Technical Annexes: Market structure, Investment and Quality in the Mobile Industry

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A1. Empirical studies on the linkage between market structure, investment & quality

Studies on the linkage between changes in market concentration and investment

Several studies have considered the relationship between changes in mobile market concentration and investment

A1.1 Given the wave of mergers in mobile markets between 2012 and 2016, several studies have attempted to analyse the impact of changes in mobile market concentration on investment. Table A1.1 summarises the main empirical studies conducted.

Table A1.1: Summary of empirical studies on the linkage between market concentration and investment

With fewer networks:	Frontier Economics (2015)[1]	WIK Consult (2015)[2]	Jeanjean and Houngbonon (2017)[3]	Genakos et al. (2018)[4]	GSMA (2020)[5, 6]
Study period	2000 – 14	2005 – 13	2006 – 15	2006 – 14	2011 – 18
More concentration leads to: • MNO investment	—	—	▲	▲	— (number of MNOs and HHI as market concentration measure) ▲ (Lerner index as market concentration measure)
• MNO investment/ subscriber	—	n/a	n/a	n/a	n/a
• Industry investment	—	—	▼ short- run ▲ long-run	—	n/a

MNO: *mobile network operator;* ***n/a:*** *No results were obtainable using the method*

— *Not statistically significant;* ▲ *positive relationship;* ▼ *negative relationship*

These studies provide limited insight to the consumer impact of mobile consolidation

A1.2 All of the studies summarised above focus on the impact of consolidation on investment. As a general note, we do not consider such studies by themselves capable of providing much insight to the question of consumer outcomes. For the reasons explained in Section 3, an increase in investment does not necessarily mean that consumer outcomes will improve.

A1.3 This is particularly true when looking at mobile network operator (MNO)-level data on investment, as investment of the merged MNO will inevitably increase when two MNOs merge. [7] This is because the combined capital expenditure of the merged firm is larger than

[1] Assessing the case for in-country mobile consolidation: A report prepared for the GSMA, Frontier Economics, May 2015. Available at https://www.gsma.com/publicpolicy/wp-content/uploads/2015/05/Assessing_the_case_for_in-country_mobile_consolidation.pdf

[2] Elixmann, D., Godlovitch, I., Henseler-Unger, I., Schwab, R., Stumpf, U., 2015. Competition & investment: An analysis of the drivers of investment and consumer welfare in mobile telecommunications

[3] Francois Jeanjean, Georges V Houngbonon. Market structure and investment in the mobile industry. 2017. Information Economics and Policy, 38, p.12-22.

[4] Genakos, C., Valleti, T., Verboven, F. (2018): Evaluating market consolidation in mobile communications. Economic Policy, 33(93): 45-100.

[5] Mobile market structure and performance in Europe: Lessons from the 4G era, February 2020. Available at https://data.gsmaintelligence.com/research/research/research-2020/mobile-market-structure-and-performance-in-europe-lessons-from-the-4g-era

[6] The GSMA considered different measures of concentration to account for mobile concentration. In particular, they considered the number of MNOs, the Herfindahl-Hirschman Index (HHI) and EBITDA margins as an approximation of the Lerner Index. Only the specification using EBITDA margins as an approximation of the Lerner Index was found to have statistically significant results. However, the positive impact of EBITDA on investment could be reflecting the fact that MNOs with larger EBITDA margins tend to be larger in size (i.e. incumbent MNOs tend to have larger margins) and thus haver higher investment requirements.

[7] In the sense of being greater than average operator-level investment pre-merger: though potentially less than the sum of the two merging parties' pre-merger investment levels, reflecting merger synergies.

the average capital expenditure of each of the merging firms pre-merger: unless the merged firm cuts back dramatically on investment. In other words, finding a positive impact of a merger on MNO investment may just reflect that, in more concentrated markets, each individual network is larger on average. As a result, we do not consider the studies by Jeanjean and Houngbonon (2017) and Genakos et al. (2018), which find that with fewer networks there is increased MNO level investment, can be interpreted as evidence that consumer outcomes improve with consolidation. The same limitation applies to the GSMA (2020) model of MNO-level investment, where we consider no inference can be drawn from their finding of no statistically significant effect from consolidation.

A1.4 For similar reasons, we do not consider the GSMA's findings (2020) of a significant relationship between MNO investment and the EBITDA margin (as a proxy for the Lerner Index, i.e. market power) to be evidence of consumer benefits from consolidation. Again, their results could be explained by MNOs in more concentrated markets generally earning higher margins while, at the same time, requiring more investment due to their larger-size networks.

A1.5 This critique does not apply to the same extent to studies that control for number of subscribers or that look at industry-level investment. However, we still find significant limitations with these studies that mean we do not draw any inference from their findings. In relation to Jeanjean and Houngbonon (2017), we note their results are a construct of the regression specification and parameters that the authors assume. As we do not consider these assumptions fully capture the dynamics of the investment process, we do not place weight on their findings.[8]

A1.6 In relation to Frontier (2015), we note the relatively wide confidence intervals around their estimated effects means it is difficult to draw robust conclusions from their analysis. We also disagree with their inference that finding no significant difference in the level of investment per subscriber between three and four player countries means there is necessarily more investment in capacity in three player countries (than in four player countries, where they argue there will be more duplication of coverage). We agree this is a possibility, which is why we have focussed on our quality analysis in our own study: but we disagree this result automatically follows, hence why we consider it important to test the proposition with data.

A1.7 We consider the study conducted by WIK Consult (2015) suffers significant data limitations as well as some methodological issues. In particular, the combination of a very short time series (comprising only 9 observations) and the inclusion of highly collinear variables (e.g. HHI and EBITDA) makes it unsurprising they found no statistically significant effect. We also note they did not attempt to control for potentially endogenous variables such as HHI and EBITDA.

[8] The authors assume a first order autoregressive model (i.e. a single lag for investment), and their findings are in part dependent on this assumption. They do not provide evidence to support their assumption and we note that our own analysis suggests significantly more lags should be included to capture fully the dynamics of the investment process.

A1.8 Finally, we note that Genakos et al. (2018) consider their country-level results are not conclusive. The authors find no statistically significant relationship between country-level investment and changes in market concentration, but consider this is due to imprecision in their estimates and so do not draw any inference from their results.[9]

A1.9 Overall, we consider the existing evidence is unclear regarding the relationship between mobile consolidation and investment.

Studies on the impact of increased concentration in the mobile market on quality

The GSMA has looked at the causal effect of mobile concentration on quality

A1.10 As we explain in Section 3, looking at the impact of consolidation on quality gives more direct insight into the impact of mergers on consumer outcomes. WIK (2015) considers the link between investment and quality, but does not control for potentially confounding factors which may affect quality outcomes in its analysis and so we do not place weight on these findings. Only the GSMA has previously looked at this relationship using statistical techniques which may potentially control for these confounding factors. Although the GSMA consider both reports lend support to the view that consolidation improves quality outcomes for consumers, we disagree given the limitations we find with their approach in both reports.

A1.11 In 2017, the GSMA analysed the impact of the 2012 Hutchison/Orange merger in Austria (a four-to-three merger) on quality using quantitative techniques (specifically using difference in difference (DD) and synthetic control methods).[10] They estimate results for both MNO and industry level data, using a range of measures for quality.[11] Based on these results, the GSMA concluded that the merger in Austria had a positive and statistically significant effect on quality outcomes.

A1.12 In 2020, the GSMA published another study. This considered the impact of changes in mobile market concentration on quality using panel data techniques (specifically pooled OLS) and applied these to MNO-level data for the period 2011 to 2018.[12] The GSMA considers this analysis shows that markets with more concentrated structures were able to deploy 4G more quickly and were better at delivering higher performances.

[9] The authors do note that finding an insignificant impact of consolidation on country-level investment suggests that consolidation gives rise to fixed cost savings and possibly benefits from coordinating investment: as theoretical studies find that in the absence of efficiencies, consolidation will lead to a reduction in industry investment. However, we consider no inference can be drawn in this case from the lack of statistical significance, which due to the noise in their dataset and lack of sufficient variability across countries and over time could reflect imprecision in their estimates rather than a lack of true effect.

[10] Assessing the impact of mobile consolidation on innovation and quality: An evaluation of the Hutchison/Orange merger in Austria, 2017. Available at https://www.gsma.com/publicpolicy/resources/evaluation-hutchison-orange-merger-austria

[11] Specifically, 4G coverage, 4G download speeds, 4G upload speeds, 3G download speeds and 3G upload speeds.

[12] Reference at footnote 5.

We consider there are significant limitations with the GSMA studies

A1.13 In both cases, we consider there are significant limitations with the approach and data which mean we do not place weight on their findings.

A1.14 In relation to the 2017 study, we note that a fundamental issue relates to the lack of appropriate pre-merger data. This is critical because without a good picture of download speeds in Austria pre-merger, it is not possible to estimate the effect that the merger had on these speeds using either the DD or synthetic counterfactual approach. This limitation applies to both their MNO and country-level results.

A1.15 In relation to MNO-level data, the GSMA did not have access to any pre-merger data on 4G download and upload speeds for Hutchison. They do not attempt synthetic control analysis of MNO-level data for this reason but do present results of what they say is a DD analysis. Strictly speaking, it is not a DD analysis (which should subtract the difference in outcomes for the control group from the difference in outcomes for the treated group): as they do not have the data to calculate the difference in outcomes pre- and post-merger for the treated group. Instead, what they estimate are MNO and country fixed effects indicating that Austria had greater download and upload speeds than the control group.[13] This finding provides no evidence these faster speeds were linked to the merger as Austria may have had faster speeds pre-merger too.

A1.16 The national-level data used by the GSMA also has serious limitations. Not only do they have limited pre-merger data (four quarters of data based on a single MNO, A1), but they also change the way they measure quality for the post-merger period (from download speeds for A1 only to the average speeds of all Austrian MNOs). This is a limited amount of pre-merger data to rely on and assumes that A1's download speeds are representative of all Austrian MNOs. This is a strong assumption to make because whilst A1 is the largest MNO in Austria, it represents less than half of the market. Moreover, changing the unit of measurement for the post-merger period means any estimated effect of the merger may simply reflect the fact they are not comparing like with like. This approach may be valid if the excluded MNOs, T-Mobile, and Hutchison had speeds pre-merger that broadly average out to the same as A1 in each period. It is unclear however whether this is a reasonable assumption, and the contrasting results of Hutchison compared to other national MNOs in Austria may suggest otherwise.

[13] The GSMA appears to recognise this in some parts of their report. For example, in A.3.2.1, p.21, they note: "Due to the lack of pre-merger data on 4G performance for Hutchison, the only model that we can estimate is the Base specification, for which the Parallel Trends assumption cannot be directly inspected. In this case, the DD framework cannot be estimated using the standard approach, since the double difference cannot be calculated. … These merger coefficients represent, in effect, an operator fixed effect for Hutchison. These capture whether the merged entity had better or worse network performance than would have been expected by assessing other operators' network performance and controlling for other distinctive factors between the treatment and the control…"

A1.17 Again fundamentally, we do not consider it appropriate to apply DD analysis to assess the impact of individual mergers – see discussion in Annex A5. In summary, this is because when we only observe one treated unit in the treatment group (i.e. one merger):

i) the common trends assumption required for DD Is unlikely to hold (and it is also not possible to properly test whether it does – see below on standard errors);

ii) equally weighting all countries in the control group is unlikely to provide a good comparison for the single treated unit affected by the intervention; and

iii) it is not possible to test whether any estimated effect is statistically significant (because there is no variation within the treated group to generate standard errors).

A1.18 Finally, while Synthetic control (SC) methods are better suited to this type of question, the GSMA were unable to use this method for most specifications.[14] Where it was possible, they conclude that the merger had a positive effect on 4G coverage for Austria as a whole. However, we note that the study does not account for 4G coverage obligations which are widely applied during the spectrum auctions at the time. These, by definition, will drive the level of 4G coverage and so we do not place weight on these results.

A1.19 Likewise, we have concerns with the GSMA 2020 panel data study. Firstly, the use of country dummies to control for unobserved fixed effects, rather than using the fixed effects transformation, has the drawback of creating collinearity issues between the market concentration measure and the country dummies. Given market concentration is relatively fixed for most countries in the GSMA's sample (only eight countries had a change in number of MNOs over the study period), there is likely to be a high degree of collinearity between the market concentration measures and country dummies (particularly in the case of the number of MNOs specification, which also relies on a dummy variable to capture differences in market concentration).[15] This collinearity is likely to lead to less reliable results, as the country dummies are likely to pick up some of the effects of market concentration on quality (and vice versa).

A1.20 Secondly, their model does not include controls for technology cycles and lagged investment, which we consider to be important drivers of quality and also likely to be related to the measures of concentration used in their models. We consider that this is likely to have biased their estimates for the reasons explained in more detail in Section 3.

A1.21 Thirdly, a key result the GSMA uses to support its findings is a positive and significant 'incremental' effect of market concentration on download speeds once an MNO reaches 90% 4G coverage. However, the estimated coefficient on this incremental effect is not meaningful by itself. What matters for the overall effect of market concentration after 90% coverage is achieved (in the context of their model) is the cumulative impact of two

[14] This is because of a lack of pre-merger data.

[15] To verify this, we ran an OLS regression whereby the measure of market concentration is the dependent variable and the country dummies are the explanatory variables. For number of MNOs the R square we obtain from running the regression is nearly 0.6 and for HHI is 0.5, which is indicative of a strong relationship between the market concentration measure and the country dummies. The coefficient of the country dummies are all statistically significant (except one) in both instances.

effects: (i) the average effect of consolidation on quality and (ii) the incremental effect of consolidation after reaching 90% 4G coverage. Although the combined impact of the GSMA's estimates is positive, it looks unlikely to be significant given the variance in both estimates.[16] Moreover, when we attempted to replicate the GSMA's results, we not only found the cumulative impact to be insignificant but we also found a significant and negative average effect of consolidation on 4G download speeds.

A1.22 Finally, we think the instruments used for endogenous variables are unlikely to be valid:

a) *3G spectrum holdings*. We consider there are two potential issues with this instrument.

i) The instrument is likely to place most weight on those countries which did not experience significant changes in market structure, when we are primarily interested in the outcomes in countries which did see significant changes. This is because some of the most significant changes in HHI during the period studied (2011 to 2018) arose when 4G auctions allowed for new market entry. Holdings of 3G spectrum will be a poor predictor for these changes in HHI. Instead, 3G spectrum holdings are likely to be more closely correlated with HHI during the 4G era for those countries in which there were no significant changes in market structure. This is likely to mean the instrument places less weight on countries which did experience structural changes vis-à-vis those which did not, with the effect that results are not that informative to the question of interest.

ii) Separately, we consider the instrument is likely to be invalid because it will be correlated with the error term. This is because the share of 3G spectrum holdings is likely to be correlated with the share of 4G spectrum holdings (e.g. a market with fewer MNOs is likely to have more concentrated holdings of both 3G and 4G spectrum). The share of 4G spectrum is likely to affect quality (as measured by 4G download speeds) but is not included in the model, so will be captured by the error term. This may induce a correlation between 3G spectrum holdings and the error term, making the instrument invalid.

b) *Time since entry*. We expect time since entry to be correlated with the MNO's EBITDA margin. This is because incumbent MNOs (which have been active in the market for longer time) are likely to enjoy economies of scale and first mover advantages and thus earn higher margins in comparison to entrant MNOs. However, time since entry is also likely to be linked to other factors that may explain network quality, such as the stock of capital – i.e. incumbent MNOs are more likely to have a larger stock of capital given cumulated investment over time, and this may influence network quality. Given that stock of capital is omitted from the analysis and will therefore be captured by the error term, time since entry is likely to be correlated with the error term and thus an invalid instrument for the EBITDA margin.

[16] It is not possible to test this statistically as the standard error of the cumulative impact is a combination of the standard errors of the two individual impacts, which the GSMA does not report.

A1.23 The issues with the GSMA studies identified above mean that causal inference about the impact of changes in market concentration on quality is problematic and therefore the results of these studies need to be treated with caution.

A2. Data

A2.1 To investigate the impact of changes in market concentration on investment and quality outcomes in the mobile industry, we construct a panel consisting of quarterly data from 2000 to 2018 for MNOs operating in 30 European countries.

A2.2 In this annex, we describe the data and the sources that we have used to construct this panel. We also set out the modifications that we have made to the original data.

Panel data used in our analysis

To investigate the impact of market concentration on investment and quality we use quarterly data from GSMA across 30 European countries

A2.3 As set out in Section 5, we use panel data and counterfactual analysis techniques to assess the impact of changes in market concentration on investment and quality outcomes in the mobile sector. To conduct this analysis, we require data on investment and quality outcomes, and on the factors affecting these outcomes, for multiple countries and time periods.[17]

A2.4 We have sourced our data primarily from GSMA Intelligence. This dataset offers quarterly data on a wide range of metrics for an extensive number of MNOs worldwide, and has been used by previous studies looking at the impact of mobile concentration on investment and quality (see Annex A1).

A2.5 We limit the panel of countries to Europe as we consider that MNOs operating in markets within Europe are more likely to face similar regulatory conditions. For example, the regulatory framework which the mobile sector is subject to is relatively harmonised across European countries, but this can vary significantly outside Europe. This is consistent with the approach taken by previous studies.[18]

A2.6 We have made a number of exclusions and modifications to the GSMA Intelligence dataset.

a) we exclude mobile virtual network operators (MVNO)s as they do not typically invest in the network and instead use the network of a MNO;

b) we also exclude MNOs that solely provide mobile broadband services and/or provide these services over frequencies that are not designated for 3G/4G services (e.g. 450 MHz band); and

c) given the focus of our study on country-level effects, we also exclude MNOs that are only active in parts of the country, such as islands or small regions within a country.

[17] Panel data techniques exploit variation across time and across panels (i.e. countries) to assess the average impact of market concentration on investment and quality outcomes. Counterfactual analysis requires building a control group as a proxy to determine the counterfactual outcome had the merger not taken place.

[18] For example, both the GSMA (2017) and GSMA (2020) studies, as well as the joint report written for the European Commission, the Dutch and the Austrian regulators (2015) take a similarly wide approach to constructing their panels.

A2.7 For full transparency, the operators that we have excluded from our analysis are listed in Table A2.2.

A2.8 In addition to excluding some operators, we have also limited the dataset to the period from 2000 to 2018. This is the period for which investment data is available and over which 3G and 4G networks were built throughout Europe. Therefore, our final panel dataset is comprised of observations for 93 MNOs across 30 countries (listed in Table A2.1) and for the period from 2000 to 2018.

Table A2.1: List of countries included in the sample

Countries included in our dataset		
Austria	Germany	Norway
Belgium	Greece	Poland
Bulgaria	Hungary	Portugal
Croatia	Ireland	Romania
Cyprus	Italy	Slovakia
Czech Republic	Latvia	Slovenia
Denmark	Lithuania	Spain
Estonia	Luxembourg	Sweden
Finland	Malta	Switzerland
France	Netherlands	United Kingdom

Table A2.2: List of operators excluded from the sample

Operator	Country	Reason
Telsim	Cyprus	Turkey/Northern Cyprus MNO
Turkcell	Cyprus	Turkey/Northern Cyprus MNO
Nordic Telecom	Czech Republic	450 MHz, mobile broadband
Net1	Denmark	450 MHz, mobile broadband
KOU	Estonia	450 MHz
Tallinn Mobile	Estonia	Never launched
Ukko Mobile	Finland	Covers only Åland Islands
NetCologne	Germany	Covers only Cologne region. Primarily fixed provider, MVNO.
MVM Net	Hungary	450 MHz
MEZON	Lithuania	Mobile broadband
UAB Comliet	Lithuania	Infrastructure provider
LOL Mobile	Luxembourg	MVNO
Ziggo	Netherlands	Cable operator, MVNO
Aero2	Poland	Mobile broadband, infrastructure provider
Aero2 (Midas)	Poland	Mobile broadband, infrastructure provider
Nordisk Polska	Poland	450 MHz
Zapp	Portugal	Mobile broadband
Net 1	Sweden	450 MHz, mobile broadband
Spring Mobil	Sweden	Business market
Relish	United Kingdom	Mobile broadband

Our data captures a number of changes in market structure

A2.9 There were a number of changes in market structure over the period of our study. Table A2.3 shows these changes, which include merger, entry and exit events.

Table A2.3 – Structural changes in market concentration in our data

Country	Year	Structural change	Merger status	Impact on MNO market structure
Austria	2000	Entrant – Three		3 to 4
	2003	Entrant – Tele.ring		4 to 5
	2006	Merger – T-Mobile/Tele.ring		5 to 4
	2012	Merger – Three/Orange	Cleared conditionally	4 to 3
Bulgaria	2001	Entrant – Telenor		2 to 3
Croatia	2005	Entrant – Tele2		2 to 3
Cyprus	2004	Entrant – MTN		1 to 2
	2015	Entrant – PrimeTel	N/A	2 to 3
Denmark	2003	Entrant – Three		4 to 5
	2004	Merger – Telia/Orange		5 to 4
Finland	2001	Entrant – DNA		2 to 3
France	2012	Entrant – Free Mobile	N/A	3 to 4
Germany	2014	Merger – Telefónica/E-Plus	Cleared conditionally	4 to 3
Greece	2002	Entrant – Q Telecom		3 to 4
	2007	Merger – WIND/Q Telecom		4 to 3
Ireland	2001	Entrant – Eircom		2 to 3
	2005	Entrant – Three		3 to 4
	2014	Merger – Three/ Telefónica	Cleared conditionally	4 to 3
Italy	2003	Entrant – Three		3 to 4
	2016	Merger – Three/WIND	Cleared conditionally	4 to 3 – but new entrant established
	2018	Entrant – Iliad	N/A	3 to 4
Latvia	2005	Entrant – Bite		2 to 3
	2005	Entrant – Triatel		3 to 4
Luxembourg	2004	Entrant – Orange		2 to 3
	2014	Entrant – Join experience		3 to 4
Malta	2000	Entrant – Go		1 to 2
	2009	Entrant – Melita Mobile		2 to 3
Netherlands	2005	Merger – KPN/Telfort		5 to 4
	2007	Merger – T-Mobile/Orange		4 to 3
	2015	Entrant – Tele2		3 to 4
Norway	2008	Entrant – Network Norway		2 to 3

Poland	2007	Entrant – Play		3 to 4
Romania	2007	Entrant – Digi Mobil		4 to 5
	2009	Merger – OTE/Zapp		5 to 4
Slovakia	2007	Entrant – O2		2 to 3
	2015	Entrant – 4ka	N/A	3 to 4
Slovenia	2001	Entrant – Vega		2 to 3
	2006	Exit – Vega		3 to 2
	2007	Entrant – Telemach		2 to 3
	2008	Entrant – T-2		3 to 4
Spain	2006	Entrant – Yoigo		3 to 4
Sweden	2003	Entrant – Three		3 to 4
Switzerland	2007	Entrant – Tele2		3 to 4
	2008	Merger – Sunrise/Tele2		4 to 3
UK	2003	Entrant – Three		4 to 5
	2010	Merger – T-Mobile/Orange		5 to 4

Source: Ofcom research

Some of our analysis conducted restricts the sample size

A2.10 In our panel analysis, we use the whole of our data set. However, in our counterfactual analysis, because we are constructing a control group to measure how a market would have developed absent the merger, depending on the merger and when it took place, the size of the sample used in the analysis differs. This is discussed in the results annexes - see Annexes A6 to A8.

Key variables used in our study

Measures of investment used in our study

A2.11 In Section 3 of the main report we set out the hypotheses that we test in relation to the impact of market concentration on investment and quality. One of these hypotheses is the indirect impact that changes in market concentration have on network quality by changing MNOs' ability and incentives to invest.

A2.12 To test this hypothesis, we use capex data sourced from the GSMA Intelligence database. As already mentioned, this database offers a comprehensive MNO dataset for the period under analysis. Although capex data is not always available for all MNOs operating within the sampled countries, this is available at an aggregate country-level.

A2.13 The GSMA defines capex as the total capital expenditure incurred in the period (quarter), including both intangible and tangible assets. This mostly excludes expenditure in acquiring spectrum rights. In analysing the data, however, we observed, for a few countries, spikes in certain quarters that could only be explained by the inclusion of spectrum auction prices. Although some MNOs included their auction spending as capital expenditure, most did not. Thus, where possible, we corrected for this inconsistency in the data by manually removing spectrum auction prices using Analysys Mason's spectrum auction tracker. Removing spectrum expenditure from the capex data allows us to obtain a consistent measure of investment across our panel and test the indirect mechanism by which changes in market concentration may impact network quality by affecting MNOs' ability and incentives to invest.[19]

A2.14 Table A2.4 below lists the spectrum auctions that we identified as being present in the database and manually corrected for.

Table A2.4 – List of spectrum auctions removed from capex data

Country	Operator	Date	Auction price (€m)
Austria	T-Mobile	2013q4	654
	A1	2013q4	1,030
Czech Republic	T-Mobile	2014q1	94
Estonia	Tele2	2014q1	5
France	SFR	2011q1	300
	SFR	2012q1	1,070
Germany	O2	2010q2	1,379
Hungary	Magyar	2012q1	35
	Magyar	2014q4	187
Ireland	Eir	2012q3 - 2013q2[20]	100
Italy	Wind	2011q4	1,120
Netherlands	T-Mobile	2013q1	911
Portugal	NOS	2011q4	113
Slovenia	A1	2014q2	64

A2.15 In conducting our panel data and counterfactual analysis, we have taken a slightly different approach in using this capex data. While the panel data analysis uses industry capex per capita as the investment measure, the counterfactual analysis uses industry capex.

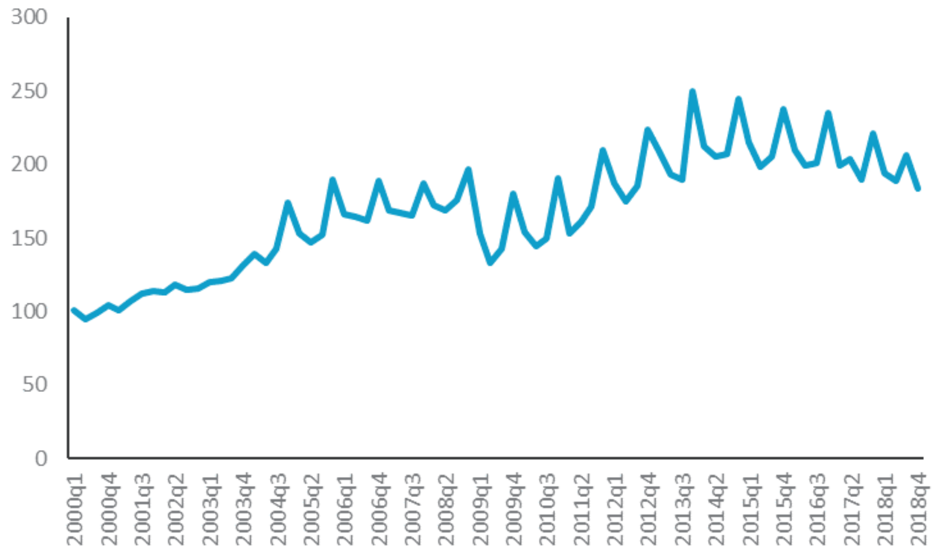
A2.16 The reason for this difference in approach is because our panel data analysis spans a 19-year time horizon where population size varied. As explained in Section 3, population size

[19] Although changes in market concentration may also affect network quality by changing the way that spectrum is utilised (see Section 3 of the main report), we test this via a separate direct link as set out in Section 5.
[20] The spectrum auction spend in the data was spread across the financial year.

can be a driver of investment and can be linked to market concentration as larger countries are more likely to support a larger number of competing MNOs. To control for this in our panel data analysis, we therefore use industry capex per capita as the investment measure. In contrast, our counterfactual analysis considers a shorter time period (i.e. a period of 8-9 years pre- and post-merger) because the aim of it is to estimate the counterfactual investment outcomes in a country that experienced an actual merger. Given the shorter time period being analysed, there is much less variation in population size within each of the countries considered over that time span.[21]

A2.17 Looking at the 19-year time horizon, the country-level capex in our data ranges from €1.3m to €1,360m, with a mean average of €169m and standard deviation of €247m. For illustration, Figure A2.1 shows the weighted average country-level capex across our sample (weighted by number of mobile connections). It shows a slight general upward trend in the level of capex, with seasonal spikes every fourth quarter. Note there is a dip in 2008-09 when the financial crisis took place. In addition, capex appears higher during the 4G investment cycle (2010-2018) than during the 3G cycle (2000-2009).[22]

Figure A2.1: Weighted average country-level capex across the sample (€ millions)



Source: Ofcom based on GSMA Intelligence dataset. Country data weighted by number of mobile connections.

Measure of network quality used in our analysis

A2.18 In investigating the relationship between market concentration and network quality we recognise that there are various dimensions to network quality. In particular, network

[21] Note that in our counterfactual analysis we also use an indexed version of industry capex where investment is set to a base level at the time of the merger studied. We do this in our synthetic control analysis, whenever we are unable to create a weighted average of the countries that did not have a merger that matches the chosen pre-treatment characteristics of the country with the merger.

[22] The first 4G rollouts started in Sweden in late 2009.

quality comprises multiple aspects which affect consumer experience, including download speeds, upload speeds, latency, network coverage and service reliability.

A2.19 Latency is the measure of time delay between data transmission from one device to another. In terms of a consumer's experience, this is the amount of time it takes between a user inputting a command and the action itself taking place. This is a particularly important measure of network quality for services that require short response times, for example video calls, gaming, and in the future, self-driving cars. We cannot however analyse the potential effect of mergers on latency as we did not have access to panel data on network latency.

A2.20 Data on network coverage is also limited. To provide network coverage, MNOs use different networks, including 2G, 3G and 4G. While we have coverage data for 4G networks, we do not have coverage data for 2G and 3G networks. Any analysis on network coverage would be therefore at best partial. In addition, given that 4G networks were rolled out at varying time periods across Europe, focusing the analysis on 4G coverage would not provide a sufficiently large dataset to obtain reliable results. Finally, coverage obligations are commonly attached to 4G licenses across Europe. As a result, 4G network coverage will be significantly driven by the timeframes required by the obligation.

A2.21 In light of the above, we have focused our analysis on download speeds. As the measure of download speed in our analysis, we use average quarterly download speeds from Ookla. Average quarterly download speed information is provided by Ookla at the MNO level since 2011.[23] Ookla also provided data on upload speeds. We found that download and upload speeds were highly correlated[24] and chose therefore to focus our analysis on the former.

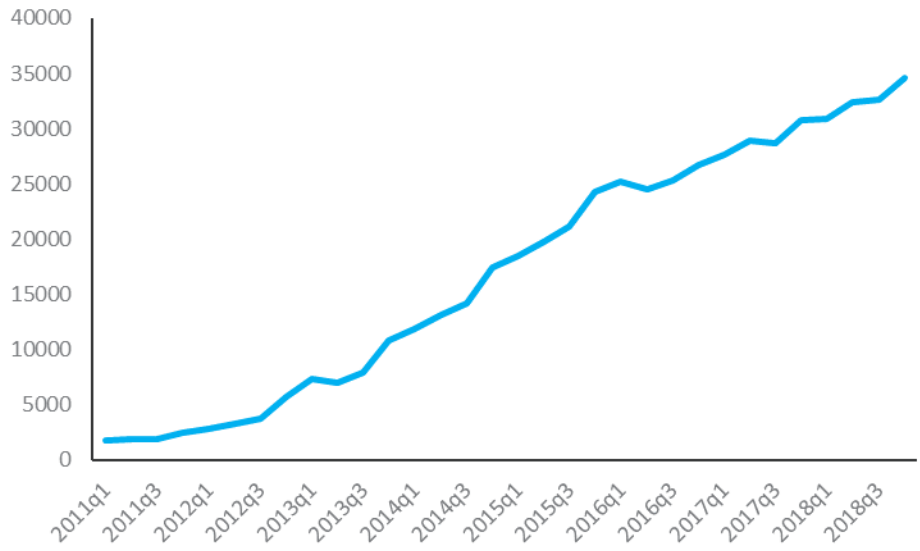
A2.22 Ookla calculates the average speeds based on an aggregation of speed tests conducted by users of its app.[25] These tests are applied to users using both 3G and 4G mobile networks. Test runs on the app are user-initiated and results are aggregated to produce measures of average quarterly download speeds at the MNO and national level.[26] This means that the data collected reflects actual consumer experience.[27] We use this MNO-level data to calculate weighted averages at the national level.

A2.23 Figure A2.2 shows the evolution of average download speeds over the period from 2011 to 2018 across our sample of countries. It shows that the weighted[28] average download speed

[23] More recently Ookla has also provided this information broken down by 2G, 3G and 4G technology. As these technology-specific series are only available for shorter periods and for selected MNOs in the data set, we focus on the average download speed metrics.
[24] The correlation between download and upload speeds was 0.946.
[25] Ookla, Speedtest, https://www.speedtest.net/apps/mobile.
[26] Aggregating individual readings reduces the risk of the data being skewed by test-specific factors. Such factors include but are not limited to; device used, weather conditions, time of day, network congestion, location, indoor/outdoor, and geographic topography. The ability to which these test-specific factors average out across MNOs depends on the sample size for each MNO and distribution of disturbances across them
[27] To account for repeated tests from the same user at a given time or location, observations are averaged in a two-step process. First, observations are averaged into samples by user, location and day. Second, these samples are averaged. This mitigates against results being skewed by repeated tests.
[28] The download speed from each country was weighted by the total number of mobile connections in each country.

has increased significantly from 2 Mbps at the start of 2011 to 35 Mbps by the end of 2018. Over this period, the trend has been quite consistent: with the weighted download speed changing by an average of 1 Mbps per quarter (though with some seasonality). The average download speeds in our data range from 1 Mbps to 66 Mbps, with an average mean of 18 Mbps and standard deviation of 14 Mbps.

Figure A2.2: Weighted average download speeds across sample (Kbps per second)



Source: Ofcom based on Ookla data. Country data weighted by number of mobile connections.

Measures of market concentration used in our analysis

A2.24 We use two measures of market concentration in our panel data analysis: number of MNOs and the Herfindahl-Hirschman Index (HHI).

A2.25 HHI is an indicator of market concentration which ranges from 0 to 10,000, with the indicator increasing with market concentration. It is calculated by the formulae $HHI = \sum_{i=1}^{n} s_i^2$, where $s_i$is the market share of firm "i" and "n" in the number of firms in the market.

A2.26 Both of these measures capture a range of structural changes in market concentration including mergers, exit and entry. This means that our analysis does not distinguish between changes in market concentration because of mergers, exit or entry. The fact that most of the structural changes in our data are entry events means that our estimated effect of changes in concentration on investment and quality will be driven mainly by entry effects. We consider this approach reasonable given that we would expect entry, merger and exit events to have a similar impact on investment and quality of service in the long-run. We do however test this assumption as part of our robustness checks. In addition, the HHI measure captures non-structural changes associated with changes in the relative market shares of MNOs. This allows us to exploit as much variation as possible in the data.

A2.27 We construct these measures using data from the GSMA Intelligence database. As mentioned above, we have made adjustments to the data to remove some operators – see paragraph A2.6.

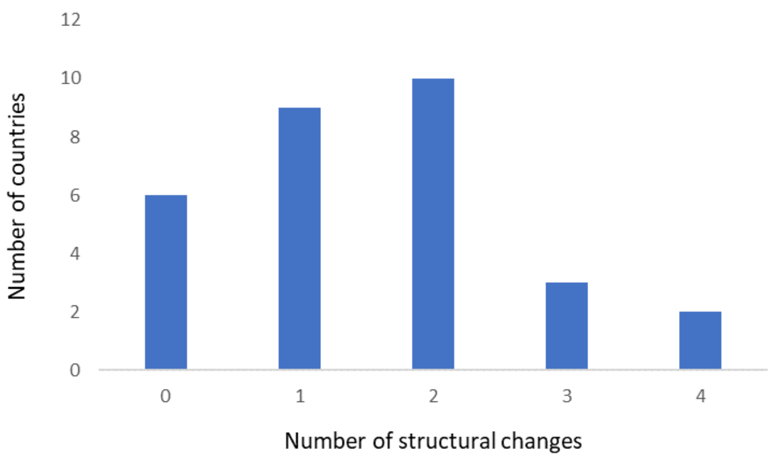
A2.28 In the following paragraphs, we provide a description of the data that we have used to build these measures.

Number of MNOs

A2.29 This metric captures the number of MNOs active in each quarter for each country within our sample. Although the value of the metric ranges from one to five MNOs in our data, the vast majority of our observations (86%) reflect countries which have three or four MNOs. Of this subset of observations, 62% are three MNO markets and 48% are four MNO markets. The average number of MNOs in our data is 3.28, with a standard variation of 0.72.

A2.30 Although changes in the number of MNOs in a country are rather infrequent, our dataset captures 46 changes (see Table A2.3). Of these changes, 12 are mergers, 33 are entries and one is exit. These changes are not spread evenly across our sample. As shown in Figure A2.3 below, while some countries had four structural changes over the study period, others had a single change or no change at all. Half of the countries in our sample had two or more structural changes over the period under analysis.

Figure A2.3: Distribution of countries by number of structural changes in market concentration



Source: Ofcom.

HHI

A2.31 As mentioned above, the HHI indicator captures both structural changes in market concentration (i.e. mergers, entry, exit) and non-structural changes (i.e. relative shifts in market share). This means that HHI is likely to offer more variation in the data than number of MNOs. However, this variation may not always be large enough as to be able to statistically isolate its impact from other factors that also drive investment and network quality. To illustrate this point, while HHI changes by 500-600 points when a merger occurs, it only changes by an average of 22 points when no structural change occurs.

A2.32 The HHI variable in our data ranges from 2,245 (UK) to 10,000 (Malta), with a mean of 3,745 and a standard deviation of 8,754.

Control variables

A2.33 As set out in Sections 3 and 5, to help isolate the impact of market concentration on investment and quality outcomes, we have considered a number of controls in our analysis. The controls and data sources used to construct these controls are listed in Table A2.6.

A2.34 Table A2.5 below provides the descriptive statistics of the main variables in our study.

Table A2.5: Descriptive statistics of main variables used in our study

	Unit	Mean	Std. Dev.	Min	Max
Number of MNOs	#	3.28	0.72	1	5
HHI	#	3,735	8,754	2,245	10,000
Capex	€m	169.00	247.00	1.30	1,360.00
Capex per capita	€m	10.00	5.55	0.29	46.70
GDP per capita	€k	35.20	15.80	9.24	98.54
Population density	#	168.19	232.91	12.28	1,352.10
Rurality of population	%	27.65	12.06	2.00	49.27
4G dummy	#	0.31	0.46	0	1.00
4G spectrum holdings	Mhz	80.18	126.00	0	400.00
Download speeds	Mbps	18.34	13.63	1.20	66.21
Unemployment rate	%	8.56	4.56	1.70	27.48
ARPU	€	32.02	14.14	8.67	93.86
Total revenue	€m	1200.00	1630.00	9.75	6450.00

Source: Ofcom

Table A2.6: Control variables considered in our study

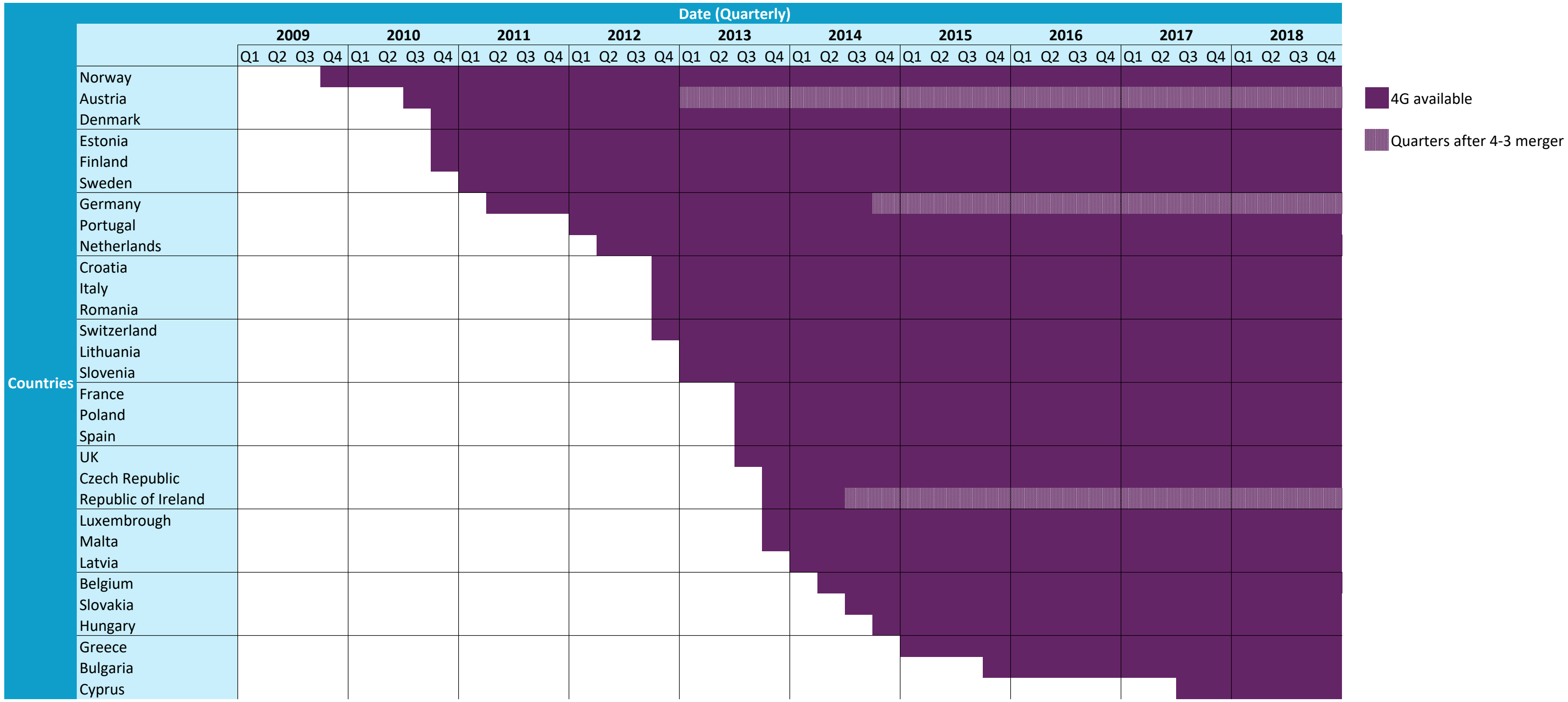
Variable	Description	Source	Frequency
GDP per $capita_{ct}$	GDP per capita, in constant 2011 USD prices.	IMF (World Economic Outlook Database)	interpolated quarterly from annual data
Population $density_{ct}$	Population density, in persons per square km.	UN (World Population Prospects)	interpolated quarterly from annual data
% rural $population_{ct}$	Difference between total population and urban population as defined by national statistical offices, in percent.	UN (World Urbanization Prospects)	interpolated quarterly from annual data

4G dummy	Takes a value of 1 from the quarter when 4G rollout commenced in a country; and 0 otherwise.	Analysys Mason	quarterly
Post 90% 4G network coverage	Takes a value of 1 from the quarter when 4G network coverage in a country reaches 90% of households; and 0 otherwise.	Analysys Mason	quarterly
4G spectrum holdings	Total spectrum holdings in the 800, 1800 and 2600 MHz bands allocated for the deployment of 4G networks	Analysys Mason spectrum tracker	quarterly from daily data
Unemployment rate$_{ct}$	Total labor force unemployed, in percent.	IMF (World Economic Outlook Database)	interpolated quarterly from annual data
d% prepaid connections	Proportion of prepaid connections as a percentage of total connections.	GSMA proprietary database	quarterly
ARPU	Average revenue per user (ARPU). Total recurring revenue generated per unique subscriber per month in the period.	GSMA proprietary database	quarterly
% smartphone adoption	Smartphone connections as a proportion of total connections	GSMA proprietary database	quarterly

i = operator, c = country, t= time period. Note: the information on these variables is unbalanced.

A2.35 We control for 4G investment cycles in our analysis using information on when 4G networks first began to be rolled out in each country. This is presented in Figure A2.7. In the panel analysis, we use a 4G dummy variable to represent when the shift in technology cycle occurred in each country. On the other hand, in the country-specific analysis, where we can do so, we normalise time around the date that 4G rollout commenced, as explained in Annex A5. In normalising the time around the start date of 4G rollout, this inevitably means that the the size of the sample used in the analysis is reduced. In the figure below, the striped areas in this table represent the time after the four-to-three mergers take place in the analysed countries.

Figure A2.7 – 4G rollout by country



Source: Global mobile Suppliers Association, Evolution to LTE reports

A3. Panel Data Methodology

A3.1 In Section 3 we explained that changes in market concentration could impact quality indirectly by affecting firms' ability and incentives to invest in network quality improvements. In addition, we said that changes in market concentration could also affect quality through direct means, by affecting consumer usage, network congestion and the way that mobile spectrum is utilised. Given these two possible mechanisms by which market concentration can impact quality, we analyse the following two models:

a) **Investment model** to estimate the impact of changes in market concentration on investment in the mobile sector; and

b) **Quality model** to estimate the:

i) direct effect of changes in mobile market concentration on average download speeds that arise from consumer usage changes and the way that mobile spectrum is utilised; and

ii) indirect effect of changes in mobile market concentration on quality that arise from changed firms' ability and incentives to invest in network quality improvements.

A3.2 In this annex we provide further details on how we specify these models and the panel data methods that we use to estimate them.

Investment model

A3.3 As set out in Section 3, we consider (based on the way that MNOs make investment decisions) that the relationship between changes in market concentration and investment is best characterised by a dynamic model whereby investment is a function of past investment, the technology cycle, market concentration and other controls (including country fixed effects).

A3.4 That is, we investigate a model of the form:

$$I_{it} = \propto + \sum_{\omega=1}^{n} \rho_{i\omega} L^{\omega} I_{it} + \sum_{k=1}^{K} \delta_k X_{ikt} + \beta_2 T_t + \beta_3 MC_{it} + v_i + \varepsilon_{it} \qquad [1]$$

A3.5 Where:

a) I_{it} is industry capex per capita in country i in quarter t;

b) $\sum_{\omega=1}^{n} \rho_{i\omega} L^{\omega} I_{it}$ is the weighted sum of lagged values of I_{it} up to the level of *n* where L^{ω} is the lag operator such that *L* is equivalent to (t-1), L^2 refers to (t-2) and so on;

c) X_{ikt} is a matrix of a control covariates which may influence I_{it}. These may include the technology cycle, GDP per capita, population density and the rurality of the population, and are set out in more detail below;

d) T_t is a vector of time variables including seasonal dummies and a time trend to capture any unobserved variation in consumer outcomes that can be attributed to specific time variant factors (e.g. new handset and mobile application releases);[29]

e) MC_{it} is the market concentration measure, either the number of MNOs or HHI in country i and quarter t – see discussion in Annex A2;

f) v_i are country fixed effects and capture any unobserved variation in consumer outcomes that can be attributed to specific characteristics of each country (e.g. geography and topology); and

g) ε_{it} is a random error term.

A3.6 Given that our investment model is a function of previous investment[30], the estimated impact of a change in market concentration on investment outcomes depends on the time horizon considered. In the short-run, the immediate impact of a change in market concentration can be identified by β_3. This identifies the contemporaneous effect of a change in market concentration on investment levels in any given quarter. In the long-run, the impact can be identified by $\beta_3/(1-\rho(L))$ where L is the lag polynomial.[31] This takes into account the fact that a change in market concentration has an impact on investment levels after the period in which the change in concentration occurs – because investment in future periods depends in part on investment today.

A3.7 We explain our approach to adopting the models that we estimate, and the methods that we use to estimate these models in turn. Before that, we set out the factors influencing planned investment in the mobile industry to clarify how we account for these in our modelling approach.

Factors influencing planned investment in the mobile industry

A3.8 Factors affecting the level of investment (and quality) in the mobile industry may be classified into demand and supply side factors.

Demand-side factors:

A3.9 **Population/market size** – population size in a country will influence the scale of demand for mobile services and network quality requirements. This will influence the scale of investment required to build a network to serve the population with the required network quality. This relationship is likely to be non-linear given the presence of scale economies.

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[29] We have tested different forms of time trend: linear year trend, linear year trend with a break in 2016 and a non-linear trend using year dummies.

[30] This includes the investment made by MNOs which have left the market in previous periods (see Annex A2). We believe that it is important to capture these investments in our analysis as they determine in part the network capacity we see today through the transfer of assets as part of a merger or acquisition. It also removes the risk that countries with longer established incumbents incorrectly appear as having higher levels of investment.

[31] Using the lag polynomial whereby $\rho(L)I_{it} = \rho LI_{it} = \sum_{\omega=1}^{n} \rho_{\omega i} L^{\omega} I_{it}$, and re-arranging the equation gives $(1-\rho L)I_{it} = \propto + \sum_{j=1}^{k} \delta_j X_{ijt} + \beta_2 T_t + \beta_3 MS_{it} + v_i + \varepsilon_{it}$. Using this equation, we can then derive the long-run impact of market structure on investment as $\beta_3/(1-\rho(L))$

A3.10 **Income per capita** – income is likely to affect demand for mobile services and network quality and the willingness to pay of consumers. The higher the income, the higher the demand for mobile services and network quality and the greater the willingness to pay of consumers. We would expect this to have a positive impact on the investment plans of MNOs.

A3.11 **New mobile applications** – new applications like instant messaging, video streaming and mobile banking have stimulated the demand for mobile internet services. These services require good mobile network quality. New applications may therefore stimulate demand for mobile services and could open new revenue streams. This could lead MNOs to increase their investment in network capacity, improving the quality of their networks in the longer term.

A3.12 **Smartphone take-up** – smartphone users tend to use mobile internet more intensively and require good mobile network quality. Thus, we would expect in countries where smartphone take-up is higher, that the willingness to pay for good mobile network quality will be higher and so this could lead to MNOs investing in network capacity to improve the quality of their networks.

A3.13 **New mobile technologies** (e.g. 4G at the time of the analysis) – the development of new technologies and the improvements they bring can increase willingness to pay from consumers for new services and better network quality that can lead to MNOs investing in these technologies.

Supply-side factors:

A3.14 **Population density / rural population** – Given that mobile antennas have a limited geographic coverage, unit costs tend to be lower in more densely populated/urban areas where more users can be served from the same antenna. Hence, we would expect investment per capita to be lower in more urban areas, i.e. in countries with larger urban population. However, it is worth noting that the more densely populated the country, the more likely that mobile network capacity is shared among users, leading to congestion. Therefore, depending on the extent of mobile network congestion, network quality could be affected detrimentally in more densely populated areas, unless sufficient investment is made to prevent the problem.

A3.15 **Geography of country** – network coverage/quality can be degraded by the presence of hills/mountains. Thus, we would expect network costs to be lower and network quality to be higher in countries with a flatter geography.

A3.16 **Spectrum holdings** – the more spectrum an MNO holds, the more network capacity and better network quality it can deliver for a given level of investment in physical infrastructure. Larger spectrum holdings may therefore require lower levels of investment to achieve the same quality level.

A3.17 **New mobile technologies** (e.g. 4G at the time of the analysis) – advances in new mobile technologies enable a more efficient use of spectrum, and thus greater network capacity/quality. Given the increasing demand for capacity from mobile users, new technologies are likely to result in new waves of investment from MNOs.

Model selection

Determining the control variables in the investment model

A3.18 As we set out above, there are a range of factors that drive investment in mobile markets. To ensure that our econometric results are reliable, we need to control for these factors. This is particularly important for those factors which may also be linked to market concentration as controlling for these factors would allow us to isolate the impact of changes in market concentration on investment.

A3.19 To select which controls to include in our models we use a general-to-specific methodology by which we start by including the widest possible set of controls and then drop those which are not statistically significant. [32] To verify the robustness of our results, we then test alternative models which include controls that were excluded as a result of this approach.

Determining the number of lags of the investment variable in the investment model

A3.20 We also adopt a general-to-specific approach in determining the number of lags of the investment variable which we include as explanatory variables in our investment model.

A3.21 As we explain in Section 3, MNOs make their investment plans over a multi-year horizon to ensure that their network capacity continues to grow in a way that supports the anticipated growth in demand. Therefore, we would expect investments across different periods to be somewhat inter-related, as more investment in one period could mean less investment is required in subsequent periods.

A3.22 In setting our most general model for investment, we start by including the first 20 quarterly lags of investment. We base this on our coverage data which suggests that 4G networks were built over an average period of 3-4 years.[33] In moving to a more parsimonious model with a smaller number of lags, we run an iterative sequence of linear restriction tests on the coefficients of the lags of investment in the unrestricted 20 lag model.[34]

Model estimation for investment model

A3.23 We estimate equation 1 using the following two methods:

a) Fixed effects (FE); and

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[32] In selecting the widest possible set of controls, degrees of freedom dictated how many controls we could include in our starting specification. We nonetheless tested different iterations of this starting specification to ensure that the choice of initial controls did not drive the final specification.
[33] We could consider additional lags but this would come at the expense of degrees of freedom in our analysis, and could lead to higher standard errors in our estimates.
[34] These linear restrictions are tested using a F-test.

b) Arellano-Bond (AB) estimator.

Fixed effects (FE) method

A3.24 We start by estimating equation 1 using the FE method. This method allows us to control for unobserved factors that affect investment but that are fixed over time, such as geography of a country. This is important because the impact of changes in market concentration cannot be isolated unless we control for those factors (observed and unobserved) that drive investment as well as market concentration.

A3.25 The FE method accounts for unobserved time invariant factors by applying the demeaning transformation, i.e. the mean value of each variable in the model gets subtracted from each observation. For time invariant factors, each observation is equal to the mean value, so applying the demeaning transformation has the effect of removing these factors from the analysis.[35]

A3.26 However, applying the demeaning transformation in a dynamic setting (i.e. where lags of the dependent variable are included as explanatory variables in the model) creates a simultaneity issue between that dependent variable and the lagged explanatory dependent variables. This is because the mean of the dependent variable is comprised by both past and future observations of the variable, so, by taking the mean value out from the dependent variable, the dependent variable and lagged explanatory dependent variables become simultaneously determined in the model. This introduces bias to the FE estimators. However, this bias tends to reduce with T, the number of time periods in the dataset.[36] Given that we have 76 periods/quarters in our data, this bias is likely to be small as suggested by Judson R.A. and Owen A. (1999).[37] We therefore believe that there is merit in using the FE method to assess equation 1. Nonetheless, we also apply the Arellano-Bond estimator because, even though we have a long-time horizon in our data, there may still be bias in our FE estimator.

Arellano-Bond (AB) estimator

A3.27 An approach that is used to address the simultaneity issue created by applying the demeaning transformation to a dynamic model is to transform the model to first

[35] For a detailed description of this method see Wooldridge, "Econometric Analysis of Cross Section and Panel Data", The MIT Press, 2nd edition, 2010; and Cameron & Trivedi, "Microeconometrics: Methods and Applications", 2005.
[36] Nickell, S. (1981). Biases in Dynamic Models with Fixed Effects. Econometrica, 49(6), 1417-1426. The author demonstrates that in a dynamic panel setting the demeaning process which subtracts the individual's mean value of y and each X from the respective variable creates a correlation between regressor and error. The mean of the lagged dependent variable contains observations 0 through (T − 1) on y, and the mean error—which is being conceptually subtracted from each ε_{it}—contains contemporaneous values of ε for t = 1 . . . T. The resulting correlation creates a bias in the estimate of the coefficient of the lagged dependent variable, which is not mitigated by increasing N, the number of individual units. Nickell demonstrates that the inconsistency of $\hat{\rho}$ as N → ∞ is of order 1/T, which may be quite sizable in a "small T" context.
[37] Judson R.A. and Owen A. (1999), Estimating dynamic panel data models: a guide for macroeconomists. Economics Letters, 1999, vol. 65, issue 1, 9-15. The authors found that even with a time dimension T as large as 30, they found that the bias may be equal to as much as 20% of the true value of the coefficient of interest. Asymptotically, the bias in the FE estimate is proportional to 1/T (where T is the number of time periods). Given that we have 76 periods in our data, the size of the bias in our FE analysis would be proportional to 0.01.

differences and construct instruments for the lagged explanatory dependent variable. Transforming the model to first differences means that the simultaneity problem would only arise between the dependent variable and the first lag of the dependent variable (included as a covariate), so further lags of the dependent variable can be used as instruments for the first lagged explanatory dependent variable.

A3.28 The Arellano-Bond (AB) Generalised Method of Moments (GMM) estimator[38] implements this instrumental variable strategy. Under this method, the necessary instruments are 'internal': that is, based on lagged values of the instrumented variable(s). Lags of the instrumented variables are likely to be good proxies for the variable needing to be instrumented for and secondly, they will likely not be simultaneously determined by the model because, as mentioned above, they no longer appear in the left hand side of the equation (as occurs when applying the FE method).

A3.29 In applying this approach to our analysis[39], the investment model is specified as a system of equations, one per time period. The AB-GMM estimator then uses different numbers of lagged level instruments for the first differenced explanatory investment variable that are available for each time period: one for t = 2, two for t = 3, and so on. In other words, for each successive time period in the model, there are an increasing number of available instruments to be used in the estimation process. Being able to make use of this additional data ensures the efficiency of the AB estimator.

A3.30 One disadvantage of this strategy though is that the number of instruments produced will be quadratic in T, the length of the timeseries. This is problematic because having too many instruments can lead to overfitting the instrumented variable and, consequently, to failing to clear out the endogeneity within it.[40] Roodman (2009) proposes restricting the number of lags used as instruments as a solution to this problem. However, there is little guidance on the optimal number of instruments. In applying the AB-GMM estimator we have therefore run the analysis for a range of instrument counts – see discussion in paragraph A4.5.

A3.31 We note that a potential weakness in the AB-GMM estimator is that lagged levels are often rather poor instruments for first differenced variables. To deal with this problem, an extension of this approach is to include lagged levels as well as lagged differences as instruments for the first differenced variables. This is known as system GMM.[41] Including more instruments though has the disadvantage of exacerbating the instrument proliferation problem cited above. It has also been shown that the weakness of the AB-

[38] Arellano & Bond, "Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations", Review of Economic Studies 58: 277–297, 1991;

[39] For a detailed description of this method see Cameron & Trivedi, "Microeconometrics: Methods and Applications", 2005; and Bond, "Dynamic panel data models: A guide to micro data methods and practice", Working Paper CWP09/02, Cemmap, Institute for Fiscal Studies. Available at http://cemmap.ifs.org.uk/wps/cwp0209.pdf

[40] This creates bias in the AB estimate – see Roodman, "A Note on the Theme of Too Many Instruments", Oxford Bulletin of Economics and Statistics, 71, 1(2009). Windmeijer (2005) reports that reducing the instrument count from 28 to 13 reduces the average bias of the parameter of interest by 40%.

[41] Arellano & Bover, "Another look at the instrumental variable estimation of error-components models", Journal of Econometrics 68 (1995) 29-51

GMM instruments holds when the coefficient of the first differenced explanatory dependent variable is moderately high (i.e. above 0.6) and the number of time periods in data is small.[42] This is not the case in our analysis[43]; hence we have not applied the "system GMM" estimator.

Testing for serial correlation in the error term

A3.32 Serial correlation in the errors is likely to arise in a dynamic setting like ours. This is particularly the case if the dynamics of the dependent variable are not properly accounted for. For example, if relevant lagged dependent variables are not included as explanatory variables in the model, these variables would end up being captured in the errors. Given that these lagged variables are likely to be correlated between each other, the omission of them in the model would give rise to serial correlation in those errors.

A3.33 The AB-GMM estimator described above assumes that the errors in the model are not serially correlated. If the errors are serially correlated, lags of the dependent variable would no longer serve as valid instruments for the lagged differenced dependent variables included as explanatory variables in the model. This is because further lags of the dependent variable might be correlated with the error term (i.e. a serial correlation of order two would mean that the second lag of the dependent variable could not be used as a valid instrument for the first lagged differenced dependent variable). Therefore, to verify the validity of the instruments that we use for the lagged differenced dependent variables, we must satisfy ourselves that the errors in our analysis are not serially correlated. We do this by applying the AB test for serial correlation.[44]

A3.34 There are other tests that could be run to verify the validity of our instruments. One of these tests is the Sargan test of over-identifying restrictions. This test, however, assumes homoskedastic errors, which is rarely assumed for panel data models like ours (so we use robust errors to correct for this). An extension of this test for heteroskedastic errors is the Hansen test. This test is suitable for datasets that have a small T. For longer datasets (like ours), the Hansen test becomes weak, due to instrument proliferation, and tends to never reject the validity of instruments.[45] We therefore do not place weight on the results from these tests.

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[42] Blundell & Bond, "Initial conditions and moment restrictions in dynamic panel data models", Journal of Econometrics, 87, Issue 1, November 1998, Pages 115-143.

[43] We have a long timeseries of 76 periods and the magnitude of the coefficient of the first differenced explanatory investment variable in the models that we estimate is below 0.5 – see Annex A4.

[44] The null hypothesis of this test is that there is no serial correlation in the first-differenced errors (for details on the implementation of this test see https://www.stata.com/manuals13/xtxtabondpostestimation.pdf). Rejecting the null hypothesis for first-differenced errors at an order greater than one would suggest serial correlation. This is because rejecting no serial correlation in the first differenced errors at an order of one would be expected of first differenced errors that are independent and identically distributed – see Arellano and Bond, "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations", Review of Economic Studies (1991) 58, 277-297, available here: http://people.stern.nyu.edu/wgreene/Econometrics/Arellano-Bond.pdf.

[45] See Roodman (2009) for a detailed discussion on the weakness of the Hansen test when there is a high instrument count. A variant of the Hanen test is the difference-in-Hansen test which checks the validity for a subset of instruments. As explained by Roodman (2009), this test is also affected by high instrument count.

Instrumenting for market concentration in the investment model

A3.35 In Section 5, we explain that to isolate the impact of changes in market concentration on investment, changes in market concentration need to be independent of changes in investment. In other words, market concentration should not be impacted by investment, nor by unobserved factors that also drive investment.

A3.36 We further explain that while changes in investment are unlikely to impact the number of MNOs in a market within the same quarter (once other factors that affect investment are controlled for), they are may impact the level of HHI. This is because changes in investment (for example, in marketing and/or network quality) could drive higher network take-up, and this could affect the level of HHI in the market. Therefore, while we believe that investment and number of MNOs are not simultaneously determined in our investment model, we consider that investment and HHI are.

A3.37 To address this simultaneity issue, we use the lag of HHI two quarters ago as an instrument for contemporaneous HHI.[46] We consider that HHI two quarters ago is a valid instrument in this case given that it is likely to be a good predictor of current HHI, while we do not expect changes in current investment to affect the level of HHI two quarters ago.

A3.38 We implement this instrumental variable strategy for each of the two methods discussed above:

a) **FE method:** we apply a two-stage least squares procedure to estimate our FE model (FE-2SLS); and

b) **AB-GMM method:** we add the lag of HHI two quarters ago as an additional instrument in the system of equations to which we apply the AB-GMM estimator.

A3.39 In the following paragraphs, we discuss each of these two methods.

FE-2SLS estimation for HHI measure

A3.40 Instruments can be implemented in the FE methodology by applying a two-stage least squares procedure. This is done by first regressing the instrumental variable (i.e. the lag of HHI two quarters ago) and the set of controls (included in the investment model) on HHI.

A3.41 That is, we estimate a model of the form:

$$HHI_{it} = \theta_0 + \sum_{\omega=1}^{n} \theta_{1i\omega} L^{\omega} I_{it} + \sum_{k=1}^{K} \theta_{2k} X_{ikt} + \theta_3 T_t + \theta_4 HHI_{it-2} + v_i + u_{it}$$

A3.42 Where:

a) HHI_{it} is the market concentration measure;

[46] We use the lag of HHI two-quarters ago as an instrument, rather than the first quarter lag of HHI. This is because, when taking first differences of the investment equation, the lag of HHI a quarter ago is part of the first differenced investment equation. Therefore, to address the simultaneity problem between HHI and investment we need to use the lagged value of HHI starting from two periods ago. We stress test the choice of this lag by re-running our analysis using the lag of HHI four quarters ago as a robustness test.

b) $\sum_{\omega=1}^{n} \rho_{i\omega} L^{\omega} I_{it}$ is the weighted sum of lagged values of I_{it} up to the level of *n* where L^{ω} is the lag operator such that L is equivalent to (t-1), L^2 refers to (t-2) and so on;

c) X_{ikt} is a matrix of a control covariates. These may include the technology cycle, GDP per capita, population density and the rurality of the population;

d) T_t is a vector of time variables including seasonal dummies and a linear time trend to capture any unobserved variation in consumer outcomes that can be attributed to specific time variant factors (e.g. new handset releases, emergence of new mobile applications);

e) HHI_{it-2} is the instrument for HHI_{it};

f) v_i are country fixed effects; and

g) u_{it} is a random error term.

A3.43 We then use the predicted values of HHI from this first-stage regression to estimate equation 1 in a second stage regression.

AB-IVGMM estimation for HHI measure

A3.44 Instruments can be easily introduced in the AB framework. This is because the AB-GMM method already uses instrumental variables to address the simultaneity issue caused by the inclusion of lags of the dependent variable (in this case investment) as a regressor in the model. Therefore, to implement our instrumental variable strategy, we have specified the lag of HHI two quarters ago as additional instrument in our AB-GMM estimator.[47]

Quality model

A3.45 To assess the impact of changes in market concentration in the mobile sector on quality, we sought to estimate the following two effects:

a) the direct effect of changes in mobile market concentration on average download speeds that arise from consumer usage changes and the way that mobile spectrum is utilised; and

b) the indirect effect of changes in mobile market concentration on quality that arise from changes in firms' ability and incentives to invest in network quality improvements.

A3.46 To estimate these two effects, we consider a model of the form:

$$Y_{it} = \gamma + \sum_{\omega=m}^{n} \varphi_{i\omega} L^{\omega} I_{it} + \sum_{k=1}^{K} \tau_k X_{ikt} + \mu_2 T_t + \mu_3 MC_{it} + v_i + \varepsilon_{it} \qquad [2]$$

A3.47 Where:

a) Y_{it} is the quality variable (i.e. average download speeds);

[47] For further details on how this method works see footnotes 38 and 39.

b) $\sum_{\omega=m}^{n} \varphi_{i\omega} L^{\omega} I_{it}$ is the weighted sum of lagged values of I_{it} (capex per capita) up to the level of *n* where *m* in this equation is one – see paragraph A3.42 for an explanation as to why we do not include contemporaneous investment. L^{ω} is the lag operator such that *L* is equivalent to (t-1), L^2 refers to (t-2) and so on;

c) X_{ikt} is a matrix of a control covariates which may influence Y_{it}. These may include the technology cycle, GDP per capita, population density, the rurality of the population and post-90% 4G coverage;

d) T_t is a vector of time variables including seasonal dummies and a global linear time trend to capture any unobserved variation in consumer outcomes that can be attributed to specific time variant factors (e.g. new handset releases, emergence of new mobile applications);

e) MC_{it} is the market concentration measure, either the number of MNOs or HHI in country i and quarter t – see discussion in Annex A2;

f) v_i are country fixed effects and capture any unobserved variation in consumer outcomes that can be attributed to specific characteristics of each country (e.g. geography and topology); and

g) ε_{it} is a random error term.

A3.48 The direct effect of changes in market concentration on average download speeds is identified by the parameter μ_3. The indirect effect of changes in market concentration on average download speeds is identified by combining the results from equations 1 and 2 above. The short-run indirect effect is identified by $\varphi L \beta_3$;[48] and the long-run indirect effect is identified by $\varphi L \times [\beta_3/(1-\rho(L))]$.[49]

Model selection

Determining the control variables in the quality model

A3.49 Similar to our analysis on investment, we use a general to specific methodology to determine which controls to include in our quality model.

A3.50 In addition to the controls that we consider for our investment model, i.e. the technology cycle, GDP per capita, population density and the rurality of the population, we consider the following controls:

a) dummy variable indicating whether 4G coverage has reached 90% of households (we refer to this control as 'post-90% 4G coverage'); and

[48] The short-run impact captures two effects. The first is the short-run impact of market structure on investment equal to β_3. The second short-run impact captures the effect of past investment on network quality equal to φL - see footnote 50 for more detail

[49] Using the lag polynomial: $\varphi(L)I_{it} = \varphi L I_{it} = \sum_{\omega=m}^{n} \varphi_{i\omega} L^{\omega} I_{it}$. Inserting this into equation 2 and then taking the partial derivative with respect to I_{it} gives φL

b) interaction between the post-90% 4G coverage dummy and our measure of market concentration.

A3.51 As we explain in Section 5, the inclusion of these additional controls allows us to test the hypothesis that MNOs may sequence their investment by focusing on extending coverage first and then ensuring enough capacity is there to meet user demand. Previous studies have assumed that the shift from coverage- to capacity-led investment happens once MNOs reach 90% coverage (see Annex A1). Therefore, the interaction term tests whether changes in market concentration may impact quality outcomes by affecting the ability and incentives to invest in further network capacity, once 90% coverage is achieved.

Determining the lags of investment to include in the quality model

A3.52 As implied by equation 2, we assume that average download speeds are partly determined by lagged investment, rather than by contemporaneous investment.[50] We understand that investment in mobile networks takes time to implement and feed into quality outcomes, particularly as capex is typically incurred at the time that an order is placed (rather than when it is received and ready for use). The delay in the impact of investment on network quality will vary depending on the asset type the investment relates to. For example, installing a new cell site could take several months to complete and for it to then become operational. Upgrading an existing cell site (e.g. by installing an additional carrier) on the other hand is likely to require less time to implement.[51] Although there is some variation in the extent of delay, it is our understanding that it would be very unusual for investment to impact network quality in the same quarter in which the expenditure is incurred on given the lead times between placing an order, receiving the equipment and making it operational.

A3.53 To determine the lags of investment to include in our quality model, we adopt the same general to specific approach that we apply for selecting the controls in our model. In setting our most general model for network quality, we start by including the first eight quarterly lags of the investment variable. We consider that eight quarters is a reasonable timeframe to consider given that it is unlikely for incremental discrete investments in mobile networks to take more than two years to get implemented and for them to then feed into quality outcomes. In moving to a more parsimonious model with a smaller number of lags, we then run an iterative sequence of linear restriction tests on the coefficients of the lagged investment variables considered in our most general model.

Model estimation for quality model

A3.54 We take a different approach for estimating equation 2 depending on the measure of market concentration used.

[50] We nonetheless test the inclusion of contemporaneous investment in the quality model to ensure that we are not omitting relevant variables from the model. Its inclusion does not impact our results.
[51] We believe that the delay in the impact of investment on download speeds could be amplified in our data given that our data is quarterly and we understand that MNOs typically record capex spend when they place an order (i.e. in advance of when they receive and are able to deploy it).

A3.55 For number of MNOs, we estimate equation 2 using the FE method.[52] That is, we assume that any unobserved factors that affect download speeds, and that are linked to market concentration, are fixed over time; and thus can be removed from the model by applying the demeaning transformation. As mentioned above, these factors may include geography and network topology.

A3.56 For HHI, we take a slightly different approach. Although we also use the FE method to estimate equation 2, we apply instrumental variable techniques to address the simultaneity issue arising between HHI and download speeds. We explain this problem, and how we address this, in the next paragraphs.

Instrumenting for HHI in the quality model

A3.57 As discussed in Section 5, we believe that HHI can be impacted by changes in average download speeds and vice versa, i.e. there is reverse link between HHI and average download speeds. This is because if a network were to offer enhanced download speeds to mobile users, this could improve the relative attractiveness of joining the network (relative to other networks). To the extent that users value getting higher download speeds, changes in download speeds could therefore affect the network's share of subscribers in the market and thus the level of HHI.[53] This reverse link between HHI and average download speeds introduces bias to our FE estimate.

A3.58 To address this issue, we apply instrumental variable techniques. Similar to our analysis on investment, we use the lag of HHI two quarters ago as an instrument for HHI.[54] As mentioned above, HHI two quarters ago is likely to be a good predictor of current HHI, and changes in current download speeds are unlikely to affect HHI two quarters ago.

A3.59 To implement this instrumental variable strategy, we estimate equation 2 by applying the 2SLS procedure described in paragraphs A3.40 - A3.43. That is, we first estimate a model of the form:

$$HHI_{it} = \theta_0 + \sum_{\omega=m}^{n} \theta_{1_{i\omega}} L^{\omega} I_{it} + \sum_{k=1}^{K} \theta_{2k} X_{ikt} + \theta_3 T_t + \theta_4 HHI_{it-2} + v_i + u_{it}$$

A3.60 Where:

a) HHI_{it} is HHI in country *i* in time *t*;

b) $\sum_{\omega=m}^{n} \varphi_{i\omega} L^{\omega} I_{it}$ is the weighted sum of lagged values of I_{it} (capex per capita) up to the level of *n* where *m* in this equation is one – see paragraph A3.52 for an explanation as to why we do not include contemporaneous investment. L^{ω} is the lag operator such that *L* is equivalent to (t-1), L^2 refers to (t-2) and so on;

[52] See footnote 34 for references providing a more detailed description of the FE method.

[53] We do not consider this holds true for the relationship between number of MNOs and download speeds. That is because changes in download speeds are unlikely to affect the number of MNOs in the market. Number of MNOs is a result of entry/merger/exit decisions made by MNOs. These decisions are based on a consideration of future profitability and we believe that changes in download speeds in a single quarter are unlikely to trigger changes in these decisions.

[54] See footnote 46 for an explanation on the choice of this lag as an appropriate instrument for contemporaneous HHI.

c) X_{ikt} is a matrix of a control covariates which may influence Y_{it}. These may include the technology cycle, GDP per capita, population density, the rurality of the population and post-90% 4G coverage;

d) T_t is a vector of time variables including seasonal dummies and a linear time trend;

e) HHI_{it-2} is the instrument for HHI_{it};

f) v_i are country fixed effects; and

g) u_{it} is a random error term.

A3.61 Using the predicted values of HHI from the equation above, we then estimate equation 2 in a second stage regression.

A4. Panel Data results

A4.1 This annex presents the results of our panel data analysis in more detail. The annex is structured as follows:

a) investment model results; and

b) quality model results.

Investment model

A4.2 In the below paragraphs we set out the results of our analysis on investment. We first present the results for our models using number of MNOs as the measure of market concentration. We then present the results for our models using HHI.

Number of MNOs

Results

A4.3 Table A4.1 presents the results for our preferred investment model specifications using number of MNOs as the measure of market concentration. The dependent variable in these models is the log of capex per capita. Column 1 of the table shows the results for our preferred FE model. Columns 2 to 4 show the results for our preferred AB-GMM models. The implied estimate of the long-run impact of changes in the number of MNOs on the log of capex per capita is reported in the same table in the row with the heading 'Long-run impact'.

A4.4 Following the methodology set out in Annex A3, our preferred specifications include 16 quarterly lags of the investment variable and a set of controls. The number of lags of investment included in our preferred specifications is unsurprising given that, as we explain in Section 3, MNOs tend to have multi-year investment plans. Therefore, we would expect current investment to be partly determined by investment in a number of previous quarters. For presentational purposes, we only report our estimate for the first lag of investment and the sum of the coefficients of the 16 lags of the investment measure.[55]

A4.5 Our preferred AB-GMM models use different numbers of lags of investment as instruments for the first lagged differenced investment variable. We use 12, 18 and 30 lags respectively, starting from lag 2.[56] This captures the range of instruments over which the analysis produces stable results. When using over 30 lags, the results do not change. When we use fewer than 12 lags, our estimate of the long-run impact of changes in number of MNOs increases significantly, together with the standard errors. However, the difference

[55] This is a linear combination of the coefficients of the lags of the investment variable. This represents the cumulative effect on current investment of a 1% increase in past investment in every period. [56] When we use 12 lagged instruments in our analysis means that the set of instruments used spans from lag 2 to lag 13 of the investment variable.

[56] When we use 12 lagged instruments in our analysis means that the set of instruments used spans from lag 2 to lag 13 of the investment variable.

between these estimates and those which rely on at least 12 lagged instruments is not statistically significant.[57]

A4.6 The signs of the coefficients of the control variables in our preferred models are as expected. The coefficient of GDP per capita is positive which suggests that capex per capita is higher in countries with higher GDP per capita. The coefficient of the 4G dummy is also positive, implying an upward level shift in the capital spent during the 4G cycle relative to the 3G cycle.

A4.7 We also tried including other controls, such as spectrum holdings, rurality of population and population density, but the coefficients of these controls were not statistically significant. We believe that this could be explained by spectrum holdings being somewhat captured by our 4G dummy variable.[58] This is because the beginning of a technology cycle often coincides with the allocation of additional spectrum, and/or with the refarming of spectrum previously used for other uses (e.g. 2G). Similarly, rurality of population and population density may be somewhat captured by the country fixed effects as these factors do not tend to vary much over time.[59]

A4.8 With respect to the long-run impact of changes in the number of MNOs on the log of capex per capita, we find that this impact is always positive and statistically significant, implying that industry capex is lower in more concentrated markets. The level of the estimated impact ranges from 13% to 19%. The fact that our AB-GMM estimates sit around our FE estimate suggests that the latter is robust to the Nickell bias – see Annex A3 for an explanation of this bias.

Table A4.1: Main results of our analysis on investment – Number of MNOs as market concentration measure

	[1]	[2]	[3]	[4]
Method:	**FE**	**AB-GMM**	**AB-GMM**	**AB-GMM**
# of lagged GMM instruments		12	18	30
Year	-0.001 (0.003)	0.002 (0.005)	0.001 (0.005)	-0.001 (0.003)
Q2	0.015 (0.012)	0.011 (0.011)	0.012 (0.012)	0.014 (0.012)
Q3	0.048*** (0.011)	0.042*** (0.010)	0.043*** (0.010)	0.048*** (0.010)
Q4	0.113*** (0.015)	0.108*** (0.015)	0.109*** (0.014)	0.113*** (0.015)

[57] We have tested whether the point estimates for different instrument counts are statistically distinct, even below 12 lags. We cannot reject the null hypothesis that the estimates are the same.
[58] The correlation factor between the 4G dummy and spectrum holdings is over 0.8.
[59] For these two measures, the observations in our sample vary 5-10% around the mean value for each country over the 19-year period.

Lag1_ln_capex_pop	0.418***	0.334***	0.345***	0.413***
	(0.038)	(0.041)	(0.041)	(0.038)
Σ(Lag_ln_capex_pop)	0.762***	0.540***	0.570***	0.749***
	(0.029)	(0.071)	(0.071)	(0.031)
Ln_gdp_pop	0.040	0.312**	0.345**	0.045
	(0.078)	(0.157)	(0.146)	(0.015)
4G dummy	0.047**	0.055**	0.060***	0.046**
	(0.020)	(0.027)	(0.023)	(0.020)
Num_MNO	0.040**	0.085***	0.057**	0.041***
	(0.015)	(0.032)	(0.027)	(0.015)
Long-run impact	0.168***	0.185**	0.132**	0.164***
	(0.061)	(0.074)	(0.062)	(0.058)
# of obs	1,800	1,800	1,800	1,800
# of groups	30	30	30	30
F statistic	371			
Wald statistic		1,406	2,883	10,469
R-square	0.87			

*Note: *p-value <0.1; **p-value <0.05; ***p<0.01. We use robust standard errors. These are shown within brackets.*

A4.9 All our AB-GMM models pass the test of no serial correlation in the errors. This suggests that the lagged instruments that we use for the first lagged explanatory investment variable are valid – see paragraph A3.33 for a discussion on the interpretation of this test.

Robustness tests

A4.10 To test the robustness of our results to key methodological choices in our analysis, we have carried out the following sensitivities on our preferred investment models:

i) **Lag structure**. Following a general-to-specific methodology, our preferred model specification includes 16 lags of investment. A drawback of including that many lags though is that this may lead to model overfitting. Therefore, we have estimated a more parsimonious model with nine lags;[60]

ii) **Spectrum holdings.** As mentioned in Section 3, spectrum is likely to be an important driver of investment and quality. As discussed in the preceding subsection, we believe that changes in spectrum holdings are likely to be captured by our 4G dummy. To test the robustness of our model to including the 4G dummy, we estimate a model that includes country 4G spectrum holdings as an additional control;

iii) **Additional controls.** We also verify the robustness of our results to the inclusion of other controls such as rurality of population and population density;

[60] We choose to include the first 9 lags because beyond this lag, few lags appeared statistically significant so this seemed like a sensible cut-off point.

iv) **Form of time trend.** In Annex A2 we presented data that suggested a slight upward trend of investment over time, but which appears to taper off towards the end of the study period. We therefore test a quadratic term in the form of the time trend as a robustness check;

v) **Time horizon.** Our period of analysis goes from 2000 to 2018. This period covers both the 3G and 4G investment cycles. However, we were not able to collect data on 3G spectrum holdings. Entry of new MNOs often coincides with the allocation of new spectrum, so there is the risk that our results could be picking-up the effects of the release of 3G spectrum. To test this, we have restricted the time horizon in our data to exclude the period over which 3G spectrum was allocated across Europe, i.e. 2000-2005. That is, we re-run our analysis for the period from 2006 to 2018;

vi) **Separating out merger and entry effects.** In Annex A2, we explain that our measures of market concentration do not distinguish between merger and entry effects. That is, our model implicitly assumes that the impact of a merger and entry is similar. We recognise that our dataset has a larger number of entry events (33) compared to the number of merger/exit events (13). Therefore, to verify whether the direction of the effect is the same, we have tested two interaction variables in our investment models. These variables interact number of MNOs with two dummy variables: one indicating when a merger/exit takes place and another one indicating when entry occurs; and

vii) **Functional form of the relationship between market concentration and investment.** Some empirical studies have assumed an inverted U-shape relationship between market concentration and investment. To examine whether our results are robust to this form of relationship, we estimate a model that includes a quadratic term for number of MNOs.[61]

A4.11 Table A4.2 presents the results of these robustness tests. They suggest that our estimate of the long-run impact of number of MNOs on the log of capex per capita is robust to a 9-lag investment structure, the form of the time trend, the inclusion of other controls[62], and a shorter time horizon which excludes the period when 3G spectrum was allocated throughout Europe.

A4.12 With regards to the separation of entry and merger effects, as noted above, our dataset has a larger number of entry events compared to merger/exit events. Therefore, it is to be expected that entry effects will have a larger weight in our estimated average effect than the merger/exit effect. However, more importantly, our analysis shows that the effects are still positive implying that industry capex is lower in more concentrated markets.

[61] We have also tested a variant of this model where instead of a quadratic term we use dummy variables to capture different market types: one capturing 4-player markets and another one capturing 5 or more player markets (as in Genakos et al. (2018)) and get similar results.

[62] We have also tested a variant of this model but rather than including country 4G spectrum holdings we include the average 4G spectrum per MNOs and get similar results.

A4.13 Furthermore, our robustness tests do not provide support for an inverted U-shape relationship between market concentration and investment as the coefficient of the square term for number of MNOs is either not significant or not significant enough to suggest that an inverted U-shape relationship applies over the range of our sample (one to five MNOs).

Table A4.2: Robustness tests on our estimate of the long-run impact of changes in the number of MNOs on investment

Method [# of lagged GMM instruments]	FE	AB-GMM [12]	AB-GMM [18]	AB-GMM [30]
0. Preferred specification	0.17***	0.19**	0.13**	0.16***
1. Lag structure	0.18***	0.19***	0.12**	0.18***
2. Spectrum holdings	0.18***	0.19**	0.14**	0.17***
3. Rurality of population	0.17***	0.17**	0.12*	0.16***
4. Population density	0.17***	0.20***	0.15**	0.17***
5. Form of time trend	0.16***	0.07**	0.08**	0.16***
6. Time horizon	0.14***	0.14**	0.11*	0.14***
7. Merger and entry effects	Entry: 0.24***	0.15***	0.14***	0.23***
	Merger: 0.13*	0.10	0.08	0.14*
8. Functional form	Sq. term not significant	Sq. term not significant	Sq. term not significant	Sq. term is significant but small

Note: *p-value <0.1; **p-value <0.05; ***p<0.01. We use robust standard errors.

HHI

Results

A4.14 Table A4.3 presents the results of our preferred investment model specifications using HHI as the measure of market structure. These models use the same dependent variable and controls as our preferred models using number of MNOs. However, as set out in Annex A3, given the likely reverse link between investment and HHI, we use the lag of HHI two quarters ago to instrument for HHI.

A4.15 Column 1 of the table reports the results from our 2SLS procedure on our FE-IV model. Columns 2 to 4 report the results for our AB-IVGMM models.[63] Note that the number of

[63] We implement the standard version of the AB method by applying the AB-GMM estimator to the system of equations in first differences. To determine the lag of investment in first differences to be used as an instrument, we start at lag 2 (i.e.

lagged instruments used in the three AB-IVGMM models reported in the table differs to that in our analysis using number of MNOs. We use 6, 12 and 18 lagged instruments, respectively. This covers the range over which the standard errors of our long-run impact estimate of market concentration are stable. Using more than 18 lagged instruments does not alter the results, while using less than 6 lagged instruments produces higher standard errors. The direction and significance of the results however are consistent irrespective of the number of lagged instruments used.

A4.16 The sign of the coefficients of the controls in our preferred models is as expected. The coefficient of GDP per capita is positive, though not always statistically significant. The coefficient of the 4G dummy is also positive and statistically significant. The coefficients of these controls are similar in magnitude to those in our models using number of MNOs.

A4.17 As to the relationship between HHI and investment we find that this is always negative and statistically significant, implying that industry capex is lower in markets with higher HHI (i.e. more concentrated markets). The results vary across our preferred models, with our estimate of the long-run impact of an additional 100 points in HHI varying between -1.4% and -3.3%. For a typical four-to-three merger this would represent an impact of -8.4% to -19.8%.[64] This is comparable in magnitude to our estimate using number of MNOs, though the range of our estimates is wider. We believe that this could be explained by the sensitivity of our AB-GMM estimates to the instrument count (see discussion in paragraph A3.30). In our models using HHI, this sensitivity gets amplified by the additional instruments used to address the simultaneity issue between HHI and investment. This however does not seem to affect the direction and significance of our results.

$\Delta LogCapex_{t-2}$). Similarly, for HHI, we restrict the set of instruments to start from lag 2 (i.e. ΔHHI_{t-2}) – see paragraph A3.37 for an explanation on the choice of the lag for the HHI instrument.

[64] Four-to-three mergers in our data are associated with an average increase in HHI of 600 points.

Table A4.3: Main results for our investment model – HHI as market concentration measure

	[1]	[2]	[3]	[4]
Method:	**FE-IV2SLS**	**AB-IVGMM**	**AB-IVGMM**	**AB-IVGMM**
# of lagged GMM instruments		6	12	16
Year	-0.003 (0.003)	-0.009 (0.006)	-0.002 (0.005)	-0.002 (0.003)
Q2	0.014 (0.013)	0.008 (0.012)	0.009 (0.012)	0.014 (0.012)
Q3	0.048*** (0.011)	0.033*** (0.012)	0.041*** (0.010)	0.047*** (0.002)
Q4	0.112*** (0.015)	0.099*** (0.016)	0.105*** (0.014)	0.112*** (0.015)
Lag1_ln_capex_pop	0.421*** (0.039)	0.305*** (0.044)	0.320*** (0.041)	0.420*** (0.038)
Σ(Lag_ln_capex_pop)	0.763*** (0.039)	0.505*** (0.095)	0.446*** (0.078)	0.758*** (0.038)
Ln_gdp_pop	0.038 (0.079)	0.456** (0.180)	0.429*** (0.158)	0.037 (0.077)
4G dummy	0.048** (0.021)	0.075** (0.030)	0.053* (0.027)	0.049** (0.020)
HHI	-0.003** (0.002)	-0.017*** (0.004)	-0.014*** (0.003)	-0.004** (0.002)
LR impact	-0.014** (0.006)	-0.033*** (0.005)	-0.024*** (0.005)	-0.015*** (0.005)
# of obs	1800	1800	1800	1800
# of groups	30	30	30	30
F statistic				
F statistic 1st stage	10,526			
Wald statistic	6,436	2,913	2,167	7,742
R-square				

*Note: *p-value <0.1; **p-value <0.05; ***p<0.01. We use robust standard errors. These are shown within brackets.*

A4.18 We have considered the validity of our instrument for HHI. As already mentioned, a valid instrument ought to be correlated with the variable being instrumented, but uncorrelated with other factors that explain the dependent variable. The first criterion can be examined by looking at the F-statistic from the first stage regression of the 2SLS procedure.[65] An F-

[65] We acknowledge that this is only one element of what makes a good instrument – the other one being that the instrument should be independent of the dependent variable (i.e. uncorrelated with the error term).

statistic lower than 10 is indicative of a weak instrument.[66] In our model, the F statistic is above 10,000 (see table above), suggesting that a weak instrument problem is not present. We tested whether this result is sensitive to the choice of the lag of HHI. To test this, we re-ran the analysis using the lag of HHI four-quarters ago[67] as the instrument for HHI and get similar results (see robustness tests further below). For the second criterion, we could apply the Sargan and Hansen tests of over-identifying restrictions. However, as explained in Annex A3, these tests are not suitable for the data in our study and thus we have not placed weight on the results from these tests.

A4.19 With regards to the validity of our instruments for the lagged differenced investment variable, we have applied the test of no serial correlation. Again, all our AB-GMM models pass this test, suggesting that our instruments for the lagged differenced investment variable are valid.

Robustness tests

A4.20 To test the robustness of our results, we have carried out the same sensitivities as those applied to our investment models using number of MNOs – see paragraph A4.10. In addition, we have tested whether our results are robust to the choice of the lag of HHI as the instrument for HHI. We do this by estimating a variant of our AB-IVGMM model using the lag of HHI four-quarters ago as the instrument for HHI.[68]

A4.21 Table A4.4 presents the results of these tests. These suggest that our estimates are robust to a more parsimonious lag structure for lagged investment, the form of the time trend, the inclusion of other controls and the use of the lag of HHI four-quarters ago as the instrument for HHI.

A4.22 When we restrict the time horizon to exclude the period over which 3G spectrum was allocated in Europe, we find a weaker impact of changes in HHI on investment. This is in part expected given that standard errors of our estimates tend to increase with a reduced number of observations. This however does not affect our conclusion that we do not observe evidence of a positive relationship between market concentration and industry investment.

A4.23 Similar to our analysis for number of MNOs, we tried to separate out the effects of merger/exit from those of entry events. Again, our analysis shows that the effects are still positive implying that industry capex is lower in more concentrated markets. Note that we could not quantify the effect of entry events. This is because changes in HHI due to entry are spread over a number of quarters/years as the entrant builds up its customer base.

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[66] Stock J, Yogo M., "Testing for Weak Instruments in Linear IV Regression. In: Andrews DWK Identification and Inference for Econometric Models", Cambridge University Press, 2005, pp. 80-108.

[67] We use the four-period lag as we believe it is still likely to be a good predictor of current HHI (i.e. current HHI and HHI a year ago are likely to be correlated), while at the same time addresses the issue of simultaneity between HHI and average download speeds.

[68] We use the four-period lag as we believe it is still likely to be a good predictor of current HHI (i.e. current HHI and HHI a year ago are likely to be correlated), while at the same time addresses the issue of simultaneity between HHI and average download speeds.

A4.24 Furthermore, our robustness tests suggest that we cannot reject the hypothesis that the relationship between HHI and investment is non-linear as the quadratic term of HHI is significant across our sensitivities. However, these results do not provide support for an inverted U-shape relationship between HHI and investment as the relationship remains negative over the relevant range of HHI across our sample.

Table A4.4: Robustness tests on our estimate of the long-run impact of changes in HHI on investment

	FE-IV2SLS	AB-IVGMM		
# of lagged GMM instruments		6	12	16
0. Preferred specification	-0.014**	-0.032***	-0.023***	-0.015***
1. Lag structure	-0.018***	-0.031***	-0.024***	-0.019***
2. Spectrum holdings	-0.014**	-0.036***	-0.025***	-0.016***
3. Rurality of population	-0.014**	-0.033***	-0.023***	-0.016***
4. Population density	-0.014**	-0.032***	-0.023***	-0.015***
5. Form of time trend	-0.014**	-0.022***	-0.020***	-0.013**
6. Time horizon	-0.007	-0.025***	-0.018***	-0.008
7. Merger and entry effects	Entry: n/a Merger: -0.016*	n/a	n/a	n/a
8. Functional form	Sq term is significant	Sq term is significant	Sq term is significant	Sq term is significant
9. Instruments	-0.014**	-0.033***	-0.024***	-0.015***

Note: *p-value <0.1; **p-value <0.05; ***p<0.01. We use robust standard errors.

Conclusion on our analysis on investment

A4.25 Our panel data analysis suggests that country-level investment is lower in more concentrated markets. We find that moving from a 4-player market to a 3-player market reduces country-level investment by 13-19%. We get a similar result when using HHI as the measure of market concentration, though our range estimate is wider than for number of MNOs partly due to our use of instruments for HHI.[69]

A4.26 This finding is robust to different model specifications. In particular, we find that our estimate of the long-run impact of changes in market concentration on investment is robust to a more parsimonious lag structure for lagged investment, a quadratic time trend, the inclusion of other controls, and the use of the lag of HHI four-quarters ago as instrument for HHI.

A4.27 Given that we do not have data to control for the impact of 3G spectrum holdings on investment, we tested restricting the data to exclude the period over which 3G spectrum was allocated to MNOs. Although the estimated impact of market concentration seems to weaken, the results are broadly consistent with those using the unrestricted data. Moreover, these results do not alter our broader conclusion that we do not find evidence of a positive link between market concentration and industry investment.

A4.28 We also investigated whether our impact estimate could vary between entry and merger/exit events. Although we have few merger/exit events in our data, and therefore these more granular results should be treated with caution, our analysis shows that the effects are still positive - implying that incremental changes in market concentration are associated with reductions in industry capex.

A4.29 We note that our results are different to those in previous studies which have not found a statistically significant impact of market concentration on country-level capex (see Annex A1). We believe that this could be due to our expanded and more complete dataset on investment, as well as the inclusion of lagged investment and technology cycles as additional controls.

A4.30 While our analysis finds that country-level investment is lower in more concentrated markets, this is not necessarily a poor outcome for consumers. This is because there could be significant cost savings from less duplication of networks and economies of scale whereby less investment can achieve the same or better network quality outcomes. On the other hand, as noted in Section 3, if changes in market concentration adversely affect consumer outcomes, because firms' incentives to invest in network quality improvements declines, then this may be of more concern.

A4.31 In the following paragraphs we discuss our analysis of the relationship between changes in concentration and average download speeds.

Quality model

A4.32 In this subsection we present the results of our analysis on network quality. We first provide the results for our models using number of MNOs, and then those using HHI.

[69] Our results for the AB models are sensitive to the number of instruments. Given that we use more instruments in our AB models when using HHI as the measure of market concentration, our results for HHI tend to vary more than those using number of MNOs.

Results for number of MNOs

Main results

A4.33 Table A4.5 presents the results for our preferred model specifications for equation 2 (see Annex A3). The dependent variable in these models is the log of average download speeds. Column 1 of the table shows the results for our preferred model assuming no direct impact of market concentration on download speeds. This model implies that market concentration impacts download speeds solely through investment (i.e. there is no direct impact). Columns 2 and 3 of the table show the results for our preferred models assuming both a direct and indirect impact of market concentration on download speeds. Our preferred model in column 2 assumes that the direct impact is independent of the level of 4G coverage, while our model in column 3 assumes this impact may change once 90% coverage is achieved. For each of our preferred models, we report our estimates of the long-run indirect and direct impact of a change in the number of MNOs on the log of average download speeds.

A4.34 Following the approach set out in paragraphs A3.52 to A3.53, our preferred models include lags two to five of the investment measure as regressors. This is consistent with our expectation that there is likely to be a delay in the impact of investment on network quality and that this is likely to vary by investment type.

A4.35 The sign of the coefficients of the controls in our preferred models is as expected. The coefficient of the 4G dummy is positive and significant, implying that download speeds are higher during the 4G cycle relative to the 3G cycle. The coefficient of 4G coverage is also positive, though not significant, suggesting that download speeds increase with 4G coverage, as anticipated. The coefficient of GDP per capita is negative, suggesting that, once we control for other factors, positive shocks in GDP per capita lead to lower download speeds. This relationship could be explained by higher income levels driving higher network usage, which, for a given level of network capacity, may result in network congestion and thus in lower download speeds.

A4.36 With regards to the impact of market concentration we find the following:

a) A positive and significant **indirect impact** of changes in number of MNOs on download speeds. This means that more concentration (fewer networks) leads to lower industry investment, which in turn leads to lower download speeds. This impact is similar across our preferred models at around 5-7%, implying that one less player in the market reduces download speeds by 5-7%.

b) No significant **direct impact** of changes in number of MNOs on download speeds, even when controlling for 4G coverage. This suggests no significant impact of changes in number of MNOs on download speeds other than through investment.

Table A4.5: Main results of our quality analysis – Number of MNOs as market concentration measure

Method: FE	[1]	[2]	[3]

Year	0.393*** (0.019)	0.393*** (0.018)	0.386*** (0.020)
Q2	0.050*** (0.014)	0.050*** (0.014)	0.048*** (0.015)
Q3	0.093*** (0.015)	0.093*** (0.015)	0.091*** (0.016)
Q4	0.266*** (0.020)	0.266*** (0.012)	0.262*** (0.023)
Lag2_ln_capex	0.115** (0.053)	0.114** (0.053)	0.112** (0.053)
Lag3_ln_capex	0.096*** (0.029)	0.096*** (0.029)	0.092*** (0.029)
Lag4_ln_capex	0.098** (0.040)	0.098** (0.040)	0.098** (0.040)
Lag5_ln_capex	0.090* (0.050)	0.090* (0.050)	0.087* (0.049)
Ln_gdp_pop	-2.094*** (0.535)	-2.08*** (0.520)	-2.099*** (0.500)
4G dummy	0.451*** (0.078)	0.449*** (0.082)	0.453*** (0.078)
Post-90% coverage			0.440 (0.397)
Num_MNO		0.018 (0.101)	0.067 (0.111)
Post_90%_cov_num_MNO			-0.117 (0.114)
Σ(Lag_ln_capex)	0.400*** (0.126)	0.398** (0.125)	0.389*** (0.124)
Indirect impact	0.053*** to 0.074***	0.053** to 0.074**	0.051*** to 0.072***
Direct impact			
Ave. direct impact		0.018 (0.101)	0.067 (0.111)
Cum. impact post 90% coverage			-0.049 (0.139)
# of obs	960	960	960
# of groups	30	30	30
F statistic	500	487	479
R-square	0.60	0.61	0.60

*Note: *p-value <0.1; **p-value <0.05; ***p<0.01. We use robust standard errors. These are shown within brackets.*

Robustness tests

A4.37 To test the robustness of our results we have carried out the following sensitivities on our preferred models:

i) **Contemporaneous investment.** In our preferred specifications we assume a delay in the impact of investment on download speeds. To test the robustness of our results to the exclusion of contemporaneous investment, we have estimated a model that also includes current investment as a control.

ii) **Spectrum holdings.** Our preferred models exclude spectrum holdings as a control. In Section 3, we explain that spectrum holdings is a factor that may affect network quality. Therefore, to test the robustness of our results to the exclusion of spectrum holdings as a regressor in our models, we estimate a model which includes country-level 4G spectrum holdings as an additional control.[70]

iii) **Additional controls.** We also verify the robustness of our results to the exclusion of other factors that may affect network quality such as rurality of population and population density.

iv) **Interaction between market concentration and investment**. It could be argued that our estimates of the indirect impact of market concentration on download speeds could be driven by investment duplication in less concentrated markets. In other words, higher industry investment in countries with a larger number of competing networks could be a result of network duplication. To test this, we have interacted number of MNOs with lagged investment. A negative sign of the coefficient of this interaction would indicate that investment is more efficient in driving higher download speeds in more concentrated markets, whereas a positive sign would indicate otherwise.

A4.38 The results of these tests are presented in Table A4.6 below. They suggest that our findings are robust to the inclusion of contemporaneous investment and other controls such as spectrum holdings, rurality of population and population density.

A4.39 As to the interaction between lagged investment and number of MNOs, the sign of the coefficient of the interaction is negative albeit not statistically significant. This suggests that investment is no more efficient in driving higher download speeds in more concentrated markets than in less concentrated markets. This indicates that our estimates of the indirect impact of changes in number of MNOs on download speeds are not being driven by network duplication in less concentrated markets.

[70] We also tested a model that includes both the 4G dummy and country spectrum holdings but the latter is insignificant due to collinearity between the two variables (correlation factor between the two variables is above 0.8).

Table A4.6: Robustness tests on our estimate of the impact of a change in number of MNOs on average download speeds

	Σ(Lag_ln_capex)		Num_MNO		Cum Num_MNO post 90% coverage	Interaction: Num_MNO & investment	
Coverage interaction	No	Yes	No	Yes	Yes	No	Yes
0. Pref specification	0.398**	0.389***	0.018	0.067	-0.049		
1. Current investment	0.437***	0.423***	0.017	0.065	-0.048		
2 Spectrum holdings	0.392***	0.383***	0.022	0.072	-0.047		
3. Rurality of population	0.392***	0.383***	0.020	0.068	-0.048		
4. Population density	0.409***	0.399***	0.006	0.055	-0.061		
5. Interaction						-0.004	-0.003

*Note: *p-value <0.1; **p-value <0.05; ***p<0.01. We use robust standard errors.*

Results for HHI

A4.40 Table A4.7 presents our findings for our preferred models using HHI as the measure of market concentration. These models also use the log of average download speeds as the dependent variable and have the same controls as our models using number of MNOs. The only difference with respect to our models using number of MNOs is that to address the possible reverse link between HHI and download speeds we apply instrumental variable techniques (as discussed in Annex A3). Column 1 of the table show the results for our preferred model assuming no direct impact of market concentration on download speeds. That is, this model assumes that market concentration impacts download speeds only through its impact on investment. Columns 2 and 3 of the table present the results for our models assuming both a direct and indirect impact of market concentration on download speeds. Again, our model in column 2 assumes that the direct impact of market concentration is independent of 4G coverage, while our model in column 3 assumes that this may vary once 90% 4G coverage is achieved.

A4.41 The sign of the coefficients of the GDP per capita and 4G dummy are as expected and have a similar magnitude as those from our models using number of MNOs. The sign of the coefficient of the post-90% coverage dummy is negative albeit not significant. The discrepancy in the sign of the coefficient between our models using number of MNOs and HHI for the post-90% coverage dummy could be explained by the limited data (2 years' worth of data for most sampled countries) that we have post 90% 4G coverage. This is likely to impact the accuracy of our estimation for the coverage dummy variable.

A4.42 With regards to the impact of HHI on download speeds, our results can be summarised as follows:

a) We find that there is a negative and significant **indirect relationship** between HHI and download speeds. This means that more concentration (higher HHI) leads to lower investment, and this in turn leads to lower download speeds. Our estimate of this indirect impact is similar across our preferred models at -0.5 to -1.3% for a 100-point increment in HHI.

b) We find no statistically significant **direct impact** of a change in HHI on download speeds.

A4.43 Our findings are consistent to those from our analysis using number of MNOs. While we find a significant negative indirect impact of changes in market concentration on download speeds, we find no significant direct impact. Based on the average HHI increase associated with the four-to-three mergers in our sample, our results for HHI suggest that a four-to-three merger would reduce download speeds by 3% to 8%. This is similar in magnitude to our impact estimate using number of MNOs.

Table A4.7: Main results for quality analysis – HHI as the measure of market concentration

Method: FE-IV2SLS	[1]	[2]	[3]
Year	0.393*** (0.019)	0.395*** (0.019)	0.390*** (0.019)
Q2	0.050*** (0.014)	0.050*** (0.014)	0.051*** (0.014)
Q3	0.093*** (0.015)	0.094*** (0.015)	0.093*** (0.016)
Q4	0.266*** (0.020)	0.268*** (0.019)	0.266*** (0.022)
Lag2_ln_capex	0.115** (0.053)	0.115** (0.053)	0.106** (0.050)
Lag3_ln_capex	0.096*** (0.029)	0.097*** (0.030)	0.088*** (0.028)
Lag4_ln_capex	0.098** (0.040)	0.098** (0.040)	0.092** (0.038)
Lag5_ln_capex	0.090* (0.050)	0.090* (0.049)	0.083 (0.050)
Ln_gdp_pop	-2.094*** (0.535)	-2.122*** (0.506)	-2.166*** (0.504)
4G dummy	0.451*** (0.078)	0.457*** (0.081)	0.448*** (0.080)
Post-90% coverage			-0.441 (0.472)
HHI		0.006 (0.016)	-0.001 (0.014)

Post_90%_cov_hhi			0.014
Σ(Lag_ln_capex)	0.400*** (0.126)	0.401*** (0.126)	0.370*** (0.117)
Indirect impact	-0.006** to -0.013***	-0.006** to 0.013***	-0.005** to -0.012***
Direct impact			
Ave. direct impact		0.006 (0.016)	-0.001 (0.014)
Cum. impact post 90% coverage			0.013 (0.020)
# of obs	960	960	960
# of groups	30	30	30
F statistic (1st stage)		245	2,123
F statistic (2nd stage)	500		428
Wald statistic (2nd stage)		6,099	
R-square	0.60	0.60	0.59

*Note: *p-value <0.1; **p-value <0.05; ***p<0.01. We use robust standard errors. These are shown within brackets.*

A4.44 We have considered the validity of our instrument for HHI. Looking at the relationship between our instrument and the instrumented variable, the F-statistic from the first stage regression of our models suggests that the instrument is not weak (i.e. F-statistic is well above 10). We have also tested the sensitivity of our results to the choice of the lag by re-running our analysis using the lag of HHI four quarters ago as the instrument for HHI. As explained further below, our results are robust to the use of the lag of HHI four quarters ago as the instrument for HHI.

Robustness tests

A4.45 To test the robustness of our results we have carried out the same sensitivities as for our analysis using number of MNOs – see paragraph A4.37. In addition, we have tested whether our results are robust to the choice of the lag of HHI as an instrument for HHI. We have done so by estimating a variant of our FE-IV model using the lag of HHI four quarters ago as the instrument.[71]

A4.46 Table A4.8 presents the results of these tests. It suggests that our findings are robust to the inclusion of contemporaneous investment and other factors that may also affect network

[71] We use the four-period lag as we believe is still likely to be a good predictor of current HHI (i.e. current HHI and HHI a year ago are likely to be correlated), while, at the same time, addresses the issue of simultaneity between HHI and average download speeds.

quality such as spectrum holdings, rurality of population and population density. Our results are also robust to the use of the lag of HHI four quarters ago as instrument for HHI.

A4.47 Our robustness tests also indicate that the interaction between investment and HHI is not statistically significant. This suggests that investment is no more efficient (in driving higher download speeds) in less concentrated markets. Again, this means that our results are not being driven by higher investment due to network duplication in less concentrated markets.

Table A4.8: Robustness tests on our estimate of the impact of changes in HHI on average download speeds

	Σ(Lag_ln_capex)		HHI		HHI post 90% coverage	Interaction: HHI and investment	
Coverage interaction	No	Yes	No	Yes	Yes	No	Yes
0. Pref specification	0.401***	0.370***	0.006	-0.001	0.013		
1. More lags	0.442***	0.405***	0.006	-0.001	0.013		
2. Current investment	0.439***	0.408***	0.006	-0.001	0.013		
3. Spectrum holdings	0.396***	0.366***	0.005	-0.002	0.012		
4. Rurality of population	0.395***	0.361***	0.005	-0.003	0.015		
5. Population density	0.410***	0.357***	0.006	-0.003	0.015		
6. Interaction						0.0003	0.0001
7. Instrument	0.394***	0.377***	0.0004	-0.001	0.014		

*Note: *p-value <0.1; **p-value <0.05; ***p<0.01. We use robust standard errors.*

Conclusion on our analysis of network quality

A4.48 Our panel data analysis on network quality suggests a negative relationship between market concentration and download speeds. We find that this relationship is an indirect one through the impact that higher market concentration has on investment. That is, we find that an increase in market concentration tends to be associated with a reduction in industry investment, which in turn means lower download speeds. We find no evidence of a significant direct impact of market concentration on download speeds (e.g. by affecting the way that mobile spectrum is utilised).

A4.49 This finding is robust to different measures of market concentration (i.e. number of MNOs, HHI) and the inclusion of contemporaneous investment, and other controls (i.e. spectrum holdings, rurality of population and population density). Our results are also robust to the use of the lag of HHI four quarters ago (as opposed to the lag of HHI two quarters ago) as an instrument for HHI.

A4.50 We have also examined whether our findings could be in part driven by investment duplication in less concentrated markets. We have tested this by interacting lagged investment and the market concentration measure. In particular, this tests the hypothesis that investment could be more efficient in driving higher download speeds (potentially due to economies of scale) in more concentrated markets. Our analysis does not support this hypothesis as we find that the efficiency of investment (proxied by the interaction term) is not distinct between more and less concentrated markets.

A5. Merger specific counterfactual analysis methodology

A5.1 In Section 3, we said that changes in market concentration could impact quality indirectly by affecting firms' ability and incentives to invest in network quality improvements. In addition, we said that changes in market concentration could also affect quality through direct means, by affecting consumer usage, network congestion and the way that mobile spectrum is utilised.

A5.2 To investigate this, we use both panel data techniques and counterfactual methods. Panel data methods, as outlined above in Annex A3, are useful for obtaining a broad view from the data of the impact of changes in market concentration, on investment and quality outcomes. They provide a mechanism to estimate the average effect on investment and quality outcomes across a number of different mergers (as well as entry events). However, the impact of any merger will depend on many factors that are unique to the merger in question and an identical change in market concentration in different markets (arising from a merger) can have very different effects depending on the market context, the underlying rationale for the transaction and remedies applied.

A5.3 For example, in mobile markets with a small number of operators, MNOs may choose to merge to directly reduce competition and exploit increased market power. On the other hand, MNOs may choose to merge to sharpen competition by taking advantage of efficiency savings from reduced duplication of networks. Put differently, the effect of a merger on quality outcomes does not arise simply from the change in market structure – the incentives that drive the merger, the pre-merger share of merging parties, local conditions and the remedies imposed by the relevant merger control authority are all important factors in how investment and quality outcomes turns out post-merger. This is why counterfactual methods to explore the impact of individual mergers are useful because they allow us to complement our findings on the average merger effect with some individual case studies that allow for variation in the merger effect between different merger events.[72]

A5.4 As set out in Section 7 of the main report, we favour the use of the synthetic control, a counterfactual technique, in the country-specific analysis to explore the impact of individual four-to-three mergers on investment and quality outcomes. This method provides a means to estimate the counterfactual investment or quality outcomes in the absence of a merger. The difference between this counterfactual and the actual post-merger investment or quality outcomes provides an estimate of the effect of the merger on these outcomes. In this annex, we provide a more detailed discussion of why we favour

[72] Note, we are unable to quantitatively disaggregate the contributions of various merger-specific contextual factors and the effect of the merger itself. This means therefore that in analysing these four-to-three mergers, we cannot use the results from a particular merger to comment generically about for example, the effectiveness of different remedies.

[73] Abadie, A., & Gardeazabal, J. (2003). The economic costs of conflict: A case study of the Basque Country. *American economic review, 93*(1), 113-132.

the synthetic control method. We then discuss the method and the robustness tests used to test the sensitivity of the models. We expect this annex to be of interest mainly to practitioners in the field wanting an in-depth understanding of our analysis.

Synthetic control

Why we favour the synthetic control and the assumptions underpinning it

A5.5 As set out in Section 7, to assess the impact that a merger had on investment and quality outcomes, it is necessary to consider what outcomes are likely to have resulted had the merger not occurred. To minimize bias (i.e. the risk that the effect we identify is due to factors other than the merger), we need to establish an appropriate control group or counterfactual. To do this, different study designs use different methods to mimic the trend in the outcome variable in the absence of the intervention.

A5.6 Once such method is Difference-in-Difference (DD). DD estimates the treatment effect by taking the difference between the change in the treated unit and the change in the control units in the periods before and after the intervention. Provided that treated and control units had parallel pre-treatment trends, and there were no other events affecting one unit but not the other, a constant difference would be expected to continue in the post intervention period. If there is a difference between the treated and control unit changes in the post intervention period, this can be interpreted as a treatment effect. If pre-treatment trends are not parallel, or an event occurred that affected only one of the units, some of the difference between the units will be the difference in the trend rather than the effect of the intervention, and the estimate of the treatment effect will be biased.

A5.7 In our study, we have one country that has experienced a merger (i.e. a single treated unit) and a larger set of countries that have not experienced a change in number of MNOs (through a merger or entry/exit event). We consider the DD method inappropriate for this context for the following reasons:

i) *The common trends assumption required for DD Is unlikely to hold* (and it is also not possible to properly test whether it does – see below on standard errors). While we have limited the panel of countries in our dataset to Europe because MNOs in Europe are more likely to face similar market conditions, it is still likely to be difficult to establish concretely (under the DD method) whether the parallel trends assumption is met. In particular, we cannot discount that there may be some unobserved confounding factors, for example political decisions, that may have time-varying effects on the outcomes we are interested in, which means that the parallel trends assumption is violated. This is especially true given that the research question we are investigating involves solely one country that has experienced a merger (i.e. a single treated unit) and a larger set of countries that have not experienced a change in number of MNOs (through a merger or entry/exit event). We note that a confounding event in a country experiencing a merger is likely to have a larger impact on estimated treatment outcomes when it is the only country in the treated group (compared to a situation in which there are multiple countries in the treated group to dilute the impact of the confounding event on treated outcomes);

ii) *Equally weighting all countries in the control group is unlikely to provide a good comparison for the single treated unit affected by the intervention*. Where there is a single treated unit, it is less likely that the unweighted average experience of a group of control countries will be a good comparator for this one country (compared to a situation where there are multiple treated units, and the research question is to approximate average outcomes across this group of treated units). Instead, it is more likely that a small number of countries in the control group provide a closer approximation to outcomes in the country experiencing the merger (and indeed this is what we find in our analysis – see Annexes 6, 7 and 8 detailing results of our country-specific studies); and

iii) *It is not possible to test whether any estimated effect is statistically significant* (because there is no variation within the treated group to generate standard errors). An underlying assumption to regression methods like DD is not just large sample sizes but also many occurrences of the treatment. Without that, it is difficult to make statistical inference as standard errors are based on variation within the treated group. With only one country in the treated group, it is not possible to generate standard errors for regression estimates: and so it is not possible to make statistical inference about any estimated results.

A5.8 Synthetic control methods relax the restrictive parallel trends assumption that the DD method relies upon and allow for both observed and confounding unobserved characteristics to vary with time. The synthetic control framework was first proposed in Abadie and Gardeazabal (2003)[73] and later built on in the seminal paper by Abadie, Diamond & Hainmueller (2010).[74] This contributed to the comparative case studies literature and offers an approach better suited for situations where only a single or a small number of units are subject to a treatment while a larger set of units remain untreated.

A5.9 Synthetic control methods provide a data driven procedure to select the control group units that are used to estimate a counterfactual. Specifically, the method assigns weights to each unit (i.e. each country in our analysis) in the control group based on how closely they resemble the treated unit for a set of covariates. This is in contrast to the DD approach where all units have equal weights in a standard regression framework. As in the DD approach, the control group is made up of countries in our sample that did not experience a change in the number of MNOs.

A5.10 The synthetic control method relies on a number of underlying assumptions:[75]

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[73] Abadie, A., & Gardeazabal, J. (2003). The economic costs of conflict: A case study of the Basque Country. *American economic review, 93*(1), 113-132.
[74] Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American statistical Association, 105*(490), 493-505.
[75] See: Abadie, A (2020). Using Synthetic Controls: Feasibility, Data Requirements and Methodological Aspects. *Forthcoming in Journal of Economic Literature* https://economics.mit.edu/files/17847, Doudchenko, N. & Imbens, G (2016).

a) *No treatment in donor countries*. No country in the pool of potential donor countries (the group of countries from which the counterfactual unit is constructed from) should have experienced a change in number of MNOs (through a merger or entry/exit event). As discussed in Annex A2, for each of the mergers we investigate, we exclude countries that experienced a change in number of MNOs from the pool of potential donor countries to satisfy this assumption;

b) *No spillover from treated to control*. The effect of a merger should not spillover on to MNOs in other countries included in the control group. This assumption appears to be plausible given that MNOs that fall under larger telecoms group are generally run as separate business entities. We recognise though that spillover effects could occur due to a merger impacting on flows of international investment or resulting in knowledge spillovers. This means that this assumption may not fully hold to the extent that many MNOs fall under larger telecoms groups that operate internationally and may be subject to capital market constraints;

c) *No effects before treatment applied*. The merger should not affect investment and quality outcomes before the merger takes place. If this assumption does not hold then we would not be evaluating the full impact of the merger (and the treated unit may be matched to a control which is a poor predictor of post-treatment outcomes. Whilst we cannot test this assumption directly, we are more likely to be able to detect issues if we have data on a reasonably long pre-intervention period. If a good fit can be generated between the pre-intervention trends in the treated area and the synthetic control for a reasonably long pre-intervention period, the less likely it is that this bias will arise. In our study, we match against twelve quarters pre-intervention. We consider that this provides a a reasonably long pre-intervention period to guard against bias;

d) *No post-treatment shocks in treated unit*. There should be no large shocks in the country with a merger other than the merger itself that will affect investment and quality outcomes differently to that of the control group. If there was a large shock to the country with a merger post-merger that the control group did not experience, then the effect picked up by the analysis would be biased. In the context of our analysis of particular mergers, there is less certainty over results further into the post-merger period as the timeframe for these shocks occurring increases. Based on when the four-to-three mergers occurred, our analysis is focused on a period of 4-5 years after a merger; [76] and

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Balancing, regression, difference-in-differences and synthetic control methods: a synthesis. NBER Working Paper Series: Working Paper 22791. http://www.nber.org/papers/w22791. Craig, P. (2015). Synthetic controls: a new approach to evaluating interventions. What Works Scotland Working Paper found at http://whatworksscotland.ac.uk/publications/synthetic-controls-a-new-approach-to-evaluating-interventions/.

[76] Ideally, there should also be no large shocks to the countries in the synthetic control. This is often however considered less of an issue because a shock in a single country may not have a large impact unless the country has a large weight attached to it in the estimation. In these circumstances, to test the sensitivity of our results, we carry out a number of robustness tests, which are discussed in this annex.

e) *Donor pool outcomes sufficiently similar to treated*. The values for investment/quality outcomes for the country with a merger should not be outside any linear combination of the values for the donor pool. This makes it possible that the counterfactual outcome of the country with a merger can be approximated by a weighted combination of the countries in the control group. This assumption relies on ensuring that the outcome variables of the country with a merger are not outliers. There are a few cases where it was not possible to create a weighted combination of the countries in the control group to approximate the country with a merger. Where this occurs, we state this and do not present results.

A5.11 In particular, the credibility of the synthetic control result relies on achieving a good pre-intervention fit for the outcome of interest between the treated unit and the synthetic control. If a good pre-intervention fit is established over an extended period of time, a discrepancy in the outcome variable following the intervention can be interpreted as an intervention effect. It is critical though that the synthetic control is built from a pool of potential controls that are similar to the treated unit. In the subsequent paragraphs, we discuss the approach taken to synthetic control estimation.

Synthetic control estimation

A5.12 In the context of our study, the synthetic counterfactual approach estimates the merger effect in a particular country by subtracting the weighted average of outcomes experienced by countries in the control group from the outcome experienced by the country of interest, where the weights used are chosen to minimise the difference between pre-treatment outcomes in the synthetic counterfactual compared to the treated unit.

A5.13 There are three steps to this process. First, we estimate a set of weights to minimise the distance between the weighted average of each chosen pre-treatment characteristic (covariate) and its value in the treated unit. At this stage, there are different weights for each country and for each covariate, with the weights for an individual covariate summing to 1 across all the countries in the control group. Next, we estimate a set of weights to apply to the covariates themselves to arrive at an overall weighting for each country in the control group. These weights are chosen to minimise the distance between the value of the outcome variable in the pre-treatment period for the synthetic counterfactual and the treated unit. Finally, we construct the synthetic counterfactual in the post-treatment period by taking the weighted average of the outcome variable across the control group using these overall country weightings.

A5.14 Formally, the synthetic control is a two-step optimisation process. First, we choose the optimal weights W^* to apply to each untreated unit by minimising the following expression:

$$\underset{W}{\operatorname{argmin}}(X_T - X_{UT}W)'V(X_T - X_{UT}W)$$

A5.15 Where:

a) X_T is a vector of covariates for the treated unit;

b) X_{UT} is a matrix of covariates for each untreated unit;

c) W is a vector of weights for each untreated unit; and,

d) V is an arbitrary symmetric and positive semidefinite matrix.

A5.16 Note that $W^* = f(V)$.

A5.17 Second, we choose the optimal weights V^* to apply to each covariate. We choose this based on goodness-of-fit, where the mean square prediction error is minimised. Formally, V^* is chosen from the set of diagonal positive definite matrices to minimise the following expression:

$$\underset{V}{\operatorname{argmin}}\big(Y_T - Y_{UT}W^*(V)\big)'\big(Y_T - Y_{UT}W^*(V)\big)$$

A5.18 Where:

a) Y_T is a vector of pre-treatment outcomes for the treated unit;

b) Y_{UT} is a matrix of pre-treatment outcomes for each untreated unit;

c) $W^*(V)$ is the vector of weights chosen in the previous step; and,

d) V is a diagonal positive definite matrix.

A5.19 We implement this using the synth and synth_runner packages in Stata.

Specification selection

A5.20 To obtain the country-specific weights described above, we need a set of covariates that are predictive of the outcome variables. These may include observations of the outcome variable itself in the pre-treatment period.

A5.21 We consider a range of potential covariates in our analysis including the covariates included in our panel analysis – GDP per capita, population density, and the rurality of the population. In addition, we also include as covariates financial indicators and macroeconomic variables that could influence the investment and quality outcome variables. These are HHI, average revenue per subscriber, revenue, and unemployment rate.

A5.22 We use a sub-set of these covariates to determine the synthetic control weights in each model. There is no consensus on the method of choosing the combination of covariates that should be used to estimate the synthetic control weights. In this study, we use the same approach as in GSMA (2017) and also commonly used in the general programme evaluation literature. We select variables based on the group of covariates which minimises the deviation between the outcomes experienced by the synthetic and the treated unit in the pre-merger period, measured in terms of the the root mean square prediction error (RMSPE). This method typically results in a different sub-set of covariates being chosen to determine the synthetic counterfactual weights in each case.

A5.23 Note that RMSPE is an absolute measure of predictive error rather than a relative measure. Therefore, it is vital to compare this to the magnitude of the value that is being predicted. As an example, a prediction with an RMSPE of 10 would be a poor estimator of a true outcome value of 20 but a very good estimator of a true outcome value of 2,000. If the synthetic control closely matches the development of the outcome variable (i.e. industry capex or average download speeds) in the treated country before the merger, we assume that this would have continued to be the case post-merger absent the merger.

A5.24 In addition to RMSPE values for the outcome variable pre-merger, we also present and compare the values of the covariates used to construct the weights for the country with a merger and the synthetic counterfactual. This can also provide an indication of how good a match the counterfactual estimate is by showing how similar the synthetic and treated units are in terms of variables which are predictive of the outcome variable.

A5.25 In testing combinations of covariates, we use a three-year pre-treatment period[77]. Most of our preferred specifications include a small number of pre-merger observations of the outcome variables and a number of covariates – see Annexes A6 to A8 for more detail of the results from our analysis.

A5.26 We recognise that there are alternative approaches that are used in applied studies that use synthetic control methods[78]. Another common approach is to solely match against all pre-treatment observations of the outcome variable.[79,80, 81] Directly matching on all pre-treatment outcomes controls for all unobservable and endogenous variables that may affect the dependent variables in the pre-treatment period. However, this comes at a trade-off. Kaul et al. (2015)[82] show that matching solely against all pre-treatment outcomes leaves other covariates that may be included in the matching process irrelevant. They suggest therefore that this may, in some cases, result in biased results. In particular, the study shows empirically that there are notably different results in matching solely against all pre-treatment outcomes versus matching against pre-treatment outcomes and other covariates.

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[77] The three-year pre-treatment period was chosen for our quality analysis because for all our mergers, we wanted to have a consistent pre-treatment period and the maximum pre-treatment quality data we held for our earliest merger was three years. In the case of investment, we wanted to ensure that our analysis was consistent with our quality analysis and because recent investment data is likely to be more relevant to predict future investment.

[78] Where the data sample is large, it can be split into two groups, a training dataset and validation dataset. The training dataset can then be used to find the optimal covariates and specification, which can be used on the validation dataset to provide an unbiased evaluation of the model fit. However, this is not possible for our study due to the limited dataset available.

[79] Hinrichs, P. (2012). The effects of affirmative action bans on college enrolment, educational attainment, and the demographic composition of universities. *Review of Economics and Statistics, 94*(3), 712-722.

[80] Billmeier, A., & Nannicini, T. (2013). Assessing economic liberalization episodes: A synthetic control approach. *Review of Economics and Statistics, 95*(3), 983-1001.

[81] Bohn, S., Lofstrom, M., & Raphael, S. (2014). Did the 2007 Legal Arizona Workers Act reduce the state's unauthorized immigrant population?. *Review of Economics and Statistics, 96*(2), 258-269.

[82] Kaul, A., Klößner, S., Pfeifer, G., & Schieler, M. (2015). Synthetic control methods: Never use all pre-intervention outcomes together with covariates.

A5.27 Given that there is no consensus in this area, we include the matching process using all pre-treatment observations (not just the three-year pre-treatment period) as a robustness test. We find results from these two approaches to be consistent.

A5.28 Given that the timeframe of many of our mergers covers the period when 4G networks began to be rolled out, we conduct the synthetic control analysis using standard time and 4G rebased time. For the latter, we normalise time for each country relative to when 4G networks began to be rolled out. This allows us to isolate the impact of the merger on investment and quality outcomes, whilst controlling for mobile technological cycles. In particular, this allows us to normalise across countries (i) the expectation and willingness to pay from consumers for new services and better network quality; and (ii) the timing of MNOs investing in these technologies. It is worth noting though that this is only done where there is sufficient data remaining after rebasing for all countries as part of the analysis. This is illustrated in Figure A2.7.

Placebo tests

A5.29 A drawback of the synthetic control method is that the usual statistical assessment of standard errors is not possible. Instead robustness tests are carried out through placebo tests as proposed in Abadie, Diamond & Hainmueller (2010 and 2015). [83]

A5.30 The placebo test comprises of running the synthetic control method for all countries in the control group in turn, treating them as if they experienced a merger at the same time as the real merger took place.

A5.31 We then compute a ratio of the post-merger RMSPE to the pre-merger RMSPE for each country. The post-merger RMSPE is equivalent to the average magnitude, i.e. the absolute value, of the merger effect. We use a ratio relative to the pre-merger RMSPE to take into account the accuracy of the synthetic control match.[84] If the ratio is higher in the treated country, compared to the placebo ratios for other countries in the control group, then this suggests that the estimated merger effect (i.e. the difference between actual outcomes and the outcomes in the synthetic unit) is statistically significant. Conversely, if the placebo ratios for the control group countries (i.e. countries that had not experienced a change in number of MNOs) are greater than the treated country, this would suggest the estimated merger effect obtained from the synthetic control is not statistically significant. We use a rule of thumb that results are statistically significant when they are ranked 1st or 2nd in the placebo tests.

Robustness tests in the synthetic control estimation

A5.32 We conduct a number of robustness checks to test the sensitivity of results to changes in the model. These robustness tests can be divided into two categories; changes to the covariates and changes to the control group. For these robustness tests we report both the

[83] Abadie, A., Diamond, A., & Hainmueller, J. (2015). Comparative politics and the synthetic control method. *American Journal of Political Science, 59*(2), 495-510.
[84] See footnote 73 and 74

magnitude of estimated merger effects and the implied level of significance based on the post-/pre-merger RMSPE ratio rank.

A5.33 The selection of the preferred specification is decided by the covariates that resulted in the minimum pre-merger RMSPE. The alternative specifications, i.e. where chosen variables were omitted and a range of alternative variables were tested, act as robustness tests for changes in model specification.

A5.34 For the preferred specification, we test the sensitivities of our results by restricting the countries available to be selected in the donor pool (the group of countries from which the counterfactual unit is constructed from). This comprises two tests:

a) First, we omit the countries that were given the greatest weights to test how reliant results are on particular control group countries. If results are vastly different after removing, for example, the country with the greatest weight, this might be an indication that the results are driven by a single country and therefore are not robust; and

b) Second, we restrict the control group to countries that are 4-player markets. As in the DD, it may be the case that these countries are most likely to represent the competitive conditions absent the merger in the countries that we study. However, note that, regardless of this hypothesis' validity, accuracy of the pre-merger synthetic unit will weakly worsen. This is because all countries available in the restricted donor pool are also available in the unrestricted donor pool and so in restricting the pool, we are in effect restricting the data available for the algorithm to work effectively.[85]

A5.35 Finally, we test two alternative matching processes:

a) First, we match against solely the three-year pre-merger observations of the outcome variable with no covariates included as described in paragraphs A5.26 and A5.27. This is suitable for both our quality and investment analysis; and

b) Second, specifically for the investment analysis where a longer time series was available, we matched against the full extended pre-treatment period of the outcome variable with no covariates included. This matching process was not suitable for our quality analysis as we did not have data beyond the three-year pre-merger period to do this test. It is worth noting though that this robustness test for our investment analysis does need to be treated with caution because given the industry we are studying, it ignores a number of technology cycles that occur at different points in time for each country. In addition and more importantly, given that our dataset spans almost a twenty-year period, almost all countries experienced some change in number of MNOs during this time. Excluding all affected countries would have significantly limited our control group. For our test therefore we maintain the same control group as the primary analysis as we cannot feasibly exclude all affected countries.

[85] Alternatively, this tests the validity of the hypothesis that 4-player markets more accurately reflect conditions in the country experiencing a merger. Restricting the control group countries to 4-player markets assumes the hypothesis to be true and we would expect the results to be similar to that of the unrestricted control group.

A6. Austria – Hutchison/Orange – 2012

Purpose of this annex

A6.1 This annex presents the detailed modelling results and statistical tests from our counterfactual analysis (using the method discussed in Section 7 of the main report and Annex A5) for the Hutchison/Orange merger in 2012 in Austria. The annex is structured as follows:

a) First, we set out the context of the merger;

b) Second, we set out the detailed results and robustness tests of the impact of this merger on investment outcomes;

c) Third, we set out the detailed results and robustness tests of the impact of this merger on average download speed outcomes; and

d) Finally, we provide a brief discussion of our results for the Hutchison/Orange merger in 2012 in Austria.

A6.2 Our counterfactual analysis suggests the merger is associated with a temporary reduction in download speeds compared to the counterfactual, but this effect disappears after two years. This effect is statistically significant and robust to a number of sensitivities. Our results on the impact of the merger on investment are inconclusive because of data limitations.

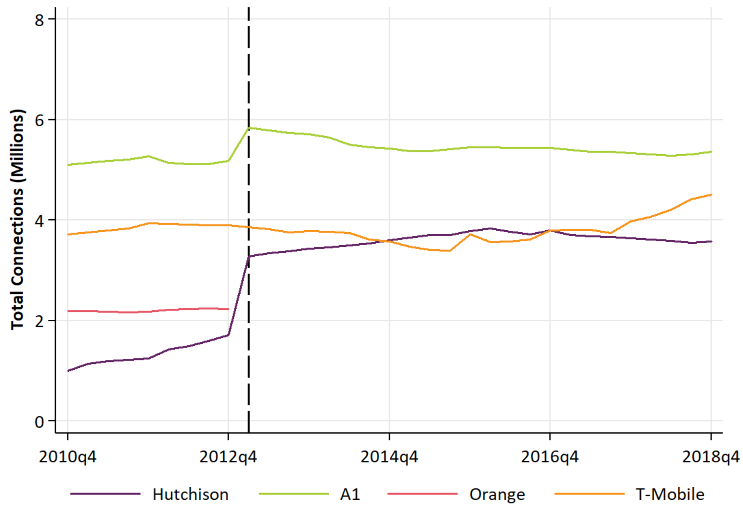
A6.3 We expect that paragraphs A6.9 to A6.22 to be of interest mainly to practitioners in the field wanting an in-depth understanding of our analysis.

Context of the merger

A6.4 In December 2012, the European Commission (EC) approved the acquisition of Orange by Hutchison following a Phase II Merger Investigation, with the deal completed in January 2013. This reduced the number of MNOs in Austria from four to three. Just prior to this merger being approved, a second transaction was also authorised in which the Orange sub-brand Yesss! was spun off to A1.

A6.5 Figure A6.1 shows the market shares of each of the MNOs measured by total connections. Prior to the merger, Hutchison and Orange were the two smallest MNOs in the four-player market, which included rivals A1 (the market leader) and T-Mobile. Post-merger, the combined entity was still the smallest MNO in terms of connections but, the merger overall created a more symmetric market in terms of total connections by MNO.

Figure A6.1: Austria – Total connections by MNO



Source: GSMA Intelligence

A6.6 In its assessment, the EC found that Hutchison and Orange were close competitors and Hutchison was viewed as an important competitive constraint in the market. Given the EC's concerns with the loss of a competitive constraint in the market, the merger was solely approved on the basis of three remedies:

a) Hutchison committed to divest spectrum to a potential new mobile network operator and to provide national roaming, collocation rights on existing Hutchison sites, and the sale of sites to this potential entrant;

b) Hutchison agreed to host up to 16 MVNOs, which would have access up to 30% of its capacity on pay-as-you-go-terms; and

c) Hutchison committed not to complete the acquisition of Orange before it entered into a wholesale access agreement with at least one MVNO.

A6.7 The first commitment did not bite as there was no new MNO wanting to enter the market. In accordance with the third commitment, Hutchison signed a MVNO agreement with UPC, primarily a cable company. However, in practice the competitive constraint imposed by the MVNO remedies was delayed as UPC did not enter the market until December 2014 and other MVNOs did not enter the market until 2015.

A6.8 It is worth noting that in December 2017, T-Mobile announced that it would acquire UPC which was approved by the European Commission in July 2018.

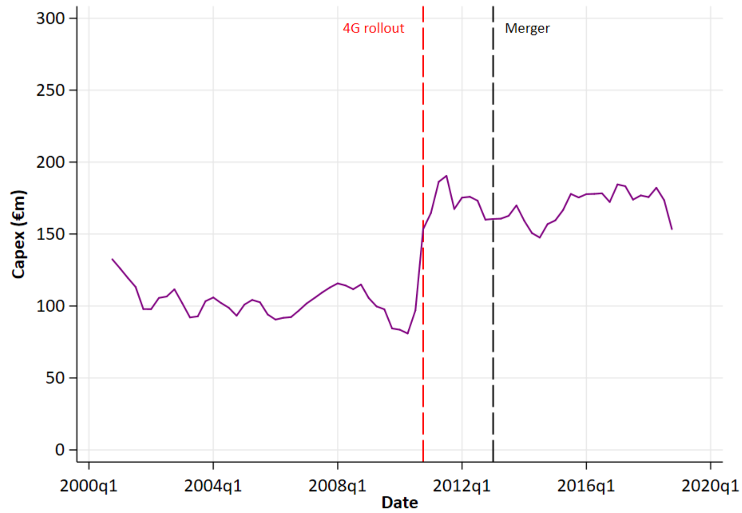
Analysis on investment

A6.9 Our results on the impact of the merger on investment are inconclusive because of data limitations.

A6.10 In particular, we are unable to construct a synthetic counterfactual that matches the pre-merger outcomes sufficiently well to provide a useful guide to the post-merger effect. This is because industry capex in Austria is significantly higher than in the countries in the control group, at least from 2010 onwards when there appears to be a level change in expenditure per quarter. This is likely due to the start of 4G rollout in Austria towards the end of 2010 which could have required a higher level of initial and ongoing investment (see Figure A6.2 below).

A6.11 This pattern of industry capex in Austria, means that Austria's industry capex is considerably higher than the capex of countries in the control group. This violates assumption (e) as set out in paragraph A5.10 (i.e. outcomes for treated unit should not be too dissimilar from control group) and means that that we cannot create a weighted combination of the countries in the control group to approximate the country Austria.

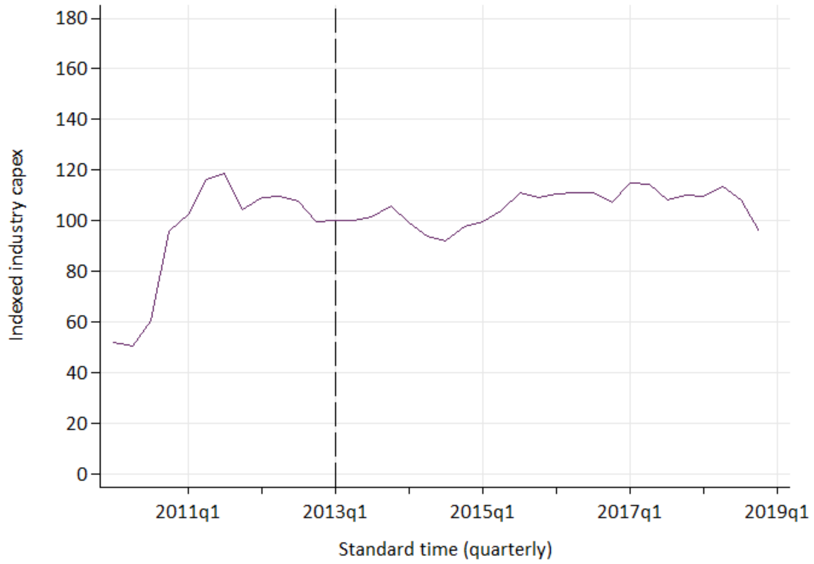
Figure A6.2 – Total industry capex in Austria



A6.12 To try to get around this issue, we attempted to analyse the effect of the merger on an indexed version of industry capex (where investment is set to a base level at the time of the merger studied) – see Annex A2. Once indexed in this way, the values for Austria are no longer outliers. However, Figure A6.3 shows that the jump in industry capex is still very much present. This may make it difficult to identify a synthetic control which is a good match for Austria for the whole of the pre-merger period.[86]

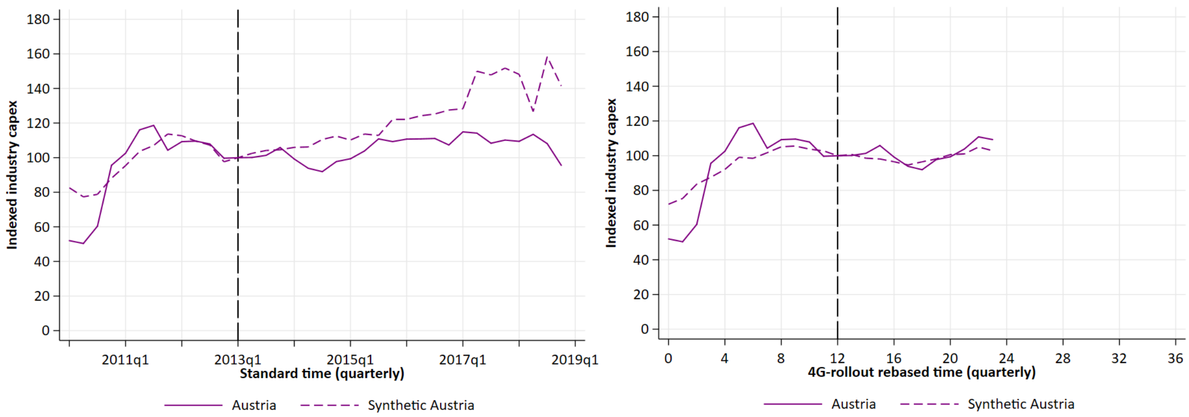
[86] We note that after rebasing around 4G rollout, only 11 countries were available in the control group.

Figure A6.3 – Trends in industry investment index, Austria



A6.13 We attempted to construct a synthetic control for indexed industry capex using both standard time and 4G rebased time. The RMSPE was high relative to the outcome variable (14.44 and 14.43 respectively compared to a pre-merger average indexed industry capex of ~94.4), even once accounting for 4G rollout. A graphical illustration of results for both models shows a very poor fit in the earlier periods of our control period, which suggests assumption (c) and (e) in paragraph A5.10 may not be satisfied. Specifically, we cannot generate a good fit between the treated unit and the synthetic control based on a linear combination of the donor pool countries for a long enough pre-merger period, therefore the treated unit may be matched to a control which is a poor predictor of post-treatment outcomes. This can be seen in Figure A6.4.

Figure A6.4 – Synthetic control results, industry capex index (Austria)[87]



Source: Ofcom analysis

[87] The time axis on the right-hand panel represents the quarter relative to the start of 4G rollout. This has been standardised and, for simplicity, set to zero at the origin.

A6.14 We are therefore unable to assess the effect that the Hutchison/Orange merger in Austria had on industry investment and turn instead to the effect of the merger in Austria on average download speeds.

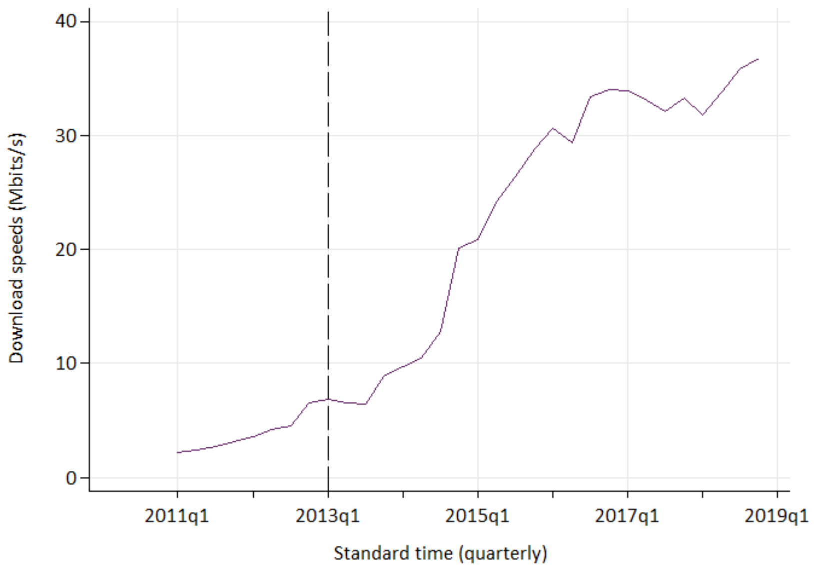
Analysis on quality

A6.15 The main finding from our analysis is that average download speeds in Austria are lower than the synthetic control group for the first two years after the merger. This effect is statistically significant and robust to a number of sensitivities. However, this effect disappears for the years studied beyond that.

Specification

A6.16 Figure A6.5 shows the trends in download speeds in Austria. We observe that average download speeds increase across the whole time period from 2011 to 2018 with a faster rate of growth from 2014 to 2016, shortly after the merger in 2013. In our analysis, we try to estimate a counterfactual for what would have happened to average download speeds in Austria in the absence of a merger using synthetic control methods. As outlined in Annex A2, the dataset on download speeds that we had access to was only available from 2011, two years prior to the merger in Austria.

Figure A6.5 – Trends in download speeds, Austria



A6.17 Based on the approach outlined in Annex A5, our preferred specification for the synthetic counterfactual in this case incorporates one observation of the dependent variable and two covariates – GDP per capita and the proportion of the population living in rural areas. The covariates included in this specification match closely (see right-hand panel of Table A6.1) and the pattern of average download speeds in the synthetic unit closely matches those observed in Austria in the pre-merger period (see Figure A6.6). This is reflected in the low pre-merger RMSPE value of 0.163 relative to average pre-merger outcome values of ~4.0.

A6.18 Based on these covariates, we obtain the country-weights as set out in the left-hand of Table A6.1. The synthetic Austria is made up of Malta and Romania, with Romania dominating with an 82% weighting. These countries do not intuitively feel the closest to the Austrian market. Table A6.2 below presents summary statistics for the covariates included in this specification for Austria and each of the countries that have been assigned weight in estimating the synthetic counterfactual. This shows that Austria is well matched to the covariates included in the specification and does not have outlier values. This therefore suggests that the assumptions as set out in Annex A5 are satisfied. In addition, we have satisfied ourselves that the change in these variables is relatively stable after the merger, including for Malta and Romania. Notwithstanding, given that we recognize that a high weight towards a single or small number of countries creates a risk that shocks to download speeds in Romania may disproportionately affect results, we tested the exclusion of Romania and Malta as part of the robustness checks (discussed below). This did not change our findings.

Table A6.1: Synthetic unit country weights and pre-merger accuracy, quality, standard time (Austria)

Standard time	
Unit	**Weight**
Malta	0.178
Romania	0.822

Standard time		
Variable	**Austria**	**Synthetic**
Download speeds (2011q4)	3.1	3.0
GDP per capita (log)	10.7	9.9
Rural population (%)	42.8	38.9
Pre-merger RMSPE	**0.163**	

Source: Ofcom analysis

Table A6.2 – Summary statistics for countries weighted in synthetic unit, quality, standard time (Austria)

	Download speeds			GDP per capita (log)			Rural population (%)		
Country	**Mean**	**Min**	**Max**	**Mean**	**Min**	**Max**	**Mean**	**Min**	**Max**
Austria	19.0	2.1	36.7	10.7	10.7	10.7	42.4	41.7	42.9
Malta	26.1	2.2	54.2	10.4	10.3	10.6	5.7	5.4	6.0
Romania	18.4	2.2	32.2	9.9	9.7	10.1	46.1	46.0	46.3

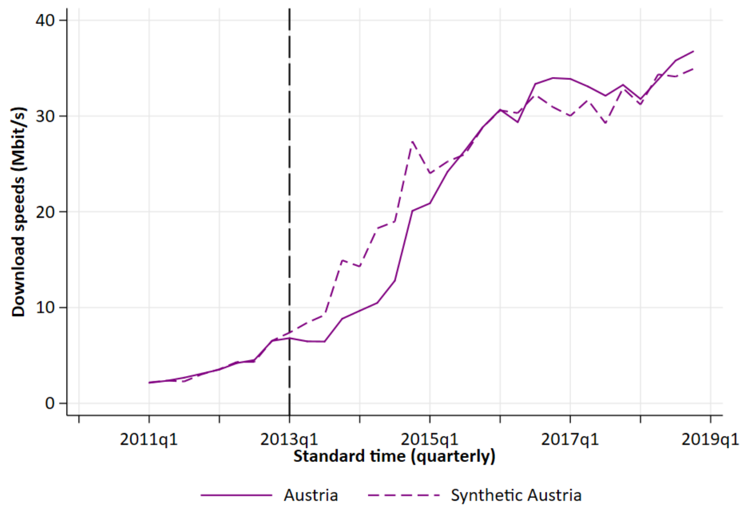
A6.19 We were unable to conduct this analysis using 4G rebased time for our quality analysis. This is because of insufficient data given the early start of 4G rollout in Austria, as seen in Figure A2.7. This was exacerbated by the fact that our data on download speeds was only available from 2011 which further restricted the control group after rebasing.

Results

A6.20 Based on the specification discussed above, Figure A6.6 graphically presents the results of our synthetic control analysis against Austria's actual outcomes. Immediately following the merger, we find Austrian download speeds are between 4-8 Mbit/s lower than the synthetic control. After two years, the growth in download speeds catch-up to the synthetic control, and we then a small positive effect of +3Mbit/s before outcomes in Austria and the counterfactual converge again.

A6.21 According to the placebo tests, across the whole period studied, this specification has a reasonably strong level of statistical significance with a rank of 2/19. Breaking out the post-/pre- merger RMSPE ratios by period, we find that the effects in the two years following the merger are ranked 1st and thus highly significant, whilst after this two-year period estimates are statistically insignificant. However, whilst we observe differing rates of growth in download speeds between Austria and the counterfactual estimate, looking at the analysis as a whole, there isn't any clear lasting effect of the merger.

Figure A6.6: Synthetic control results, quality (Austria)



Source: Ofcom analysis

Robustness tests

A6.22 As discussed in Annex A5, we conduct a number of robustness checks to test the sensitivity of results to changes in the model. Table A6.3 presents the results of the robustness tests on the variables included within the preferred specification.[88] Table A6.4 presents results on changes to the donor pool and on alternative approaches to the matching process.

A6.23 In all specifications tested, we find a negative effect in the first two years. However, after two years, results are far less robust and generally not statistically significant (except in the specifications which estimate the largest negative effects). As we have no reason to prefer these specifications over the others, and results are generally insignificant and non-robust, we infer from this therefore that the merger had no effect on average download speeds beyond two years post-merger.

[88] It is worth noting that while a couple of the specifications presented here have lower average pre-merger RMSPEs, the reason they were not adopted as the preferred specification was because these specifications did not create a good match to average download speeds in Austria immediately prior to the merger. For identification of the impact of the merger, it is important that the synthetic unit and the treatment unit behave similarly pre-merger. These results are in any case consistent with the preferred specification.

Table A6.3: Synthetic control variable robustness tests, quality, standard time (Austria)

	Preferred specification	Omitting variables		Additional variables					
Variables									
Download speed (2011q4)	✓		✓	✓	✓	✓	✓	✓	✓
GDP per capita (log)	✓	✓		✓	✓	✓	✓	✓	✓
Rural population (%)	✓	✓	✓	✓	✓	✓	✓	✓	✓
Prepaid connections (%)				✓					
Unemployment rate (%)					✓				
ARPU						✓			
Population density							✓		
HHI								✓	
Smartphone penetration (%)									✓
Pre-merger RMSPE	0.163	0.929	0.163	0.100	0.163	0.163	0.226	0.163	0.133
Merger effect									
+4 quarters	-6.1	-8.4	-6.1	-5.1	-6.1	-6.1	-5.0	-6.1	-5.6
+8 quarters	-7.3	-2.3	-7.3	-3.8	-7.3	-7.3	-2.4	-7.3	-5.8
+12 quarters	0.0	4.5	0.0	-1.2	0.0	0.0	-1.0	0.0	0.4
+16 quarters	3.0	9.8	3.0	0.3	3.0	3.0	-3.0	3.0	3.8
+20 quarters	0.3	1.9	0.3	-7.0	0.3	0.3	-10.7	0.3	0.1
+24 quarters	1.8	2.2	1.8	-5.9	1.8	1.8	-9.1	1.8	0.7
Rank (RMSPE ratio)	2/19	10/19	2/20	1/20	5/20	5/20	4/19	5/20	3/20

Source: Ofcom analysis

Table A6.4: Synthetic control other robustness tests, quality, standard time (Austria)

	Donor pool robustness tests			Alternative matching approaches
	Omitting Romania	Omitting Malta	4-player markets only	Three year pre-merger average download speeds with no covariates
Pre-merger RMSPE	0.253	0.278	0.342	0.089
Merger effect				
+4 quarters	-8.2	-5.0	-5.3	-5.0
+8 quarters	-7.9	-2.4	-2.6	-3.4
+12 quarters	-3.9	-1.9	2.9	0.2
+16 quarters	-0.7	-5.0	1.1	2.1
+20 quarters	-13.1	-13.1	-2.2	-4.6
+24 quarters	-10.7	-10.7	-2.6	-4.5
Rank (RMSPE ratio)	2/19	2/19	4/7	10/19

Source: Ofcom analysis

Discussion

A6.24 As set out in paragraphs A6.9 to A6.13, for our analysis on investment, we are unable to satisfy the key assumption of the synthetic control method that pre-merger outcomes of the treated and control group are similar. This means we cannot draw any conclusions from our analysis of the Hutchison/Orange merger on country-level investment.

A6.25 Our analysis on network quality suggests that there was a decline of between 4-8 Mbit/s in the two years post-merger relative to the synthetic unit (absent the merger) and this is statistically significant. It is worth noting though that, after this two-year period, we find that Austrian average download speeds recover and return to a similar level to what we would predict without the merger.[89] As the MNO remedy did not take effect in Austria and the entry of MVNOs was delayed, this raises a question as to whether weakened competitive pressure in the Austrian market negatively affected MNOs' incentive to invest in their network in the initial years after the merger. As discussed in Section 3 of the main report, it is difficult to quantitatively disaggregate the contributions of various merger-specific contextual factors and the effect of the merger itself.

A6.26 Overall, we find no evidence in our analysis that there is a positive effect on average download speeds from this merger.

[89] There is a small positive effect estimated around four years after the merger but this is statistically insignificant.

A7. Ireland – Hutchison/Telefónica – 2014

Purpose of this annex

A7.1 This annex presents the detailed modelling results and statistical tests from our counterfactual analysis (as discussed in Section 7 of the main report and Annex A5) for the Hutchison/Telefónica merger in 2014 in Ireland. The annex is structured as follows:

a) First, we set out the context of the merger;

b) Second, we set out the detailed results and robustness tests of the impact of this merger on investment outcomes;

c) Third, we set out the detailed results and robustness tests of the impact of this merger on average download speed outcomes; and

d) Finally, we provide a brief discussion of our results for the Hutchison/Telefónica merger in 2014 in Ireland, noting that this analysis evaluates the merger as a whole, including the remedies that the merger is subject to and the national market conditions that are present.

A7.2 The main finding from our analysis of this merger is that it appears to have had a negative effect on both industry investment and average download speeds. Industry investment appears to have been immediately affected whereas average download speeds appear to have been affected with some delay, but an enduring effect is seen in both. The effects we find are statistically significant and consistently negative across all robustness tests.

A7.3 We expect that paragraphs A7.8 to A7.38 to be of interest mainly to practitioners in the field wanting an in-depth understanding of our analysis.

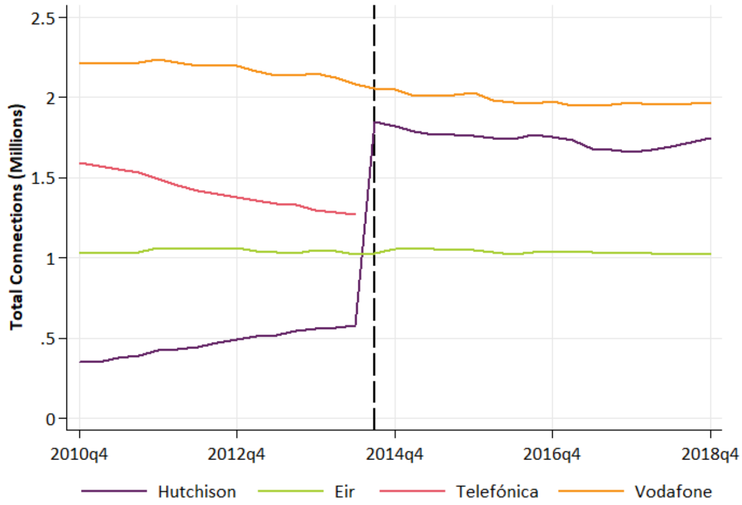
Context of the merger

A7.4 The EC was formally notified of Hutchison's intention to acquire Telefónica Ireland in October 2013. After a Phase II merger investigation, the Commission cleared the merger subject to a number of commitments and the merger was completed in July 2014.

A7.5 Pre-merger, four MNOs were active in the Irish market; Vodafone, Telefónica, Eir, and Hutchison. Hutchison, the most recent entrant to the market was gaining market share, particularly at Telefónica's expense (see Figure A7.1), driving a gradual decline in market concentration. MVNOs played a negligible role in the market. Excluding Tesco Mobile[90], the joint subscriber market share of MVNOs was less than 3% at the time of the merger.

[90] Telefónica held a 50% stake in Tesco Mobile so it was unlikely to pose a competitive constraint post-merger

Figure A7.1: Total connections by MNO (Ireland)



Source: GSMA Intelligence

A7.6 To gain approval of the EC, Hutchison made three main commitments:

a) First, it committed to enter capacity agreements of up to 30% with two MVNOs, one of which it would enter into prior to the merger concluding. This aimed to mitigate the loss of competition whilst addressing the concern that MVNOs are less effective competitors than MNOs. These MVNOs would have a strong incentive to compete aggressively in order to fully utilise the network capacity. The MVNO that obtained a capacity agreement pre-merger was further given the option to acquire a sub-brand of the merged entity.

b) Second, Hutchison offered to divest spectrum to one of the MVNOs to allow them to become a fully-fledged MNO. This offer of spectrum would be available for ten years starting in 2016.

c) Third, Hutchison offered a package aimed at ensuring that Eir remained an effective competitor going forward. Hutchison committed to continue the existing network sharing agreement on improved terms between Eir and Telefónica. This ensured that Eir would remain as an effective and viable competitor with national coverage and enabled Eir's own rollout plans.

A7.7 Although two new MVNOs (iD Mobile and Virgin Mobile) entered the market, neither succeeded in making an impact. iD Mobile launched in August 2015, but after failing to achieve sufficient scale or profitability, exited the market in 2018. Virgin Mobile was the second MVNO to enter and launched its services in October 2015. In Q3 2017 Virgin commanded a revenue and subscriber share of 0.9% in Ireland.[91]

[91] BEREC (2018). See footnote 88.

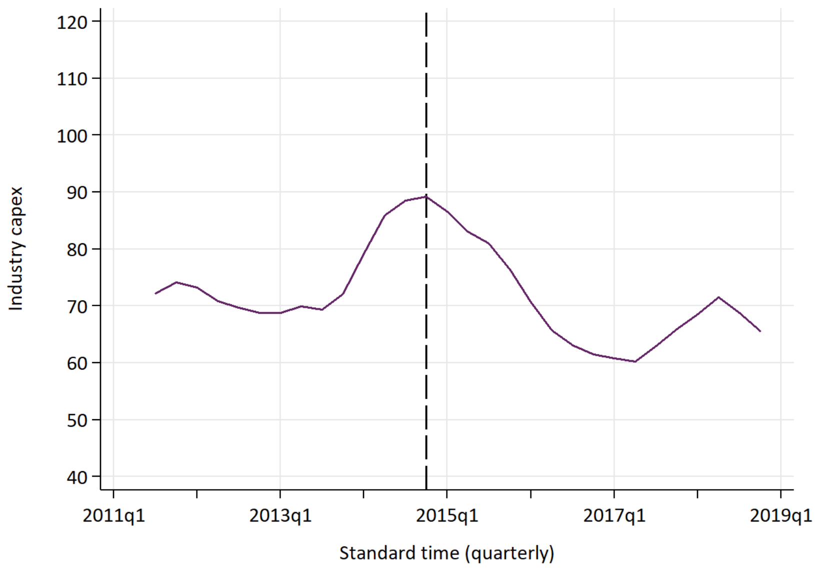
Analysis on investment

A7.8 For investment, we find a good counterfactual for what would have happened to industry investment in Ireland in the absence of the merger that is robust to the sensitivity tests outlined in Annex A5. Using both standard time and 4G rebased time specifications, our analysis finds that the merger in Ireland had a strong negative effect on industry investment. Below, we set out the detail of the specifications estimated, and the tests conducted to arrive at this finding.

Specification

A7.9 Figure A7.2 shows the pattern of industry investment in Ireland over the period 2011 to 2018. Investment increased during 2013 as MNOs started to invest in 4G services.[92] However following the merger, we observe industry investment in Ireland falls to levels below that of the pre-merger period. In our analysis, we try to estimate a counterfactual for what would have happened to investment in Ireland in the absence of a merger using synthetic control methods.

Figure A7.2 – Trends in industry investment, Ireland



A7.10 As explained in Annex A5, to obtain the country-specific weights, we need a set of covariates that are predictive of industry capex in Ireland that minimise the deviation between the pre-treatment outcomes experienced by the synthetic and the treated unit. As explained in Annex A5, this is done iteratively using combinations of covariates.

[92] Telefónica: https://www.thejournal.ie/o2-ireland-4g-network-675504-Nov2012/

Vodafone, Three and Eir: https://www.independent.ie/business/technology/vodafone-and-meteor-take-lead-in-race-for-4g-launch-29521593.html

A7.11 Based on this approach, we arrive at a specification with a RMSPE of 0.917 relative to average pre-merger outcomes of ~74.0, which suggests a good match for Ireland's industry capex. Details on this specification are presented in Table A7.1 below.

A7.12 This specification includes three observations of the outcome variable and four covariates – GDP per capita, proportion of prepaid connections, population density, and total industry revenue. As can be seen from the table, the synthetic results match reasonably well to the covariates with the exception of population density where Ireland has a noticeably lower population density than its synthetic counterpart.

A7.13 Table A7.2 below presents summary statistics of the covariates included in this specification for Ireland and each of the countries that have been assigned weight in estimating the synthetic counterfactual. This suggests that Ireland is not an outlier relative to the other units in the control group.

A7.14 Based on these covariates, we obtain the country-weights as set out in the left-hand side of Table A7.1. The synthetic Ireland unit is made up of six countries with Czech Republic given the greatest weight of approximately 1/3, followed by Lithuania with weight of 25%, and Greece and Portugal also having weights greater than 10%. Given the possible concern that the weights attributed to Czech Republic and Lithuania could lead to spurious results, we carried out a sensitivity test which excluded these countries from the donor pool. We found that results were not sensitive to this test, see discussion below.

Table A7.1 – Synthetic unit country weights and pre-merger accuracy, industry capex, standard time (Ireland)

Standard time	
Unit	Weight
Czech Republic	0.332
Greece	0.185
Lithuania	0.249
Portugal	0.177
Switzerland	0.056

Standard time		
Variable	Ireland	Synthetic
Industry capex (2012q1)	73.2	71.4
Industry capex (2013q4)	72.1	72.9
Industry capex (2014q1)	79.1	78.5
GDP per capita (log)	10.7	10.2
Rural population (%)	38.0	29.6
Prepaid connections (%)	59.9	55.1
Population density	67.9	105.6
Pre-merger RMSPE	**0.917**	

Source: Ofcom analysis

Table A7.2 – Summary statistics for countries weighted in synthetic unit, industry capex, standard time (Ireland)

	Industry capex (€m)			GDP per capita (log)			Rural population (%)			Prepaid connections (%)			Population density		
Country	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Ireland	72.1	60.3	89.3	10.9	10.7	11.2	37.6	36.8	38.3	54.3	47.3	64.6	68.5	67.8	70.0
Czech Republic	83.1	67.5	108.3	10.3	10.2	10.4	26.6	26.2	26.8	38.5	33.0	44.2	137.3	136.9	137.6
Greece	82.6	63.5	97.0	10.1	10.1	10.2	22.2	20.9	23.4	70.2	68.5	71.8	87.2	86.4	88.5
Lithuania	10.7	8.0	14.5	10.2	10.0	10.4	32.8	32.3	33.3	46.2	37.3	59.6	47.1	45.8	49.0
Portugal	142.0	108.0	191.5	10.2	10.1	10.3	36.9	34.8	39.0	63.0	53.9	73.1	113.9	112.2	115.9
Switzerland	149.2	99.0	220.3	10.9	10.9	11.0	26.3	26.2	26.4	35.9	29.7	39.8	209.8	201.2	217

A7.15 The rise in investment in Ireland in 2013 (see Figure A7.2) is likely attributable in part to the introduction of 4G technology by all Irish MNOs following spectrum being awarded in November 2012. As can be seen in Figure A2.7 in Annex A2, this occurred towards the end of 2013. As such, we also carried out the synthetic control analysis relative to the first commercial rollout of 4G. This allows us to test whether the development of national 4G networks rather than the actual date is better at explaining the Irish pre-merger investment levels.

A7.16 The preferred specification for 4G rebased time is presented in Table A7.3 below and is similar to the standard time specification in terms of the covariates included. Note though that for 4G rebased time, this analysis is only conducted for three years post-merger to avoid loss of units in the control group. Figure A2.7 in Annex A2 illustrates this showing the start of 4G rollout in each country for which time is rebased to as well as the date of the merger in Ireland. The 4G rebased time specification for industry capex has a RMSPE of 0.967 relative to average outcomes of ~74.0 representing a good match although this is slightly worse than that of the standard time specification.

A7.17 Table A7.4 below presents summary statistics of the covariates included in this specification for Ireland and each of the countries that have been assigned weight in estimating the synthetic counterfactual.

A7.18 Based on these covariates, we obtain the country-weights as set out in the left-hand side of Table A7.3. The synthetic Ireland unit is made up of four countries. As with the standard time specification, Czech Republic is assigned a large weight but alongside Finland which has the largest weight overall. As part of the robustness tests, we test the sensitivity of our results to the omission of these countries.

Table A7.3 – Synthetic unit country weights and pre-merger accuracy, industry capex (Ireland), 4G rebased time

4G rebased time	
Unit	Weight
Czech Republic	0.389
Finland	0.479
Malta	0.019
Sweden	0.112

4G rebased time		
Variable[93]	Ireland	Synthetic
Industry capex (9)	69.3	70.0
Industry capex (10)	72.1	73.0
Industry capex (12)	85.9	84.7
GDP per capita (log)	10.7	10.5
Population density	67.9	89.4
Prepaid connection (%)	59.9	27.1
Pre-merger RMSPE	**0.967**	

Source: Ofcom analysis

Table A7.4 – Summary statistics for countries weighted in synthetic unit, industry capex, 4G rebased time (Ireland)

	Industry capex (€m)			GDP per capita (log)			Population density			Prepaid connections (%)		
Country	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Ireland	72.1	60.3	89.3	10.9	10.7	11.2	68.5	67.8	70.1	54.3	47.3	64.6

[93] Industry capex (+2Q) and industry capex (+6Q) represent matching against 2 and 6 quarters after the start of 4G rollout respectively.

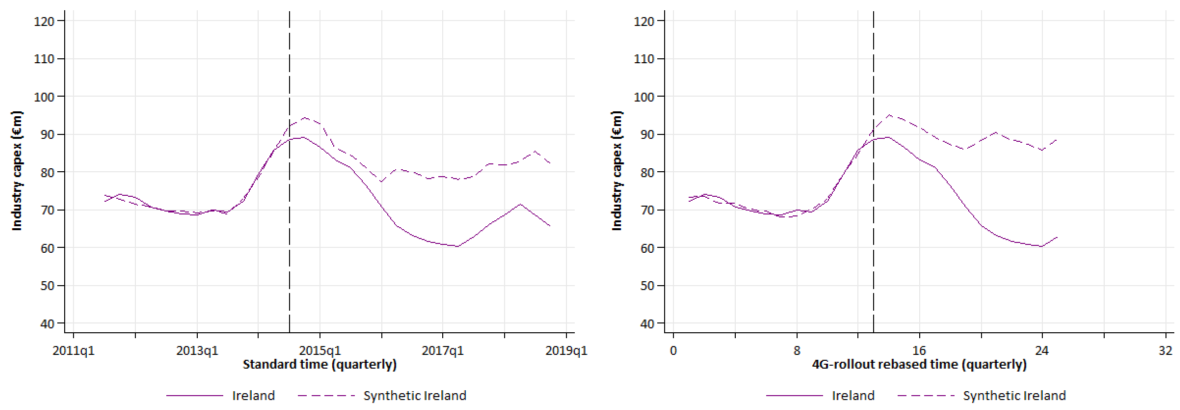
Czech Republic	83.1	67.5	108.3	10.3	10.2	10.4	137.3	136.9	137.6	38.5	33.0	44.2
Finland	100.1	75.1	138.3	10.6	10.6	10.6	18.0	17.7	18.3	9.2	8.4	10.6
Malta	6.7	5.8	7.6	10.5	10.3	10.6	1333.6	1309.3	1352.1	73.7	65.6	81.3
Sweden	133.7	116.3	153.0	10.7	10.7	10.8	23.8	23.2	24.4	26.5	20.8	35.5

Results

A7.19 Based on the specifications discussed above, Figure A7.3 below is a graphic representation of the synthetic control results, plotting the counterfactual estimates against the actual observed outcomes in Ireland. The left-hand panel is the results under standard time whilst the right-hand panel is for 4G rebased time.

A7.20 In the years following the merger, we observed that industry investment in Ireland fell before recovering slightly. In contrast, the synthetic counterfactual is predicted to generally remain flatter, seeing a smaller decline in industry investment. Overall, this estimates a negative effect on industry investment in all periods from the merger. The 4G rebased time specification predicts a similar path and slightly larger negative effect from the merger, although this can only be estimated for three years after the merger.

Figure A7.3 – Synthetic control results, industry capex (Ireland)[94]



Source: Ofcom analysis

A7.21 In terms of statistical significance from the placebo tests, Ireland is ranked 2nd in the standard time specification and 1st in the 4G rebased time. This indicates that the negative effect estimated from this analysis is statistically significant in both specifications.

Robustness tests

A7.22 Table A7.5 presents the results of the robustness tests on the standard time specification. The top half of the table presents the sensitivity of results to changes to the variables included with in the preferred specification and the bottom half other table presents the sensitivity of results to changes to the donor pool and on alternative approaches to the

[94] The time axis on the right-hand panel represents the quarter relative to the start of 4G rollout. This has been standardised and, for simplicity, set to zero at the origin.

matching process. Table A7.6 presents the robustness tests in the same format described for the 4G rebased time specification.

A7.23 Across all specifications tested in the robustness tests for both standard and 4G rebased time specifications, we find a consistent reduction in industry investment in all periods after the merger. This provides a strong indicator that industry investment declined in Ireland follow the Hutchison/Telefónica merger. However, our robustness tests provide a wide range of estimates for the merger effect and therefore the magnitude of the decline is unclear.

Table A7.5 – Synthetic control robustness tests, industry capex, standard time (Ireland)

	Preferred Specification	Omitted variables							Additional variables			
Variables												
Industry capex (2012q1)	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry capex (2013q4)	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry capex (2014q1)	✓	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓
GDP per capita (log)	✓	✓	✓	✓		✓	✓	✓	✓	✓	✓	✓
Rural population (%)	✓	✓	✓	✓	✓		✓	✓	✓	✓	✓	✓
Prepaid connections (%)	✓	✓	✓	✓	✓	✓		✓	✓	✓	✓	✓
Population density	✓	✓	✓	✓	✓	✓	✓		✓	✓	✓	✓
HHI									✓			
ARPU										✓		
Smartphone penetration (%)											✓	
Unemployment rate (%)												✓
Pre-merger RMSPE	0.917	1.618	1.295	1.535	1.134	1.063	1.083	0.944	1.122	0.971	1.103	0.986
Merger effect (€m)												
+4 quarters	-5.5	-5.3	-0.7	0.1	-6.3	-8.3	-8.8	-7.8	-1.9	-9.3	-8.2	-2.1
+8 quarters	-19.2	-13.0	-19.4	-17.1	-21.3	-23.5	-25.9	-23.5	-15.7	-26.2	-28.5	-18.0
+12 quarters	-23.1	-15.6	-21.1	-18.0	-22.7	-29.9	-30.3	-26.1	-17.1	-30.2	-32.3	-19.2
+16 quarters	-17.3	-14.5	-5.6	-1.9	-11.3	-27.3	-22.5	-19.6	-5.3	-22.5	-20.4	-6.3
Rank (RMSPE ratio)	2/19	1/19	2/19	2/19	2/19	2/18	2/19	2/19	3/19	2/19	2/18	2/19

Source: Ofcom analysis

	-	Donor pool robustness tests				Alternative matching approaches	
	Preferred specification	Omitting Czech Republic	Omitting Lithuania	Omitting Czech Republic + Lithuania	4-player markets only	All pre-merger outcomes	Long pre-merger time series
Pre-merger RMSPE	0.917	1.649	0.924	1.601	1.587	0.708	5.527
Merger effect (€m)							
+4 quarters	-5.5	-3.0	-8.0	-6.2	-6.6	-4.8	8.8
+8 quarters	-19.2	-26.7	-22.8	-32.2	-32.8	-20.3	-15.9
+12 quarters	-23.1	-33.3	-26.2	-37.2	-38.0	-23.2	-15.1
+16 quarters	-17.3	-16.5	-18.7	-19.0	-19.7	-11.4	-9.8
Rank (RMSPE ratio)	2/19	2/17	2/18	2/16	1/7	2/19	3/18

Source: Ofcom analysis

Table A7.6 – Synthetic control robustness tests, industry capex, 4G rebased time (Ireland)

	Preferred Specification	Omitted variables						Additional variables				
Variables												
Industry capex (2012q1)	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry capex (2013q4)	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry capex (2014q1)	✓	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓

GDP per capita (log)	✓	✓	✓	✓		✓	✓	✓	✓	✓	✓	✓
Prepaid connections (%)	✓	✓	✓	✓	✓		✓	✓	✓	✓	✓	✓
Population density	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Rural population (%)								✓				
HHI									✓			
ARPU										✓		
Smartphone penetration (%)											✓	
Unemployment rate (%)												✓
Pre-merger RMSPE	0.967	1.414	2.907	2.601	1.463	1.285	1.320	0.983	0.967	0.965	1.198	1.289
Merger effect (€m)												
+4 quarters	-8.7	-14.5	-9.8	-1.2	-7.0	-12.1	-11.6	-8.7	-8.7	-8.7	-2.9	-12.4
+8 quarters	-22.5	-35.0	-25.8	-41.2	-26.0	-30.7	-30.6	-22.8	-22.5	-22.5	-20.1	-29.4
+12 quarters	-25.6	-39.0	-30.1	-29.5	-31.9	-36.5	-38.3	-26.1	-25.6	-25.6	-20.5	-30.4
Rank (RMSPE ratio)	1/18	1/18	1/18	1/19	1/18	1/18	1/18	1/18	1/19	1/18	1/18	1/18

Source: Ofcom analysis

		Donor pool robustness tests				Alternative matching approaches	
	Preferred specification	Omitting Czech Republic	Omitting Lithuania	Omitting Czech Republic + Lithuania	4-player markets only	All pre-merger outcomes	Long pre-merger time series
Pre-merger RMSPE	0.967	1.289	1.298	1.336	1.580	0.906	6.341
Merger effect (€m)							
+4 quarters	-8.7	-12.4	-15.8	-14.7	-18.6	-8.7	-13.1
+8 quarters	-22.5	-29.4	-42.6	-36.4	-43.0	-19.1	-33.3
+12 quarters	-25.6	-30.4	-56.4	-42.1	-58.0	-22.0	-28.0
Rank (RMSPE ratio)	1/18	1/17	1/17	1/16	1/7	1/18	2/18

Source: Ofcom analysis

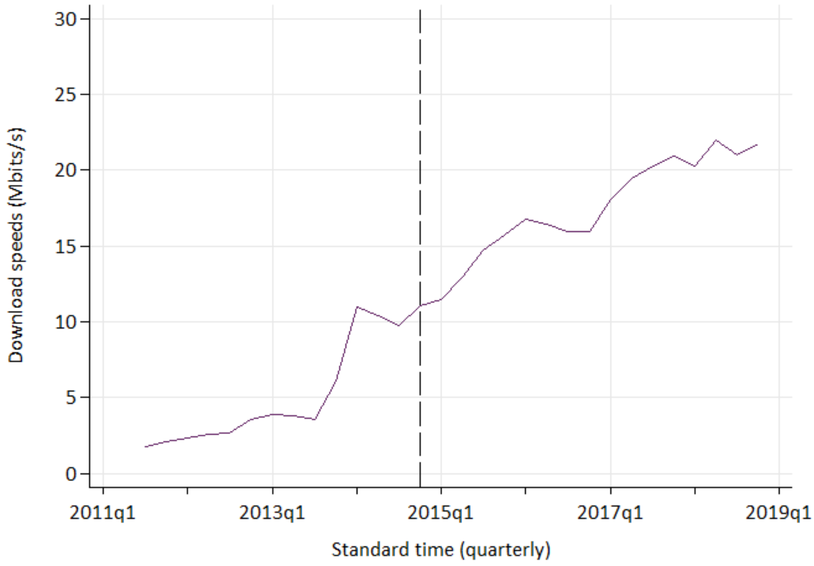
Analysis on quality

A7.24 When we adjust for the date of 4G roll-out, we find a good counterfactual for what would have happened to average download speeds in Ireland in the absence of the merger that is robust to the sensitivity tests outlined in Annex A5. This suggests that there was an enduring negative effect of the transaction on average download speeds, which is statistically significant and is consistently negative across all robustness tests. Below, we set out the detail of the specifications estimated, and the tests conducted to arrive at this finding.

Specification

A7.25 Figure A7.4 shows the trends in industry average download speeds over the period 2011 to 2018. During the full period, we observe a general steady increase in download speeds in Ireland and a noticeable sharp increase towards the end of 2013 which is likely attributable in part to the introduction of 4G technology by all Irish MNOs following spectrum being awarded in November 2012. As can be seen in Figure A2.7 in Annex A2, the start of this rollout occurred towards the end of 2013.

Figure A7.4 – Trends in download speeds, Ireland



A7.26 Based on our synthetic control approach explained in Annex A5, we arrive at a specification shown in Table A7.7. Whilst visually a good fit, the standard time specification suggests a relatively poor match for Ireland average download speeds with a RMSPE of 0.739 relative to average pre-merger outcomes of ~4.9.

A7.27 This specification includes one observation of the outcome variable, 2013q3, and three covariates – GDP per capita, population density, and the proportion of the population living in rural areas. Based on these covariates, we obtain the country-weights as set out in the left-hand side of Table A7.7. The synthetic unit is made up solely of Estonia which suggests it is the country that is best able to replicate the pre-merger trends in download speeds. We can see how closely Estonia matches in the pre-merger period on the right-hand side of Table A7.7. Table A7.8 shows the summary statistics of the covariates included in this specification for Ireland and Estonia. These tables suggest that Estonia is a reasonable match for Ireland for some covariates but less so for others, including average download speeds. Given this, and a possible concern that this large weight attributed to Estonia could lead to spurious results, we carried out a sensitivity test of our results by excluding Estonia from the donor pool of countries and found the results robust to this, see discussion below. Nonetheless, we do not place much weight on these results given the relatively poor fit of the specification for the pre-treatment period.

Table A7.7 – Synthetic unit country weights and pre-merger accuracy, download speeds, standard time (Ireland)

Standard time	
Unit	Weight
Estonia	1.000

Standard time		
Variable	Ireland	Synthetic
Download speed (2013q3)	3.6	4.6
GDP per capita (log)	10.7	10.1
Population density	67.9	31.2
Rural population (%)	38.0	32.0
Pre-merger RMSPE	**0.739**	

Source: Ofcom analysis

Table A7.8 – Summary statistics for countries weighted in synthetic unit, download speeds, standard time (Ireland)

	Download speed (Mbits/s)			GDP per capita (log)			Population density			Rural population (%)		
Country	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Ireland	11.9	1.8	22.0	10.9	10.7	11.2	68.5	67.8	70.1	37.6	36.8	38.3
Estonia	17.4	1.9	36.0	10.2	10.1	10.3	31.1	30.8	31.3	31.7	31.1	32.0

A7.28 Whilst the synthetic control method under standard time found Estonia to be the best match for download speeds in Ireland, we note that as seen in Figure A2.7, 4G rollout started in Estonia much earlier than in Ireland – 2010 compared to 2013. If the 2013 increase in download speeds in Ireland was due to 4G rollout, then accounting for this may provide better insight into how download speeds would have developed absent the merger. Therefore, we also carried out the synthetic control analysis relative to the first commercial rollout of 4G spectrum.

A7.29 The preferred specification for 4G rebased time is presented in Table A7.9 below and includes more or less the same covariates as the standard time specification. The sole difference relates to the observations of average download speeds in the pre-merger period, which is not surprising given that the data is rebased to the first commercial rollout of 4G spectrum.

A7.30 The 4G rebased time specification provides a much better match for Ireland download speeds with a pre-merger RMSPE of 0.291 relative to a pre-merger download speeds of ~4.9. As in the investment analysis, we have limited the post-merger period to three years due to loss of data after rebasing time. However, in this case there is also some loss of data in terms of the control group which is smaller at 14 countries.

A7.31 Table A7.10 below presents summary statistics of the covariates included in this specification for Ireland and each of the countries that have been assigned weight in estimating the synthetic counterfactual. This suggests that Ireland is not an outlier relative to the other units in the control group. It also suggests a better match on all covariates except population density, with a good match particularly on download speeds.

A7.32 Based on these covariates, we obtain the country-weights as set out in the left-hand table of Table A7.9. This shows a better distribution of weights amongst a wider range of countries with Croatia being assigned the greatest weight of 0.456. This is followed by Switzerland with weight of 0.290 and Slovenia with a weight of 0.185. We test the omission of the two countries that are assigned the greatest weight, Croatia and Switzerland, as part of our robustness tests discussed below. These results do not suggest that the countries being assigned the greatest weight are driving results.

Table A7.9 – Synthetic unit country weights and pre-merger accuracy, download speeds, 4G rebased time (Ireland)

4G rebased time	
Unit	Weight
Croatia	0.456
Hungary	0.029
Malta	0.040
Slovenia	0.185
Switzerland	0.290

4G rebased time		
Variable[95]	Ireland	Synthetic
Download speed (+5Q)	3.9	3.6
Download speed (+7Q)	3.6	3.8
GDP per capita (log)	10.7	10.3
Population density	68.0	168.8
Rural population (%)	38.0	37.8
Pre-merger RMSPE	**0.291**	

Source: Ofcom analysis

Table A7.10 – Summary statistics for countries weighted in synthetic unit, download speeds, 4G rebased time (Ireland)

	Download speeds (Mbit/s)			GDP per capita (log)			Population density			Rural population (%)		
Country	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Ireland	10.8	2.3	20.3	10.9	10.7	11.1	68.3	67.9	69.3	37.7	37.1	38.2
Croatia	11.0	2.0	26.2	9.9	9.9	10.0	76.2	75.2	77.2	44.3	43.7	44.8
Hungary	31.0	10.7	49.7	10.1	10.0	10.3	107.9	106.9	108.8	29.5	28.7	30.3
Malta	25.5	3.7	49.9	10.4	10.3	10.6	1332.0	1313.1	1347.4	5.7	5.5	5.8
Slovenia	15.1	2.0	25.4	10.3	10.2	10.3	102.7	102.0	103.3	46.6	46.0	47.2
Switzerland	12.5	1.9	28.9	10.9	10.9	10.9	206.9	200.1	213.1	26.3	26.3	26.4

Results

A7.33 Based on the specifications discussed above, Figure A7.5 below is a graphical representation of the synthetic control results, plotting the counterfactual synthetic estimates against the actual observed outcomes in Ireland. The left-hand panel is the results under standard time whilst the right-hand panel is for 4G rebased time.

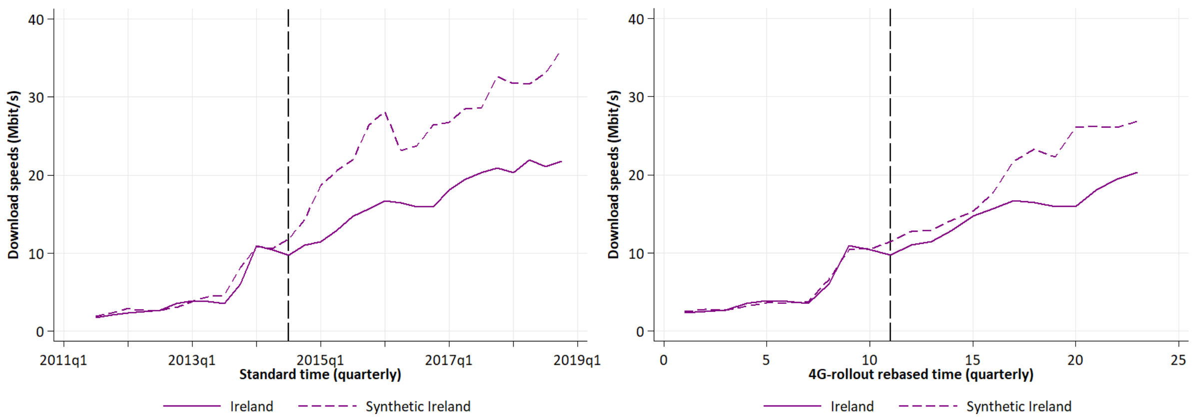
A7.34 In both specifications we can see a general negative effect of the merger in terms of average download speeds. Under standard time, we find an immediate post-merger deviation between the observed download speeds and those predicted by the synthetic control. In particular, this analysis shows download speeds may have been around 10 Mbit/s slower than they would have been but for the merger. However, we have some concerns around this model given the relatively poor match for pre-merger outcomes and being matched to Estonia only. Therefore, we do not place as much weight on these findings.

A7.35 When we rebase for 4G, which is the better model from our discussion above, the counterfactual download speeds closely follow that observed in Ireland for one-and-a-half

[95] Download speed (+5Q) and download speed (+7Q) represent matching against 5 and 7 quarters after the start of 4G rollout respectively.

years before the negative effect emerges. In addition, this effect is smaller at around -7Mbit/s. We place more weight on these results.

Figure A7.5 – Synthetic control results, quality (Ireland) [96]



Source: Ofcom analysis

A7.36 The ranking of post-/pre- merger RMSPE ratios from the placebo tests rank Ireland 1st for both specifications, suggesting that the estimated effects are statistically significant.

Robustness tests

A7.37 As per the discussion in Annex A5, Table A7.11 presents the results of the robustness tests on the standard time specification. The top half of the table presents the sensitivity of results to changes to the variables included with in the preferred specification and the bottom half other table presents the sensitivity of results to changes to the donor pool and on alternative approaches to the matching process. Table A7.12 presents the robustness tests in the same format described for the 4G rebased time specification.

A7.38 Generally, we estimate a negative effect across all robustness test specifications estimated. For rebased 4G time which we place most weight on, all results are statistically significant with an estimated reduction of download speeds post-merger which range between -6 Mbit/s to -10 Mbit/s. We also find an overall negative effect for the sensitivity analysis based on standard time, although this effect is not always found to be statistically significant.

Table A7.11 – Synthetic control robustness tests, download speeds, standard time (Ireland)

Standard time									
	Preferred Specification	Omitted variables				Additional variables			
Variables									
Download speeds (2013q3)	✓		✓	✓	✓	✓	✓	✓	✓
GDP per capita (log)	✓	✓		✓	✓	✓	✓	✓	✓
Population density	✓	✓	✓		✓	✓	✓	✓	✓
Rural population (%)	✓	✓	✓	✓		✓	✓	✓	✓

[96] The time axis on the right-hand panel represents the quarter relative to the start of 4G rollout. This has been standardised and, for simplicity, set to zero at the origin.

HHI						✓			
Unemployment rate (%)							✓		
Prepaid connections (%)								✓	
Smartphone penetration (%)									✓
Pre-merger RMSPE	0.739	4.798	0.758	0.826	0.816	0.745	0.739	0.997	1.238
Merger effect (Mbit/s)									
+4 quarters	-7.6	-9.7	-7.6	-7.6	-7.8	-7.6	-7.6	-7.8	-6.4
+8 quarters	-6.7	-7.1	-6.7	-7.0	-6.8	-6.7	-6.7	-6.6	-8.1
+12 quarters	-9.1	-9.5	-9.1	-9.2	-9.2	-9.1	-9.1	-8.9	-11.5
+16 quarters	-9.7	-12.9	-9.7	-10.2	-9.9	-9.7	-9.7	-9.8	-12.4
Rank (RMSPE ratio)	1/20	7/20	1/20	1/20	1/20	1/20	2/20	2/20	2/20

Source: Ofcom analysis

Standard time			
	Donor pool robustness tests		Alternative matching approaches
	Omitting Estonia	4-player markets only	Three-year pre-merger quality outcomes with no covariates
Pre-merger RMSPE	1.957	1.856	0.739
Merger effect (Mbit/s)			
+4 quarters	-6.5	-4.2	-7.6
+8 quarters	-9.2	-5.9	-6.7
+12 quarters	-14.0	-8.4	-9.1
+16 quarters	-14.4	-9.8	-9.7
Rank (RMSPE ratio)	4/19	3/9	9/20

Source: Ofcom analysis

Table A7.12 – Synthetic control robustness tests, download speeds, 4G rebased time (Ireland)

4G-rollout rebased time										
	Preferred Specification	Omitted variables					Additional variables			
Variables										
Download speeds (5)	✓		✓	✓	✓	✓	✓	✓	✓	✓
Download speeds (7)	✓	✓		✓	✓	✓	✓	✓	✓	✓
GDP per capita (log)	✓	✓	✓		✓	✓	✓	✓	✓	✓
Population density	✓	✓	✓	✓		✓	✓	✓	✓	✓
Rural population (%)	✓	✓	✓	✓	✓		✓	✓	✓	✓
HHI							✓			
Unemployment rate (%)								✓		
Prepaid connections (%)									✓	
Smartphone penetration (%)										✓
Pre-merger RMSPE	0.291	0.734	0.463	0.780	0.321	0.386	0.497	0.346	0.346	0.362
Merger effect (Mbit/s)										
+4 quarters	-1.3	-1.6	-2.4	1.0	-1.2	-0.7	-1.9	-1.3	-1.3	-1.5
+8 quarters	-6.9	-5.8	-7.1	-7.7	-6.4	-6.7	-6.0	-7.4	-7.4	-7.0
+12 quarters	-6.6	-6.6	-8.4	-7.2	-6.1	-9.7	-8.4	-7.0	-7.1	-9.5
Rank (RMSPE ratio)	1/15	1/15	1/14	1/15	1/14	1/15	2/15	1/15	1/15	1/15

Source: Ofcom analysis

4G-rollout rebased time				
	Donor pool robustness tests			Alternative matching approaches
	Omitting Croatia	Omitting Switzerland	4-player markets only	Three-year pre-merger quality outcomes with no covariates
Pre-merger RMSPE	0.364	0.963	0.963	0.285

Merger effect (Mbit/s)				
+4 quarters	-2.1	-4.4	-4.4	-1.0
+8 quarters	-6.9	-6.9	-6.9	-6.5
+12 quarters	-9.0	-8.1	-8.1	-6.2
Rank (RMSPE ratio)	1/14	1/13	2/6	1/15

Source: Ofcom analysis

Discussion

A7.39 Our analysis suggests that the merger in Ireland had a strong negative effect on industry investment for the period studied. The exact magnitude of the effect on investment is however uncertain as the robustness tests provided a wide range of point estimates. A decline in industry investment is not necessarily a poor outcome for consumers, as there could be significant cost savings from less duplication of networks and economies of scale that may provide MNOs' with the ability to invest in quality improvements.

A7.40 We do, however, also find an enduring and negative effect of the transaction on average download speeds in Ireland. After accounting for when 4G rollout takes place in each country, which we consider is the most appropriate way to construct a synthetic counterfactual in this context, we find that this effect appears after one-and-a-half years after the merger and endures for the rest of the post-merger period considered.[97] This lag might indicate the amount of time it took for the reduction in investment in Ireland following the merger to feed through to tangible effects on download speeds.

[97] Under standard time, we find a larger negative effect occurring with no time lag but rebasing for 4G appears to produce a better counterfactual match.

A8. Germany – Telefónica/E-Plus – 2014

Purpose of this annex

A8.1 This annex presents the detailed modelling results and statistical tests from our counterfactual analysis (as discussed in Section 7 of the main report and Annex A5) for the Telefónica/E-Plus merger in 2014 in Germany. The annex is structured as follows:

a) First, we set out the context of the merger;

b) Second, we set out the detailed results and robustness tests of the impact of this merger on investment outcomes;

c) Third, we set out the detailed results and robustness tests of the impact of this merger on average download speed outcomes; and

d) Finally, we provide a brief discussion of our results for the Telefónica/E-Plus merger in 2014 in Germany.

A8.2 The main finding from our counterfactual analysis of this merger is that for the first two years after the merger, there does not appear to be any effect on industry capex. However, in the third and fourth year following the merger, we find a statistically significant decline in industry capex of between 15-20% relative to the counterfactual. A decline in industry investment is not necessarily however a poor outcome for consumers as there could be significant cost savings from less duplication of networks and economies of scale. Therefore, we also considered the effect of the merger on average download speeds. Although we found a consistently negative effect on download speeds in Germany from two years after the merger due to stagnating download speeds, this negative effect was found to be statistically insignificant.

A8.3 We expect that paragraphs A8.13 to A8.40 to be of interest mainly to practitioners in the field wanting an in-depth understanding of our analysis.

Context of the merger

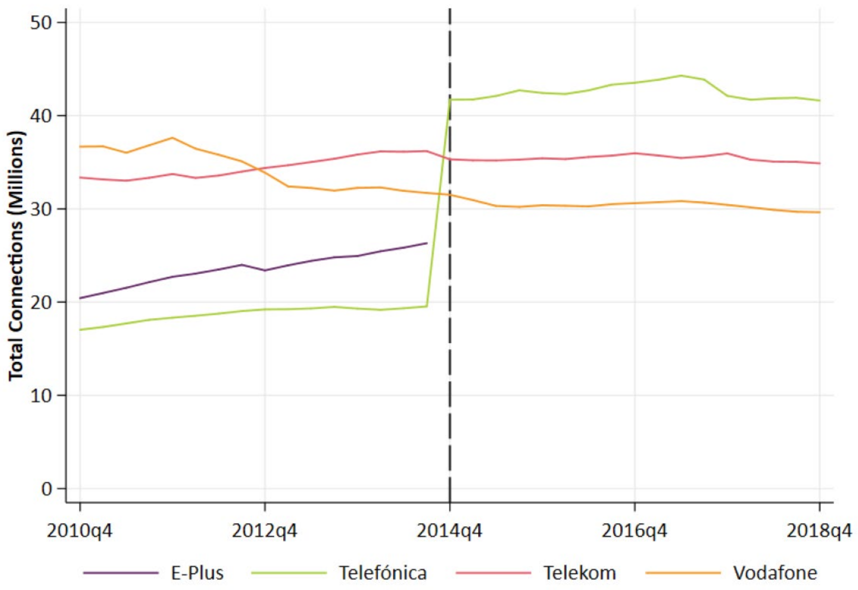
A8.4 In July 2014, the EC approved the acquisition of E-Plus[98] by Telefónica following a Phase II Market Investigation. The takeover was completed in October 2014 which, reduced the number of MNOs in Germany from four-to-three.

A8.5 While this transaction led to an identical change in the number of MNOs as the other mergers analysed, the incentives that drove the merger, the pre-merger share of merging parties, local conditions and the remedies imposed by the relevant merger control authority differ. Figure A8.1 shows the market shares of each of the MNOs measured by total connections. Prior to the merger, E-Plus and Telefónica were the third and fourth

[98] E-Plus was Dutch operator KPN's German mobile telecommunications business.

largest MNOs in the market respectively. Following the merger, they became the largest player in Germany.

Figure A8.1: Germany – Total connections by MNO



Source: GSMA Intelligence

Remedies

A8.6 The EC had concerns that this merger would reduce competitive pressures in the market and so lead to negative outcomes for consumers. Therefore, conditional upon a package of commitments submitted by Telefónica, the merger was approved:[99]

a) First, an **MVNO remedy** – Telefónica committed to agree mobile bitstream access[100] of up to 30% of the merged company's network capacity to up to three MVNOs. This ensured the entrance of at least one MVNO with the aim of mitigating the loss of competition whilst addressing the concern that MVNOs are less effective competitors than MNOs. These MVNOs would have a strong incentive to compete aggressively in order to fully utilise the network capacity.

b) Second, an **MNO remedy** – Telefónica committed to offer spectrum, national roaming, divestiture of sites, passive radio network sharing, and the sale of shops in order to enable the entry of a new MNO. Further, they committed that if the offer was not taken up by a new entrant, Telefónica would offer this to the MVNOs in the first remedy.

[99] European Commission, Case M.7018 – Telefónica Deutshland/ E-Plus, 2 July 2014. Accessed: 24 June 2019.
[100] Mobile bitstream access – this refers to dedicated access to an MNOs network capacity.

c) Third, a **general remedy** – Telefónica committed to extend existing wholesale agreements with Telefónica's and E-Plus' existing wholesale partners, and to further offer wholesale 4G services to all interested players at "best prices under benchmark conditions". In addition, Telefónica committed to allow its wholesale partners to switch their customers to another MNO without penalty.

Post-merger developments

A8.7 Following the merger, no new MNO entered the market, and therefore the MNO remedy failed to bite. Instead, the MVNO remedy was taken by the MVNO Drillisch in July 2015. Drillisch was given access to 20% of Telefónica's network with the option to increase this to 30% by 2020, at guaranteed prices until at least 2030.

A8.8 The mobile bitstream access agreement allowed Drillisch to be more flexible in the mobile market relative to other MVNOs with access on a per usage basis. In the years following the merger, Drillisch increased its number of subscribers by 65% from 2.07 million at the end of 2014 to 3.43 million at the end of 2016. Over the same period, total SIM card circulation increased by only 15%.[101]

A8.9 In 2017, Drillisch was acquired by United Internet, primarily a fixed telecommunications provider, which was seen to strengthen Drillisch's position in the broader telecommunications market. In 2018, the combined entity served about 4.36 million DSL and 8.31 million mobile customers. The merger allowed United Internet to offer combined broadband and mobile bundles more competitively through its 1&1 brand. Pre-merger, 1&1 had used Vodafone's network. Post-merger, Drillisch contracts allowed these customers to be served more cheaply using Telefónica's network.

A8.10 However, given that Drillisch was not guaranteed usage of Telefónica's network at current guaranteed rates from 2030, the long-term success of Drillisch was not assured. Consequently, Drillisch competed in the recently completed 5G spectrum auction where it acquired 70MHz of spectrum for €1.1 billion.[102]

A8.11 In February 2019, the EC began an investigation into whether Telefónica breached the general remedy. The Commission's preliminary view is that Telefónica failed to properly implement its obligations under the wholesale 4G access obligation. Specifically, the EC considered that Telefónica had excluded certain existing favorable wholesale agreements from its benchmark, which meant that the benchmark did not reflect 'best prices'.[103]

Analysis on investment

A8.12 In summary, from our analysis we find no effect on industry investment in the first two years following the merger but we find a large and statistically significant decline in

[101] BEREC (2018), p.29.
[102] Bundesnetzagentur, Spectrum auction comes to an end, 12 June 2019. Accessed: 28 June 2019.
[103] European Commission, Mergers: Commission alleges Telefónica breached commitments given to secure clearance of E-Plus acquisition, 22 February 2019. Accessed: 28 June 2019.

industry investment in Germany after this period until the end of our dataset. This result is consistent with the robustness tests conducted, outlined in Annex A5.

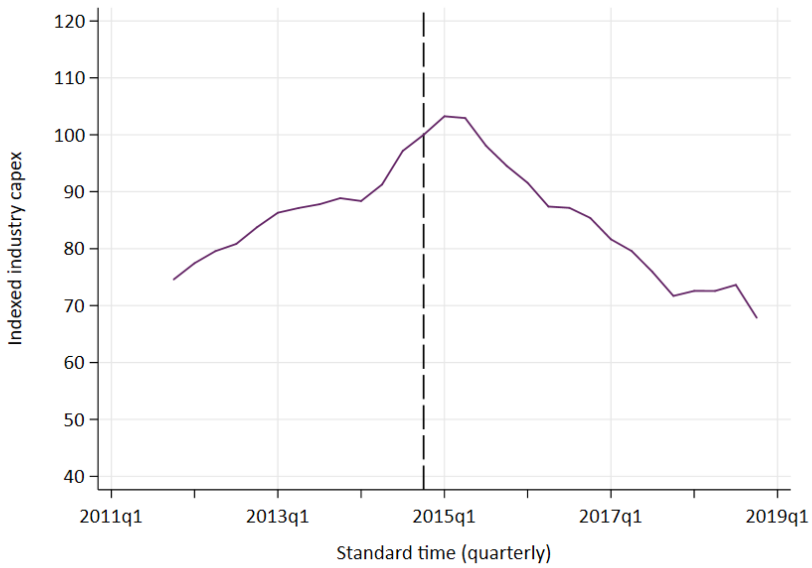
Specification

A8.13 We could not identify an appropriate synthetic counterfactual for total industry capex. This is because Germany had a higher level of industry capex relative to any of the other countries in the control group. This violates assumption (e) as set out in paragraph A5.10 and means that we cannot create a weighted combination of the countries in the control group to approximate the country with a merger. Given this, instead, we conduct the synthetic control analysis on an indexed version of industry capex (where investment is set to a base level at the time of the merger studied) – see Annex A2.

A8.14 Note that in this case we did not consider 4G rebased time as there was insufficient data to conduct meaningful analysis on. This was because of a combination of Germany's early 4G adoption in 2011, see Figure A2.7, and the date of the merger in 2014 which, after rebasing around the start of 4G rollout, resulted in a significant reduction in the number of countries and quarters remaining in the post-merger period.

A8.15 Figure A8.2 shows the trends in indexed industry capex in Germany. We observe that investment in Germany increased prior to the merger but fell sharply from 2015 onwards. In our analysis, we try to estimate a counterfactual for what would have happened to investment in Germany in the absence of a merger using synthetic control methods.

Figure A8.2: Trends in industry investment index, Germany



A8.16 As explained in Annex A5, to obtain the country-specific weights, we need a set of covariates that are predictive of our investment variable in Germany and allow us to select a set of weights that minimises the deviation between the outcomes experienced by the synthetic and the treated unit. As explained in Annex A5, this is done iteratively using combinations of covariates.

A8.17 Based on this approach, we arrive at a specification with a RMSPE of 1.928 relative to average pre-merger outcomes of ~86.4 which suggests a good match for Germany's indexed industry capex.[104] Details on this specification are presented in Table A8.1 below.

A8.18 This specification includes one observation of the dependent variable and four covariates. In particular, the specification includes the indexed industry capex for the fourth quarter of 2012. The inclusion of this appears sensible given the pattern of investment in Germany – see Figure A8.2. In addition, this specification includes the log of GDP per capita, the proportion of population living in rural areas, the percentage of prepaid connections and the unemployment rate, which as discussed in Section 3 of the main report are likely to strongly influence investment decisions in the mobile sector. As can be seen from the table, the synthetic results match reasonably well to the covariates.

A8.19 Based on these covariates, we obtain the country-weights as set out in the left-hand table of Table A8.1. This suggests that the synthetic Germany unit is made up of four countries with the UK being assigned the greatest weight.[105]

A8.20 Table A8.2 below presents summary statistics for the covariates included in this specification for Germany and each of the countries that have been assigned weight in estimating the synthetic counterfactual. This shows that Germany is not an outlier and that the covariates included in the specification for the donor pool countries are not significantly dissimilar to Germany. This therefore suggests that the assumptions as set out in Annex A5 are satisfied. In addition, we have satisfied ourselves that the change in these variables is relatively stable after the merger, including for the UK.

A8.21 Notwithstanding, given a possible concern that the large weight attributed to the UK could lead to spurious results, we carried out a sensitivity test of our results by excluding UK from the donor pool of countries. This showed our results are not sensitive to this – see discussion below. We also carried out numerous other sensitivity tests, which again showed the stability of our results. These are discussed below.

Table A8.1 – Synthetic unit country weights and pre-merger accuracy, industry capex index, standard time (Germany)

Unit	Weight
Belgium	0.083
Czech Republic	0.183
Malta	0.205
United Kingdom	0.529

Variable	Germany	Synthetic
Industry capex index (2012q4)	83.8	83.8
GDP per capita (log)	10.7	10.5
Rural population (%)	22.8	15.8
Prepaid connections (%)	51.8	51.8
Unemployment rate (%)	5.2	7.0
Pre-merger RMSPE	**1.928**	

[104] We note that one specification had a slightly lower pre-merger RMSPE as can be seen in Table A8.3. This was not selected due to a large divergence immediately pre-merger. The estimates of this specification were very similar to the preferred specification.

[105] We excluded Switzerland from the control group in this analysis. This is because Switzerland was being weighted heavily and resulted in an implausible counterfactual that fluctuated widely in the post-merger period. Switzerland is an outlier for its capex data due to an unusually large spike in capex of €1.2bn to €3.9bn from 2017q3 to 2017q4, which drove an implausible spike in the counterfactual when included. We test and present the re-inclusion of Switzerland as one of the robustness tests.

Source: Ofcom analysis

Table A8.2 – Summary statistics for countries weighted in synthetic unit, industry capex index, standard time (Germany)

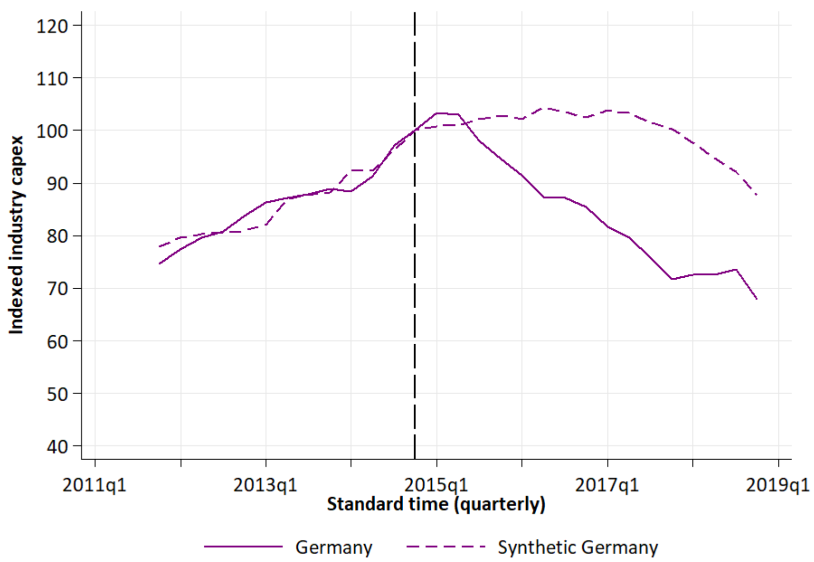
	Industry capex index			GDP per capita (log)			Rural population (%)			Prepaid connections (%)			Unemployment rate (%)		
Country	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Germany	85.1	67.9	103.3	10.7	10.7	10.8	22.8	22.7	22.8	47.4	40.7	55.3	4.5	3.4	5.6
Belgium	80.0	50.1	102.1	10.6	10.6	10.7	2.1	2.0	2.3	41.7	18.3	54.5	7.7	5.9	8.5
Czech Republic	76.8	62.4	100.0	10.3	10.2	10.4	26.6	26.2	26.8	38.3	33.0	43.7	4.9	2.5	7.0
Malta	96.3	83.7	109.2	10.5	10.3	10.6	5.6	5.4	5.9	73.5	65.6	81.3	5.2	4.0	6.3
UK	94.5	78.8	105.0	10.6	10.5	10.6	17.5	16.6	18.4	39.3	30.4	49.6	5.8	4.1	8.0

Results

A8.22 Based on the specification discussed above, Figure A8.3 below is a graphical representation of the synthetic control results, plotting Germany's actual observed outcomes against the estimated synthetic counterfactual.

A8.23 Whilst we observe a flat level of investment in the donor pool as a whole, as seen in Figure A8.2, these results predict that in a synthetic counterfactual scenario where Germany did not experience a merger, industry capex would have held steady for two years before then seeing a gradual quarter-by-quarter decline. This contrasts with the actual observed outcomes in Germany, which suggests a strong decline in the industry capex index relative to the counterfactual approximately two years after the merger.

Figure A8.3: Synthetic control results, industry capex index (Germany)



Source: Ofcom analysis

A8.24 The results of the placebo tests suggest a relatively weak level of significance with a post-/pre-RMSPE rank of 4/19 when considering the full period. However, looking in more detail, we see that the level of significance increases over time as the estimated negative effect increases. Up to 2017, the coefficients are ranked lower than the overall 4/19 whereas from 2017 onwards, the negative effect has a much stronger level of statistical significance with a rank of 2/19.

Robustness tests

A8.25 The robustness tests described in paragraphs A5.32 to A5.35 are presented below. Table A8.3 presents results of robustness tests on the variables included within the preferred specification whilst Table A8.4 presents results on changes to the donor pool and on alternative approaches to the matching process.

A8.26 In all the specifications tested, we found a consistent reduction in industry investment two years after the merger, with further reductions in the following years. There is a wide variation of results; by the fourth year, industry capex is between 12-26%[106,107] lower than it would have been absent the merger.

A8.27 The robustness tests do not consistently support the small positive effect on investment immediately after the merger from the preferred specification. Therefore, due to the uncertainty around the magnitude and direction of effect, we cannot conclude that there was any effect from the merger on industry investment in the initial year after the merger, with the decline in investment only occurring from the second year after the merger.

Table A8.3: Synthetic control variable robustness tests, industry capex index, standard time (Germany)

	Preferred Specification	Omitting variables					Additional variables			
Variables										
Industry capex index (2012q4)	✓		✓	✓	✓	✓	✓	✓	✓	✓
GDP per capita (log)	✓	✓		✓	✓	✓	✓	✓	✓	✓
Rural population (%)	✓	✓	✓		✓	✓	✓	✓	✓	✓
Prepaid connections (%)	✓	✓	✓	✓		✓	✓	✓	✓	✓
Unemployment rate (%)	✓	✓	✓	✓	✓		✓	✓	✓	✓
Population density							✓			
HHI								✓		
ARPU									✓	
Smartphone penetration (%)										✓
Pre-merger RMSPE	1.928	2.261	3.627	1.928	3.101	2.361	2.361	2.276	2.261	1.866
Merger effect										
+4 quarters	1.4	-4.1	5.9	1.4	-4.1	1.4	-5.0	-4.3	-4.1	1.7
+8 quarters	-8.7	-16.4	-2.0	-8.7	-15.4	-8.7	-16.9	-15.9	-16.4	-8.4
+12 quarters	-16.2	-25.6	-9.9	-16.2	-24.5	-16.2	-25.9	-24.9	-25.6	-15.8
+16 quarters	-14.5	-18.5	-15.5	-14.5	-19.4	-14.5	-19.2	-18.5	-18.5	-14.2
Rank (RMSPE ratio)	4/19	1/18	7/19	4/18	5/18	4/19	3/19	3/18	3/19	4/18

Source: Ofcom analysis

[106] As discussed, our analysis is on industry capex index. These percentages have been converted back in terms of Germany's industry capex.

[107] This excludes the specification that tests the re-inclusion of Switzerland into the donor pool. As discussed, this produced an unrealistic counterfactual with an even larger negative effect on industry capex.

Table A8.4: Synthetic control other robustness tests, industry capex index, standard time (Germany)

	Donor pool robustness tests				Alternative matching approaches	
	Omitting UK	Omitting Malta	4-player markets only	Including Switzerland	Three year pre-merger capex with no covariates	Extended pre-merger time series of capex with no covariates
Pre-merger RMSPE	2.174	2.280	2.264	5.542	1.229	5.407
Merger effect						
+4 quarters	7.5	-4.5	-3.2	8.7	2.0	5.5
+8 quarters	-0.8	-16.1	-15.4	3.8	-16.1	-6.8
+12 quarters	-5.8	-25.1	-24.4	-13.0	-25.5	-19.9
+16 quarters	-9.4	-18.6	-16.8	-39.8	-34.8	-22.7
Rank (RMSPE ratio)	6/18	2/17	1/5	4/19	4/20	9/20

Source: Ofcom analysis

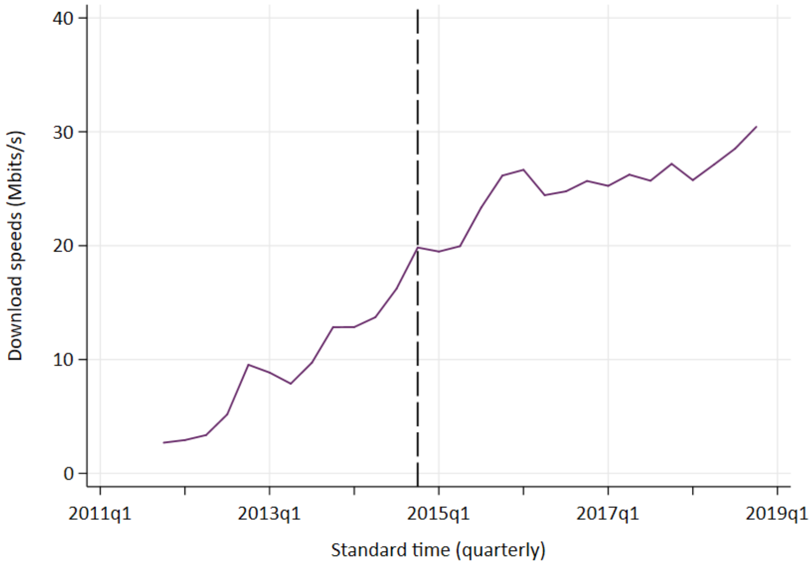
Analysis on quality

A8.28 For our analysis on quality, whilst our results consistently estimate a negative impact on download speeds driven by a two-year stagnation in the rate of growth, these are not statistically significant. Therefore, we cannot conclude that the Telefónica/E-Plus merger has had any effect on download speeds in Germany.

Specification

A8.29 Figure A8.4 shows the trends in download speeds in Germany. We observe that download speeds in Germany increased in a linear fashion from before the merger until 2016. Download speeds in Germany then stagnate, seeing no growth for two years. In 2018, the final year of our dataset, we see some evidence of download speeds returning to a similar rate of growth as prior to 2016. In our analysis, we try to estimate a counterfactual for what would have happened to download speeds in Germany in the absence of a merger using synthetic control methods.

Figure A8.4 – Trends in download speeds, Germany



A8.30 Using synthetic control techniques to analyse the effect of the Telefónica/E-Plus merger on quality, the preferred specification incorporates one observation of the dependent variable and four covariates – GDP per capita, proportion of population living in rural areas, proportion of prepaid connections, and unemployment rate. The covariates included in this specification all appear sensible given the pattern of average download speeds in Germany - see Figure A8.4 and are consistent with the specification for investment. The process for selecting the variables in this model is outlined in paragraphs A5.20 to A5.25.

A8.31 Based on these covariates, we obtain the country-weights as set out in the left-hand of Table A8.5. This suggests that the synthetic Germany unit is made up of four countries with Switzerland and the UK being assigned the greatest weights.

A8.32 Table A8.6 below presents summary statistics for the covariates included in this specification for Germany and each of the countries that have been assigned weight in estimating the synthetic counterfactual. This shows that Germany is not an outlier and that the covariates included in the specification for the donor pool countries are not significantly dissimilar to Germany. This therefore suggests that the assumptions as set out in Annex A5 are satisfied. In addition, we have satisfied ourselves that the change in these variables is relatively stable after the merger, including for the UK and Switzerland.

A8.33 Matching with the UK is similar to the analysis on investment. The addition of Switzerland when considering download speeds may intuitively be because of its location as a neighbouring country and as a highly developed country in Europe. To ensure that our results were not driven by any particular country included in the donor pool, we carried out sensitivity tests of our results by omitting the UK and Switzerland. This showed our results are robust to this – see discussion below.

A8.34 This specification results in a pre-merger RMSPE of 1.089 relative to average pre-merger outcomes of 9.7. The pre-merger synthetic Germany matches very well to that of actual Germany in terms of all covariates. This is presented in Table A8.5.

Table A8.5 – Synthetic unit country weights and pre-merger accuracy, download speeds, standard time (Germany)

Unit	Weight
Malta	0.186
Romania	0.127
Switzerland	0.356
United Kingdom	0.331

Variable	Germany	Synthetic
Download speeds (2014q3)	16.2	16.3
GDP per capita (log)	10.7	10.5
Rural population (%)	22.8	22.3
Prepaid connections (%)	51.8	50.8
Unemployment rate (%)	5.2	5.5
Pre-merger RMSPE	**1.089**	

Source: Ofcom analysis

Table A8.6 – Summary statistics for countries weighted in synthetic unit, download speeds, standard time (Germany)

	Download speeds (Mbit/s)			GDP per capita (log)			Rural population (%)			Prepaid connections (%)			Unemployment rate (%)		
Country	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Germany	18.4	2.7	30.4	10.7	10.7	10.8	22.8	22.7	22.8	47.4	40.7	55.3	4.5	3.4	5.6
Malta	28.6	3.7	54.2	10.5	10.3	10.6	5.6	5.4	5.9	73.5	65.6	81.3	5.2	4.0	6.3
Romania	20.1	2.9	32.2	9.9	9.8	10.1	46.1	46.0	46.1	54.7	45.8	63.1	6.1	4.2	7.1
Switzerland	21.3	2.5	46.4	10.9	10.9	11.0	26.3	26.2	26.4	35.8	29.7	39.7	3.1	2.6	3.3
UK	16.2	2.2	27.6	10.6	10.5	10.6	17.5	16.6	18.4	39.3	30.4	49.6	5.8	4.1	8.0

A8.35 As in the analysis on investment outcomes, we did not consider 4G rebased time. This is because of insufficient data as a result of Germany's early 4G adoption in 2011 and the date of the merger in 2014 which, after rebasing around the start of 4G rollout, resulted in a significant reduction in the number of countries and quarters remaining in the post-merger period. In addition, our data on download speeds is only available from 2011 which further restricted the control group after rebasing.

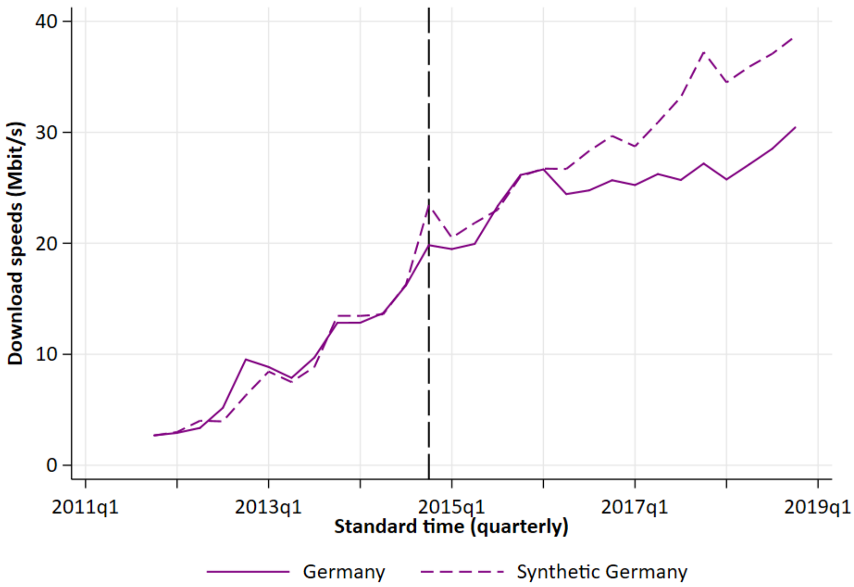
Results

A8.36 Based on the specification discussed above, Figure A8.5 below is a graphical representation of the synthetic control results, plotting Germany's actual outcomes against the estimated synthetic counterfactual.

A8.37 The results from this analysis estimate that downloads speeds in Germany closely followed that of the counterfactual in the first year after merger. After this, from the second year Germany's download speeds stagnated whereas the estimated download speeds for the synthetic counterfactual continues to increase. This stagnation lasts until 2018 where, in the last year of our dataset, download speeds in Germany appear to return to a similar rate of growth predicted in the counterfactual scenario with the negative effect of the merger on download speeds stabilising between 7-9 Mbit/s.

A8.38 However, the placebo tests result in a rank of 10/20 relative to the other countries in the control group. This suggests that the results are not statistically significant.

Figure A8.5: Synthetic control results, quality (Germany)



Source: Ofcom analysis

Robustness tests

A8.39 The robustness tests described in paragraphs A5.32 to A5.35 are presented below. Table A8.7 presents results of robustness tests on the variables included within the preferred specification whilst Table A8.8 presents results on changes to the donor pool, including omitting Switzerland and the UK which were the countries assigned the greatest weight, and on alternative approaches to the matching process.

A8.40 In all specifications tested, we found similar declines in download speeds and of similar magnitude. We also find that most specifications suggest statistically insignificant results.

Table A8.7: Synthetic control variable robustness tests, quality, standard time (Germany)

	Preferred Specification	Omitting variables					Other specifications					
Variables												
Download speed (2014q3)	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
GDP per capita (log)	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓
Rural population (%)	✓	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓
Prepaid connections (%)	✓	✓	✓	✓		✓	✓	✓	✓	✓	✓	✓
Unemployment rate (%)	✓	✓	✓	✓	✓		✓	✓	✓	✓	✓	✓
Population density							✓					
HHI								✓				
ARPU									✓			
Smartphone penetration (%)										✓		
Download speed (2013q3)											✓	
Download speed (2012q3)												✓
Pre-merger RMSPE	1.089	1.332	1.090	1.090	1.198	1.225	1.090	1.021	1.089	1.213	1.078	1.090

Merger effect												
+4 quarters	0.3	-0.9	0.3	0.4	0.1	0.3	0.3	1.7	0.4	-0.5	-0.2	0.4
+8 quarters	-3.6	-3.7	-3.7	-3.4	-3.6	-3.6	-3.6	-1.9	-3.5	-4.2	-4.5	-3.5
+12 quarters	-7.5	-10.6	-7.3	-7.2	-9.3	-7.5	-7.3	-5.8	-7.4	-11.7	-8.9	-7.5
+16 quarters	-8.6	-13.8	-8.5	-8.2	-12.2	-8.6	-8.4	-4.9	-8.4	-15.0	-9.7	-8.4
Rank (RMSPE ratio)	10/20	3/20	8/20	7/20	5/20	10/20	9/20	10/20	10/20	4/20	8/20	9/20

Source: Ofcom analysis

Table A8.8: Synthetic control donor pool robustness tests, quality (Germany)

	Donor pool robustness tests			Other tests
	Omitting Switzerland	Omitting Switzerland & UK	4-player markets only	Three-year pre-merger average download speeds with no covariates
Pre-merger RMSPE	1.148	1.149	1.318	1.015
Merger effect				
+4 quarters	1.2	1.2	1.2	1.5
+8 quarters	-2.9	-2.9	-4.2	-2.2
+12 quarters	-9.0	-9.0	-4.1	-7.1
+16 quarters	-12.6	-12.6	-4.0	-7.4
Rank (RMSPE ratio)	7/19	6/18	6/7	12/20

Source: Ofcom analysis

Discussion

A8.41 From the analysis on investment, we do not find an effect on industry capex in the first two years after the merger. However, in the third and fourth year following the merger, we find a statistically significant decline in industry capex relative to that of the synthetic counterfactual. This delay might reflect stickiness in investment and the time horizons that MNOs consider when making future investment decisions – see discussion in Section 3 of the main report. However, a decline in industry investment is not necessarily a poor outcome for consumers. There could be significant cost savings from less duplication of networks and economies of scale, which means that more focus can be given to improving consumer outcomes.

A8.42 As noted previously, we recognise that there are various dimensions which affect consumer experience. For the purposes of this study, as discussed previously, we have particularly focused our attention on network quality and in particular average download speeds. In light of the investment analysis, we proceeded to consider the effect of the merger on network quality. We found a consistently negative effect on download speeds in Germany from the second year after the merger driven by a stagnation in download speeds. However, we found this negative effect to be statistically insignificant.

A8.43 Therefore, the main finding from our analysis of this merger is that we found no positive effect on industry investment or average download speeds. This finding comes from considering a range of different models and robustness/sensitivity checks- all of which are consistent with there being no positive effect on investment/average download speeds.