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# A Retrospective Study of State Aid Control in the German Broadband Market\*

Tomaso Duso,<sup>†</sup> Mattia Nardotto,<sup>‡</sup> and Jo Seldeslachts<sup>§</sup>

16 August 2024 – This is an updated version of the 2021 Discussion Paper.

**Abstract** We provide an evaluation of the impact of German public subsidy schemes in municipalities of Bavaria and Lower Saxony aimed at supporting the deployment of basic broadband infrastructure in rural Germany. Such subsidies are subject to state aid control by the European Commission and may only be granted if the potential market failure is addressed without distorting competition. We first analyse the consequences of the subsidies on reducing the digital divide between urban and rural areas. Second, and more novel, we examine the impact on different measures of competition: market entry and the resulting effect on prices and broadband offerings. We use an instrumental variables (IV) approach based on the interaction between technical features of the programme and political cycles in order to exploit plausibly exogenous variation in the allocation of subsidies. We find that the subsidies significantly increased broadband coverage. More interestingly, the number of internet service providers (ISPs) increased significantly in municipalities that received subsidies, which, in turn, led to a relative increase in the number of local broadband plans and a relative decrease in average prices. Our results show that well-designed state aid need not distort competition and can even enhance it.

**Keywords:** State Aid, Ex-Post Evaluation, Broadband, Coverage, Entry, Competition, Prices.

**JEL Codes:** C23, D22, L1, L4, L64

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# 1 Introduction

Given the importance of information and communication technology (ICT) in modern economies (e.g., Czernich et al., 2011; Akerman, Gaarder and Mogstad, 2015), developing a fast and reliable broadband infrastructure is a priority for countries in the European Union (EU) and it was the core of the “Digital Agenda for Europe” (DAE).<sup>1</sup> However, the fixed costs that must be undertaken to set up a telecommunication network are sizable.<sup>2</sup> Therefore, telecom internet service providers (ISPs) are reluctant to roll out up-to-date technologies in areas where the expected demand for internet services is not sufficiently high, such as rural regions (Greenstein, 2020). Thus, most EU countries are allocating large amounts of public resources with two goals: (i) to sustain investment in new infrastructure, and (ii) to ensure that areas that are less attractive for ISPs do not fall too far behind.<sup>3</sup>

In principle, subsidies by national governments to companies, i.e., “state aid,” are forbidden by EU treaty because they are likely to distort competition and adversely affect trade. However, the EU rules recognise that state aid, in general, and investment in broadband, in particular, can contribute to economic development.<sup>4</sup> For this reason, the European Commission (EC) has the power to decide upon certain types of aid measures. As specified in Article 107 of the Treaty on the Functioning of the European Union (TFEU), state aid is allowed by the EC if it passes the so-called *balancing test*, where the beneficial effects of state aid are weighted against their potential adverse effects on competition and trade. In particular, state aid is allowed in the EU when (i) it is well designed to appropriately solve a potential market failure, and (ii) distortions to competition are limited such that the overall balance is positive.<sup>5</sup>

This balancing test is traditionally performed on an *ex-ante* base with little economic analysis (Munoz de Juan, 2018). Yet, the EC increasingly recognises the role of an *ex-post* economic analysis in controlling state aid. Indeed, the EU’s State Aid Modernisation (SAM) initiative by the Directorate General of Competition (DG Competition) in 2014 introduced the obligation for retrospective evaluations of aid schemes (Friederiszick et al., 2018). This obligation kick-started a literature on the retrospective evaluations of different types of state aid, mainly in the areas of R&D subsidies (e.g., Szücs, 2020, and references therein) and regional aid (e.g., Becker, Egger and von Ehrlich, 2018, and references therein).

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<sup>1</sup>Communication from the Commission to the European Parliament, the Council, the European Economic and Social Committee, and the Committee of the Regions, COM(2010) 245, A Digital Agenda for Europe.

<sup>2</sup>For example, for the most costly technology, fibre to the home, a 2017 study by the European FTTH Council estimates the cost to complete coverage across the EU28 countries to be €137bn (FTTHCouncil, 2017).

<sup>3</sup>Indeed, there is often a large gap between social and private incentives to invest in telecom infrastructure, as Nevo, Turner and Williams (2016) find for US broadband markets. While, in theory, public investment might crowd out private investment, the empirical findings of Wilson (2023) suggest that these two investments are complementary in the sense that public investment in fibre induces more private investment through both anticipatory effects and responses to public investment.

<sup>4</sup>See, for example, Criscuolo et al. (2019) for the impact of state aid on regional development and Briglauer et al. (2019) for an application of the impact of broadband deployment in Germany on employment.

<sup>5</sup>For a detailed exposition of the balancing act in the context of broadband state aid, see Gómez-Barroso and Feijóo (2012).

In this context, however, it is surprising that there are not yet retrospective studies performed that provide a full assessment of the balancing test. Indeed, while many studies look at the impact of subsidies on those outcomes that measure the identified market failure, there is, to the best of our knowledge, no study that investigates the impact of state aid on *competition*.<sup>6</sup> The aim of the current paper is to fill this gap. The underlying idea is make a start to bringing the evaluation of state aid control more into line with the experience in other areas of competition policy, such as merger control and abuse of dominance, where competitive assessments are at the core (Robins and Geldof, 2018).<sup>7</sup> More generally, the insights gained from our approach can also be seen as a first step towards a better understanding of how industrial policies interact with competition and how they can best be designed to be not detrimental to the functioning of markets. Piechucka, Saurí-Romero and Smulders (2023) provides an excellent overview of current issues on the topic. Specifically, they suggest that EU state aid control rules can be considered a blueprint for designing and assessing efficiency-enhancing industrial policies.

We examine the impact of a national German subsidy scheme in rural municipalities in Bavaria and Lower Saxony, announced in 2008, and subsequent regional programmes in 2009 and 2010. We restrict our treated municipalities to Bavaria and Lower Saxony, as only in these two states are data on municipal subsidies available (see the data section below). These programmes, which were implemented between 2011 and 2013, aimed to support the development of basic broadband infrastructure in rural areas and were approved by the EC. Our analysis aims to assess the impact of these aid schemes on broadband availability – the outcome that measures the market failure – as well as their impact on competition.

There are several ways to characterise the potential distortions of product market competition in the area of state aid in terms of theories of harm, as suggested by Verouden and Stehmann in Flynn (2016). We therefore look at different measures of competition. First, we examine whether the subsidies actually harmed competitors, as this is the traditional measure of competition used in state aid control in the rare cases where the EC has examined this dimension (Heidhues and Nitsche, 2006). Specifically, we assess the impact of the aid on the number of active ISPs in each municipality, with the logic that if the number of providers did not decrease after the aid, then the aid must not have harmed competitors.<sup>8</sup> However, there is a strong case for looking further at whether the aid has harmed consumers, as consumer welfare is the standard used in other areas of competition policy (see Heidhues and Nitsche,

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<sup>6</sup>There are papers that look at the effect of state aid on related outcomes, such as financial viability of firms (Heim et al., 2017), firms' growth (Cerqua and Pellegrini, 2014), or firms' productivity (Sergant and Van Cayseele, 2019) and (Criscuolo et al., 2019). Bourreau, Grzybowski and Muñoz-Acevedo (2022) look at the effectiveness of a broadband scheme in France, accounting for the endogeneity of entry. Wilson (2023) employs a structural model to investigate the entry choices made by ISPs in the US broadband market and compares various subsidy schemes to support the building of broadband infrastructure.

<sup>7</sup>Verouden and Stehmann, however, argue in Flynn (2016) that applying a more economic approach to state aid should perhaps not be understood as making it fully equal to antitrust or merger control. Indeed, it is often impossible to weigh state aid's benefits (such as regional cohesion) against its costs (such as distorting competition). Our setting, however, as we later explain, is well suited to make a precise assessment of both benefits and costs.

<sup>8</sup>Our logic is the following: if the aid induces more ISPs to be present in a municipality, then there will be more competitors. Therefore, assuming that ISPs are only present if they are profitable, the aid cannot have harmed rivals.

2006, for a detailed discussion). Therefore, in a second step, we look at how the aid affected the number of broadband offers (variety) and broadband prices, measures that are directly related to consumer welfare.

We collect and merge data from several sources and create a database that covers a panel of all municipalities in Bavaria and Lower Saxony for the years 2005-2015. The database contains information on key variables of the broadband internet market: broadband coverage (at different download speeds), number of ISPs (both in total and differentiated by technology), number of broadband plans (local and national), and corresponding prices. We complement this data with information on the subsidies allocated to the municipalities, and with other key factors driving investment decisions at the local level, such as socio-demographic characteristics, industrial presence, and geographical characteristics. Finally, we collect information on political variables, both at the regional state and at the municipality levels, as these factors played a role in the process of requesting aid.

Our identification strategy is based on an instrumental variables (IV) approach, which addresses concerns about the potential endogeneity of state aid applications across municipalities. We use a long-run within-municipality difference in outcomes and control variables. Using these within-municipality differences, which account for time-invariant unobserved factors, we estimate a model in which the municipality's use of state aid is instrumented by variables that (i) play a role in influencing the cost of applying for the grants and (ii) are related to the political cycle at the local level. In other words, we use an instrumental variables approach based on the interaction between technical features of the programme and political cycles to exploit exogenous variation in the allocation of subsidies. This should address the remaining endogeneity concerns due to time-varying unobserved heterogeneity. To further support our identification strategy, we conduct two tests: (i) a falsification or placebo test, where we estimate the same model in the years prior to the introduction of the programme, and (ii) a test to verify that our instruments are not systematically associated with trends in demographics, variables that are demand-related drivers of broadband investment.

We show that the aid schemes successfully increased broadband coverage within the aid implementing municipalities. Specifically, we measure an additional increase due to the aid ranging between 16 to over 26 percentage points (depending on the connection speed), in line with the findings of Briglauer et al. (2019). However, our main contribution is to show that the implemented schemes did not distort competition. On the contrary, the number of ISPs in the aid-implementing municipalities increased by 0.75 additional entrants between 2010 and 2015. For the most subsidised technology, digital subscriber line (DSL), this effect amounts to an increase of almost 30%. Second, we find a negative impact on the average price per Mbit/s, which is reduced by 13% due to the aid. This observed decrease in the average price goes hand in hand with a relative increase in the number of local broadband plans of more than 50%, while the number of national plans is not affected. Given that local plans are cheaper than national plans, their relative increase drives down the average price.

Finally, we disentangle the mechanism through which state aid has affected both outcomes. While

the increase in broadband coverage is directly driven by the additional infrastructure investment financed by the aid, the entry of ISPs is directly responsible for the price reductions.<sup>9</sup> Thus, our results show that well-designed state aid programmes need not distort competition and can even enhance it. A more general interpretation of these findings is that industrial policy, competition, and consumer welfare can go hand in hand.<sup>10</sup>

The paper is structured as follows. In Section 2 we provide some background information on the state aid schemes. Section 3 describes the data, whereas Section 4 shows a first look at the broadband market. We present our econometric model and empirical results in Section 5. Section 6 shows extensions and robustness checks while section 7 concludes.

## 2 Broadband in Germany and the state aid schemes

The development of the broadband internet market in Germany is akin to the rest of the EU. At the end of the 1990s, when the first versions of DSL technologies were introduced, the market was dominated by the former state-owned telecommunications company, Deutsche Telekom (DT). Following the EU Regulation EC 2887/2000 and the Directive 2002/19/EC, the German government passed the so-called *open access* policies, which mandated the incumbent to open the market to new entrants and allow them to provide internet services over DT's network. As in other EU countries, these policies allowed the rapid growth of the market share of new ISPs competing with DT for internet users accessing the internet over fixed lines. In the first decade of the 2000s, as mobile internet technologies were not yet sufficiently developed, the main alternative to DSL access was cable, another fixed-line technology, which had a market share of around 10% at the national level. With the start of the second decade of the 2000s, mobile technologies began to spread rapidly, mostly as a complement to fixed lines. More recently, the next step in fixed internet access – fibre – has begun, although with a relatively slow take-up rate compared to previous technologies.

Figure 1 shows the evolution of broadband penetration over time. Thanks to the large investments of telecom companies, internet access quickly diffused across Germany. However, despite a sustained growth of internet penetration, which in just a decade went from only 10% to more than 80%, access to the internet was not uniform. This phenomenon is often referred to as the *digital divide*, a situation

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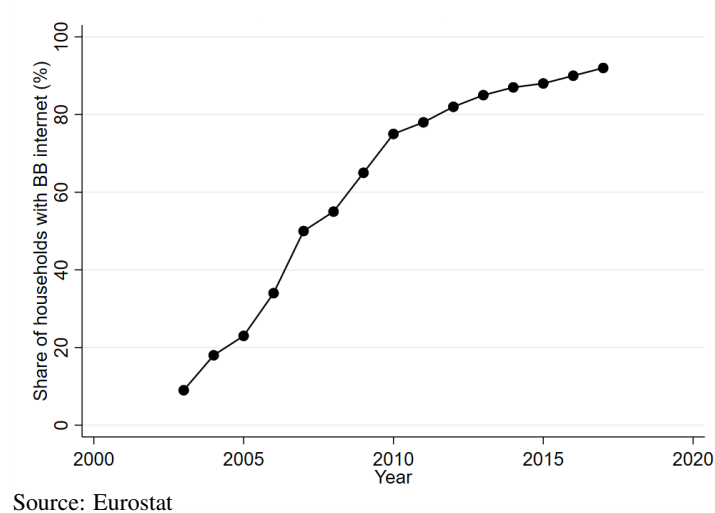
<sup>9</sup>In a related study, Nardotto, Valletti and Verboven (2015) show that market entry induced by local loop unbundling had the effect of fostering both product differentiation and large quality improvements in the UK's fixed broadband market. Another relevant paper in our context is Fan and Xiao (2015), which studies various subsidy policies designed to encourage entry. They estimate a dynamic entry game based on data of actual and potential entrants after the 1996 Telecommunications Act, which opened the monopolistic US local telephone industry to new entrants. Examples for the positive link between entry –or more in general the number of firms active in a market– and market outcomes can also be found for other sectors, such as the mobile telecommunication industry (Genakos, Valletti and Verboven, 2018), the banking sector (Cohen and Mazzeo, 2007), and the food industry (Toivanen and Waterson, 2005).

<sup>10</sup>This aligns with findings by Aghion et al. (2015). By using a dataset of medium and large enterprises in China, they show that industrial policies in competitive sectors increase productivity growth (where competition in a sector is defined based on the Lerner Index).



that the OECD (2001) defines as follows: “The gap between individuals, households, businesses and geographic areas at different socio-economic levels with regard both to their opportunities to access information and communication technologies and to their use of the internet for a wide variety of activities.” While this statement covers many alternative definitions, the divide targeted by the aid schemes studied in this paper mostly concerns the gap between rural and urban areas.

Figure 1: Broadband penetration in Germany



The difference between rural and urban areas in Germany can be summarised as follows: In 2010, a decade after the introduction of broadband internet, 19% of municipalities had internet access at speeds of 16Mbit/s or higher, but these municipalities accounted for over 40% of the total population, indicating that these are large/densely populated (urban) municipalities. This disproportionate coverage of a minority of municipalities is the result of simple economic trade-offs: urban areas provide stronger incentives for ISPs to undertake large, fixed, investments, thus generating a gap in internet access with rural areas, sometimes to the point of having no internet access at all. Reducing this gap by investing in rural areas was the aim of the state aid we study.

The schemes were set up to support the construction of basic broadband service networks, later stated in the 2009 broadband guidelines of the EC (European Commission, 2009). The main idea behind them was to intervene by closing the “profitability gap” in deploying broadband infrastructure, i.e., the difference in investment costs and profitability thresholds, to provide similar broadband services in rural areas compared to urban areas.

Intervention areas were mostly designed at the level of the municipalities, which were the implementing authorities; although the ultimate aid recipients were the companies designated as beneficiaries via tenders.<sup>11</sup> Municipalities could apply for the aid and implement the scheme if at least some parts

<sup>11</sup>We unfortunately have no information on how the tendering processes worked exactly and which exact ISP was the beneficiary, as this data was not kept systematically by national or local authorities. Therefore, the unit of observation in our

of the relevant area were identified as “white areas,” i.e., areas where no provider was offering internet services.<sup>12</sup> A key obligation of the schemes was to provide third-party wholesale access to the broadband infrastructure built with granted aid. In most municipalities, a sort of “two-stage beauty contest” was used to award the subsidy (see Bayerisches Staatsministerium für Wirtschafts, Infrastruktur, Verkehr und Technologie, 2012). First, the municipality had to launch a market consultation procedure (“Markterkundungsverfahren”) to identify which companies had the technical capacity and potential interest to implement the necessary infrastructure investments. During this market consultation process, interested ISPs had to present their preliminary plans. The second step was the selection procedure (“Auswahlverfahren”). Municipalities were in principle obliged to publish in advance all selection criteria – which had to be formulated in a supplier- and technology-neutral manner – and the selection procedure had to be based on objective standards. However, anecdotal evidence suggests that often only a subset of interested ISPs were invited to present detailed plans for infrastructure development. Furthermore, case studies show that the selection was based on the specificities of the individual project (see Bayerisches Staatsministerium für Wirtschafts, Infrastruktur, Verkehr und Technologie, 2012).

Because of data limitations, we can only investigate the impact of a national aid scheme, as well as two subsequent regional programmes, in Bavaria and Lower Saxony. All three projects received approval from the EC. First, scheme N115 was issued in July 2008 and consisted of €141 million (decision N 115/2008). The aid intensity for each infrastructure project was related to the profitability gap, initially limited to a maximum amount of €100,000 in public funding for each project. As the N115/2008 scheme was not sufficiently employed, the German authorities notified amendments with the schemes N368/2009 and N299/2010. Indeed, only 20% of the available funding was requested by the end of 2009. The remaining funds were transferred to subsequent years. The amendments were mainly aimed at modifying some of the rules approved in the previous scheme. Moreover, the maximum amount in public funding was increased to €200,000 per project.

In addition to these national schemes, which were mainly aimed at supporting the deployment of broadband lines to households, the states of Lower Saxony and Bavaria decided to mobilise additional public funding of around €45 million. These regional schemes focused specifically on broadband availability for businesses and were not expected to be implemented on top of the national scheme, i.e., projects were funded under only one scheme. However, within the same region, several projects were implemented under different schemes. The actual investments supported by these schemes occurred in the following years, mostly from 2012 onwards.

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analysis is a municipality that received aid. In some cases, several municipalities coordinated and applied jointly for the aid; see also our IV strategy.

<sup>12</sup>To be more precise, to obtain national aid, the municipality had to provide a detailed analysis of the current state of its broadband infrastructure, as well as an analysis of future infrastructure needs (“Bedarfsanalyse”). Municipalities had to provide a clear rationale for their needs, supported by maps showing which roads or parts of the municipality were underserved, i.e., where private households had internet access slower than 1Mbit/s. For regional aid, business needs were also taken into account.

In all three schemes, the EC has taken into consideration the potential qualification of the aid as *de minimis*: small subsidies are supposed to have unsubstantive competitive effects and do not need the approval of the EC.<sup>13</sup> Therefore, every single project financed under a particular scheme would not be considered to be state aid under the EC's rules. However, prolongations and amendments moved the aid amount beyond a *de minimis* level. Moreover, while the EC approved the schemes, concerns were raised as some ISPs were expected to be awarded aid for several local projects in different municipalities and, therefore, could have received cumulative aid over €200,000 over three years. Nevertheless, the EC ruled that the distortions of competition and the effect on trade were most likely still limited, and, therefore, that the overall impact of the measures was likely to be positive. This paper investigates that assumption.

There are several reasons why these schemes are interesting to study. First, the local nature of the schemes allows us to carefully address the identification issue, as subsidised municipalities can be compared to similar, unaffected municipalities within the same national market. Moreover, as the municipalities had considerable discretion in designing the different steps of the aid schemes, as well as in allocating the aid, we can address potential endogeneity issues using insights from the process, both in terms of its technical specificities and in terms of the politics involved. Furthermore, the schemes are targeted. Therefore, it is easier than for other forms of state aid to define outcomes and measure the effects of state aid. Further, data availability for Germany is particularly good, with detailed information on broadband coverage, the number of ISPs, and price data available at the municipal level. Finally, there are several academic papers analysing broadband markets at this level of aggregation (e.g., Falck, Gold and Heblich, 2014; Nardotto, Valletti and Verboven, 2015). These examples are useful to support methodological choices.

### 3 The Data

Our empirical analysis seeks to identify the impact of the aid schemes on the broadband market, and, in particular, on the availability of internet access (coverage), the entry of ISPs, the number and type of broadband plans, as well as their price. To perform this analysis, we combine several data sources described in the remainder of this section.

**Data on the state aid schemes.** State aid was accessible for municipalities in the whole country, both because of the presence of the national scheme, and because several states supplemented the national scheme with regional schemes. However, we only obtained detailed municipality-specific information for two states: Bavaria and Lower Saxony.<sup>14</sup> Two variables are reported: (i) an indicator variable for

<sup>13</sup>“*De minimis* aid refers to small amounts of state aid to undertakings (essentially companies) that EU countries do not have to notify to the EC. The maximum amount is €200,000 for each undertaking over a three-year period.”(<https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=LEGISSUM%3A0802%2>, pg. 1)

<sup>14</sup>Bavaria was the state that obtained the highest level of public support for the expansion of its broadband network. According to aggregated data obtained from the German Federal Ministry for Food and Agriculture, Bavaria alone accounted

receiving the aid, and (ii) the amount of the aid.<sup>15</sup> This data is cross-sectional, meaning that we do not know the exact moment when the funding was transferred nor when the ISP made the investment. This has implications for our empirical strategy, as we later discuss.

**Data on broadband coverage and ISP entry between 2010 and 2015.** We obtained data on broadband availability from TÜV Rheinland Consulting GmbH. TÜV collects geo-referenced information on internet coverage and on ISPs' network for the German Federal Ministry for Transport and Digital Infrastructure within the "Breitbandatlas" project.<sup>16</sup> This data is aggregated at the municipality level and spans the years from 2010 to 2015.<sup>17</sup> It reports for each municipality/year an indicator variable for broadband availability – i.e., the percentage of the households in the municipality that could access the broadband infrastructure – at different speeds (2Mbit/s, 6Mbit/s, 16Mbit/s) and a set of variables for the number of active ISPs per technology class – DSL, cable, mobile (Long Term Evolution (LTE) and Worldwide Interoperability for Microwave Access (WiMax)), and fibre to the home (FTTH).<sup>18</sup> Due to the availability of state aid data on the municipality level for Bavaria and Lower Saxony, both located in West Germany, we requested data on all West German municipalities. We do this to compare the municipalities that we use in the empirical analysis to the rest of the municipalities in the country.<sup>19</sup>

**Data on the broadband market between 2005 and 2008 and cartography data.** We complemented the previous data on the broadband market with two datasets on the network infrastructure and on the geo-conformation of the terrain. The first dataset is taken from Falck, Gold and Heblich (2014) and contains information on the topology of the network, which allows us to compute the number of the main distribution frames (MDFs) serving each municipality and the (linear) distance between each MDF and the municipality, an important factor driving the quality of broadband connections. Furthermore, this data reports the levels of coverage for the years 2005-2008 for the (basic) speed of 1Mbit/s. The second dataset was obtained from the Federal Agency for Cartography and Geodesy and is used to compute two variables, altitude and ruggedness, which capture part of the costs to deploy

for over 40% of the money spent to support broadband infrastructure in West Germany, and Lower Saxony for about 13%. Therefore, these two states cover more than half of the aid given under the schemes. The data for these two states have been provided by the Bavarian State Ministry for Economics, Media, Energy and Technology, and by the Lower Saxony Ministry for Economics and Transportation.

<sup>15</sup>We have no exact information on which applications were approved, but the evidence suggests that virtually all applications were granted. For Bavaria, of the 1,458 Bavarian municipalities that were advised by a consultancy firm commissioned by the Bavarian Ministry of Economic Affairs, 1,409 projects (i.e., 97%) received funding for infrastructure.

<sup>16</sup>See <https://bit.ly/2fB2GSN>.

<sup>17</sup>This is an update of the data that was used in Falck, Gold and Heblich (2014).

<sup>18</sup>The level of detail, both in terms of speed and technology, is important for our analysis. For example, while the aid was aimed at basic coverage (up to 2 Mbit/s), there might have been spillover effects to higher speeds. Moreover, while the aid was aimed to be technology neutral, it was only effectively granted to DSL and mobile technologies (LTE and WiMax), as cable technology could not meet the open access obligations necessary to receive the aid, whereas FTTH was too expensive at the time.

<sup>19</sup>There are good reasons to believe that the (formerly) East German broadband markets are still (differently) affected by the investment decisions made after the reunification, which would make them poor control groups (see Falck, Gold and Heblich, 2014).

the infrastructure.<sup>20</sup> Both datasets cover the entire country and, thus, also the two states on which we focus.

**Data on prices of broadband plans for the period 2010-2015.** We obtained data on the price of all broadband plans for each prefix in Bavaria and Lower Saxony for the period 2010-2015 from [www.Teletarif.de](http://www.Teletarif.de), which is a German price comparison website. The dataset contains a number of variables including a “comprehensive” monthly price, which covers activation costs per quota (assuming a contract duration of two years – the typical duration in Germany), connection speed, and other features of the plan, such as the included data (when capped to a maximum per month). We focus on broadband-only plans and thus exclude plans where broadband is bundled with other services, such as voice or TV. This choice is driven by the idea of creating a clean proxy for price effects at the broadband level, as disentangling the broadband component of bundles would be difficult and likely to be subject to noise. These data are provided at the level of telephone prefixes, which we have mapped to municipalities, where the topology of prefix and municipality mostly coincides for small towns.

**Data on socio-demographic characteristics and politics.** Data on socio-demographics characteristics and the political composition of local councils was obtained from the German Census databases collected within the Regional Statistical Data Catalogue of the Federal Statistical Office and the statistical offices of the individual states.<sup>21</sup> For each municipality, the census reports a rich set of variables, including total population, average income, education, age structure, population density, unemployment and the percentage of area in each municipality used by businesses. Most variables were available at the municipality level, while some others were only available at the more aggregated county level (each county contains an average of 16 municipalities). We use these variables in our empirical analysis as control variables and in the matching procedure used in the extension to capture the role of demand-side factors driving ISP investment decisions.

Our identification strategy relies on an instrumental variable framework in which the municipalities’ decision to apply for and allocate aid is instrumented with variation in the political, technological and competitive environment across municipalities. Therefore, the data also include information on the timing of municipal and state elections (which varies across German states), the length of terms of office (which varies between four and six years), the parties in power, the distance to the MDF, and the number of large ISPs operating in nearby municipalities. These variables are used to construct our instruments for aid allocation.

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<sup>20</sup>Bundesamt fuer Kartographie und Geodessie, see <https://www.bkg.bund.de/EN/Home/>

<sup>21</sup>See <https://www.regionalstatistik.de/genesis/online>

## 4 A first look at the broadband market

In this section, we describe the German broadband market between 2010 and 2015, i.e., the period of implementation of the aid schemes. We also compare the two states we focus on, Bavaria and Lower Saxony, with the whole West German market to show that they are representative. As noted above, the unit of observation for our analysis is the municipality, as this is the geographical unit at which both broadband and state aid data are available; other data, such as prices, have been re-mapped to this level.

Table 1 reports descriptive statistics on demographic characteristics in *Panel A*, the broadband network in *Panel B*, coverage and entry of ISPs in *Panel C*, the state aid in *Panel D*, the price and number of broadband plans in *Panel E*, and our instruments in *Panel F*.

**Socio-demographic characteristics and the network's structure.** The socio-demographic characteristics of West Germany reported in *Panel A* of the table show that municipalities have an average population of 7,600. Of these, just under 30% have a college degree and about 55% are aged between 24 and 55. The average unemployment rate is 5.6%. On average, the municipalities in Bavaria and Lower Saxony are not very different from the other municipalities in western Germany. They are slightly less populous and less densely populated, as they are slightly richer but not more educated, as measured by the share of people with a college degree.

*Panel B* reports information on the DSL network. The average distance between a node of the network (MDF) and the centroid of the municipality served by the node is 2,798 meters. This distance is an important factor driving the actual quality of DSL connections, as the signal strength decays with distance, and this factor is taken into account by ISPs when deciding where to invest. The sample of municipalities in Bavaria and Lower Saxony displays an average distance close to the full sample. The number of MDFs per municipality is 0.7. This indicates that a node often serves more than one municipality in rural areas. Finally, the panel reports the terrain ruggedness index.<sup>22</sup> The value of the index indicates that, on average, the municipalities are located in flat areas, with some variation due to the presence of mountains mainly located in the southern part of Bavaria. In this respect, the presence in the sample of a flat state, Lower Saxony, and a more mountainous state, Bavaria, makes the sample that we consider a good representation of an average municipality in the country. This variable proxies for some of the infrastructure costs that ISPs incur when they decide to dig the terrain and connect an area. These costs are greater when operating in a rugged area, and thus influence the decision to invest.

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<sup>22</sup>This index is computed as in Riley, DeGloria and Elliot (1999). It is calculated as the difference in elevation values between a centre cell and the eight cells surrounding it. It then averages the squares of the eight elevation differences. Finally, the index is the square root of this average. The index ranges from *level* (0 - 80), to *moderately rugged* (240 - 497), to *extremely rugged* (959 - 4367).

Table 1: Descriptive Statistics

	West German municipalities				Bavaria and Lower Saxony			
	Mean	Std. Dev.	Min.	Max.	Mean	Std. Dev.	Min.	Max.
<i>Panel A: Socio-demographics</i>								
Total population (1,000s people)	7.6	32.0	0.1	1429.6	6.7	31.1	0.2	1429.6
Income (€1,000s)	0.0	0.0	0.0	0.1	0.0	0.0	0.0	0.1
College degree (%)	28.1	8.2	0.0	70.3	26.3	9.5	0.0	70.3
Population between 24 and 65 (%)	54.7	2.6	31.7	74.2	54.7	2.3	36.9	71.6
Population density	210.7	294.4	3.5	4601.2	173.3	257.1	3.5	4601.2
Unemployment rate (%)	5.3	1.8	1.4	16.4	4.7	2.0	1.4	14.7
Area for firms and industry (%)	0.8	1.2	0.0	16.4	0.7	1.0	0.0	16.1
<i>Panel B: Network structure and supply-side factors</i>								
Distance MDF–municipality (in m)	2796.3	1804.1	11.5	14832.7	2777.2	1919.4	11.5	14832.7
Number MDFs serving the municipality	0.7	1.8	0.0	56.0	0.7	1.6	0.0	56.0
Ruggedness index	38.3	32.8	0.5	289.5	29.8	30.2	0.5	289.5
<i>Panel C: Coverage and entry of ISPs</i>								
DSL Coverage 1Mbit/s in 2008	0.9	0.2	0.0	1.0	0.9	0.1	0.0	1.0
DSL Coverage 2Mbit/s in 2010	62.3	36.6	0.0	100.0	65.2	32.8	0.0	100.0
DSL Coverage 2Mbit/s in 2015	86.1	24.5	0.0	100.0	90.1	17.7	0.0	100.0
DSL Coverage 6Mbit/s in 2010	40.0	37.3	0.0	100.0	43.9	35.0	0.0	100.0
DSL Coverage 6Mbit/s in 2015	72.6	33.4	0.0	100.0	79.6	25.8	0.0	100.0
DSL Coverage 16Mbit/s in 2010	17.6	24.6	0.0	100.0	18.8	23.8	0.0	100.0
DSL Coverage 16Mbit/s in 2015	55.3	36.2	0.0	100.0	61.0	31.5	0.0	100.0
Number of ISPs in 2010	2.8	1.5	1.0	8.0	2.8	1.4	1.0	8.0
Number of ISPs in 2015	5.3	1.3	2.0	11.0	5.4	1.2	3.0	11.0
<i>Panel D: State-aid</i>								
State aid – Share municipalities with aid (%)	–	–	–	–	60	–	0	100
Funding (€1,000s)	–	–	–	–	86.5	55.9	10.0	673.7
<i>Panel E: Broadband plans</i>								
Average effective price in 2010	–	–	–	–	74.3	20.6	23.1	167.3
Average effective price in 2015	–	–	–	–	52.6	14.8	21.3	109.9
Number of plans in 2010	–	–	–	–	1232.9	61.9	1199	1663
Number of plans in 2015	–	–	–	–	624.9	54.6	571	772
Number of local plans in 2010	–	–	–	–	34.6	38.6	13	324
Number of local plans in 2015	–	–	–	–	27.4	21.9	6	102
<i>Panel F: Instruments</i>								
Same party last election (%)	–	–	–	–	81.2	–	0	100
Few municipalities MDF (%)	–	–	–	–	59.9	–	0	100
Same party (%)	–	–	–	–	53.2	–	0	100
Share aid neighbours (%)	–	–	–	–	61.5	–	0	100
Large ISPs 2010 neighbours	–	–	–	–	2.35	0.78	1	4

Notes: *Total population* is the resident population (in thousands). *Income* is the average income (in thousands €). *College degree* is the share of population with a university degree or equivalent. *Population density* is the number of inhabitants per km<sup>2</sup>. *Area for firms and industry* is the share of surface in the municipality occupied by firms and factories. *Distance MDF-municipality* is the linear distance in meters between the MDF and the geographical centroid of the municipality. *Number MDFs serving the municipality* is the number of MDFs connected to the municipality. *Ruggedness index* is the terrain ruggedness index, proposed in Riley, DeGloria and Elliot (1999). Variables in Panel C on coverage, *DSL Coverage X Mbit/s in 20YY* is the share of households covered by broadband at minimum speed of X Mbit/s in year 20YY. *Number of ISPs in 2010* and *Number of ISPs in 2015* is the number of ISPs active in year 2010 and 2015, respectively. *Share of municipalities with aid* is the share of municipalities receiving state aid. *Funding* is the amount of the subsidy in thousands €. *Average effective price in 2010* and *Average effective price in 2015* are the average monthly price (including ancillary costs) for broadband connections in the municipality in 2010 and 2015, respectively. *Number of (local) plans* is the number of broadband plans offered by the (local) ISPs. *Same party last election* is the share of municipalities where the party in power in 2008 is the same as in previous electoral cycle. *Few municipalities MDF* is the share of municipalities sharing the same MDF with at most one other municipality. *Same party* is the share of municipalities that have the same party in majority as the other municipalities covered by the same MDF. *Share aid neighbours* is the share of aid proposals granted to neighbouring municipalities. *Large ISPs 2010 neighbours* is the number of large ISPs operating in nearby municipalities in 2010.

**Broadband coverage and market entry.** *Panel C* of table 1 reports information on broadband coverage at different speeds.<sup>23</sup> The first row in the panel reports the overall coverage at the speed of at least 1Mbit/s in 2008.<sup>24</sup> At this threshold, more than 90% of the municipalities were served in 2008. This market coverage, however, does not exclude laggards, as the presence of municipalities without any coverage shows (the minimum is zero). Moving to higher speed thresholds, coverage increased at all speeds during the period 2010–2015, with lower coverage rates for higher speeds.<sup>25</sup> The coverage at the minimum level of 2Mbit/s, which was only slightly above 60% in 2010, increases by more than 20 percentage points in five years, corresponding to almost full coverage once we look at the share of the population covered. Similarly to what happened for the basic speed of 2Mbit/s, coverage at the higher speeds – 6Mbit/s and 16Mbit/s – increases substantially, the latter getting close to 60%, in terms of municipalities covered, and close to 80% in terms of population. The picture is qualitatively not different in Bavaria and Lower Saxony. These two states had very similar coverage rates to the rest of the country for the 1Mbit/s speed in 2008, while they were slightly ahead in the period 2010 to 2015 compared to the rest of the country at the speeds of 2Mbit/s, 6Mbit/s, and 16Mbit/s.

The increase in market coverage went hand in hand with an increase in the number of ISPs active in the market. The average number of ISPs operating in a municipality almost doubled between 2010 and 2015, going from 2.8 to 5.3. In this dimension, Bavaria and Lower Saxony look very similar to the rest of the country.<sup>26</sup> The two technologies that experienced the strongest entry process are DSL and LTE. Starting with DSL, more than half of the municipalities had only one ISP active in 2010, while five years later the median municipality had three ISPs. The entry process in the LTE technology is even stronger. In 2010, more than three-quarters of municipalities had no operator, but five years later 24% of them have two operators, more than 50% have three ISPs and another 20% have four operators.

The number of cable and FTTH ISPs also grew during this time period but to a much lesser extent than DSL and mobile ISPs. For cable, the number of municipalities with zero ISPs reduced from almost 70% to 54.6%. In 2015, 25% of all municipalities had two cable ISPs. FTTH displayed a more modest growth with the share of municipalities with zero ISPs going down from 98.7% in 2010 to 90% in 2015. Only 0.7% of the municipalities had two or more FTTH ISPs in 2015. Finally, WiMax was only adopted in very few (2.5%) municipalities in 2010. Five years later, this number has more than doubled, with 5.3% municipalities with one WiMax operator and 0.3% with two.

<sup>23</sup>The numbers on speed are ordered. For example, if a percentage of the municipality's population is covered with 16MB/s, then an equal or higher percentage is covered with lower speeds. Therefore, coverage numbers per speed are highly correlated.

<sup>24</sup>We do not have separate figures for coverage for each technology, but only for total broadband coverage. However, DSL is the main contributor to the total.

<sup>25</sup>Figure A1 in Appendix A.1 reports the growth of coverage at difference speed levels for all West German municipalities and for Bavaria and Lower Saxony.

<sup>26</sup>Table A1 in Appendix A.1 provides additional information regarding the technology of the entrants in Lower Saxony and Bavaria. The table reports, for the years 2010 and 2015, the share of municipalities with zero to seven active ISPs, divided by technology.



**State aid.** *Panel D* of table 1 reports the share of municipalities receiving state aid in Bavaria and Lower Saxony. This share amounts to 59.9%, and conditional on receiving funding, the average grant amounts to €86,500. In the two panels of Figure A2 reported in the Appendix, we show graphically the geographical distribution of the municipalities that received state aid in the two states.<sup>27</sup>

**Broadband plans.** *Panel E* of table 1 reports the average price for broadband connections in the first and in the last years of our sample. The average broadband price substantially decreased over the five-year time we span with our data, going from €74.3 to €52.6. Among several others, one of the factors that could explain this large fall in prices is the overall entry process in the market, a hypothesis this paper will investigate.<sup>28</sup> The sample period also saw a reduction in the number of broadband plans available on the market. This reduction is mainly due to the fact that the owner of the main broadband infrastructure, DT, cut many national plans when it stopped marketing its classic T-DSL connection in 2014. DT and other ISPs used this connection to offer their broadband plans. This may also have affected the average price, as DT, as an incumbent and former monopolist, is likely to price its plans higher than its competitors.<sup>29</sup> The average number of broadband plans offered in a municipality was 1232.9 in 2010 and almost halved to 624.9 in 2015. The average number of local broadband plans is much lower and has decreased less: from 34.6 in 2010 to 27.4 in 2015.

**Instruments.** The last panel of table 1, *Panel F*, provides information on our instruments for aid adoption. In Bavaria and Lower Saxony, the political parties in the councils are very stable. In 81.2% of the municipalities, the party in power in 2008 was the same as in the previous election. As we will argue, political stability makes it more likely that long-term projects, such as applying for subsidies to support broadband investment, will be started and completed. In more than half of the cases (59.9%), only two municipalities are involved when municipalities share the MDF with another municipality. In fact, 1.46 municipalities share the same MDF on average. Moreover, more than half of the municipalities that share the MDF with another municipality have the same political party in the majority, which is likely to reduce coordination costs for politically relevant projects such as the development of broadband infrastructure, as we explain in the next section. The share of aid applications granted to neighbouring municipalities is 61.5%. We will argue that the higher this proportion, the higher the probability that the focal municipality will also apply for and receive aid. Finally, the average number of large ISPs operating in neighbouring municipalities in 2010 is equal to 2.35. This variable should

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<sup>27</sup>The maps hint at the possibility of geographic clustering in obtaining the aid. This can be the consequence of political or technical elements that make neighbouring municipalities form clusters when applying for aid. Our IV strategy is partly based on this observation.

<sup>28</sup>A negative relation between the number of competitors and market price is a standard result when the number of initial competitors is low, and is documented in many studies; see the literature that originated by the work of Bresnahan and Reiss 1991. In the case of broadband internet, see, for instance, Xiao and Orazem (2011).

<sup>29</sup>National contracts are on average more expensive than local contracts. For example, in 2010 the average price of a national contract was more than €75, while the average price of a local contract was around €45.

help to explain both the likelihood of applying for aid and the likelihood of subsequently entering the focal municipality.

## 5 Empirical analysis and identification strategy

This section presents the empirical framework through which we quantify the impact of state aid schemes on the broadband market. We first discuss how state aid is expected to affect broadband investment and competition in order to motivate the main outcome variables of our analysis: the level of broadband availability, the number of ISPs, the number of broadband plans and the average broadband price. We then present the econometric specification and the identification strategy.<sup>30</sup> Finally, the results are presented.

### 5.1 State aid, open access obligations & market expansion, and market competition

The primary expected effect of the aid is to increase broadband coverage beyond what the market would achieve in the absence of the scheme. The reason for this expectation is simple. Providing infrastructure comes at a high cost for an ISP. This high cost has to be offset by the (expected) revenues from the sale of broadband services to end users. In rural areas, where demand is low and costs are relatively high due to low population density and dispersed areas, ISPs often find this trade-off unfavourable. Other things being equal, by financing (part of) the investment costs of deploying new infrastructure, the subsidies should lead to an increase in coverage in assisted areas compared to unassisted areas where this investment gap was not covered by public funds.

The next likely effect of aid is to change the number of ISPs providing broadband services in a municipality. Aid for broadband investment can have a positive effect on the number of ISPs in several ways. Firstly, the aid may be granted to a new network operator who makes the investment and provides broadband services as an ISP. Consider, for example, a municipality where the market is served only by DT and where the subsidy is granted to Telefónica. Telefónica enters the market as a network operator, invests in new infrastructure and then acts as an ISP. In this scenario the subsidy has a direct effect on ISP entry.<sup>31</sup> In a second scenario, the aid is granted to the incumbent. Such a situation could, in principle, reduce the incentives for other ISPs to enter the market. However, one of the key features of the schemes – which was instrumental to gaining EC approval – was to impose open access to the newly built infrastructure. This meant that once the infrastructure was in place, all ISPs could use it to

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<sup>30</sup>The previous version of this paper also exploited the panel dimension of the data and used a complementary identification strategy based on propensity score matching and a difference-in-differences (DiD) approach. The estimated effects of the two approaches are qualitatively very similar, although the results based on the IV strategy appear to be slightly larger in magnitude. A summary of the results obtained with the DiD approach is discussed in section 6 of this paper.

<sup>31</sup>It is also possible that a new, typically small and local, network operator wins the contract and builds the infrastructure, but does not act as an ISP. In this scenario, it gives ISPs access to its infrastructure at a cost.

provide broadband services in the area by paying an access fee to the infrastructure owner. Therefore, this scenario could also lead to the entry of new ISPs.

In addition, the predicted increase in market coverage may provide a further incentive for ISPs to enter the market. An increase in coverage is equivalent to an increase in the potential market size, where a larger market could lead to more ISPs entering the market if the cost of entry is not too high, which is the case with open access obligations.<sup>32</sup> Thus, based on oligopoly theory and the characteristics of the schemes, the following prediction can be made: with open access obligations, a larger market can sustain a larger number of ISPs in equilibrium.<sup>33</sup>

This effect might vary across technologies. The technology most targeted by the aid, DSL, is likely to be the technology most responsible for this mechanism. However, given the high heterogeneity between projects and the complex structure of broadband networks, investments in other technologies may have taken place, which could trigger the entry of ISPs active in these technologies. Wireless technologies – such as LTE and WiMax – have been subsidised in some areas. While cable could *de facto* not be subsidised as the technology could not provide open access, fibre infrastructure could potentially be subsidised. FTTH, which is the last mile of the broadband network, was expensive at the time and was unlikely to be directly funded by the subsidies granted under the schemes analysed. However, several projects were a mix of fibre up to a certain point - in most cases the cross-connection cabinets, “fibre to the curb” (FTTC) - followed by either copper (DSL) or mobile technology (LTE and WiMax). Fibre could, therefore be part of a “mix” of supported technologies, and fibre effectively funded where projects included fibre connections to some network nodes, which were then connected to homes via DSL or mobile technologies.

Thirdly, the aid may have affected prices and the variety of broadband plans, triggered by the entry of ISPs. To understand possible mechanisms, it may be important to distinguish between the plans offered by different market players. Large incumbent ISPs, such as DT and Vodafone, offer national broadband plans at uniform prices across municipalities, so state aid targeted at specific municipalities is unlikely to have a direct impact on these plans and prices. In contrast, smaller local players do not have national coverage and often try to gain market share through aggressive (local) pricing. Therefore, a testable hypothesis is whether average price changes at the local level are mainly influenced by local plans (offered by small and local ISPs). As these plans tend to be cheaper, average prices should be lower in areas where this type of entry takes place.

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<sup>32</sup> Access regulation could significantly impact the incentive to enter a market with open access to infrastructure, as it affects profitability. While access to the DSL infrastructure (copper) of the dominant player, DT, was already regulated, this was not the case for other types of infrastructure.

<sup>33</sup> See the literature originating from the seminal work of Bresnahan and Reiss (1991) and then applied in the context of the broadband market, for example by Nardotto, Valletti and Verboven (2015).

## 5.2 Identification strategy and empirical results

To assess the effects highlighted above and identify the causal effect of state aid, the starting point of our framework is the following equation (1):

$$y_{it} = \gamma \text{State aid}_{it} + \lambda X_{it} + \eta_i + u_i, \quad (1)$$

where  $y_{it}$  is the outcome of interest in municipality  $i$  at year  $t$ ;  $X_{it}$  includes time-varying socio-demographic variables and a set of interactions between fixed effects and time trends that we use to capture the role of local trends;  $\eta_i$  are time-invariant (potentially unobserved) characteristics at the level of the municipality, and  $\text{State aid}_{it}$  is an indicator that is equal to 1 if municipality  $i$  was granted the state aid and if the infrastructure has been deployed in the year  $t$ .

While our data allow us to exploit its panel dimension, it has one limitation: it lacks the exact year in which the infrastructure financed by the aid was actually deployed. To overcome this problem, we adopt a long-difference model, reported in equation (2):

$$\Delta y_i = \gamma \Delta \text{State aid}_i + \lambda \Delta X_i + \varepsilon_i, \quad (2)$$

where  $\Delta y_i$  is the difference in the outcome of interest for municipality  $i$  between year 2015 (the last year in our sample, well after the implementation of the aid) and year 2010 (the first year in our sample, before the implementation of the grants).<sup>34</sup> The outcomes we focus on are the broadband coverage at different speeds, the number of ISPs, the number of different plans offered by the ISPs active in the municipality, and the average price of internet connections. The control variables  $\Delta X_i$  are the differences in the time-varying demand and supply conditions captured by the changes in socio-demographic variables and the differences in the local trends. Any factors that are not time-varying – i.e.,  $\eta_i$  of model (1) – are removed by taking the long difference. The explanatory variable of interest is the indicator variable  $\Delta \text{State aid}_i$ , which is equal to 1 for those municipalities that received the state aid.<sup>35</sup>

As discussed in Section 2, the primary goal of the aid is to help close the digital divide between rural and urban areas by providing the financial resources needed to build the basic broadband infrastructure where it is still missing. Thus, in general, areas receiving the aid have different characteristics than those not receiving aid, which, if not properly addressed, would produce a selection-based endogeneity

<sup>34</sup>As discussed in Section 2, the programmes were approved between 2008 and 2009. Their actual implementation in the form of deployment of new infrastructure took place after two main bureaucratic steps: (i) the presentation and the approval of the proposals made by the municipalities, and (ii) the procurement procedure to select the company in charge of the actual deployment. The follow-up reports on the state aid schemes indicate that actual deployment started in 2011 and the vast majority of the infrastructure was deployed between that year and 2013 (Bayerisches Staatsministerium für Wirtschafts, Infrastruktur, Verkehr und Technologie, 2012).

<sup>35</sup>Thus, in all specifications, we only use one observation per municipality. However, in one of the extensions, reported in Appendix A.2.2, we make use of the full panel of five years, where we adopt a different identification strategy based on matching and a DiD approach (detailed in Appendix A.2).

bias in a simple OLS regression of the outcome on the aid. Apart from excluding large cities and urban centres from our sample, the empirical model in equation (2) is a first step towards reducing such bias, thanks to the time-differencing that eliminates the endogeneity bias stemming from omitted variables that are time-invariant, such as the size of local economic activity and its mix, or local demographic characteristics (which may represent a more or less attractive potential market for ISPs).

In addition, model (2) includes as control variables the changes between 2010 and 2015 in a wide range of socio-demographic and economic factors that may influence private investment decisions, and thus the evolution of both the local broadband market in the treatment and control areas and the probability of applying for and receiving a subsidy. Assuming that all relevant factors are taken into account, estimating the equation (2) could provide an unbiased estimate of the causal effect of state aid on the outcome. However, one may be unwilling to make such an assumption because of concerns that not all factors influencing ISPs' investment decisions are considered, especially in the context of a rapidly growing broadband market.

To eliminate potential residual endogeneity bias, we propose an instrumental variable strategy based on a set of instruments that influence the probability of a municipality applying for a grant. These variables are related to the administrative and coordination costs that municipalities had to incur in preparing the applications, and possibly to the awareness of the grant itself. We use three instrumental variables. The first is an indicator variable equal to one if the municipality had the same party in power at the time of the application as in the previous political cycle. The idea behind this instrument is that since most municipalities held elections in 2006-2007, a stable administration could have worked on the proposals more efficiently and possibly produced one of higher quality. Political stability is a well-known factor influencing economic development at all levels, from the highest (see Alesina et al. 1996, Alesina, Roubini and Cohen 1997, and related literature) to the lowest, as in this paper. For example, Clingermayer and Feiock 1997 documents how political turmoil within the city, represented by leadership turnover, affects the costs of negotiating, enacting and enforcing the terms of a contract with external suppliers.

The second instrumental variable is an indicator equal to one if all municipalities sharing the same MDF have the same governing party, interacted with an indicator equal to one if the same MDF covers few other municipalities (i.e., less than two). The ability of this instrument to shift the probability of submitting a grant application package lies in the free-riding problem faced by larger groups of municipalities compared to smaller groups of municipalities that did not have to coordinate their efforts. Moreover, such coordination failures may be greater when local administrations are also of different political colours. The literature on individual contribution and effort in the context of public goods is extensive, starting with the early work of Samuelson (1954), Olson Jr (1971), Chamberlin (1974), and Marwell and Ames (1979), and has examined the role of factors such as group size and group homogeneity (for example, in terms of individual returns to the public good). The evidence reported in this literature is sometimes mixed but broadly suggests that smaller and more homogeneous groups

tend to be characterised by higher average contributions and effort from their members, suggesting that the expected effect of this instrument on the probability of receiving a grant is positive.<sup>36</sup>

The third instrument is the share of aid applications granted to neighbouring municipalities. The idea behind this instrument is that being geographically close to a municipality interested in receiving the grant could have triggered participation in the programme and possibly also reduced some of the learning costs related to the administrative procedures (e.g., due to the exchange of information on the technical aspects of the proposal). This instrument is therefore based on the role of peers, defined in our case as neighbouring municipalities. The literature has extensively studied the role of peer effects in influencing decision making and has shown that peers, in the form of neighbours, colleagues or peer institutions, can be an important driver of the decisions made by individuals, workers and organisations (see, for instance, Duflo and Saez 2003, Zimmerman 2003, Brown et al. 2008, Mas and Moretti 2009, Cai, Chen and Fang 2009, Moretti 2011, Kaustia and Knüpfer 2012). In the case of public institutions, many papers use institutional changes, new regulations or reforms among a group of peers as a driver for local institutional change or as a trigger for the introduction of similar regulations (see, among others, Giuliano, Mishra and Spilimbergo 2013, Acemoglu et al. 2019, Caselli and Reynaud 2020, and Dertinger and Hirth 2020), suggesting that the expected effect of this instrument on the probability of receiving state aid should be positive.

### **5.2.1 Effectiveness of the Aid - Broadband Availability**

Before discussing the effect of aid on broadband availability, let us briefly discuss the result of the first stage regression reported in column (1) of table 2. Our instruments are all statistically significant and have the expected sign. The probability of receiving aid is higher when the political party in power is stable, i.e., the same party is in power in the sample period as in the previous electoral cycle. Moreover, this probability is also significantly higher when political and administrative coordination costs are lower and when several other neighbouring municipalities have also subsidised their broadband infrastructure. The F-test for the joint significance of the instruments is large (116.4), as is the R-squared, indicating that our instruments explain a large part of the variation in aid allocation. We return to the plausibility of the exogeneity of the proposed instruments in section 5.2.5, where we present two statistical tests to support our identification strategy.

Columns (2)-(4) of table 2 show the effect of the aid on broadband availability. This ranges from 16 percentage points of additional broadband coverage for 2 Mbit/s lines to 26 percentage points of additional availability for 6 Mbit/s lines. These effects are economically relevant not only in absolute but also in relative terms. If we compare the estimated effect of state aid with the average increase in coverage experienced by the municipalities in our sample between 2010 and 2015 reported in table 1, the aid-induced effects are substantial: 65.5% of the average increase observed for 2 Mbit/s, 35.7% of

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<sup>36</sup>See for example Isaac and Walker (1988), Fisher et al. (1995), Haan and Kooreman (2002), Böhmelt (2012), Yang et al. (2013), and Diederich, Goeschl and Waichman (2016).

Table 2: IV regressions on broadband availability.

Dependent variable:	1 <sup>st</sup> stage	2 <sup>st</sup> stage		
	$\Delta$ State aid (1)	$\Delta$ 2Mbit/s (2)	$\Delta$ 6Mbit/s (3)	$\Delta$ 16Mbit/s (4)
$\Delta$ State aid		16.35*** (2.91)	22.83*** (3.64)	26.29*** (4.09)
Same party last election	0.07*** (0.02)			
Same party $\times$ Few municipalities MDF	0.04** (0.02)			
Share aid neighbors	0.57*** (0.03)			
$\Delta X$	YES	YES	YES	YES
Average y in 2010		65.1	43.8	18.8
Average $\Delta y$ between 2010 and 2015		25	35.8	42.2
F-Test	116.430			
R <sup>2</sup>	0.67	0.39	0.39	0.26
Observations	2974	2974	2974	2974

The dependent variable in column (1),  $\Delta$  State Aid, is the change between 2015 and 2010 of the state aid indicator. Thus, it takes the value 1 if the municipality receives state aid and 0 otherwise. The dependent variables in columns (2) to (4) are the change between 2015 and 2010 in the share of households in the municipality who at least one ISP could serve at a download speed of 2Mbit/s, 6Mbit/s, and 16Mbit/s, respectively. *Same party last election* is an indicator variable equal to 1 if the municipality at the time of the grant application had the same party in power as in the previous political cycle. *Same party* is an indicator that is equal to 1 if the municipality has the same ruling party as the other municipalities sharing the MDF and *Few municipalities MDF* is equal to 1 if few other municipalities (i.e., less than 2) are covered by the same MDF. *Share aid neighbours* is the share of aid proposals granted to neighbouring municipalities. The vector of controls includes the changes in the observed socio-demographic characteristics, the average total number of ISPs operating in neighbouring municipalities, a set of dummy variables for the party ruling the state, an indicator variable that takes value one if the MDF is within the municipality, and the share of the ruling party in 2010. The standard errors (in parentheses) are spatially clustered following the procedure in Conley (1999), using a radius of 10km around the municipalities; \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

the average increase observed for 6 Mbit/s and 62.3% of the average increase observed for 16 Mbit/s. Our analysis therefore supports the view that the aid was indeed effective in increasing availability.

Based on the estimates in table 2, we can also do a back-of-the-envelope calculation of the cost per potentially connected inhabitant in a given municipality  $i$ . For this calculation, we use the simple formula in (3):

$$Cost_i = \frac{Tot\ Aid_i}{\hat{\gamma} \times Population_i}, \quad (3)$$

where *Tot Aid* is the amount of state aid spent in the municipality and *Population* is the number of inhabitants. For example, given  $\hat{\gamma} = 16.35$  for 2 Mbit/s and the average population of 6,700 inhabitants in a municipality (see table 1), on average, the aid potentially covered an additional 1,059 inhabitants per municipality. Assuming that all the aid was invested in infrastructure to cover this speed, each

additional covered inhabitant costs on average about €79, with an average aid amount of €86,500. The figures are even more favourable – i.e., less costly per inhabitant – for higher speeds if we make a similar exercise.

It is important to note that our results on coverage do not allow us to say much about actual adoption, which is relevant from a policy perspective (see for example the discussion in Belloc, Nicita and Rossi, 2012; Boik, 2017). If bridging the digital divide requires not only access to infrastructure, but also to all the services that run on that infrastructure, then improved broadband availability alone is not sufficient for a positive evaluation of the subsidies, as one would also need to see improved adoption.<sup>37</sup> This is particularly relevant for Germany. As discussed by Girard, Mattes and Michelsen (2018), the low coverage of fibre infrastructure and the lack of willingness to pay hampers the adoption of fast internet in Germany compared to other OECD countries.<sup>38</sup> Unfortunately, disaggregated data on internet adoption in Germany are not available.

### 5.2.2 Impact on Competition - Entry

As a first measure of the competition that might have been affected by the aid, we look at the total number of ISPs active in a municipality. In addition, our data allow us to examine whether the effect of the aid was heterogeneous in terms of the technology used. While the aid was supposed to be technologically neutral, only DSL and mobile technologies (LTE and WiMax) were fully covered by the schemes, allowing ISPs to enter the market either as direct beneficiaries of the aid or through access to the subsidised infrastructure. We estimate model (2) where the dependent variable is the number of ISPs operating in a municipality. We focus first on the total number of ISPs and then on the five technologies separately: DSL, cable, LTE, WiMax and FTTH. Table 3 reports the results of the estimations. The coefficient estimates on the  $\Delta$  *State aid* indicator variable identify the treatment effect, which is heterogeneous across technologies. The estimated coefficient in column (2) indicates an increase in the number of ISPs in municipalities that received the aid, equivalent to 0.75 more entrants. This effect is statistically significant and, compared to a general average increase of 2.6 ISPs over the five years, represents an additional increase of 28.8%, a significant economic effect. Looking at the effect of the aid on different technologies, we find that, as expected, it is strongest for DSL, which was the most heavily subsidised technology. On average, treated markets had 0.47 more DSL entrants in 2010-2015 than the control group, an additional increase of 29.4%.

Cable has not received direct aid and is technologically “decoupled” from the technologies that have received aid, i.e., entry could not occur in combination with investment in other technologies.

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<sup>37</sup>In fact, countries have taken different approaches to subsidising broadband networks because of this issue. Greece, for example, subsidised the demand side rather than the supply side by giving vouchers to users. As shown by Belloc, Nicita and Rossi (2012), such demand-side subsidies can be more effective than supply-side interventions, especially when broadband diffusion is in its later stages.

<sup>38</sup>In line with this observation, Boik (2017) shows that households that already have access to DSL do not necessarily switch to faster but more expensive high-speed broadband.



Table 3: IV regressions on the number of ISPs (Entry).

Dependent variable:	1 <sup>st</sup> stage	2 <sup>st</sup> stage					
	$\Delta$ State aid	$\Delta$ ISPs					
	(1)	All (2)	DSL (3)	Cable (4)	LTE (5)	WiMax (6)	FTTH
$\Delta$ State aid		0.75*** (0.16)	0.47*** (0.12)	0.08 (0.05)	-0.31*** (0.10)	0.075** (0.03)	0.12*** (0.03)
Same party last election	0.07*** (0.02)						
Same party $\times$ Few municipalities MDF	0.04** (0.02)						
Share aid neighbors	0.57*** (0.03)						
$\Delta X$	YES	YES	YES	YES	YES	YES	YES
Average $y$ in 2010		2.8	1.74	0.29	0.3	0.006	0.008
Average $\Delta y$ between 2010 and 2015		2.6	1.6	0.21	2.88	0.065	0.077
F-Test	116.430						
R <sup>2</sup>	0.67	0.09	0.09	0.07	0.2	0.31	0.06
Observations	2974	2974	2974	2974	2974	2974	2974

The dependent variable in column (1),  $\Delta$  State Aid, is the change between 2015 and 2010 of the state aid indicator. Thus, it takes the value 1 if the municipality receives state aid and 0 otherwise. The dependent variable in column (2) is the change between 2015 and 2010 of the total number of ISPs. The dependent variables in columns (3) to (6) are the change between 2015 and 2010 in the total number of DSL, cable, LTE (mobile), WiMax (mobile), and FTTH operators, respectively. *Same party last election* is an indicator variable that is equal to 1 if the municipality at the time of the grant application had the same party in power as in the previous political cycle. *Same party* is an indicator that is equal to 1 if the municipality has the same ruling party as the other municipalities sharing the MDF and *Few municipalities MDF* is equal to 1 if few other municipalities (i.e., less than two) are covered by the same MDF. *Share aid neighbours* is the share of aid proposals granted to neighbouring municipalities. The vector of controls includes the changes in the observed socio-demographic characteristics, the average total number of ISPs operating in neighbouring municipalities, a set of dummy variables for the party ruling the state, an indicator variable that takes value one if the MDF is within the municipality, and the share of the ruling party in 2010. The standard errors (in parentheses) are spatially clustered following the procedure in Conley (1999), using a radius of 10km around the municipalities; \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

It is therefore not surprising that the coefficient, although positive, is not significant.<sup>39</sup> For WiMax, a technology supported by the aid, the estimated effect is small but positive and significant: the aid leads to an increase of 0.075 ISPs. However, compared to the low average growth of 0.006 ISPs, this represents more than a 12-fold increase in ISPs compared to non-subsidised municipalities.

On the contrary, for LTE, the mobile technology also potentially supported by the aid, the effect is negative: a reduction of 0.31 ISPs. This means that the growth of the number of LTE ISPs was reduced by about 10.8%. It is not easy to explain why the arrival of aid in a municipality has slowed down investment in this technology, but we offer some explanations based on our knowledge of the industry. First, the overall growth of LTE infrastructure has been astonishing over the period covered

<sup>39</sup>To explain the positive effect, one can perhaps speculate that one ISP, Vodafone, invested in several technologies, including cable, at the time of the aid schemes. Perhaps it bundled infrastructure investments across technologies for large projects. All in all, larger projects tend to be more beneficial in terms of additional entry, as we further explain in section 6, providing additional analysis.

by our data, so even small delays in LTE deployments, possibly due to aid, could result in a negative coefficient. For example, it could be that the aid reduced the speed of antenna deployment compared to the private market, e.g., due to the additional bureaucracy of tendering procedures or because ISPs were simply “waiting to see” whether public investment would become available, resulting in a lower number of LTE ISPs by the end of our sample period in 2015. In other words, there may be fewer ISPs in the treated municipalities because the antennas (paid for by state aid) took longer to be deployed.

Second, it might be that the arrival of a good fixed-line connection discouraged the marginal LTE player from entering the market, especially in areas with relatively more ISPs already competing in that same technology area. Indeed, by the end of 2015, the median municipality in Germany had three fixed-line (DSL) ISPs and three LTE operators, thus making the market for broadband access quite crowded. Third, in line with the previous explanation, some marginal LTE players might have been discouraged from entering areas where other (similar) mobile technologies were already deployed or subsidised, such as WiMax.<sup>40</sup> As shown in column (6), the decline in LTE is partly offset by the growth of WiMax, which in some cases was perhaps preferred to LTE antennas by the municipalities allocating state aid. While we do not know the extent to which each scenario has played out, the last two scenarios are consistent with the substitutability of these technologies.

We also find that the aid had a significant positive effect of 0.12 more entrants in the FTTH technology, which was not directly supported by the aid schemes because it was too expensive. Considering the low growth of this technology in five years, the estimated effect is large in relative terms, as it amounts to more than doubling the growth rate of entrants. This suggests the existence of spillovers from subsidised technologies to FTTH. The most likely explanation for this result is that FTTH technology shares most of its infrastructure with DSL and mobile technologies, as a large part of the broadband infrastructure (up to the last mile) for all technologies is fibre. The additional cost for ISPs to deploy the additional last mile may have been reduced, potentially facilitating entry.

Overall, the scheme led to more entry in the assisted municipalities. Thus, state aid *did not harm competitors*, the standard competition and welfare measure used in state aid, as the number of ISPs increased relative to non-aided municipalities. Some technologies experienced a significant boost to entry, while others grew less rapidly. In particular, fixed technologies benefited strongly from state aid, namely DSL (+0.47 ISPs, +29.4%) and FTTH (+0.112 ISPs, +155.8%). The impact on mobile technologies is more mixed, with LTE growing more slowly (-0.31 ISPs, -10.8%), while WiMax experienced a boost (+0.075 ISPs, +115%). In addition, we see a relative decrease in an aided technology (LTE) and a relative increase in an unaided technology (FTTH), suggesting some substitutability and spillovers between different technologies.

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<sup>40</sup>There is anecdotal evidence that this technology was supported in several rural municipalities with disperse infrastructure (see Bayerisches Staatsministerium für Wirtschafts, Infrastruktur, Verkehr und Technologie, 2012).

### 5.2.3 Impact on Competition - Product offerings and prices

In this section we complement the previous analysis by examining two other important dimensions of competition that directly affect consumer welfare and are therefore more in line with other areas of competition policy: product offering and prices. We focus on how the average prices of broadband plans in a municipality respond to state aid. Since both the composition of plans offered at the municipal level and their corresponding prices affect average prices, we additionally analyse the impact of aid on the number of broadband plans offered to gain further insights into pricing. To this end, we use model (2) to estimate the effect of state aid on three outcomes: i) the average prices per Mbit/s download speed (to standardise prices) offered by ISPs in the municipality; ii) the *total* number of broadband plans available to consumers in the municipality; and iii) the number of *local* plans (i.e., plans that are offered by smaller local ISPs).<sup>41</sup>

The estimation results are reported in table 4. The first column reports the first stage of our 2SLS estimation and is the same as in the previous tables. Turning to the results reported in columns (2) to (4), the estimates first show a reduction in the price of €0.18 per Mbit/s of download speed following the introduction of state aid. To assess the magnitude of the effect, it is worth looking at the general evolution of the average price per Mbit/s. In 2010, it was around €11.8. Five years later, this price drops to €1.36 per Mbit/s.<sup>42</sup> Thus, a consumer living in a municipality receiving state aid enjoyed a further reduction in the average price of €0.18/1.36 per Mbit/s in 2015, i.e., paid on average 13% less than a user living in a municipality not receiving state aid.

Columns (3) and (4) provide further insight into the effects of state aid and a mechanism for the observed price effect. While the difference in the total number of plans between 2010 and 2015 did not change significantly between municipalities with and without state aid (the estimated effect is positive but not statistically significant), the aid led to a substantial and statistically significant relative increase in the number of local broadband plans, as shown in column (4). This increase amounts to 14.2 additional local plans due to state aid, which corresponds to 51.9% more local plans. This is particularly notable at a time when ISPs have been reducing and rationalising the complex tariffs they offer to internet users.<sup>43</sup>

These findings offer a possible explanation for the patterns observed: state aid has led to the entry of small local ISPs and a relative increase in local plans offered by these ISPs. On the other hand, the number of national plans has not changed due to state aid, as these plans are set at the national level. Local plans tend to be much cheaper than national plans: the average price of a local plan is, on average,

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<sup>41</sup>The total number of broadband plans includes both national plans offered by large national ISPs, such as DT and Vodafone, and local plans offered by local ISPs. Ideally, one would like to look at national plans versus local plans. However, national plans have too little variation between municipalities to be used as a separate category. This is not surprising as national ISPs set their plans (and corresponding prices) nationally.

<sup>42</sup>The average (theoretical) download speed increased more than sixfold from 6.35 Mbit/s in 2010 to 39.4 Mbit/s in 2015.

<sup>43</sup>As discussed in section 4, the number of broadband plans observed in the database fell dramatically in 2014, when almost 50% of the several hundred plans on offer were discontinued, mainly due to the withdrawal of DT's T-DSL connection, on which many ISPs offered their own plans.

Table 4: IV regressions on broadband prices and the number of offered plans.

Dependent variable:	1 <sup>st</sup> stage	2 <sup>st</sup> stage		
	$\Delta$ State aid	$\Delta$ Avg. Price Mbit/s	$\Delta$ Number Plans	$\Delta$ Number Loc. Plans
	(1)	(2)	(3)	(4)
$\Delta$ State aid		-0.18** (0.09)	9.03 (9.84)	14.21** (5.56)
Same party last election	0.07*** (0.02)			
Same party $\times$ Few municipalities MDF	0.04** (0.02)			
Share aid neighbors	0.57*** (0.03)			
$\Delta X$	YES	YES	YES	YES
F-Test	116.43			
R <sup>2</sup>	0.67	0.2	0.21	0.26
Observations	2974	2974	2974	2974

The dependent variable in column (1),  $\Delta$  *State Aid*, is the change between 2015 and 2010 of the state aid indicator. Thus, it takes the value 1 if the municipality receives state aid and 0 otherwise. The dependent variables in columns (2) to (4) are the change between 2015 and 2010 of: (2) the average price of broadband plans per Mbit/s of download speed; (3) the total number of plans offered by ISPs providing services in the municipality; and (4) the total number of local plans offered in the municipality. *Same party last election* is an indicator variable that is equal to 1 if the municipality at the time of the grant application had the same party in power as in the previous political cycle. *Same party* is an indicator that is equal to 1 if the municipality has the same ruling party as the other municipalities sharing the MDF and *Few municipalities MDF* is equal to 1 if few other municipalities (i.e., less than 2) are covered by the same MDF. *Share aid neighbours* is the share of aid proposals granted to neighbouring municipalities. The vector of controls includes the changes in the observed socio-demographic characteristics, the average total number of ISPs operating in neighbouring municipalities, a set of dummy variables for the party ruling the state, an indicator variable that takes value 1 if the MDF is within the municipality, and the share of the ruling party in 2010. The standard errors (in parentheses) are spatially clustered following the procedure in Conley (1999), using a radius of 10km around the municipalities; \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

about €20 lower than a national plan (which costs on average €75). As a result, the relative increase in cheaper local plans has decreased average prices in the aided municipalities. In short, the aid has not distorted competition. On the contrary, it has induced the entry of new (local) ISPs, which in turn has led to an increase in the number of (local) broadband plans offered, and a resulting downward effect on average prices.

#### 5.2.4 Further evidence on the effect of the state aid programme

In this section, we shed further light on the mechanism through which the state aid programme has produced the observed effects. The analysis aims to identify the main driver(s) of the two key outcomes of the aid: the increase in broadband services (coverage) and the decrease in the average price paid by end users. The two main causes of these outcomes, which we test jointly, are the granting of state aid

to the municipality, which financed the infrastructure rollout, and the increase in the number of ISPs competing in the municipality.

By including the state aid and the number of ISPs in the same regression, we aim to disentangle the effect of each on our outcomes to better understand the mechanisms at play. This is of interest, as the state aid is, *prima facie*, only intended to build infrastructure. However, the aid could also impact other outcomes, such as a reduction in prices, and this impact could have occurred directly or indirectly through the channel of ISP entry. Similarly, the number of ISPs might not only have an impact on prices but also on the infrastructure, as more ISPs might build more broadband infrastructure. More generally, the exercise aims to show the relative importance of creating the conditions for a market (building the infrastructure for internet access) versus promoting competition within that market.

Therefore, we extend the model in equation (2) by allowing outcomes to depend on both state aid and the number of ISPs, which are assumed to be endogenous. The extended model in equation (4) retains the long-difference approach to control for time-invariant characteristics of the municipality  $i$ . The long-difference is taken between 2015 and 2010, and the same set of controls for changes in the time-varying variables is used as in the original model. The estimated equation is:

$$\Delta y_i = \gamma \Delta \text{State aid}_i + \Delta \text{Num. ISPs} + \lambda \Delta X_i + \varepsilon_i. \quad (4)$$

For both endogenous variables, we use the same set of instruments as in the original model: the same party in power in the municipality as in the previous election cycle, the same party in power in municipalities sharing the MDF interacted with few municipalities sharing the MDF, and the share of neighbouring municipalities receiving a subsidy. We then add a fourth variable to instrument for the number of ISPs: the number of large ISPs operating in neighbouring municipalities. We introduce this additional instrument primarily to have enough identification power to estimate the model with both state aid and ISPs treated as endogenous variables. The rationale for this instrument is that a higher number of large ISPs in neighbouring municipalities prior to the arrival of aid will affect the number of ISPs considering entering the local market. Potential entrants may consider the entry of these large ISPs as more likely, thus narrowing the market space. This instrument is also related to state aid. Having large ISPs nearby may reduce the incentive for municipalities to start a costly application process in the expectation that these ISPs might invest anyway.

Table 5 reports the results. The table is split into two panels. In both, the first two columns report the results for the first stage estimations. In columns (1) and (2) we report the first stage regression for the state aid indicator and for the change in the number of ISPs in the municipality between 2015 and 2010, respectively. Both first stage regressions show the expected effects for all instruments. The probability of receiving aid is statistically positively affected by the previously used instruments, while only a high share of neighbouring municipalities receiving aid affects entry. The new instrument also has the expected effect and is significant. A larger number of large ISPs in neighbouring municipalities is detrimental both to state aid and to the absolute change in the number of ISPs.

Table 5: IV regressions on broadband prices and the number of offered plans.

Dep. variable:	1 <sup>st</sup> stage			2 <sup>st</sup> stage			1 <sup>st</sup> stage			2 <sup>st</sup> stage		
	Δ State aid (1)	Δ Num. ISP (2)	Δ Coverage 6MB/s (3)	Δ Price MB/s (4)	Δ State aid (5)	Δ Num. DSL ISP (6)	Δ Coverage 6MB/s (7)	Δ Price MB/s (8)				
Δ State aid			22.46*** (5.44)	-0.14 (0.16)			22.28*** (5.83)	0.02 (0.16)				
Δ Number of IPSs			0.43 (1.07)	-0.20*** (0.03)								
Δ Num. DSL ISPs							1.14 (3.59)	-0.66*** (0.12)				
Same party last election	0.07*** (0.02)	0.03 (0.05)			0.07*** (0.02)	0.06 (0.04)						
Same party × Few municipalities	0.05*** (0.02)	-0.04 (0.04)			0.05*** (0.02)	0.03 (0.04)						
MDF												
Share aid neighbors	0.56*** (0.04)	0.31*** (0.09)			0.56*** (0.04)	0.22*** (0.08)						
Large ISPs 2010 neighbors	-0.03** (0.01)	-0.96*** (0.04)			-0.03** (0.01)	-0.30*** (0.04)						
ΔX	YES	YES	YES	YES	YES	YES	YES	YES				
R <sup>2</sup>	0.171	0.350	0.395	0.280	0.171	0.160	0.395	-0.405				
F-test	38.606	14.866			38.606	14.866						
Observations	2974	2974	2974	2974	2974	2974	2974	2974				

The dependent variable in column (1) and (5),  $\Delta$  State Aid, is the change between 2015 and 2010 of the state aid indicator. Thus, it takes the value 1 if the municipality receives state aid and 0 otherwise. The dependent variables are the change between 2015 and 2010 of: (2) and (4) the change in the number of ISPs operating in the municipality; (3) and (6) the change in internet coverage at a speed of 6Mbit/s; (4) and (8) the change in average price of broadband plans per Mbit/s of download speed.  $\Delta$  Number of (DSL) ISPs is the change between 2015 and 2010 in the number of (DSL) ISPs; *Same party last election* is an indicator variable that is equal to 1 if the municipality at the time of the grant application had the same party in power as in the previous political cycle. *Same party* is an indicator that is equal to 1 if the municipality has the same ruling party as the other municipalities sharing the MDF and *Few municipalities MDF* is equal to 1 if few other municipalities (i.e., less than 2) are covered by the same MDF. *Share aid neighbors* is the share of aid proposals granted to neighbouring municipalities. *Large ISPs 2010 neighbors* is the number of large ISPs operating in nearby municipalities in 2010. The vector of controls includes the changes in the observed socio-demographic characteristics, the average total number of ISPs operating in neighbouring municipalities, a set of dummy variables for the party ruling the state, an indicator variable that takes value 1 if the MDF is within the municipality, and the share of the ruling party in 2010. The standard errors (in parentheses) are spatially clustered following the procedure in Conley (1999), using a radius of 10km around the municipalities; \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

The corresponding second-stage regressions are reported in columns (3) and (4). We focus on the two outcome variables that have already been investigated in our previous analysis: the change in coverage and the change in the average price per download speed (Mbit/s).<sup>44</sup> Column (3) shows that the increase in broadband coverage is entirely due to the granting of aid, which financed the deployment of the infrastructure. The positive but small and statistically insignificant coefficient of  $\Delta \text{Number of ISPs}$  indicates that the contribution of additional ISPs entering the local market is negligible. Column (4) shows that the price reduction documented in section 5.2.3 is entirely due to the entry of new ISPs, while the state aid (i.e., the new infrastructure) does not *per se* lead to lower prices. These findings are confirmed by the results reported in the right panel of table 5 in columns (5) to (8), where we estimate the same model but we replace the change in the number of all ISPs operating in the municipality, with the change in the number of DSL ISPs, which were the main beneficiaries of the state aid as shown in the previous Section 5.2.2.

### 5.2.5 IV tests

The identification strategy proposed in section 5, like any instrumental variable strategy, relies crucially on the set of instruments used to generate exogenous variation in the endogenous variables. However, the exogeneity of the instruments – the *exclusion restrictions* – cannot be directly tested and has to be assumed. Nonetheless, we provide supporting evidence for this assumption by providing two tests. First, we use the data from a period before the state aid was granted. Since our data start in 2010, we use the data from Falck, Gold and Heblich (2014), who report information on broadband coverage at 1 Mbit/s between 2005 and 2008.<sup>45</sup>

The idea behind this placebo test is straightforward: if the instruments that we employ were *not exogenous* to local factors affecting the development of broadband, then the estimates of the same model as in Section 5, where we replace our measures of broadband coverage in 2010-2015 with the broadband coverage in 2005-2008 – i.e., well before any state aid was granted – would reflect this correlation (i.e., they would also be sizable and statistically significant), thus invalidating our identification strategy. If, instead, we found no effect of the state aid – with our instruments – on broadband development between 2005 and 2008, that would be consistent with the fact that the instruments are not part of a causal chain that is linked to the development of local broadband infrastructure, absent the (actual) arrival of the state aid programme.

Thus, the empirical model that we estimate is the same as in (2) with the difference that the outcome variable is the change in broadband coverage at 1 Mbit/s (within the same municipality) between 2005 and 2008. Results are reported in table 6 where columns (1) and (2) show the estimated coefficients of the model without control variables (thus, with only the instruments) and columns (3) and (4) show the

<sup>44</sup>We report only the intermediate download speed of 6Mbit/s for the sake of compactness, but results are consistent for other download speeds.

<sup>45</sup>The data, which also come from the German Breitbandatlas, can be found in the additional material of the published paper, see <https://www.aeaweb.org/articles?id=10.1257/aer.104.7.2238>

Table 6: IV regressions - Placebo test using 2005-2008 data.

	1 <sup>st</sup> stage	2 <sup>st</sup> stage	1 <sup>st</sup> stage	2 <sup>st</sup> stage
Dep. variable:	State aid	$\Delta$ 1MB/s	State aid	$\Delta$ 1MB/s
	(1)	(2)	(3)	(4)
State aid		0.01 (0.02)		-0.02 (0.02)
Same party last election	0.06*** (0.02)		0.07*** (0.02)	
Same party $\times$ Few municipalities MDF	-0.01 (0.02)		0.04** (0.02)	
Share aid neighbors	0.60*** (0.03)		0.57*** (0.03)	
$\Delta X$	NO	NO	YES	YES
R <sup>2</sup>		0.015		0.090
F-test		160.593		126.463
Observations	2974	2974	2974	2974

The dependent variable in columns (1) and (3),  $\Delta$  *State aid*, is the change between 2015 and 2010 of the state aid indicator. Thus, it takes the value 1 in 2015 (post-treatment) if the municipality receives state aid and 0 otherwise. The dependent variables in columns (2) and (4) are the change between 2008 and 2005 of the share of households in the municipality who could be served by at least 1 ISP at a download speed of 1Mbit/s as reported in Falck, Gold and Heblich (2014) additional material. *Same party last election* is an indicator variable that is equal to 1 if the municipality at the time of the grant application had the same party in power as in the previous political cycle. *Same party* is an indicator that is equal to 1 if the municipality has the same ruling party as the other municipalities sharing the MDF and *Few municipalities MDF* is equal to 1 if few other municipalities (i.e., less than two) are covered by the same MDF. *Share aid neighbours* is the share of aid proposals granted to neighbouring municipalities. The vector of controls includes the changes in the observed socio-demographic characteristics, the average total number of ISPs operating in neighbouring municipalities, a set of dummy variables for the party ruling the state, an indicator variable that takes value 1 if the MDF is within the municipality, and the share of the ruling party in 2010. The standard errors are clustered at the municipality level; \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively. \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

estimated coefficient of the model that includes the set of control variables that we used in the analysis in Section 5.2.

Estimating the model before the actual implementation of the state aid programme leads to very small and statistically insignificant coefficient estimates (see columns (2) and (4)). In other words, the set of instruments shifts the probability of receiving state aid - thus making some municipalities more likely to receive state aid than others - in a way that is not correlated with factors affecting broadband development in the absence of the actual implementation of the state aid programme.

Second, we employ another test, which consists of regressing on the instruments the changes in the main demographic variables that are generally associated with broadband deployment (population size, population density, income and education) experienced by the municipalities in our sample, in order to show that the latter do not correlate with local trends in factors related to broadband deployment. In fact, although in our model (2) we control for observed and unobserved, time-invariant factors that may explain broadband development (e.g., population density), one may still be concerned that the



instruments are correlated with changes in these factors (i.e., their trends). We, therefore, run the following regressions:

$$\Delta y_i = \gamma Z_i + \beta \Delta X_i + u_i, \quad (5)$$

where  $\Delta y_i$  are the changes between 2015 (last year of our data) and 2007 (before the state aid programme was announced) in total population, population density, income and the share of population with a college education. The vector  $Z_i$  contains the three variables we use as instruments in our main analysis: same party last election, same party x few municipalities MDF, and share of aid neighbours, and  $\Delta X$  are the changes over time of the same controls we used in the previous regressions. Table 7 reports the results.

Table 7: Regression on the changes in main demographics

Dep. variable:	$\Delta$ Population	$\Delta$ Population density	$\Delta$ Income	$\Delta$ College Education
	(1)	(2)	(3)	(4)
Same party last election	-0.029 (0.018)	-0.473 (0.731)	0.143 (0.245)	-0.117 (0.111)
Same party $\times$ Few municipalities MDF	-0.01 (0.014)	0.91 (0.562)	0.001 (0.191)	0.093 (0.107)
Share aid neighbors	0.011 (0.019)	-0.721 (1.02)	0.814** (0.344)	0.128 (0.22)
$\Delta X$	YES	YES	YES	YES
R <sup>2</sup>	0.212	0.165	0.001	0.518
Observations	2974	2974	2974	2974

The dependent variables in columns (1) to (4) are the change in *Total population* in the municipality, the change in *Population density* of the municipality, the change in average *Income* in the municipality, and the change in *Education* in the municipality, measured as the share of population with college education, respectively. *Same party last election* is an indicator variable that is equal to 1 if the municipality at the time of the grant application had the same party in power as in the previous political cycle. *Same party* is an indicator that is equal to 1 if the municipality has the same ruling party as the other municipalities sharing the MDF and *Few municipalities MDF* is equal to 1 if few other municipalities (i.e., less than two) are covered by the same MDF. *Share aid neighbours* is the share of aid proposals granted to neighbouring municipalities. The vector of controls includes the other changes in the observed socio-demographic characteristics, the average total number of ISPs operating in neighbouring municipalities, a set of dummy variables for the party ruling the state, an indicator variable that takes value one if the MDF is within the municipality, and the share of the ruling party in 2010. Controls in  $X$  include the same controls employed in previous analysis, taken at the year 2007. The standard errors (in parentheses) are spatially clustered following the procedure in Conley (1999), using a radius of 10km around the municipalities; \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

We find no systematic evidence of a correlation between local trends in observed demographics traditionally associated with broadband development on the one hand and the instruments on the other hand. Only 1 coefficient out of 12 is significant at the 5% level. These results, together with the placebo test discussed above, provide strong support for the IV strategy that we propose.

## 6 Additional analyses

In this section, we summarise the results obtained with an alternative estimation strategy based on a difference-in-differences (DiD) approach coupled with matching.<sup>46</sup> This alternative approach not only provides a check on the robustness of our main identification strategy, but also helps to shed further light on the mechanism at work. Within this framework, we can further uncover some dynamic effects by exploiting the time dimension of the panel, while at the same time identifying heterogeneous effects based on the amount of aid.

### 6.1 Difference-in-differences with matching

The DiD approach compares municipalities in Bavaria and Lower Saxony that applied for and implemented the aid with municipalities in the same states that did not. Since the allocation of aid was not a random process, we first reduce selection bias due to observable characteristics by identifying treated and control municipalities through a matching procedure (see Appendix A.2 for a detailed discussion of our matching procedure). Specifically, we identify those municipalities that did not implement the state aid but were *ex-ante* as attractive for broadband investment as those that implemented the aid.

The results of the static DID analysis largely confirm our IV findings. Municipalities that received aid experienced a large and significant increase in broadband availability compared to the corresponding matched municipalities that did not receive aid (see table A4 in the appendix A.2.1). This effect ranges from an additional increase of more than 14 percentage points for the availability of connections at 2 Mbit/s to an increase of around 21 percentage points for the availability of connections at 6 Mbit/s and speeds above 16 Mbit/s. Qualitatively, these results are similar to our main results. However, the failure to account for unobserved heterogeneity – which is instead captured in our IV approach – biases the estimates downwards.

Regarding entry, we estimate an increase in the number of ISPs in municipalities that received aid, which amounts to 0.21 more entrants (see table A5 in annex A.2.1). This effect is again strongest for DSL, which was the most subsidised technology. On average, treated markets observed 0.16 more DSL entrants over the period 2010-2015 compared to the control group, an additional increase of 10%. For LTE and WiMax, the other technologies that received partial support, the effect is negative but insignificant for the former and positive and significant for the latter. We also observe that the aid had a significant positive effect of 0.06 more cable ISPs and 0.05 more entrants for FTTH technology. Thus, these results for entry are similar to those obtained with our preferred IV strategy (see footnote 39 for a possible explanation of the result for cable).

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<sup>46</sup>This was the main identification approach in an earlier version of the paper. We report only a subset of the results here. The full paper is available at <https://repec.cepr.org/repec/cpr/ceprdp/DP15779.pdf>.

## 6.2 Dynamic effects

To better exploit the time variation in our panel, we estimate a specification of the DiD model that exploits all years, considering that aid was gradually introduced between 2011 and 2014. Thus, we estimate a model where we interact the aid variable with yearly indicators to capture how the impact of state aid builds up over time. Such an approach, however, has pros and cons. On the one hand, it gives a good idea of the progressive build-up of the effect of aid. On the other hand, the estimated interaction coefficients cannot be interpreted as a true treatment effect (even assuming that the fixed effects solve all endogeneity problems), because we know that, especially in the early years, the estimated coefficients capture the change in outcome resulting from a mix between municipalities that actually have the new infrastructure and municipalities that still do not have it.

The results are reported in table A6 in Appendix A.2.2. The effect of aid on coverage increased rapidly. This effect increases until 2013, when most projects have been completed. The additional broadband availability due to aid remains fairly stable thereafter, suggesting that control municipalities do not catch up quickly. The results for entry, on the other hand, show a lagged response to the aid. This suggests that it took some time for additional entrants to use the new infrastructure. Alternatively, or in addition, this finding may indicate that firms waited to see whether the additional investment would generate enough additional demand to make the market attractive enough for entry. ISPs entered the DSL market (the directly subsidised technology) quickly, whereas they entered cable and FTTH technologies later. For LTE, after the initial surge in entry observed in 2011 and 2012, the untreated municipalities quickly closed the gap with the treated ones. Finally, the impact on entry in WiMax technology quickly catches up and remains positive and significant over the years, suggesting that the aid had a rapid and lasting impact on entry in this technology.

## 6.3 Aid amount

So far, we have estimated the extensive margin or the average treatment effect of the aid. However, given the heterogeneity in the amount of aid, it is also important to understand the intensive margin. Our core IV approach cannot be easily adapted to capture heterogeneous effects. We therefore analyse this extension in the more flexible DiD framework. We again slightly modify our main model (1) by introducing three interactions based on the amount of aid with the *Post* treatment indicator. The three categories of aid amounts are €50,000 or less (small project); between €50,000 and €100,000 (medium project); and above €100,000 (large project).

The results reported in table (A7) show that small projects have the lowest impact on coverage and only a marginal impact on entry. Only for WiMax do we estimate a positive effect of aid, probably due to lower infrastructure deployment costs. Medium-sized projects do not perform worse than large projects in terms of additional coverage, but large projects lead to more entry overall. It is also worth noting that only large projects generate enough demand to trigger the entry of cable and FTTH operators. A possible explanation for these results is that larger projects tend to be concentrated in

larger municipalities where the positive effect on availability generates larger demand effects (and see footnote 39 for a possible explanation for the cable result). This in turn attracts more new entrants. For FTTH, it is also likely that projects with a larger budget include some investment in fibre, making FTTH deployment more likely. Overall, larger projects tend to be more beneficial, especially regarding additional entry.

## 7 Conclusions

The analysis presented in this paper shows that the state aid schemes implemented in Bavaria and Lower Saxony to incentivise private ISPs to deploy broadband infrastructure and subsequently provide broadband services in rural areas have been successful. First, the schemes were effective in increasing coverage within the municipalities that implemented the aid. Specifically, we measure an additional increase in broadband coverage due to the aid of between more than 16 percentage points for connections of 2 Mbit/s and around 26 percentage points for connections of more than 16 Mbit/s.

Second, and more importantly for this study on state aid control, we find that the implemented schemes did not impair competition. On the contrary, we find that the number of ISPs competing in a local market increased significantly in aided municipalities compared to non-aided municipalities, by about 29% above the general trend. This is true not only for DSL markets – which received the most aid – but also for other technologies such as WiMax and even fibre. Third, the aid led to lower average prices by 13%, due to an increased number of cheaper local broadband packages offered by local ISPs.

Finally, we show that while the increase in coverage is a direct effect of the aid, the downward pressure on average prices is entirely due to market entry. In other words, the aid affected competition *for* the market (i.e., entry), which in turn affected competition *within* the market (i.e., prices and variety). Taken together, our results suggest the following narrative: state aid drove investment that expanded the availability of broadband infrastructure. This, coupled with an open access obligation, led to additional entry. In particular, new local ISPs offered relatively more (cheaper) local broadband packages in aided municipalities, driving down average prices. In other words, our evidence confirms that infrastructure deployment alone does not lead to lower prices, but that the resulting increased competition between firms is needed for consumers to fully benefit.

In summary, our paper highlights an intuitive – but perhaps not well-understood – lesson about regulation and competition policy in network industries: infrastructure subsidies need not be harmful to competition. For entry to occur, regulators and competition authorities need to create the right conditions. In this case, the subsidy programme may have been successful not only because it subsidised infrastructure and technologies that are pro-competitive – i.e., the infrastructure could be technically shared – but also because it imposed an open access obligation on the subsidised infrastructure.

To our knowledge, this paper is the first to provide a full quantitative ex-post analysis of EU state aid control with a focus on competition. As such, it represents a first step in establishing a framework

for assessing an important and under-researched policy area, with a view to stimulating further academic research. Indeed, while there is a large body of empirical research assessing the effectiveness of subsidies, the evidence on their impact on competition – an important dimension under the control of the European Commission in its state aid framework – is scarce.

The more general lesson to be drawn from our results is that, if well designed, state aid need not harm competition. On the contrary, it can even stimulate competition. Thus, industrial policy and competition policy can go hand in hand and complement each other to increase welfare.

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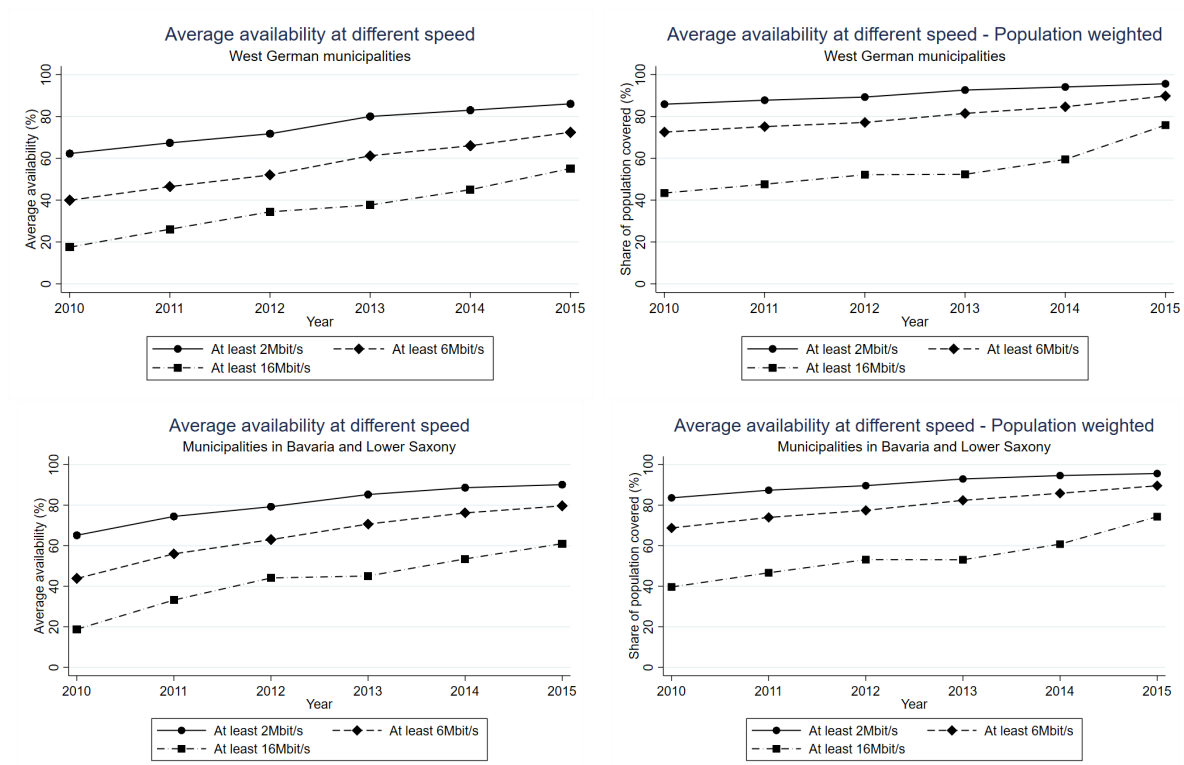
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## A Appendices

### A.1 Additional graphs and tables on the German broadband market

Figure A1: Broadband coverage at different speeds



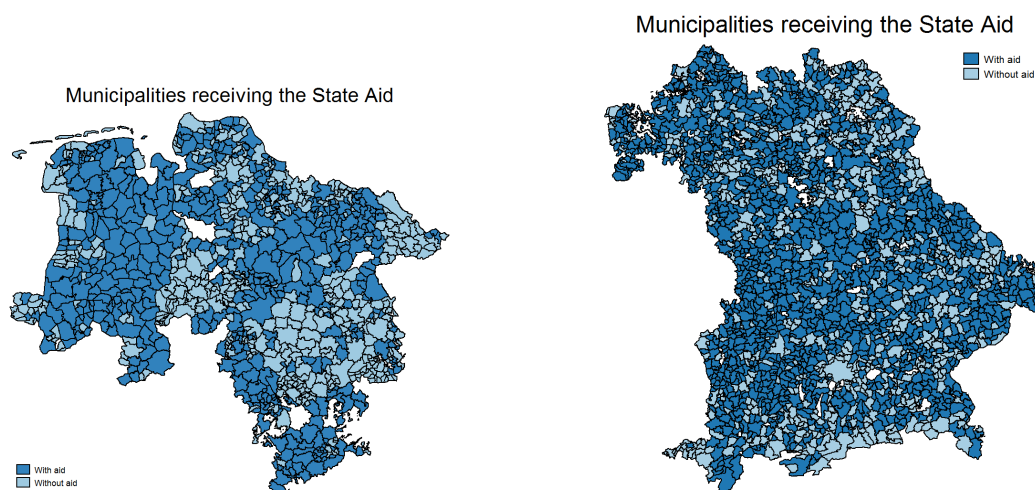
Source: Our elaboration on Breitbandatlas data. Left panel displays the average availability calculated as the share of municipalities covered at 2Mbit/s or more, 6Mbit/s or more, and 16Mbit/s or more; Right panel displays the share of population covered at 2Mbit/s or more, 6Mbit/s or more, and 16Mbit/s or more. The top panels report the two measures of coverage using all West German municipalities while the bottom panels report the two measures of coverage using only municipalities in Bavaria and Lower Saxony.

Table A1: Frequencies of the number of ISPs in 2010 and 2015, by technology, for the main technologies in Lower Saxony and Bavaria.

Number of ISPs	DSL		Cable		LTE		WiMax		FTTH	
	2010	2015	2010	2015	2010	2015	2010	2015	2010	2015
0	4.5	0.2	68.7	54.6	77.1	0.2	97.5	94.4	98.7	90
1	54.5	0.1	30.5	17	21.5	1.2	2.5	5.3	1.3	9.3
2	17.8	35.3	0.8	25.6	1.4	24	0	0.3	0	0.6
3	11.8	37.5	0	2.6	0	52.9	0	0	0	0.1
4	10.3	18.2	0	0.2	0	20	0	0	0	0
5	1	7.2	0	0	0	1.7	0	0	0	0
6	0.1	1.3	0	0	0	0	0	0	0	0
7	0	0.2	0	0	0	0	0	0	0	0

Source: Our elaboration on Breitbandatlas data. The table reports, for the years 2010 and 2015, the share of municipalities hosting 0 to 7 ISPs adopting the following technologies: DSL (all types), Cable, LTE, WiMax, and FTTH.

Figure A2: : Treated municipalities in Lower Saxony (left) and Bavaria (right)



Source: Our elaboration on data from the Bavarian State Ministry for Economics, Media, Energy and Technology, as well as the Lower Saxony Ministry for Economics, and Transportation.

## A.2 Propensity score matching

In order to identify the causal effects of the different state aid schemes on broadband availability and entry, we use a DiD approach where we compare municipalities in Bavaria and Lower Saxony that applied for and implemented the aid with municipalities in the same states that did not. A simple DiD model would allow us to identify the average treatment effect of the aid if the treatment were random. However, the allocation of aid was probably not a random process. Therefore, as a first step, we try to reduce selection bias due to observable characteristics (see e.g. Aguzzoni et al. (2016)). We do this by identifying appropriate treatment and control municipalities through a matching procedure.

Specifically, we identify those municipalities that did not implement the aid, but which had the same attractiveness for broadband investment *ex-ante* as those that implemented the aid. In this way, the municipalities that did not implement the aid provide the counterfactual outcome for how the subsidised municipalities would have performed without the subsidy. This would therefore mimic a situation where the treatment, i.e. the aid, was randomly assigned.

To implement the matching procedure, we first run a regression to recover the likelihood, i.e., the “propensity score,” that a municipality implemented the aid based on its observable characteristics. Second, for each treated municipality, we match a non-treated municipality that is as similar as possible, i.e. that has a propensity score as close as possible to the treated municipality in question. Note that this regression does not aim to explain why a municipality received the aid. We only look for municipalities among the non-treated that are as similar as possible to the treated with regards to observed characteristics.

The matching is done using variables from 2008 (or the year closest in time) to avoid any spurious correlation with the effect of the treatment. By choosing a period well before the treatment, we are more certain to include only variables that are unaffected by the aid.<sup>47</sup> Furthermore, following the matching literature, we use a measure of the pre-treatment outcome (e.g., Heckman, Ichimura and Todd, 1997). Specifically, we use DSL availability from Falck, Gold and Heblich (2014), which covers internet availability at the municipal level for the years 2005-2008. This variable is also used again below in the context of the “common trend” assumption needed for a clean DiD analysis.

When doing matching, the trade-off lies between a model that is rich enough to capture the main drivers of investment and entry, but that also ensures balance is achieved between treated and control areas. We converged to a specification entailing what we consider to be the core set of characteristics. These include demand-related variables (population, income, education, percentage area for firms and industry) and supply-related variables (distance to the MDF, ruggedness).

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<sup>47</sup>Our empirical strategy is based on the so-called conditional independence assumption (e.g. Heckman, Ichimura and Todd, 1998). This assumption requires that the outcome variable is independent of the treatment, conditional on the propensity score. Therefore, when implementing the matching procedure, we select a set of explanatory variables that satisfy this condition. Specifically, we only include variables that are expected to simultaneously influence both the treatment and the outcome.

The model we estimate using a logit regression as a first step in the matching procedure is:

$$State\ aid_m = \alpha + \eta X_m + u_m, \quad (6)$$

where  $State\ aid_m$  is an indicator for the municipality that received state aid and  $X_m$  is a vector of demand and supply characteristics: population and population squared, population density, income, unemployment, the share of inhabitants with a college degree, the share of population between 16 and 64 years old (and thus at working age), the ruggedness index (calculated as described in footnote 22), the distance between the municipality's centre and the MDF, the share of the municipality's area devoted to business and industry, and the coverage of 1 Mbit/s in 2008.

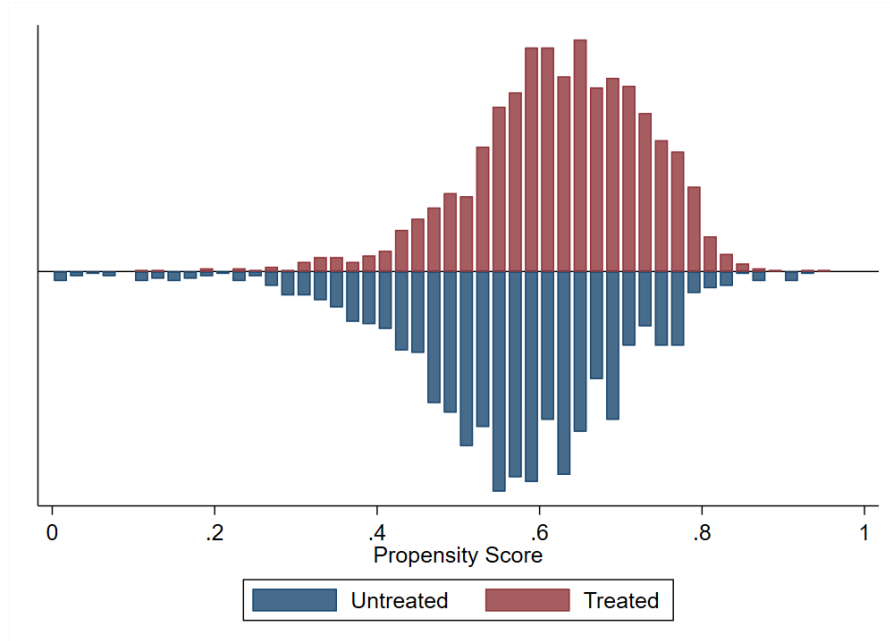
Table A2 reports the estimates for equation (6), while Figure A3 shows the frequency distributions of the propensity scores for the treated and untreated municipalities. As the figure shows, the frequency distributions of the propensity scores of the two groups of municipalities are similar, indicating that there is a good set of municipalities without aid that can be matched with those receiving aid.

Table A2: Propensity score regression

<b>Dependent variable: State aid</b>				
	Coeff.	Std. err.	dy/dx	Std. err.
Population	0.356***	(0.059)	0.08***	(0.013)
Population <sup>2</sup>	-0.006***	(0.001)	-1e-3***	(0.3e-3)
Population density	-0.002***	(0.000)	-4e-4***	(0.6e-4)
Income	0.003	(0.008)	7e-4	(0.001)
College degree	-0.017***	(0.006)	-4e-3***	(0.001)
Work age	0.011	(0.017)	2.5e-3	(0.004)
Unemployment	-0.102***	(0.024)	-0.02***	(0.005)
Distance to MDF	0.198***	(0.024)	0.04***	(0.005)
Ruggedness	-0.002	(0.001)	-0.4e-3	(0.3e-3)
Area firms and industry	0.081	(0.056)	0.018	(0.013)
DSL 2008	0.535	(0.362)	0.12	(.0812)
Constant	-0.271	(1.046)		
Observations	3009			
Log-likelihood	-1927.168			
Pseudo R <sup>2</sup>	0.049			

The dependent variable *State Aid* is an indicator which takes value 1 if the municipality receives state aid. *Population* is the number of inhabitants in the municipalities (in thousands) and *Population*<sup>2</sup> is its squared value. *Population density* is the number of inhabitants per km<sup>2</sup>. *Income* is the average income in the municipality (in thousands €). *Work age* is the share of population between 16 and 64 years old. *Unemployment* is the unemployment rate in the municipality. *Distance to the MDF* is the linear distance in km between the centroid of the municipality and the MDF. *Ruggedness* is the ruggedness index computed as described in footnote 22. *Area firms and industry* is the share of land in the municipality devoted to firms and industry. *DSL 2008* is the coverage of 1Mbit/s in 2008 in the municipality. Robust standard errors in parentheses. \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively. Marginal effects are computed with the delta-method.

Figure A3: Propensity score distributions.



Empirical distributions of the propensity score estimated in model (6) for municipalities which received versus not receive the state aid.

Based on this information, we perform a nearest-neighbour matching without replacement. When there is enough overlap between units in the treated group and a subset of the control group, then matching without replacement is the better choice (see Dehejia and Wahba, 2002). The matching algorithm paired 2,086 out of 3,009 treated municipalities.

The quality of the match is represented in tables A3. For each variable, it shows how matching improves the balance between treated and control. For example, in the case of population density, the pre-match difference amounts to more than 22%, and the difference between treated and untreated is significantly different. After the matching, the bias is only just above 2%, which amounts to a reduction in bias of more than 90%. Moreover, the difference post-match between the treated and untreated is no longer significant. Overall, the covariate imbalance before matching amounts to 13.5% and after matching to only 3.2%, which indicates a clear improvement due to matching on observables.<sup>48</sup>

Finally, we provide a last test for our identification strategy by examining trends in the outcome variable before the start of our treatment. Due to data limitations, we cannot assess the common trend assumption for all our outcome variables, but we can show it for the broadband coverage at the speed

<sup>48</sup>This is further confirmed by the fact that the pseudo- $R^2$  of a probit regression of the propensity score on all variables is almost 20 times larger (0.04) for the unmatched sample than for the matched one (0.002). Moreover, the likelihood-ratio test cannot reject the joint significance of all regressors when using the unmatched sample but it can reject this hypothesis when using the matched sample.

Table A3: Balancing

Variable	Sample	Mean		(%) bias	(%) reduction $\ bias\ $	t-test	
		Treated	Control			t	$p >  t $
Population	Unmatched	6.07	7.73	-5.0		-1.46	0.145
	Matched	6.42	5.81	1.8	62.9	0.82	0.410
Pop. dens.	Unmatched	149.43	210.95	-22.6		-6.49	0.000
	Matched	166.5	160.67	2.1	90.5	0.67	0.505
Income	Unmatched	32.258	32.483	-3.5		-0.97	0.333
	Matched	32.163	32.19	-0.4	87.8	-0.10	0.919
College	Unmatched	22.148	23.886	-22.0		-5.98	0.000
	Matched	23.576	23.086	6.2	71.8	1.43	0.151
Work age	Unmatched	54.313	54.043	11.5		3.11	0.002
	Matched	54.08	54.143	-2.7	76.8	-0.62	0.536
Unemployment	Unmatched	5.256	5.673	-20.4		-5.52	0.000
	Matched	5.712	5.525	9.1	55.3	2.03	0.043
Distance MDF	Unmatched	3.095	2.475	34.1		9.18	0.000
	Matched	2.557	2.624	-3.7	89.2	-0.88	0.377
Ruggedness	Unmatched	29.87	29.79	0.2		0.07	0.946
	Matched	29.96	30.42	-1.5	-504.5	-0.33	0.739
Area firms	Unmatched	0.599	0.702	-10.3		-2.84	0.005
	Matched	0.641	0.602	4.0	61.5	1.03	0.301
DSL 2008	Unmatched	0.918	0.923	-5.3		-1.43	0.152
	Matched	0.921	0.921	-0.4	92.0	-0.09	0.925

*Population* is the number of inhabitants in the municipalities (in thousands) and *Population*<sup>2</sup> is its squared value. *Population density* is the number of inhabitants per km<sup>2</sup>. *Income* is the average income in the municipality (in thousands €). *Work age* is the share of population between 16 and 64 years old. *Unemployment* is the unemployment rate in the municipality. *Distance to the MDF* is the linear distance in km between the centroid of the municipality and the MDF. *Ruggedness* is the ruggedness index computed as described in footnote 22. *Area firms and industry* is the share of land in the municipality devoted to firms and industry. *DSL 2008* is the coverage of 1Mbit/s in 2008 in the municipality.

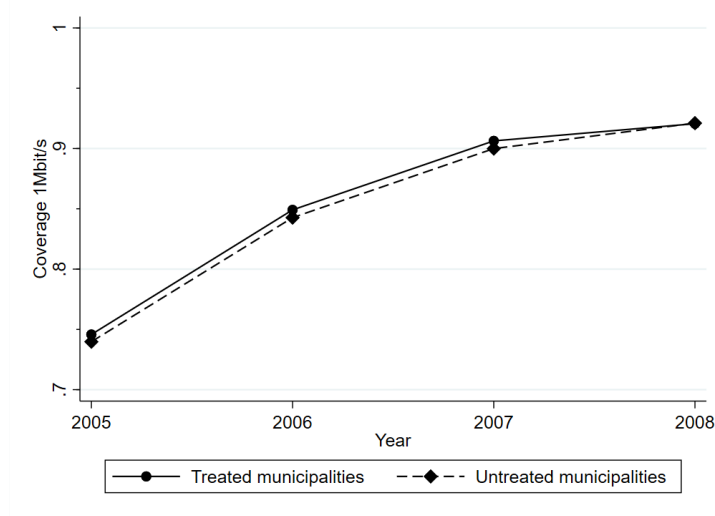
of 1Mbit/s.<sup>49</sup> The graph shown in Figure A4 clearly indicates that, for this speed, the levels of coverage in the two groups of municipalities evolve in such a way that the common trend hypothesis is satisfied.

We believe that our specification is reasonable for three reasons. First, the chosen specification is “theoretically” sound as it entails what the literature considers to be the core set of observable characteristics that might be important determinants of broadband investments. Second, the chosen specification works well, not only in terms of balancing the observable characteristics between treated and control groups but also in terms of providing not just parallel but even identical trends in the outcome variable for treated and control municipalities. Finally, and perhaps most importantly, the main results obtained with the PSM and DiD approach are similar both in terms of size and in terms of significance to those obtained with our IV identification strategy.

<sup>49</sup>As discussed in Section 3, our main dataset covers the years 2010 to 2015, with the bulk of state aid schemes being implemented between 2011 and 2013. The second data on the broadband market used in Falck, Gold and Heblich (2014) covers the period 2005 to 2008 and, thus, it is suitable for a common trend analysis pre-treatment. However, in this data, the only variable on broadband diffusion is coverage at 1Mbit/s, while information on coverage at higher speeds or on the number of ISPs is not available.



Figure A4: Trends 1Mbit/s coverage



### A.2.1 Static difference-in-difference analysis

The empirical model that we employ is reported in the equation below, where  $p$  indicates two paired municipalities (by the matching procedure), and  $t$  is time.

$$\Delta y_{pt} = \alpha + \gamma Post_t + \lambda \Delta X_{pt} + \mu_p + \varepsilon_{pt}. \quad (7)$$

Thus,  $\Delta y_{pt}$  is the difference in the outcome of interest between a treated municipality and the paired, non-treated, control municipality at year  $t$ . The outcomes we focus on are the broadband coverage at different speeds, the number of ISPs offering internet services, and the price of broadband connections. As control variables  $X_{pt}$ , we employ the time-varying differences in market demand and supply conditions captured by the changes in socio-demographic variables. All factors that are not time-varying are captured by the municipality-pair fixed-effects  $\mu_p$ . The explanatory variable of interest is the indicator variable  $Post_t$ , which is equal to one for the post-intervention year 2015, whereas the year of comparison (the base year) is the year 2010. Therefore, the estimated coefficient  $\gamma$  measures the average treatment effect of the state aid, as it captures the difference over time of the difference in outcomes across treated and matched non-treated municipalities. This coefficient quantifies the additional variation experienced by the outcome of those municipalities, which were affected by the scheme, with respect to the average outcome change for those municipalities, which were not implementing the aid. The error term  $\varepsilon_{pt}$  is assumed to be heteroskedastic and correlated among municipality-pairs; results are robust to clustering errors at the Kreis (the next administrative unit) or state-level.

**Coverage.** Table A4 reports the coefficient estimates for equation 7 when the outcome variable is broadband coverage at different speeds. Municipalities that received aid experienced a large and significant increase in availability if compared to the latter to the corresponding matched municipality

that did not receive any subsidy. This effect ranges between an additional increase of more than 14 percentage points for availability of connections at 2 Mbits/s to about an 21 percentage points increase for availability of connections at 6 Mbits/s speed as well as speed larger of 16 Mbit/s. Qualitatively, these results mimic our main findings. However, not accounting for unobserved heterogeneity biases the estimates downward, as expected.

Table A4: Regressions for broadband coverage.

<b>Dependent variable: <math>\Delta</math>Coverage at different speeds</b>			
	2Mbit/s (1)	6Mbit/s (2)	16Mbit/s (3)
Post	14.40*** (1.00)	21.14*** (1.25)	20.56*** (1.29)
$\Delta X$	YES	YES	YES
$\mu$	YES	YES	YES
Average coverage in 2010	65.1	43.8	18.8
Average change between 2010 and 2015	25	35.8	42.2
$R^2$	0.167	0.216	0.196
Observations	2086	2086	2086

The dependent variables are the changes between 2015 and 2010 of the differences between paired municipalities in the share of population that can access the internet at a speed of at least 2Mbit/s, 6Mib/t and 16Mibt/s. *Post* is an indicator that takes value 1 in 2015 (post-treatment) and 0 in 2010 (pre-treatment). The vector of controls  $\Delta X$  includes the changes in *Population* and *Population*<sup>2</sup>, *Population density*, *Income*, *Work age*, *Unemployment*, and *Area firms and industry*.  $\mu$  is the municipality-pair fixed effect. The standard errors in parentheses are clustered at the level of the paired municipalities. \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

**Entry.** We estimate model 7 where now the dependent variable is the number of ISP operating in a municipality. First, we look at the total number of ISPs, and then we focus on the 4 main technologies: DSL, Cable, Mobile (LTE), and fibre (FTTH). Table A5 reports the results of the estimations. The estimated coefficient in the first column indicates an increase in the number of ISPs in the municipalities that received the aid that amounts to 0.21 more entrants. This effect is statistically significant and, compared with a general average increase of 2.6 ISPs over the 5 years, amounts to an additional increase of 8 percent. As expected, this effect is the strongest for the DSL market, which was mostly subsidised. On average, treated markets observe 0.16 more DSL entrants over the period 2010-2015 compared to the control group: an additional increase of 10 percent. For mobile markets, the other technology that was partially supported, the effect is instead not significant. We also observe that the aid had a significant positive effect of 0.06 more cable ISPs as well as 0.05 more entrants in the FTTH technology. Also in this case, results are similar to the findings obtained with our preferred IV strategy, although estimates appear again to be downward biased.

Table A5: Regressions for the number of ISPs.

<b>Dependent variable: <math>\Delta</math> Number of ISPs</b>						
	All (1)	DSL (2)	Cable (3)	LTE (4)	WiMax (5)	FTTH (6)
Year <sub>2015</sub>	0.21*** (0.05)	0.16*** (0.04)	0.06*** (0.02)	-0.02 (0.04)	0.04*** (0.01)	0.05*** (0.01)
$\Delta X$	YES	YES	YES	YES	YES	YES
$\mu$	YES	YES	YES	YES	YES	YES
Avg. num. of ISPs in 2010	2.8	1.74	0.29	0.3	0.006	0.008
Avg. change between 2015 and 2010	2.6	1.6	0.21	2.88	0.065	0.077
R <sup>2</sup>	0.023	0.028	0.015	0.019	0.033	0.054
Observations	2086	2086	2086	2086	2086	2086

The dependent variables are the changes between 2015 and 2010 of the differences between paired municipalities in the number of ISPs. *Post* is an indicator that takes value 1 in 2015 (post-treatment) and 0 in 2010 (pre-treatment). The vector of controls  $\Delta X$  includes the changes in *Population* and *Population*<sup>2</sup>, *Population density*, *Income*, *Work age*, *Unemployment*, and *Area firms and industry*.  $\mu$  is the municipality-pair fixed effect. The standard errors in parentheses are clustered at the level of the paired municipalities. \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

### A.2.2 Effect of the state aid over time

The analysis conducted in section A.2.1 is based on the comparison of outcomes between the pre-treatment year (2010) and the last year in the sample (2015). This is partly justified by the fact that we do not have precise information on the local implementation of the state aid. Thus, we chose to compare a clear pre-treatment period to a clear post-treatment period, as we are sure that by 2015 all projects were concluded. In order to better exploit the time variation in our panel, in this subsection we present the result of an empirical specification that exploits all years, keeping in mind that the aid was gradually rolled out between 2011 and 2014. Hence, we estimate an empirical model, reported in equation (8), where we include a set of yearly indicator variables to capture how the impact of the state aid built up over time, compared to the control municipalities (year 2010 being the baseline):

$$\Delta y_{pt} = \alpha + \gamma_1 Year_{pt}^{2011} + \gamma_2 Year_{pt}^{2012} + \gamma_3 Year_{pt}^{2013} + \gamma_4 Year_{pt}^{2014} + \gamma_5 Year_{pt}^{2015} + \lambda \Delta X_{pt} + \mu_p + \varepsilon_{pt}. \quad (8)$$

The outcome variables  $\Delta y$  are again the differences in the (yearly) change between matched municipalities of the coverage variables and of the entry variables. As in model 7, we include the set of control variables  $\Delta X$  and the set of municipality-pair fixed effects  $\mu$ . Table A6 reports the results.

### A.2.3 Amount received

In this subsection, we investigate whether the amount of aid granted to the project has a differential impact on coverage and entry. We again slightly modify our main model (2) by introducing three interactions, based on the amount of the aid, of the *Post* treatment indicator. The three categories of the

Table A6: Effect of the aid on coverage and entry. All years.

Dep. vars.:	$\Delta$ Coverage			$\Delta$ Number of ISPs					
	2MB/s (1)	6MB/s (2)	16MB/s (3)	All ISPs (4)	DSL (5)	Cable (6)	LTE (7)	WiMax (8)	FTTH (9)
Year <sub>2011</sub>	9.78*** (0.81)	12.95*** (0.99)	11.75*** (0.98)	0.08** (0.04)	0.04 (0.02)	-0.00 (0.01)	0.08*** (0.03)	0.02*** (0.01)	0.00 (0.00)
Year <sub>2012</sub>	14.78*** (0.86)	20.57*** (1.10)	19.15*** (1.11)	0.12*** (0.04)	0.06** (0.03)	0.01 (0.01)	0.06* (0.04)	0.02*** (0.01)	0.00 (0.00)
Year <sub>2013</sub>	15.57*** (0.89)	22.67*** (1.13)	20.33*** (1.15)	0.18*** (0.05)	0.15*** (0.03)	0.04*** (0.02)	-0.03 (0.03)	0.03*** (0.01)	-0.00 (0.01)
Year <sub>2014</sub>	15.14*** (0.97)	22.38*** (1.24)	21.41*** (1.28)	0.23*** (0.05)	0.15*** (0.04)	0.05*** (0.02)	-0.02 (0.03)	0.04*** (0.01)	0.02** (0.01)
Year <sub>2015</sub>	14.40*** (1.00)	21.14*** (1.25)	20.56*** (1.29)	0.21*** (0.05)	0.16*** (0.04)	0.06*** (0.02)	-0.02 (0.04)	0.04*** (0.01)	0.05*** (0.01)
$\Delta X$	YES	YES	YES	YES	YES	YES	YES	YES	YES
$\mu$	YES	YES	YES	YES	YES	YES	YES	YES	YES
R <sup>2</sup>	0.111	0.134	0.102	0.008	0.009	0.008	0.003	0.005	0.009
Observations	6258	6258	6258	6258	6258	6258	6258	6258	6258

The dependent variables in columns (1) to (3) are the yearly changes of the differences between paired municipalities in the share of population that can access the internet at a speed of at least 2Mbit/s, 6Mib/t and 16Mibt/s. The dependent variables in columns (4) to (8) are the yearly changes of the differences between paired municipalities in the number of ISPs. Year<sub>2011</sub> - Year<sub>2015</sub> are indicator variables for the corresponding years. The vector of controls  $\Delta X$  includes the changes in *Population* and *Population*<sup>2</sup>, *Population density*, *Income*, *Work age*, *Unemployment*, and *Area firms and industry*.  $\mu$  is the municipality-pair fixed effect. The standard errors in parentheses are clustered at the level of the paired municipalities. \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.

amount of the state aid are: €50,000 or less (small project); between €50,000 and €100,000 (medium-sized project), and above €100,000 (large project). The estimated model is reported in equation 9 below:

$$\Delta y_{pt} = \alpha + \gamma_1 Post_{pt}^{Aid \leq 50} + \gamma_2 Post_{pt}^{50 > Aid < 100} + \gamma_3 Post_{pt}^{Aid \geq 100} + \lambda \Delta X_{pt} + \mu_p + \varepsilon_{pt}. \quad (9)$$

Table A7: Effect of the aid on coverage and entry. All years.

Dep. vars.:	$\Delta$ Coverage			$\Delta$ Number of ISPs					
	2MB/s (1)	6MB/s (2)	16MB/s (3)	All ISPs (4)	DSL (5)	Cable (6)	LTE (7)	WiMax (8)	FTTH (9)
$\text{Post}^{\text{Aid} \leq 50}$	7.32*** (1.89)	9.64*** (2.35)	7.70*** (2.43)	0.17* (0.10)	-0.00 (0.07)	0.01 (0.04)	-0.18** (0.07)	0.06** (0.02)	0.02 (0.02)
$\text{Post}^{50 > \text{Aid} < 100}$	17.22*** (1.90)	25.08*** (2.37)	26.17*** (2.45)	0.21** (0.10)	0.18** (0.07)	0.06* (0.04)	-0.08 (0.07)	0.04 (0.02)	0.02 (0.02)
$\text{Post}^{\text{Aid} \geq 100}$	15.91*** (1.46)	25.25*** (1.83)	25.29*** (1.89)	0.23*** (0.08)	0.23*** (0.06)	0.09*** (0.03)	0.12** (0.06)	0.03 (0.02)	0.08*** (0.02)
$\Delta X$	YES	YES	YES	YES	YES	YES	YES	YES	YES
$\mu$	YES	YES	YES	YES	YES	YES	YES	YES	YES
$R^2$	0.200	0.250	0.234	0.024	0.034	0.018	0.029	0.034	0.060
Observations	2086	2086	2086	2086	2086	2086	2086	2086	2086

The dependent variables in columns (1) to (3) are the yearly changes of the differences between paired municipalities in the share of population that can access the internet at a speed of at least 2Mbit/s, 6Mib/t and 16Mibt/s. The dependent variables in columns (4) to (8) are the yearly changes of the differences between paired municipalities in the number of ISPs.  $\text{Year}_{2011} - \text{Year}_{2015}$  are indicator variables for the corresponding years. The vector of controls  $\Delta X$  includes the changes in *Population* and *Population*<sup>2</sup>, *Population density*, *Income*, *Work age*, *Unemployment*, and *Area firms and industry*.  $\mu$  is the municipality-pair fixed effect. The standard errors in parentheses are clustered at the level of the paired municipalities. \*, \*\*, and \*\*\* denote significance at the 10, 5, and 1 percent level, respectively.