications for the optimal number of operators.

Gruber (2005, 2007) builds a simple analytical framework for studying entry into the mobile telecommunication industry and examines developments on European markets from this perspective. One of Gruber's main conclusions is that regulators just mechanically applied an "$n + 1$ rule" meaning that there should be one more active operator with the introduction of a new technology generation and the corresponding spectrum released (3G in 2000s) in order to guarantee effective competition. However, he presents convincing evidence that this hypothesis was not well founded and was based much more on industry policy objectives than demand-side considerations. Operators also overestimated the profitability of entry and often entered if a possibility arose. Garrard (1998) also finds similar evidence for mobile market developments till 1997. These findings are important for our empirical analysis, as they indicate that entries could plausibly be considered exogenous shocks.

[8] Angrist and Pischke (2010) actively promote the use of such techniques and discuss their advantages.

[9] See Ormosi's (2012) contribution to the 2012 debate at the OECD on this subject, summarizing both the available methods and the academic contributions.

[10] See for example Hastings (2004), Dafny (2009) and Choné and Linnemer (2012).

[11] For a slightly distant application, see Przybyla and Roma (2005) studying the effect of increasing product market competition on inflation in a panel of European countries divided by sectors.

3. Developments on European mobile telecommunication markets between 2000 and 2012

At the very beginning of the 2000s the widespread expectation was that the third generation mobile services capable of providing fast data transmission in addition to voice services would be an imminent success story for mobile markets. Though the technology, services and business models were not ready and tested, these inflated expectations influenced the timing of spectrum assignments in Western Europe. The allocation process started in 1999 and many countries started releasing the 3G spectrum as soon as possible. Some countries chose auctions as allocation method while others used beauty contests. Early auctions in 2000 resulted in lot of new applicants and extremely high bids for the spectrum, and therefore unexpectedly high revenues to the state budgets.

This environment also encouraged regulators in many countries to set the allocation procedures in a way that offered entry opportunities for newcomers to the increasingly mature mobile markets. By the beginning of the 2000s we observe 3 or 4 active operators on the mobile markets of most developed countries. This was a result of a gradual increase in the number of operators from the analogue mobile service era. There were always entry opportunities whenever a new spectrum band was released. The 900 MHz spectrum for GSM was available for two players, and the subsequent 1800 MHz spectrum was allocated in a way to create opportunity for at least one new player. It seemed that the growing demand form mobile telephony made it possible to strengthen competition by increasing the number of operators. This hypothesis that new spectrum makes new entry possible and viable was not questioned and thought to be working by regulators (see the discussion of Gruber's view on the "n + 1 rule" in the previous Chapter).

After the allocation of the spectrum in many Western European countries by 2002, there were lot of failed expectations concerning entry or large auction revenues in those markets where the process started later. In countries where new players won licenses around 2000, the entry boom turned to bust as some entries were delayed or even aborted. In other countries, the auctions failed to attract operators others than the incumbents. The reasons were manifold, but the technological problems concerning UMTS networks and data speed provided, the unavailability of handsets and services, and the lack of demand were certainly among them. Nevertheless, the first new networks were turned on, commercial offers appeared on the market in 2003 and the slower than expected take-up started.[12] Some regulatory ease of the

[12] See for example Gruber (2007) and Dippon (2012).

coverage requirements or even license fees, and some leniency concerning network sharing in some countries were applied to support the advance of the new networks. By the second half of the decade, the implementation of HSPA enhancement to the UMTS networks and the availability of new devices like USB modems and later smartphones made 3G a much demanded service. However, the question of viability of changing market structure and competition by inviting new entrants still remained an issue to be judged by the subsequent market developments.

For the ex-post evaluation of entries, it is very important to note that winning a spectrum license does not necessarily mean that the mobile operator actually entered the market. Several companies returned the spectrum without effectively using it and wrote down the material and goodwill losses. There were cases when the regulator withdrew the licenses because the license holder did not fulfill its commitments. Actual commercial entry offering voice and data services for customers occurred in many cases only 1-3 years later than the legal entry of the company to the respective national market. Therefore, official statistics (like the ones published by the Directorate General for Information Society of the European Commission, DG InfoSoc) on the actual number of mobile operators can be misleading. In order to draw the real competitive landscape for each country, we complemented and corrected official statistics with information available on mobile operators' websites or other industry information sources on the time of effective entry on each market.

The next table demonstrates how the number of active operators evolved between 2000 and 2012 in the current 27 Member State of the European Union.[13] These aggregate numbers show the results of effective entries and mergers in each years. We do not observe exits in this period in the sense that an already supplying mobile operator stopped its service that was available for years.[14] Until 2003, that is before the launch of 3G networks, the number of operators only refers to the number of active 2G players. The numbers after 2000 show that in some countries new 2G networks were launched earlier and 3G was awarded later, but in other cases these two licenses were awarded together for newcomers. Operators with only 2G or 3G licenses and operators with both type of licenses were always counted as one player.[15]

[13] We consider only the operators using GSM (2G) or WCDMA (3G) technology having license in the 900 MHz, 1800 MHz and 2100 MHz bands.

[14] The only case that could be interpreted as market exit following its failure to win a 3G license is the case of Blu, a 2G operator in Italy, but we consider it as an acquisition that happened few month after (by another operator Wind).

[15] We also consider operators as economic entities in competition policy terms, so two legally distinct companies with the same owner are counted as one single entity.

Table 2. Number of countries with 1-5 active operators between 2000 and 2012 (as effective at the 1st of January of each year)

	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
1 operator	3	2	1	1	1	0	0	0	0	0	0	0	0
2 operators	8	6	6	6	5	6	4	4	2	1	1	1	1
3 operators	11	13	14	15	14	14	15	14	15	14	14	14	14
4 operators	4	5	5	4	3	4	6	8	9	9	10	11	12
5 operators	1	1	1	1	4	3	2	1	1	3	2	1	0

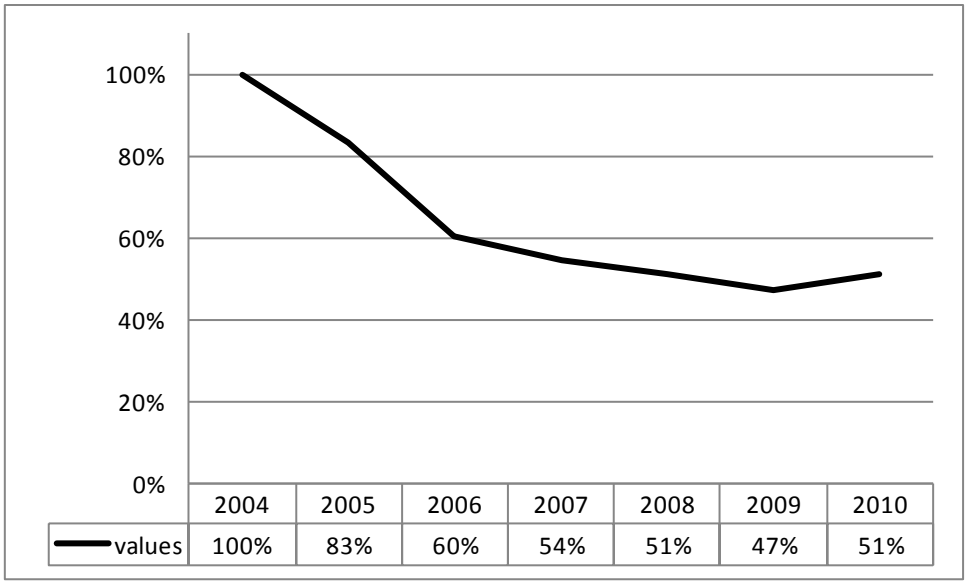
We can observe from in this table that although there was large variance in the number of operators in 2000, this number stabilized as a result of entries and mergers at the level of 3 or 4 operators for all countries (except Cyprus) by 2012. We identified 27 entries and 8 mergers between 2000 and 2012.

The average number of operators per country increased from 2.70 in 2000 to 3.41 by 2012. The 0.7 increase is closer to one, and might be hastily interpreted as being in line with the hypothesis that the $n + 1$ rule has been working. However, all the 7 of the 28 entries before 2003 were late 2G entries. From 2003 only 3G entries occurred, so the average increase from 2.93 in 2003 to 3.41 in 2012 is below 0.5.

The structural changes on many markets happened because of regulatory steps releasing new spectrum for providing mobile services with the intention or expectation of enhancing competition on the market. The success of this policy must be judged by the results. This huge number of changes in the structures of national mobile markets provides a very good opportunity for assessing the intertemporal changes due to entries (and mergers) and whether this entry-enhancing policy has contributed to decreasing prices.

The graph below shows that average prices were indeed significantly falling in the observed period. For this illustration, we include prices only for countries where we have observations for all years, so we start from 2004 when 10 new Member States joined the European Union.

Graph 3. Average level of OECD mobile voice basket prices for 24 European Member States between 2004-2010 (base = 2004)



4. Database

Our price database builds on the regular yearly reports prepared by Teligen for DG InfoSoc, available for years between 2003 and 2010. As there exists no universal price indicator for mobile services, the OECD defined three product baskets for low, medium and high usage levels in 2002.[16] The OECD baskets were redefined in 2006 and 2009 in order to take care of changing consumption patterns and facilitate inter-country comparisons, but we use the OECD 2002 basket prices for all years as we want to track the intertemporal changes of prices. For each country, the price of the respective consumption pattern was computed for all mobile packages offered in the respective period, and the price of the two cheapest alternatives are published for the two leading operators in the given country. All published prices are in Euros and include VAT.

The published prices indicate prices in August or September of the respective year. As for each country-time observation we have two prices for each basket, we construct average prices for baskets because we believe that average prices are better country-specific indicators than firm-specific prices. For each country-time observation, we also form a representative average price by taking the mean of the three average basket prices. The advantage of using

[16] Low usage basket: 25 outgoing calls (37 minutes) per month + 30 SMS messages, 42% of calls are to fixed line phones, 58% to mobile phones. Medium usage basket: 75 outgoing calls (148 minutes) per month + 35 SMS messages, 36% of calls are to fixed line phones, 64% to mobile phones. High usage basket: 150 outgoing calls (315 minutes) per month + 42 SMS messages, 40% of calls are to fixed line phones, 60% to mobile phones. Only post-paid prices are measured, and they include the monthly rental and registration charges distributed over 3 years.

multiple prices is that we can estimate our specifications for each of them in order to check the robustness of our results.

For 15 countries, we have price observations from 2003 till 2010. For 10 Member States joining in 2004, we have prices only from 2004 (except for Cyprus, for which they are available from 2007), and from 2007 for two additional Member States joining in 2007. In total, we have 195 observations in our panel.

Note that there might be some measurement errors present concerning the prices, as we observe countries for which prices were stable for 2 years and then showed a price decrease of 20-40%. Still, we do not think these values should be categorized as outliers, because drastic price decreases can happen in mobile markets. Furthermore, we observe but the lowest prices for exogenously chosen consumption baskets, not average prices. Therefore, we retained these observations, partly because our database is very small, and more importantly we could have missed some entry shocks that we wish to evaluate.

The next table summarizes all relevant dependent and control variables for each country-time observation.[17] Simple descriptive statistics on these variables can be found in Appendix 1.

Table 4. Description of main variables

Abbreviation	**Description**	**Source**
Plow	Average price for low usage mobile basket (OECD2002)	DG InfoSoc
Pmid	Average price for medium usage mobile basket (OECD2002)	DG InfoSoc
Phigh	Average price for high usage mobile basket (OECD2002)	DG InfoSoc
Pav	Mean of the average basket prices	
GDPcap	GDP per capita	Eurostat
Pop	Population	Eurostat
Exch	Average annual exchange rate (Euro/domestic currency)	ECB
Inflation	Inflation rate stacked from base year 2003	Eurostat
Vat	Value added tax on mobile services (standard rate)	EC
MTR	Average mobile termination charge	DG InfoSoc
Pen	Mobile penetration[18]	Eurostat
MVNO	Dummy variable measuring present of MVNO	Own research

The previous Chapter discussed the calculation of operators' number in a country. However, we have to differentiate between "early" events already affecting prices in the same year (note that prices are measured in August) and "later" events that could have affected prices only from the second part of the year. As we are measuring only the prices of the two main

[17] Market share numbers for the first and second mobile operators are also available, but we do not include these variables in our regressions as they are directly with entry and merger treatments. Anyway, even if correlated mobile penetration numbers were not available for 2010, we used the 2009 values for 2010.

[18] As mobile penetration numbers were not available for 2010, we used the 2009 values for 2010.

operators, the entrant's prices are not included yet in the country's average price. It is a common industrial view that adjustment to competitors' new pricing strategy requires 2-3 months. Therefore, we assume first that if an entry or merger occurred till the end of May of year *t*, than it already affected prices in August, and in this case we refer to this event as happening in year *t*. On the other hand, if the event occurred after May of year *t*, than we refer to this event happening / taking effect in year ($t + 1$). For entries, we consider the date for the launch of services available from operators' websites for this calculation. For mergers, we consider the date of the competition authority's clearing decision. We can also test the robustness of our results by checking the estimations with different periods of time till the events start to have effect - one could even study whether prices started to decrease before entry effectively took place but was already public knowledge due to the known results of the spectrum allocation.

5. Estimation strategy and separation of effects

The ideal method to estimate the effects of entries and mergers would be to conduct controlled experiments. This would mean selecting national markets with identical market structures, let the event happen for a randomly selected subgroup (treatment group) but not on the other markets (control group), and then the difference of post-event price changes between the treatment and control markets would provide the effect of interest.

As these controlled experiments are not possible with entries and mergers, we follow a quasi-experimental design by tracking the whole sample of national markets over the observation period, and for each effect to be separated we compare the pre- and post-merger price differentials between affected and non-affected markets. We are thus identifying our parameters of interest from variation in the number of players in observed countries over time. The motivation behind this difference-in-differences comparison is that observations on non-affected markets can form a counterfactual by informing us about what would have happened to the affected markets had the event not taken place. Controlling for additional factors like demand and market structure ensures that we compare as similar subjects as possible.

We implement this estimation strategy by estimating panel regressions with country and time fixed effects. Our estimated equations will take the following general form:

$$p_{it} = \sum_j \alpha_j s_{jit} + \beta \cdot controls_{it} + u_i + v_t + \varepsilon_{it},$$

where *i* indexes countries and *t* indexes time periods. A country-time observation can be subject of one or several shocks indicated by dummy variables s_{jit}, for which we estimate our main parameters of interest α_j. Unobserved heterogeneity will be captured by country fixed effects, which is important in our case also because assignment to treatment and control status is not random. Time fixed effects control for changes in common unobservable variables to all countries in a given period.

In the basic case, a country will be subject of an entry treatment *e* from period *t* (as discussed before, this means that entry took place before the end of May).[19] When we want to differentiate on the type of entry, treatment *exy* will refer to the situation if entry happened at a market with originally *x* active operators so it became a *y*-player market. We also want to separate the effects on the entrant's type, and so treatment *exy_big* shows if a multinational firm entered the respective market,[20] while local entrants the country is subject to treatment *exy_small*. Finally, when we separate different effects for different time periods for the same event, *exy*_1 = 1 only in the first year after the *xy* entry, *exy*_2 = 1 only in the second year after the *xy* entry, and *exy*_3 = 1 for all years from the third after the entry.[21]

Treatments are similarly defined for mergers denoted by *m*, but here the type of the firms involved is not separated. Note that the treatments take care of changes in the number of operators, so this control should not be added to the regressions as country-fixed effects implicitly include the number of operators in the first year observed. We can also add treatments together, we will for example use *deltaop* = *e* - *m* to measure how the number of operators changed up to the respective year from the first period.

Naturally, a national market can be subject to several treatments during the observed period. Note however, that the treatment and control groups are different for each effect, and this is the crucial fact that makes the identification of each effect possible. Let us take the example

[19] A possible solution would have been to exclude the interim period for the year of entry (that is to exclude it both from the treatment and the control group), but that would have decreased sample size considerably.

[20] Big multinational firms are Deutsche Telekom, Telefonica, Orange, Vodafone, and Hutchison.

[21] Naturally, one could define separate effects for each year after the event, but it can considerably decrease the degrees of freedom of the estimation, which is a problem with small databases such as ours. In competition policy analysis, it is common to evaluate effects for only 2-4 years after the event (typically mergers), so this separation into 3 different time periods seems reasonable.

of a hypothetical country A with 3 operators in 2003, entrant Hutchison starting operation in March 2006, and then a merger of any two active operators in December 2007. Then for this specific country *e*34*big*_1 = 1 for 2006, *e*34*big*_2 = 1 for 2007, *e*34*big*_3 = 1 for 2008-2010, *m*43_1 = 1 for 2008, *m*43_2 = 1 for 2009 and *m*43_3 = 1 for 2010. Country-time observation A-2006 is therefore in the treatment group of effect *e*34*big*_1, but is also in the control group of all merger effects.

The following table illustrates how the studied events affected the different European markets. To clarify again, by entry date we mean the year when the company effectively started to offer mobile services on the market, not the year when it acquired the necessary spectrum for it. Note that we also include events that are affecting prices in all periods observed, like for example Hutchison's 2003 entry in Italy. When we are assuming a universal treatment for all periods observed (like entry treatment *e*), these observations will be used by the estimation method as if they were in the control group. However, when we are estimating dynamic effects (entry treatments *e*34*big*_1, *e*34*big*_2, *e*34*big*_3), then different observations for this country will be affected by different treatments, so will belong to different treatment groups.

Table 5. Assessed entries and mergers for the observed period in 27 countries

	Countries	2003	2004	2005	2006	2007	2008	2009	2010
Stabile 2 ops	1 country								
Stabile 3 ops	9 countries								
Stabile 4 ops	2 countries								
2-3 entry: 5 countries 2 big 3 small	Latvia				Small				
	Luxembourg			Big					
	Malta							Small	
	Slovakia					Big			
	Slovenia						Small		
3-4 entry: 8 countries 3 big 5 small	Ireland				Big				
	Italy	Big							
	Luxembourg								Small
	Poland					Small			
	Romania					Small			
	Slovenia							Small	
	Spain					Small			
	Sweden		Big						
4-5 entry: 1 big 1 small	Denmark		Big						
	Poland								Small
4-3 merger: 1 case	Netherlands						X		
5-4 merger: 4 cases	Austria				X				
	Denmark			X					
	Netherlands				X				
	United Kingdom								X

There are several affected countries for most of the effects to be estimated and they occur in different time periods, which are useful properties for our estimations. Note also that among the big multinational firm entries, all 2-to-3 entries correspond to the so called "Big4" firms (T-Mobile, Orange, O2 and Vodafone) that are incumbents in some European countries, while all 3-to-4 and 4-to-5 entries are by Hutchison which is nowhere in an incumbent position. The small entries were almost always done by firms usually called "challengers" or "mavericks" (like Tele2, Bite, DigiMobil), so they do not belong to regional multinational firms that are incumbent in a country (like TeliaSonera or Telekom Austria).[22]

5.1 Discussion of econometric problems

Note that our estimated effects are guaranteed to be unbiased only if entry and merger events can be considered as exogenous country-specific shocks, or more specifically that the price developments in the respective countries did not influence the firm's decisions regarding the timing of entries and mergers. However, evidence found by Gruber (2005, 2007) and Garrard (1998) discussed before indicates that the entry choices are driven much more by the regulator's more general (and less unambiguous) objectives and not necessarily on the careful ex-ante consideration of pricing trends. Even if prices are then rationally chosen by the firms following entry, industry experiences hint at the fact that the entry decision itself is often based on factors other than price expectations.

On top of this "verbal" justification, consider it for a moment what kind of endogeneity would be problematic for our estimations. Our effects to be estimated are linearly shifting the trend of prices and the assumption behind our DID estimation method is that price changes in non-affected countries can form a valid counterfactual for the treated countries. If firms observe or expect *different price levels* in different countries and base their entry decisions on this experience, this behavior alone does not violate the critical assumption of exogeneity for our estimations. Endogeneity becomes a potential problem only if countries are selected (by the firms involved) for the specific treatment group because *different trends of price changes* are expected for them, as then comparing differences between treatment and control groups are misleading. Naturally, we cannot exclude that entries and mergers are based on these more advanced expectations (in a perfect world of rational business decisions they should), but this kind of problematic endogeneity is less likely to be satisfied in reality.

[22] The only exception is the entry of Xfera owned by TeliaSonera to Spain, but it is branded under a different name and did not gain considerable market share.

In general, there exist various econometric techniques dealing with the potentially existing endogeneity problem,[23] but these methods are hard to apply in our case. As the events happen in different time periods, exploratory "tests" of endogeneity based on comparing the time series of prices between several treatment and control groups are not applicable. Theoretically, using instruments could offer a possible solution for taking care of endogeneity, but it would be very hard to find variables correlated with the events, but not correlated with the prices.

Another important problem to address with DID estimation is that their estimated standard errors can be serially correlated, which biases the standard errors.[24] This problem should be seriously considered in our case, as serial correlation is very much expected for our decreasing trends of prices (and in fact will be always found). There are various estimation techniques available to correct for serial correlation,[25] we use the most intuitive (although not the most efficient) method of estimating our specification on the differentiated time series. In this way, we lose one period of observations, but as country-fixed effects are not estimated anymore, the degree of freedom of our estimations is basically unchanged.

6. Empirical results

Now we are presenting our main results using the empirical framework defined in the previous Chapter. We run our regressions for the log of prices, so the estimated effects for the treatments can be interpreted as percentage changes in prices due to the event. We cluster standard errors by country to account for within-group correlation. As we have four prices (low, medium and high basket and the average of these three) for all country-time observations, we use the results for all of them to look for robust patterns. We consider a result stronger if more regressions provide a significant estimate of the same sign.

If we are estimating our regression on the level of variables, we encounter the problem of serial correlation mentioned in the previous Chapter.[26] Therefore, in the main text we display only the results from the estimation on first differences, and Appendix 2 includes estimation results on levels for the first and the last specification. We should note that our qualitative results change considerably, so we should indeed be careful of the serial correlation problem when evaluating the price effects of telecom entries and mergers.

[23] See for example Wooldridge (2002) for a detailed overview.
[24] See Bertrand et al (2004).
[25] Again, see Wooldridge (2002).
[26] We use the test developed by Woodridge (2002), implemented by Stata command *xtseries*.

We include the full set of possible controls presented in Chapter 4, even though a large subset of them will be consistently insignificant. When estimating in differences, the only control that is consequently and strongly significant is the population of the specific country, which can be interpreted as a demand control. On the other hand, variables controlling for country level price changes (inflation and exchange rate) and for potential cost changes influencing telecommunications prices (penetration, mobile termination rates and VAT) turn out to be mostly insignificant, even though we observe variation in some of them. Interestingly, the presence (entry) of an MVNO has a small but positive effect on prices when significant. Almost all time fixed effects will be insignificant with the estimations in differences, but we do not display estimates for them.

We start with a very basic estimation and then progress step-by-step, in order to demonstrate that the separation of effects to account for different features of entries and mergers can become very important for a correct ex-post evaluation. We give a detailed interpretation of the effects after our final specification.

First, we explain the changes in prices only with the change in the number of operators, *deltaop = e - m*. This specification is equivalent with the usual approach of price-concentration analyses where we include the number of operators in the regressions.

Table 6. Results for regressing log of prices on the change in number of operators, estimation on first differences

	(1) d_logPlow		(2) d_logPmid		(3) d_logPhigh		(4) d_logPav	
VARIABLES	coef	se	coef	se	Coef	Se	coef	se
d_deltaop	-0.12*	(0.07)	-0.12*	(0.07)	-0.12*	(0.07)	-0.12*	(0.06)
d_mvno	-0.02	(0.04)	0.14***	(0.04)	0.13**	(0.06)	0.11**	(0.05)
d_logGDPcap	0.16	(0.58)	-0.26	(0.54)	0.05	(0.53)	-0.03	(0.51)
d_logPop	9.08***	(2.80)	8.62***	(2.84)	10.11***	(3.11)	9.48***	(2.81)
d_logExch	0.90	(0.72)	1.14	(0.77)	0.86	(0.77)	0.92	(0.72)
d_inflation	-0.00	(0.01)	-0.00	(0.01)	-0.00	(0.01)	-0.00	(0.01)
d_vat	-0.00	(0.03)	-0.00	(0.03)	-0.02	(0.03)	-0.01	(0.03)
d_logPen	-0.43	(0.31)	-0.37	(0.32)	-0.12	(0.36)	-0.25	(0.32)
d_logTerm	-0.24**	(0.11)	-0.06	(0.10)	-0.10	(0.11)	-0.11	(0.10)
Constant	-0.06	(0.07)	-0.05	(0.06)	-0.10*	(0.06)	-0.08	(0.06)
Country FE	NO		NO		NO		NO	
Year FE	YES		YES		YES		YES	
Observations	168		168		168		168	
R-squared	0.28		0.26		0.25		0.27	
p-value for H_0: error term AR(1)	0.47		0.35		0.46		0.48	

Robust country-clustered standard errors in parentheses
Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

This rough specification provides point estimates indicating that an increase (decrease) in the number of operators leads to a weakly significant 12% price decrease (increase) on average compared to the trend of non-affected countries. If this result proved to be robust, it could provide a basis for an activist entry and merger regulation.

Now let us allow different price effects for entries and mergers. Here we assume a linear effect of entries or mergers, so if for example two entries happen between 2006 and 2008, then the estimated effect after 2008 should be interpreted as two times the coefficient for treatment *e*.

Table 7. Results for regressing log of prices on the simple treatments of entry and merger, estimation on first differences

	(1) d_logPlow		(2) d_logPmid		(3) d_logPhigh		(4) d_logPav	
VARIABLES	coef	se	coef	se	coef	Se	coef	se
d_e	-0.13	(0.09)	-0.18**	(0.09)	-0.18*	(0.09)	-0.17*	(0.09)
d_m	0.08	(0.08)	-0.02	(0.04)	-0.01	(0.04)	0.00	(0.04)
d_mvno	-0.01	(0.04)	0.15***	(0.04)	0.14**	(0.06)	0.12***	(0.05)
d_logGDPcap	0.18	(0.58)	-0.19	(0.53)	0.12	(0.52)	0.03	(0.50)
d_logPop	9.17***	(2.82)	8.96***	(2.93)	10.43***	(3.19)	9.76***	(2.89)
d_logExch	0.89	(0.72)	1.11	(0.77)	0.83	(0.76)	0.89	(0.71)
d_inflation	-0.00	(0.01)	-0.00	(0.01)	-0.00	(0.01)	-0.00	(0.01)
d_vat	-0.00	(0.03)	0.00	(0.03)	-0.01	(0.03)	-0.01	(0.03)
d_logPen	-0.42	(0.32)	-0.34	(0.33)	-0.08	(0.37)	-0.22	(0.33)
d_logTerm	-0.23**	(0.11)	-0.05	(0.10)	-0.08	(0.11)	-0.10	(0.10)
Constant	-0.06	(0.07)	-0.04	(0.06)	-0.10*	(0.06)	-0.07	(0.06)
Country fixed effects	NO		NO		NO		NO	
Year fixed effects	YES		YES		YES		YES	
Observations	168		168		168		168	
R-squared	0.28		0.27		0.26		0.28	
p-value for H0: error term AR(1)	0.41		0.28		0.46		0.41	

Robust country-clustered standard errors in parentheses
Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

We see considerable changes from previous results: entries are shown to have a weakly significant price-decreasing effects of 17-18% (except on the low-type basket), but we find no significant effects for mergers.

We can now turn to our first main research question on whether the effects depend on the number of active operators before the event. The next table shows our estimates for treatments separated by the type of entries and merger depending on the number of operators before the event.

Table 8. Results for regressing log of prices on entry and merger treatments depending on the number of operators before the event, estimation on first differences

	(1) d_logPlow		(2) d_logPmid		(3) d_logPhigh		(4) d_logPav	
VARIABLES	coef	se	coef	se	coef	se	coef	se
d_e23	-0.14	(0.19)	-0.20	(0.21)	-0.19	(0.20)	-0.18	(0.20)
d_e34	-0.11	(0.10)	-0.13	(0.08)	-0.13	(0.11)	-0.13	(0.09)
d_e45	-0.18	(0.23)	-0.26***	(0.05)	-0.30***	(0.05)	-0.28***	(0.05)
d_m43	-0.02	(0.07)	-0.11*	(0.06)	-0.09	(0.06)	-0.08	(0.06)
d_m54	0.11	(0.10)	-0.00	(0.04)	0.01	(0.05)	0.02	(0.04)
d_mvno	-0.02	(0.04)	0.15***	(0.05)	0.14**	(0.06)	0.12**	(0.05)
d_logGDPcap	0.18	(0.58)	-0.19	(0.52)	0.11	(0.51)	0.03	(0.49)
d_logPop	8.88***	(2.59)	8.41***	(2.85)	9.82***	(3.17)	9.21***	(2.82)
d_logExch	0.90	(0.73)	1.12	(0.77)	0.86	(0.77)	0.92	(0.72)
d_inflation	-0.00	(0.01)	-0.00	(0.01)	-0.00	(0.01)	-0.00	(0.01)
d_vat	-0.00	(0.03)	0.00	(0.03)	-0.02	(0.03)	-0.01	(0.03)
d_logPen	-0.41	(0.34)	-0.32	(0.35)	-0.08	(0.39)	-0.21	(0.35)
d_logTerm	-0.23**	(0.11)	-0.04	(0.10)	-0.07	(0.11)	-0.09	(0.10)
Constant	-0.05	(0.07)	-0.04	(0.06)	-0.09	(0.06)	-0.06	(0.06)
Country FE	NO		NO		NO		NO	
Year FE	YES		YES		YES		YES	
Observations	168		168		168		168	
R-squared	0.28		0.28		0.27		0.29	
p-value for H0: error term AR(1)	0.63		0.29		0.39		0.35	

Robust country-clustered standard errors in parentheses
Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

These results already show that the effect separation based on the number of active operators before the event is important. The price-decreasing effects of entries are still to be found, but they turn out to be significant only for the 4-to-5 entries (-25 to -30% on average compared to the price trend of 4-operator countries not affected by additional entry). However, we should be careful to interpret our estimates for 4-to-5 entries as general results as both of them occurred rather specially: the multinational entry in 2004 in Denmark was followed by a merger in a year, while the small entry in 2010 in Poland affected only one year in our sample.

For mergers, we mostly find insignificant price effects and never significantly positive ones. However, we can reject the hypothesis that *x*-to-*y* entries and *y*-to-*x* mergers have the same magnitude effect on prices, but only with the opposite sign (H0: d_e34 + d_m43 = 0 and H0: d_e45 + d_m54 = 0). This hypothesis is usually behind using entry studies as substitutes for merger simulations,[27] but our results provide additional evidence for the critique of this approach.

[27] This element was present, for example, in the assessment of the Ryanair /Aer Lingus merger (COMP/M.4439).

Now we examine whether the type of entrant (big multinational or small local) firm matters for evaluating the effect of entry.

Table 9. Results for regressing log of prices on entry and merger treatments depending on the number of operators before the event and the entrant's type, estimation on first differences

	(1) d_logPlow		(2) d_logPmid		(3) d_logPhigh		(4) d_logPav	
VARIABLES	coef	se	coef	Se	coef	se	coef	se
d_e23big	0.03	(0.13)	0.13	(0.10)	0.11	(0.10)	0.11	(0.10)
d_e34big	0.02	(0.17)	0.03	(0.09)	0.05	(0.05)	0.04	(0.07)
d_e45big	-0.51***	(0.06)	-0.29***	(0.05)	-0.26***	(0.05)	-0.31***	(0.05)
d_e23sm	-0.24	(0.29)	-0.41	(0.28)	-0.39	(0.29)	-0.37	(0.29)
d_e34sm	-0.23***	(0.06)	-0.26***	(0.06)	-0.29*	(0.15)	-0.27***	(0.09)
d_e45sm	0.14**	(0.06)	-0.23***	(0.07)	-0.34***	(0.07)	-0.24***	(0.07)
d_m43	-0.03	(0.07)	-0.11**	(0.06)	-0.09	(0.06)	-0.09	(0.06)
d_m54	0.11	(0.09)	0.00	(0.04)	0.02	(0.05)	0.03	(0.04)
d_mvno	-0.00	(0.05)	0.18***	(0.05)	0.16**	(0.07)	0.14**	(0.06)
d_logGDPcap	0.28	(0.58)	-0.04	(0.50)	0.27	(0.49)	0.17	(0.47)
d_logPop	8.08***	(2.50)	7.46***	(2.71)	8.95***	(3.06)	8.32***	(2.69)
d_logExch	0.65	(0.68)	0.91	(0.72)	0.70	(0.74)	0.73	(0.67)
d_inflation	-0.00	(0.01)	-0.01	(0.01)	-0.00	(0.01)	-0.00	(0.01)
d_vat	-0.00	(0.03)	-0.00	(0.03)	-0.02	(0.03)	-0.01	(0.03)
d_logPen	-0.54*	(0.31)	-0.51	(0.32)	-0.26	(0.37)	-0.39	(0.32)
d_logTerm	-0.21*	(0.11)	-0.03	(0.10)	-0.07	(0.11)	-0.08	(0.10)
Constant	-0.02	(0.07)	-0.03	(0.06)	-0.09	(0.06)	-0.06	(0.06)
Country FE	NO		NO		NO		NO	
Year FE	YES		YES		YES		YES	
Observations	168		168		168		168	
R-squared	0.32		0.32		0.30		0.32	
p-value for H0: error term AR(1)	0.67		0.31		0.21		0.27	

Robust country-clustered standard errors in parentheses
Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

This separation proves to be rather important, as we now see clear differences in the results for multinational and local entries.[28] The point estimates for the price effects of 2-to-3 and 3-to-4 multinational entries are always positive, although never significant. However, we see large price-decreasing effect for local entries, and the parameters are robustly significant for 3-to-4 small entries (-23 to -29% on average compared to the price trend of 3-operator countries not affected by additional local entry). For 3-to-4 entries, we also find that the parameter estimates for multinational and local entries are significantly different from each other.

[28] There is no qualitative change for the effects of mergers from the previous specification, which was to be expected as we did not apply a different separation for merger effects.

So far, we assumed that the effect of any event is the same for all the years after it occurred, which does not necessarily hold. In order to test this hypothesis, let us further separate the price effect of each event for the first year and the second year (short run effects) and jointly for all subsequent years after the third (long run effect), which will be our final specification.

Table 10. Results for regressing log of prices on entry and merger treatments depending on the number of operators and the entrant's type, separated for 1st, 2nd and subsequent years, estimation on first differences

	(1) d_logPlow		(2) d_logPmid		(3) d_logPhigh		(4) d_logPav	
VARIABLES	coef	se	Coef	se	coef	se	coef	se
d_e23big_1	0.03	(0.13)	0.11	(0.10)	0.08	(0.10)	0.08	(0.10)
d_e23big_2	0.13	(0.15)	0.28**	(0.13)	0.32**	(0.15)	0.28**	(0.14)
d_e23big_3	0.10	(0.19)	0.14	(0.14)	-0.06	(0.21)	0.03	(0.17)
d_e34big_1	0.00	(0.14)	0.01	(0.07)	0.03	(0.04)	0.02	(0.06)
d_e34big_2	-0.23	(0.20)	-0.18	(0.12)	-0.20*	(0.12)	-0.20	(0.12)
d_e34big_3	-0.18	(0.22)	-0.17	(0.14)	-0.25	(0.15)	-0.21	(0.15)
d_e45big_1	-0.53***	(0.06)	-0.30***	(0.05)	-0.26***	(0.05)	-0.31***	(0.05)
d_e45big_2	-0.41***	(0.14)	-0.25***	(0.08)	-0.18**	(0.08)	-0.24***	(0.08)
d_e45big_3	-0.00	(0.25)	0.28	(0.33)	0.38	(0.28)	0.29	(0.29)
d_e23sm_1	-0.22	(0.30)	-0.38	(0.29)	-0.34	(0.29)	-0.33	(0.29)
d_e23sm_2	-0.02	(0.35)	-0.28	(0.33)	-0.28	(0.32)	-0.24	(0.32)
d_e23sm_3	0.17	(0.35)	-0.17	(0.34)	-0.24	(0.33)	-0.15	(0.33)
d_e34sm_1	-0.29***	(0.07)	-0.29***	(0.10)	-0.33*	(0.19)	-0.31**	(0.13)
d_e34sm_2	-0.34**	(0.14)	-0.13	(0.13)	-0.02	(0.21)	-0.10	(0.16)
d_e34sm_3	-0.08	(0.19)	0.24	(0.18)	0.37	(0.24)	0.26	(0.19)
d_e45sm_1	0.16**	(0.07)	-0.21***	(0.07)	-0.33***	(0.08)	-0.22***	(0.07)
d_m43_1	-0.19**	(0.08)	-0.02	(0.21)	0.19	(0.13)	0.06	(0.12)
d_m43_2	-0.13	(0.11)	0.08	(0.21)	0.29**	(0.14)	0.15	(0.13)
d_m43_3	-0.11	(0.12)	0.37*	(0.22)	0.39**	(0.16)	0.29**	(0.15)
d_m54_1	0.07	(0.12)	-0.01	(0.05)	0.00	(0.06)	0.01	(0.05)
d_m54_2	-0.20	(0.25)	-0.17	(0.33)	0.04	(0.28)	-0.06	(0.28)
d_m54_3	-0.05	(0.26)	-0.26	(0.39)	-0.22	(0.30)	-0.20	(0.30)
d_mvno	-0.01	(0.06)	0.16**	(0.06)	0.13*	(0.07)	0.12*	(0.06)
d_logGDPcap	0.19	(0.63)	-0.22	(0.54)	0.08	(0.53)	-0.00	(0.50)
d_logPop	8.85***	(2.84)	8.33***	(2.80)	10.75***	(3.05)	9.70***	(2.75)
d_logPen	-0.65**	(0.33)	-0.52	(0.35)	-0.35	(0.35)	-0.46	(0.33)
d_logExch	0.83	(0.81)	1.26	(0.83)	1.11	(0.80)	1.08	(0.75)
d_inflation	-0.01	(0.01)	-0.01	(0.01)	-0.00	(0.01)	-0.01	(0.01)
d_logTerm	-0.17	(0.12)	-0.01	(0.11)	-0.07	(0.12)	-0.07	(0.11)
d_vat	0.00	(0.03)	0.00	(0.03)	-0.01	(0.03)	-0.01	(0.03)
Constant	0.01	(0.08)	-0.01	(0.07)	-0.09	(0.07)	-0.05	(0.06)
Country FE	NO		NO		NO		NO	
Year FE	YES		YES		YES		YES	
Observations	168		168		168		168	
R-squared	0.37		0.38		0.41		0.41	
p-value for H0: error term AR(1)	0.83		0.57		0.95		0.77	

Robust country-clustered standard errors in parentheses
Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Now we see a striking difference compared to our previous results, as now our estimates reveal important insights into the price dynamics of different entry types. Concerning 2-to-3 entries first, the point estimates still signal differences between the effect of multinational and local entries: the previous ones are mostly positive while the latter ones negative. However, the only effect that is statistically different from zero is the second-year effect of the entry of a multinational firm: so even if the presence of 3rd multinational entrant has a price-increasing effect in the short-run, the price trend of a country affected by this type of entry goes back to the price trend of an otherwise similar country not affected by this type of entry. Based on all the specifications, our estimations indicate the insignificant impact of 2-to-3 entries on mobile voice price between 2003 and 2010 (compared to the trend of similar countries not affected by the entry of a 3rd operator), which result is quite robust to separations based on entrant's type and the timing of the effects.

On the other hand, we do have a more refined picture for 3-to-4 entries. The point estimates for a fourth multinational big entrant (which was always Hutchison) indicate price-increasing first-year effects, but price-decreasing second-year and long-run effects, although they are almost never significant. However, if the fourth entrant is a small challenger firm, the effect mechanism is exactly the opposite: small entrants have a robust significant price-decreasing effect only in the first year (-29-33%) but none significant thereafter with the point estimates being even positive on the long-run.

We can also test whether the same year effects are the same for multinational and local entries (H0: e34big_i = e34sm_i for i = 1, 2, 3) and we find that first-year price effect is significantly larger for multinational firms (so their entry was more price-increasing / less price-decreasing on the short-run), while the long-run price effect is significantly larger for local firms. These findings support conventional industry wisdom that local and multinational entrants usually adopt different strategies: local entrants initially price more aggressively in order to quickly gain market share and reach a viable size, while multinational firms can take a more patient approach and the latter can have a more beneficial effect on the long-run.

We have already noted that we should be careful with the interpretation of results on 4-to-5 entries due to only one observation in both categories, which makes it more likely that the estimated parameters will be significant and of large magnitude (as it takes up most of the residuals for specific country-year observations).

There are important changes in the results concerning the effects of mergers as well and different price dynamics are shown for 4-to-3 and 5-to-4 mergers. For the only 4-to-3 merger

in our sample, we do not find robustly significant short-run effects, but we see a price-increase on the long run due to the merger: on average +29% compared to the price trend of 4-operator countries.[29] This result could also indicate why recent 4-to-3 mergers received thorough examination by competition authorities in the last few years and were cleared only after adopting substantial remedies.

For 5-to-4 mergers, we do not find significant effects, but the point estimates are negative on the long-run. This result is in line with general competition policy thinking on merger effects: 5-to-4 mergers were almost never found to be anticompetitive and beneficial effects are rather expected from a consolidation to a 4-player model, which seems one of the focal points to which national mobile markets converge.

6.1 Further discussion of estimation results

In this final part, we discuss some comments that naturally come to mind when looking at our approach.

First, our study analyzes only the price development of mobile voice services. As mobile operators typically offer both voice and data services, it would be beneficial to study the parallel price trends for both services, but this type of data is unfortunately not available. However, the importance of data services increased considerably only by the emergence of smartphones from 2010, and bundling became more widespread thereafter. We might thus expect that entries and mergers had a larger effect on the pricing of voice services during the studied period of 2003-2010, and so a separate study of voice price trends could offer important insights on its own.

Second, we do not have average voice prices for the respective markets, but prices for three OECD defined consumption baskets. It would have been much more useful to work with measures like average voice revenue per minute (ARPM), but this data is publicly available only for a few years (DG InfoSoc published it only for 2009 and 2010). However, the signs of the estimated effects are mostly in line with industry experience, which can give confidence to our qualitative results, but one should be more careful with interpreting the magnitude of significant price effects.

The endogeneity problem between entry and prices was already discussed in Chapter 5.1, one that does not have a perfect solution to tackle. One might also argue that there is already a selection problem in why some countries started with 3 or 4 operators in the early 2000s. One

[29] As the long-run effect is the average yearly effect after the third year of a merger , it is less problematic that we have only one merger to identify this effect from.

way to check whether this presents a problem can be that when we are measuring the effect of 3-to-4 entries, we include only countries with a stable number of 3 operators in the control group. On a conceptual level, the general difference-in-differences approach separately estimates the parameters for 3-to-4 entries from this subsample, but it is true that the parameters for the control variables are estimated from the full sample.

We can successfully perform this approach only for 3-to-4 entries, as those are the ones with a sufficient number of observations. The next table shows the important estimates for these two specifications only for the average price.[30] It turns out that the difference between the estimates on 3-to-4 price effects is minimal, so our results would be almost the same. The only meaningful difference is that in the second approach the long-run beneficial effect of multinational entries is also significant and of larger magnitude, which just strengthens the points we made before.

Table 11. Comparing results on 3-to-4 entries by regressing log of prices on all treatments based on full sample and regressing prices on only 3-to-4 treatments on the subsample of countries starting with 3 operators, with only relevant estimates displayed

	(1) d_logPav		(2) d_logPav	
VARIABLES	Full sample	se	3-operator subsample	se
d_e34big_1	0.02	(0.06)	0.02	(0.06)
d_e34big_2	-0.20	(0.12)	-0.21	(0.14)
d_e34big_3	-0.21	(0.15)	-0.22**	(0.08)
d_e34sm_1	-0.31**	(0.13)	-0.32***	(0.10)
d_e34sm_2	-0.10	(0.16)	-0.01	(0.19)
d_e34sm_3	0.26	(0.19)	0.43*	(0.21)
Country FE	NO		NO	
Year FE	YES		YES	
Observations	168		95	
R-squared	0.41		0.47	
H_0: error term AR(1) (p-value)	0.77		0.66	

We clearly have a selection problem for mergers, as the observed ones were all mergers investigated by European competition authorities. Therefore, we cannot draw the general conclusion that any 5-to-4 merger would have had no effect, only that clearing the mergers in our sample (with or without remedies) was justified by studying ex-post evidence on voice prices.

[30] The full table can be found in Appendix 3.

7. Conclusions

This paper developed an impact assessment method to evaluate the effect of entries and mergers on the price of mobile voice services in a panel database of 27 European Member States between 2003 and 2010. We showed that effects depend very much on the number of active operators and the type of entrant, and not controlling for these differences results in misleading conclusions.

We found no robust evidence for that entry has a long-lasting price-decreasing effect on markets with originally 2 operators relative the trend of decreasing prices without entry, independent from the entrant's type. We showed beneficial effects for the entry of a 4th operator, but different price dynamics according to the entrant's type. After separating entry effects for different years, we found significant price-decreasing effects for local entrants only in the first year after entry, while the price-decreasing effects for multinational entries are present in the long-run. The effects for the entry of 5th operator are significant with the dynamics also dependent on firm's type, but these latter results should be interpreted carefully. Finally, we found a long-run price-increasing effect for the only 4-to-3 merger in our sample, but none for the 5-to-4 mergers.

A main policy conclusion is that regulators should be more careful when incentivizing entry on markets with 3 or 4 active operators which is still one of the main policy issues nowadays, as one cannot mechanically expect a beneficial effect on prices. Naturally, our estimated results are only average effects, so one cannot discard the possibility that some entries of a 4th or 5th operator did have a price-decreasing effect in a country or two even where our estimates were not significant, but the overall European evidence between 2003 and 2010 does not support the "n + 1 should be better" rule. We believe that the experience from this ex-post assessment is useful as similar questions arise with the ongoing spectrum auctions for 4G services, so each regulator must carefully weigh the realistically expected benefits of additional entries when deciding on spectrum allocation rules.

Concerning competition policy, these results provide evidence that the clearance decisions of competition authorities were justified on average in the case of 5-to-4 mergers, as no merger-specific price-increasing effects were found concerning voice services. However, our results indicate that one should be more careful with 4-to-3 mergers as they bear a much higher risk of price increases on the long-run. Note that some merger investigations initially identified competitive concerns, but these were alleviated after remedies were offered by the parties. However, we cannot identify whether the remedies are responsible for the lack of price

increases or whether the mergers in their original forms would have had an at least neutral effect on voice competition.

Let us finally note that the magnitude of the effects identified from our sample of events between 2003 and 2010 cannot be mechanically expected to hold for subsequent entries or mergers from now on. When assessing the impact of future entries in mergers one should more carefully evaluate the changes brought by the deployment of 4G technologies and the convergence between voice and data services. However, our study points out that one cannot mechanically assume similar beneficial effects for all types of entries and harmful or neutral effects for mergers, and not controlling for these differences might lead to misleading policy conclusions.

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Appendix 1 : Summary statistics

Table 12. Detailed summary statistics for variables used in analysis

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
plow	195	14.89	7.209	4.170	39.47
pmid	195	30.52	14.56	6.490	85.87
phigh	195	51.50	26.51	7.820	158.9
pav	195	32.30	15.66	6.160	94.76
gdpcap	214	22,067	14,682	2,400	78,600
pop	208	1.844e+07	2.315e+07	397,296	8.254e+07
exchrate	216	0.761	0.586	0.00357	2.347
infl	216	3.020	2.528	-1.700	15.30
vat	208	19.48	2.521	15	25
pen	197	109.8	21.67	51	170.6
mtr	199	10.28	4.540	1.840	26.83
mvno	216	0.560	0.498	0	1

Table 13. Trends for yearly cross-section averages

Year	2003	2004	2005	2006	2007	2008	2009	2010
plow_av	20.513	22.980	19.531	13.960	12.546	11.818	10.797	10.810
pmid_av	41.900	48.828	40.906	29.178	25.336	24.299	22.202	19.610
phigh_av	71.059	85.744	72.824	48.789	42.745	40.701	35.669	29.005
Pav	44.491	52.517	44.421	30.643	26.876	25.606	22.889	19.808
gdpcap	18900.000	19834.615	20848.148	22266.630	23874.074	24262.926	22685.148	23662.963
pop	1.83e+07	1.84e+07	1.85e+07	1.86e+07	1.83e+07	1.84e+07	1.85e+07	1.86e+07
exchrate	0.768	0.766	0.765	0.766	0.766	0.758	0.749	0.751
infl	2.978	3.196	2.904	3.037	3.326	5.330	1.333	2.059
vat	19.404	19.404	19.244	19.324	19.485	19.485	19.522	19.967
mtr	17.067	14.355	12.578	11.115	9.874	8.275	7.035	5.458
pen	84.067	89.533	99.548	107.392	114.037	120.481	123.333	125.719
mvno	0.296	0.407	0.519	0.556	0.556	0.667	0.704	0.778

Appendix 2 : Estimation results on the level of variables

Table 14. Results for regressing log of prices on the change in number of operators, estimation on levels

	(1) logPlow	(2)	(3) logPmid	(4)	(5) logPhigh	(6)	(7) logPav	(8)
VARIABLES	Coef	se	coef	se	coef	se	coef	se
deltaop	0.06	(0.08)	0.04	(0.08)	-0.01	(0.06)	0.02	(0.07)
mvno	-0.04	(0.06)	0.06	(0.06)	0.09	(0.07)	0.06	(0.06)
logGDPcap	-0.95**	(0.41)	-1.19***	(0.35)	-1.05***	(0.33)	-1.10***	(0.32)
logPop	2.73	(2.02)	3.08	(1.86)	4.12*	(2.03)	3.58*	(1.81)
logExch	1.20*	(0.59)	1.71**	(0.63)	1.67**	(0.71)	1.56**	(0.63)
inflation	-1.74***	(0.56)	-1.50***	(0.36)	-1.27***	(0.38)	-1.42***	(0.36)
vat	0.04	(0.03)	0.06**	(0.02)	0.04	(0.03)	0.04*	(0.02)
logPen	-0.44	(0.35)	-0.40	(0.40)	-0.19	(0.42)	-0.29	(0.39)
logTerm	-0.02	(0.10)	0.03	(0.11)	-0.05	(0.11)	-0.02	(0.10)
Constant	-27.07	(35.57)	-30.02	(31.70)	-47.90	(34.14)	-38.95	(30.93)
Country FE	YES		YES		YES		YES	
Year FE	YES		YES		YES		YES	
Observations	195		195		195		195	
Within R-squared	0.75		0.78		0.80		0.81	
p-value for H_0: error AR(1)	0.00		0.01		0.00		0.00	

Robust country-clustered standard errors in parentheses
Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Table 15. Results for regressing log of prices on entry and merger treatments depending on the number of operators and the entrant's type, separated for 1st, 2nd and subsequent years, estimation on levels

	(1) logPlow	(2)	(3) logPmid	(4)	(5) logPhigh	(6)	(7) logPav	(8)
VARIABLES	coef	se	coef	se	coef	se	coef	se
e23big_1	0.01	(0.15)	0.04	(0.13)	0.09	(0.13)	0.06	(0.13)
e23big_2	0.18	(0.15)	0.27**	(0.10)	0.39***	(0.09)	0.33***	(0.10)
e23big_3	0.01	(0.14)	0.08	(0.09)	-0.04	(0.12)	0.01	(0.10)
e34big_1	0.09	(0.16)	0.04	(0.07)	0.04	(0.06)	0.05	(0.08)
e34big_2	-0.11	(0.28)	-0.23	(0.17)	-0.27	(0.16)	-0.23	(0.18)
e34big_3	-0.07	(0.14)	-0.40***	(0.09)	-0.49***	(0.09)	-0.40***	(0.07)
e45big_1	-0.59***	(0.06)	-0.32***	(0.05)	-0.28***	(0.05)	-0.34***	(0.04)
e45big_2	-0.45**	(0.20)	-0.22*	(0.11)	-0.11	(0.15)	-0.20	(0.13)
e45big_3	-0.08	(0.14)	0.51***	(0.11)	0.68***	(0.08)	0.50***	(0.08)
e23sm_1	-0.14	(0.25)	-0.29	(0.22)	-0.32	(0.21)	-0.28	(0.22)
e23sm_2	0.16	(0.16)	-0.11	(0.14)	-0.17	(0.16)	-0.09	(0.14)
e23sm_3	0.21	(0.20)	0.03	(0.15)	-0.00	(0.21)	0.04	(0.17)
e34sm_1	-0.28***	(0.09)	-0.32**	(0.15)	-0.32	(0.23)	-0.31*	(0.18)
e34sm_2	-0.23	(0.21)	-0.19	(0.13)	-0.20	(0.14)	-0.19	(0.13)
e34sm_3	-0.22	(0.17)	-0.03	(0.13)	0.01	(0.13)	-0.02	(0.13)
e45sm_1	0.27***	(0.06)	-0.10	(0.07)	-0.21**	(0.08)	-0.11	(0.07)
m43_1	-0.34***	(0.06)	-0.22**	(0.10)	0.14	(0.13)	-0.05	(0.10)
m43_2	-0.34***	(0.07)	-0.19**	(0.09)	0.19	(0.12)	-0.02	(0.09)
m43_3	-0.30***	(0.08)	0.12	(0.11)	0.29*	(0.14)	0.13	(0.11)
m54_1	-0.04	(0.21)	-0.15	(0.09)	-0.18	(0.13)	-0.14	(0.12)
m54_2	-0.21	(0.24)	-0.34	(0.26)	-0.18	(0.16)	-0.23	(0.20)
m54_3	-0.20	(0.14)	-0.50***	(0.10)	-0.54***	(0.09)	-0.46***	(0.08)
mvno	-0.04	(0.07)	0.09	(0.06)	0.12*	(0.07)	0.08	(0.06)
logGDPcap	-0.81*	(0.46)	-1.07***	(0.33)	-0.93***	(0.33)	-0.99***	(0.32)
logPop	3.72	(2.52)	5.89***	(1.87)	7.59***	(2.18)	6.46***	(1.98)
logPen	-0.55	(0.34)	-0.47	(0.40)	-0.37	(0.41)	-0.42	(0.38)
logExch	1.23*	(0.69)	1.59**	(0.62)	1.55**	(0.69)	1.48**	(0.63)
inflation	-2.17**	(0.81)	-1.74***	(0.47)	-1.49***	(0.52)	-1.67***	(0.50)
logTerm	-0.06	(0.11)	-0.08	(0.09)	-0.17*	(0.10)	-0.12	(0.09)
vat	0.04	(0.03)	0.06**	(0.02)	0.04	(0.03)	0.05*	(0.03)
Constant	-43.14	(42.67)	-75.28**	(31.77)	-103.33**	(36.06)	-85.06**	(33.23)
Country FE	YES		YES		YES		YES	
Year FE	YES		YES		YES		YES	
Observations	195		195		195		195	
Within R-squared	0.80		0.85		0.86		0.87	
Number of country	27		27		27		27	
H_0: error term AR(1) (p-value)	0.00		0.01		0.00		0.00	

Appendix 3

Table 16. Comparing results on 3-to-4 entries by regressing log of prices on all treatments based on full sample and regressing prices on only 3-to-4 treatments on the subsample of countries starting with 3 operators, all estimates displayed

	(1) d_logPav		(2) d_logPav	
VARIABLES	Full sample	se	3-operator subsample	se
d_e23big_1	0.08	(0.10)		
d_e23big_2	0.28**	(0.14)		
d_e23big_3	0.03	(0.17)		
d_e34big_1	0.02	(0.06)	0.02	(0.06)
d_e34big_2	-0.20	(0.12)	-0.21	(0.14)
d_e34big_3	-0.21	(0.15)	-0.22**	(0.08)
d_e45big_1	-0.31***	(0.05)		
d_e45big_2	-0.24***	(0.08)		
d_e45big_3	0.29	(0.29)		
d_e23sm_1	-0.33	(0.29)		
d_e23sm_2	-0.24	(0.32)		
d_e23sm_3	-0.15	(0.33)		
d_e34sm_1	-0.31**	(0.13)	-0.32***	(0.10)
d_e34sm_2	-0.10	(0.16)	-0.01	(0.19)
d_e34sm_3	0.26	(0.19)	0.43*	(0.21)
d_e45sm_1	-0.22***	(0.07)		
d_m43_1	0.06	(0.12)		
d_m43_2	0.15	(0.13)		
d_m43_3	0.29**	(0.15)		
d_m54_1	0.01	(0.05)		
d_m54_2	-0.06	(0.28)		
d_m54_3	-0.20	(0.30)		
d_mvno	0.12*	(0.06)	0.08	(0.07)
d_logGDPcap	-0.00	(0.50)	-0.75	(0.58)
d_logPop	9.70***	(2.75)	11.29***	(3.67)
d_logPen	-0.46	(0.33)	-0.41	(0.53)
d_logExch	1.08	(0.75)	1.60*	(0.91)
d_inflation	-0.01	(0.01)	0.01	(0.01)
d_logTerm	-0.07	(0.11)	0.15	(0.17)
d_vat	-0.01	(0.03)	-0.01	(0.02)
Constant	-0.05	(0.06)	-0.05	(0.06)
Country FE	NO		NO	
Year FE	YES		YES	
Observations	168		95	
R-squared	0.41		0.47	
H_0: error term AR(1) (p-value)	0.77		0.66	