

Briglauer, Wolfgang

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DOES STATE AID FOR BROADBAND DEPLOYMENT IN RURAL AREAS CLOSE THE DIGITAL AND ECONOMIC DIVIDE?

Wolfgang Briglauer^{*}, Niklas S. Dürr[†], Oliver Falck[°] and Kai Hüschelrath^{*}

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Abstract

We evaluate the impact of a major European state aid programme for broadband deployment applied to rural areas in the German state of Bavaria in the years 2010 and 2011. Using difference-in-differences estimation strategies, we find that aided municipalities have—depending on broadband quality—between 16.8 and 23.2 percentage points higher broadband coverage than non-aided municipalities. This increase in broadband coverage, closing the digital divide, results in an average increase of seven employed individuals living in the respective aid-receiving municipalities while leaving the number of employed or self-employed individuals and wages unaffected. We therefore conclude that an increase in broadband coverage through state aid protects rural areas from depopulation, but does not contribute to a further closing of the economic divide in the form of creating new jobs.

JEL Class: D62; D73; G38; H23; J23; K23; L52; L96; L98; R23

Keywords: government policy; state aid; ex-post evaluation; broadband; employment, rural areas; European Union; Germany; Bavaria

^{*} ZEW Centre for European Economic Research, MaCCI Mannheim Centre for Competition and Innovation, P.O. Box 10 34 43, D-68034 Mannheim, Germany. E-mail: briglauer@zew.de. We are grateful to Sven Heim and Nicolas van Zeebroeck as well as participants at the 14th ZEW Conference on the Economics of ICT in Mannheim in June 2016, the 43rd Annual EARIE Conference in Lisbon in August 2016 and the 14th Annual Media Economics Workshop in Zurich in October 2016 for valuable comments on earlier versions of the article and to Benedikt Kauf for excellent research assistance. We thank *axiom* for providing us with detailed data on the number of freelancers.

[†] ZEW Centre for European Economic Research, MaCCI Mannheim Centre for Competition and Innovation, P.O. Box 10 34 43, D-68034 Mannheim, Germany. E-mail: duerr@zew.de.

[°] University of Munich and Ifo Institute—Leibniz Institute for Economic Research at the University of Munich, and CESifo, Poschingerstraße 5, D-81679 Munich, Germany. E-mail: falck@ifo.de. *Corresponding author.*

^{*} University of Mannheim and ZEW Centre for European Economic Research and MaCCI Mannheim Centre for Competition and Innovation, P.O. Box 10 34 43, D-68034 Mannheim, Germany. E-mail: hueschelrath@zew.de.

1 Introduction

The interrelationship between various types of infrastructure investment and economic development has fascinated generations of researchers. While there appears to be little dispute about the positive impact of the general provision of infrastructures such as transportation or communication networks for employment, innovation and growth, the question of the socially optimal degree of network deployment in general and the most suitable financing options in particular are much more controversial.

While, historically, the (seemingly) public good character of many infrastructures suggested their entirely public provision, the liberalisation processes in many network industries in the 1980s and 1990s broadened the financing options to entirely private or public-private investment projects. The public provision of infrastructures is more and more seen as limited to cases of market imperfection—i.e. situations in which market forces alone are unlikely to provide the socially optimal level of network deployment.¹

In the European Union, the belief in the strategic importance of broadband infrastructures for economic development has long been affecting policy making. Although the European Commission first and foremost aims at strengthening the incentives of private companies to invest in both the deployment of broadband infrastructures and subscriptions through the design and implementation of appropriate regulatory frameworks, since 2003 this general strategy has included the granting of state aid in cases of particular rural areas where the private investment incentives are considered insufficient due to the interference of large deployment costs and limited revenue potentials.

¹ It should be noted here that the identification of the socially optimal level of network deployment is a complex and therefore error-prone process. For example, it cannot be ruled out that a state authority decides to provide funding for an extension of a certain infrastructure to rural areas and later learns that only a small fraction of the respective individuals is interested in using (and paying for) the respective infrastructure (thus suggesting an inefficient investment decision).

In fact, between 2003 and 2014, the European Commission approved in sum 136 state aid applications²—mostly from regions, but also entire (smaller) countries, in the European Union—for the deployment of broadband networks in rural areas, aiming at closing the digital divide and triggering welfare-enhancing externalities that are expected from a well-established broadband infrastructure as ‘general purpose technology’ (see Bresnahan and Trajtenberg, 1995).

Aiming at evaluating the impact of such state aid programmes for broadband deployment in rural areas, two consecutive general research questions suggest themselves. Firstly, from an effectivity perspective, has the granting of state aid had the desired direct effect? That is to say, in the case of broadband aid, did the granting of financial aid to firms lead to improved broadband coverage in the respective regions or municipalities (and to what degree)? Secondly, from an efficiency perspective, the broader question is raised as to whether the respective state aid scheme is socially desirable in view of the impact on local labour markets in aided municipalities. In the case of broadband aid, such a broader assessment of social costs and benefits has to go beyond the direct effects of the aid to take into consideration various (positive or negative) indirect effects of the granting of aid on, for example, competition, trade, employment, investment or economic growth.³

We are the first to assess the causal effects of one of such state aid policy on a fine geographic scale. Specifically, we are interested in the effects of the state aid programme for broadband deployment in the German state of Bavaria in the years 2010 and 2011 on broadband coverage (first stage effectivity question) and employment (second stage efficiency question). We concentrate on employment as the second stage outcome variable as it is of key interest

² See http://ec.europa.eu/competition/sectors/telecommunications/broadband_decisions.pdf (last accessed on 24 October 2016) for a full list of Commission decisions on state aid to broadband.

³ Ideally, such an analysis will also have to consider a possible ‘beggar-thy-neighbour problem’, suggesting negative effects on total welfare.

for public policy makers and likely to react rather quickly to policy changes such as the granting of state aid and the deployment of broadband infrastructure.

Applying a difference-in-differences (DiD) estimation strategy on the basis of a matched sample of 1,845 aided and non-aided rural municipalities, we find with respect to the effectivity question that the aided municipalities have—depending on broadband quality—between 16.8% and 23.2% higher broadband coverage than non-aided municipalities. Concerning answers to the subsequent efficiency question, we combine our DiD strategy with an instrumental variables (IV) strategy in order to estimate the effect of the state aid-induced increase in broadband coverage on employment. We find a significant and positive effect of the state aid programme on employed individuals living in the respective aid-receiving municipalities; however, neither the number of employed measured at place of work nor self-employed individuals nor the average wage shows any significant effect. We therefore conclude that an increase in broadband coverage attracts workers to live in these rural municipalities—or prevents them from depopulation, respectively—but without attracting additional economic activity necessary to close the economic divide.

The remainder of the article is organised as follows. The second section presents a brief review of the existing literature on the economic impacts of telecommunications networks and broadband internet. The third section continues with a description of the institutional structure of broadband state aid in the European Union in general and its implementation in the German state of Bavaria in particular. The fourth section provides a detailed characterisation of our empirical strategy. The fifth section describes our data, followed by the presentation and discussion of our estimation results in section six. Section seven concludes the paper with a review of its main results and the identification of avenues for future research.

2 Review of Related Literature

The study of the economic impact of telecommunications networks and broadband internet—together with the impact of alternative public policies on broadband deployment—has attracted a significant amount of research. For example, a recent survey by Bertschek. (2016) reviews more than 60 studies that econometrically investigate the effects of communication networks on economic growth, employment, regional development, productivity and firm/market performance. In the remainder of this section, we limit our review of the literature firstly to studies that quantitatively assess the impact of state aid (subsidies) on broadband deployment (section 2.1) and, secondly, to contributions related to the impact of broadband availability (or adoption) on employment as the key outcome variable (section 2.2).

2.1 *The Impact of State Aid on Broadband Deployment*

The impact of state aid on broadband deployment—i.e. the effectivity question—is only investigated by very few studies, all making use of highly aggregated country-level data. In an early contribution, Belloc *et al.* (2012) examine the impact of public policies on broadband adoption by utilising a data set for 30 OECD countries that contains public funding measures, as well as the countries' socio-economic and demographic conditions for the years 1995 to 2010. The authors find a positive and significant effect of demand-side policies which is higher when the broadband adoption is already developed, while the effect of supply-side policies decreases as the broadband market moves into its later stages.

Montolio and Trillas (2013) measure how the level of broadband adoption is affected by the impact of regulation, the degree of centralisation of regulatory decisions, and industrial policy. The authors employ an industrial policy variable that stands as a proxy for public policies devoted to fostering broadband penetration and is calculated as government subsidies to private and public companies as a percentage of GDP. The authors utilise data sets for

OECD and EU countries for the years 1999 to 2006 and find positive, albeit insignificant, effects of subsidies in all model specifications.

2.2 *The Impact of Broadband Deployment on Economic Outcomes*

Applying US county- or ZIP code-level data, Kandilov and Renkow (2010)—and partly also Kolko (2012)—find positive links between broadband (availability or adoption) and employment growth. Using a large Canadian data set on the municipality level from 1997 to 2011, Ivus and Boland (2016) find a similar significantly positive impact of broadband availability on employment growth in rural areas as well as overall wage growth (particularly created by the service sector). Furthermore, as shown by Atasoy (2013) for county-level US data, taking into account differences in urbanisation and workers' skills levels suggests that the effect of broadband is significant mostly in rural areas and for highly skilled workers. The stronger impact on remote areas lends support to the notion that broadband can help these regions to catch up with more economically developed urban areas. Last but not least, Fabritz (2013) investigates the impact of broadband on economic activity in rural areas of Germany. Using panel data on broadband coverage in 8,460 West German municipalities from 2005 to 2009, she finds a positive but limited relationship between local employment and local broadband infrastructure, the effect being larger in rural municipalities. A ten percentage point increase in broadband availability in rural areas is associated with a 0.09 to 0.15 percentage points increase in the local employment rate.

In addition to the effects on employment types and levels—investigated further in our empirical analysis below using microdata on the municipality level for rural areas of Bavaria—recent quantitative studies also investigate a possible impact of broadband availability (or adoption) on the wages and labour productivity of particularly highly skilled workers. In particular, for the US, Forman *et al.* (2012) find that diffusion of advanced internet comes along with significant wage and employment growth only for locations with

high IT use, high income and population density. Akerman *et al.* (2015) analyse the impact of broadband on labour productivity with Norwegian firm and worker data from 2000 to 2008, taking the workers' skills levels into account. They find that broadband availability improves the labour market outcomes and marginal productivities of highly skilled/educated workers but has a detrimental effect on unskilled/uneducated workers. Since jobs for more skilled workers are typically better paid, broadband might therefore increase the wage gap.

In sum, our review of the existing literature suggests that employment effects of broadband deployment are often found empirically; however, they tend to be rather small in size and often show some heterogeneity with respect to urban and rural areas on the one hand and low and highly skilled workers on the other.

3 Institutional Background on Broadband State Aid

In this section, we present the important institutional background to the granting of broadband state aid. While section 3.1 briefly describes the broadband state aid rules in the European Union,⁴ section 3.2 continues with a detailed characterisation of the broadband state aid programmes in Bavaria.

3.1 Broadband State Aid in the European Union

According to Article 107 of the Treaty on the Functioning of the European Union (TFEU), state aid is defined as '... any aid granted by a Member State or through State resources in any form whatsoever which distorts or threatens to distort competition by favouring certain undertakings or the production of certain goods ...'. Granting state aid is generally prohibited unless it is justified by reasons of general economic development. To ensure that this

⁴ As one out of 16 states that constitute the Federal Republic of Germany, European laws and regulations apply to the State of Bavaria.

prohibition is respected and exemptions are applied equally across the European Union, the European Commission is in charge of ensuring that state aid complies with EU rules.

For the case of telecommunications and broadband infrastructures, the European Union has long recognised its strategic importance in promoting the key objectives of creating common European markets in general and fostering economic development in the Member States in particular. In its Digital Agenda for Europe⁵, the Commission therefore envisages concrete goals in the form of the nationwide coverage of broadband above 30 Mbit/sec and 50% of the households in the EU subscribed to broadband above 100 Mbit/sec by the year 2020. Although the Commission first and foremost aims at strengthening the incentives of private companies to invest in both the deployment of broadband infrastructures and subscriptions through the design and implementation of appropriate regulatory frameworks, since 2003 it explicitly allows the granting of state aid in cases of particular rural areas where the private investment incentives are considered insufficient due to the interference of large deployment costs and limited revenue potentials.

In order to reach its self-imposed goal of ‘well-designed aid targeted at market failures in order to achieve growth-enhancing priorities’ (European Commission, 2012), the Commission has adopted detailed broadband state aid rules which specify the conditions by which public funding could be provided for broadband deployment. According to the guidelines—originally adopted in 2009⁶ and revised in 2013⁷—the Commission supports public funding for broadband network deployment in rural and underserved ‘white’ areas where no broadband infrastructure exists or where no plans by private investors to roll out such an infrastructure exist in the near future. In providing funds, member states should pursue

⁵ See <https://ec.europa.eu/digital-agenda/en> (last accessed on 24 October 2016) for further information.

⁶ European Commission (2009), Community rules for the application of state aid rules in relation to rapid deployment of broadband networks, Official Journal of the European Union 2009/C 235.

⁷ European Commission (2013), EU Guidelines for the application of state aid rules in relation to the rapid deployment of broadband networks, Official Journal of the European Union 2013/C 25.

genuine cohesion and economic development as their main objectives (§40–42). In so-called ‘grey’ areas where only one broadband network operator is present, a more detailed assessment is required for state aid approval, as market distortions become more likely in those areas (§44–46). Finally, there is, in principle, no role for state aid in competitive ‘black’ areas with two or more existing broadband infrastructures (§43).

3.2 Broadband State Aid Programmes in Bavaria

In the year 2007, the German state of Bavaria⁸—consisting of 2,056 distinct municipalities—started the initiative ‘Broadband for Bavaria’, aiming at informing local municipalities about general possibilities to foster the deployment of broadband networks in rural areas. The initiative was motivated by slower broadband deployment in Bavaria compared to other German states, for reasons such as a lower population density, a high share of rural areas with numerous far-flung municipalities, and difficult topographical conditions with medium- and high-range mountains. Moreover, the divergence in broadband coverage between rural and urban regions was substantial.

Guided by the aim of providing equivalent working and living conditions in the entire state, in November 2007, the Bavarian government decided to support the deployment of broadband in rural areas from 2008 onwards (see Bavarian Ministry of Economic Affairs and Media, Energy and Technology, 2012).

Following this initiative by one federal state, the German Federal Government notified the European Commission of its intended policy to support rural areas all over Germany in the deployment of broadband infrastructure. By the end of 2008, the European Commission

⁸ In 2015, Bavaria generated a (nominal) GDP of about €550 billion, making it the second largest German state after North Rhine-Westphalia (with a GDP of about €646 billion). Although part of the Federal Republic of Germany, Bavaria therefore had a larger GDP than entire EU member states such as Austria (about €337 billion in 2015), Belgium (about €529 billion in 2014) or Poland (about €545 billion in 2014). Data sources: Statistical Offices of the Federation and the Länder and World Bank.

decided to raise no objections against the initiative, allowing the Bavarian government to support each broadband deployment project in rural areas with aid of up to €200,000.

After the state election in 2008, the Bavarian government decided in its coalition negotiations to increase the maximum amount of aid to €500,000 per municipality project. The respective proposal was approved by the European Commission on 19 May 2009. Due to the large number of subsequent applications by the majority of Bavarian municipalities, the government later decided to extend the programme up to the end of 2011, allowing all interested municipalities to apply for funding. Eventually, 1,300 municipalities received approval for funding by the end of 2011, that being about 63% of all Bavarian municipalities. The total funding amount provided by the public authorities added up to €107.6 million, being about €83,000 per aided municipality. Funding was granted for feasibility studies and planning activities as well as for closing the profitability gap for network infrastructure deployment.

Further information on the economic geography of broadband state aid is provided in Figure 1 below by plotting the state of Bavaria and the state aid status of all of its municipalities: white indicates municipalities that did not receive any aid; light blue flags municipalities that received aid in the 2008 to 2009 period; and dark blue shows all municipalities that received broadband aid in the 2010 to 2011 period. As revealed by Figure 1, the white areas are distributed all over Bavaria and do not show apparent concentrations in particular areas of the state.

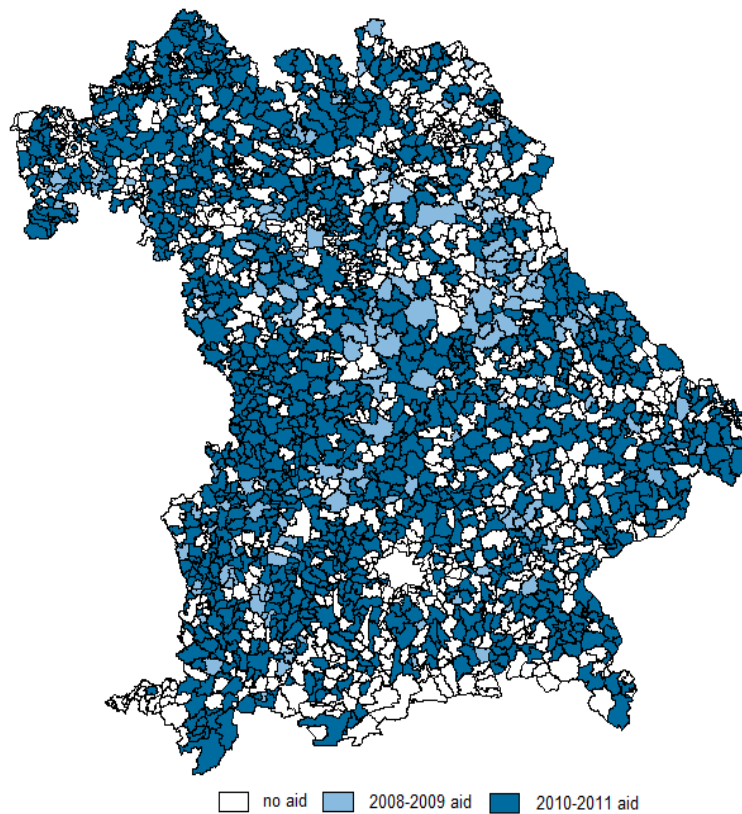


Figure 1: Aided and Non-Aided Municipalities in Bavaria

Data source: Bavarian Ministry of Economic Affairs and Media, Energy and Technology (2012)

In our empirical investigation below, we focus on the impact of broadband state aid granted by the state of Bavaria in the 2010 to 2011 period—basically because 87% of all aided municipalities received funding in this second aid period (marked in dark blue in Figure 1). Generally, the event window has to be long enough that changes in broadband infrastructure deployment related to state aid can be captured; however, it must also be short enough to avoid confounding effects from other changes that are not under control.

Accordingly, in defining our treatment period of 2010 and 2011, we excluded 171 municipalities to which state aid was granted during the first state aid programme from 2008 to 2009 (marked in light blue in Figure 1). Overall, our sample consists of 1,885 municipalities, of which 1,129 are in the treatment and 756 are in the control group of untreated municipalities.

As the deployment of broadband infrastructure is rather time-consuming and subject to technical complexities related to network planning, regulatory and legal permissions (such as rights of way and, in particular, the state aid approval procedures themselves), as well as other institutional rigidities (such as negotiations with property owners), it can take place only gradually (see Briglauer, 2015). Accordingly, we do not expect any substantial network deployment activities in the same year that state aid was approved by the government.

4 Empirical Strategy

We employ a two-stage empirical approach that, in the first stage, aims at identifying the effect of state aid on broadband coverage in Bavarian municipalities that received approval (treatment) in the period from 2010 to 2011 (the effectivity question). In order to achieve this, we employ a standard difference-in-differences (DiD) estimator as well as conditional DiD, which is based on a matched sample of aided and non-aided municipalities (section 4.1).⁹ Applying a conditional DiD means using in the control group only matching partners from a foregoing propensity score matching. So, the treatment and control group are identical with respect to all observables except the treatment. There are two advantages related to this approach: The first one is that the treatment can be taken as truly exogenous as it has been, with respect to observables, deployed randomly. The second one is that the common trend assumption is more likely to be fulfilled as the two groups show no statistically significant differences on all observables. In the second stage, we identify the effect of state-aid induced broadband deployment on employment using a two-stage least-squares (2SLS) instrumental variable (IV) estimation method (section 4.2; the efficiency question).

⁹ Please note that this kind of conditional DiD estimator relaxes the key identifying assumptions, as it combines the advantages both to abandon the linear assumption when controlling for observables and to control for unobservables, exploiting the panel dimension of the data (see Heckman *et al.*, 1998).

4.1 The Impact of State Aid on Broadband Deployment

In order to quantify the effect of state aid on broadband deployment, we are first interested in estimating the following static equation (super indexed s) on the basis of municipality-level panel data:

$$bb_hh_{it}^q = \alpha^{sq} + \beta_1^{sq} D_2 + \beta_2^{sq} D_1 D_2 + \mathbf{X}_{it}^{DiD} \beta_3^{sq} + \theta_i^{sq} + \varepsilon_{it}^q \quad (1)$$

The outcome variable, $bb_hh_{it}^q$, measures the share of household broadband coverage (i.e. availability on the supply side and not subscriptions on the demand side) in municipality i and in year t at various levels of bandwidth quality, super indexed q . θ_i^s captures municipality-specific fixed effects and ε_{it} represents the error term of the static specification. Including municipality fixed effects already captures a large share of the variation in broadband coverage, since most of the supply and demand factors show low variation over time (see also Akerman *et al.*, 2015, p. 1796). Equation (1) is estimated separately for the different quality levels. D_1 is a binary variable that indicates whether a municipality received treatment (state aid approved; $D_1 = 1$) in the funding period from 2010 to 2011. D_1 is truly exogenous as with respect to all observables there is no statistical difference between treatment and control group. The variable D_2 is also of binary nature and signals whether an observation belongs to the pre- or post-treatment period. D_2 is equal to one for the period from 2012 to 2014, as the corresponding observations were measured after the treatment, and zero otherwise. The coefficient of interest, however, is β_2^{sq} , the coefficient of the interaction term $D_1 D_2$ which is equal to one if the observation was measured after the treatment period and the observation was treated. Hence, the DiD coefficient β_2^{sq} directly captures the average treatment effect over the years 2012 to 2014. The vector \mathbf{X}_{it}^{DiD} contains the following list of time-varying covariates (discussed further in section 5 below).

- Socio-structural: working age structure, share of females

- Geo-structural: population density, accessibility of motorways and regional cities
- Economical: share of medium-sized firms, share of large firms, share of gross value added in secondary and tertiary sector
- Political: vote shares of CSU (Conservative) and SPD (Democrat) political parties in municipal elections

Secondly, as we have several time periods available, we can model the dynamics of the treatment effect more flexibly. The ‘dynamic’ (super indexed d) DiD regression framework for municipality i and year t with quality q is specified analogously to equation (1) and reads as follows:

$$bb_hh_{it}^q = \alpha^{dq} + \sum_{t=2012}^{2014} (\beta_{1t}^{dq} D_t + \beta_{2t}^{dq} D_1 D_t) + \mathbf{X}_{it}^{DiD} \beta_3^{dq} + \theta_i^{dq} + \mu_{it}^q \quad (2)$$

Instead of one interaction term in the static model, in the dynamic model we have three interaction terms, one for each year after treatment. In equation (2), t can take three distinct values corresponding to the years 2012 to 2014. Accordingly, instead of one dummy variable capturing the entire period after treatment, in the dynamic model we include three different dummies controlling for each year after treatment individually. This dynamic specification traces out the full adjustment path and thus relaxes the assumption that the policy impact is immediate or the same in every year.

The key identifying assumption underlying the DiD estimator is that both the treated and the untreated municipalities follow the same trend (parallel trends assumption). In our regressions, we include a large number of covariates to control for factors that might lead to different trends across the two groups after treatment. To ensure that the treatment and control group municipalities are even more likely to show the same trend over time, we also relate the DiD estimator to a matched sample obtained from a propensity score matching (PSM) procedure. For PSM, we include the same set of controls measured in 2010 to capture pre-

existing initial conditions while additionally including the following list of controls where we have pre-treatment information to further address the omitted variable bias at the municipality level:

- Socio-structural: number of households in 2010
- Geo-structural: type of municipality in 2010
- Economical: development of average rents in the years 2007–2009
- Initial broadband conditions: average annual growth rate in coverage of 1 Mbit/s in the years 2007 to 2009; availability of 2, 6 and 16 Mbit/s in 2010

Matching is a non-parametric estimation method: thus one of its main advantages is that it does not rely on a particular specification of functional forms and distributional assumptions about the error term, as is the case in ordinary regression methods including DiD regression frameworks. The broadest possible average causal effect is the average treatment effect (ATE) which averages across all municipalities. The ATE represents an estimator that can be applied to a sample drawn from a larger population where each individual observation is considered. However, in most policy contexts, the particular interest is in the average treatment effect on the treated (ATT), for whom the policy treatment was designed. In deciding whether a policy was effective, the focus would then be on those municipalities who received the treatment and not on the average effect of all municipalities (see Heckman *et al.*, 1997). In our case, the focus is thus on ATT; however, we calculate both the ATT and the ATE in our treatment analysis as part of our robustness checks.¹⁰

Identification of the ATT requires that all (pre-treatment) covariates that influence treatment assignment and potential outcomes have to be simultaneously observed, meaning that there

¹⁰ Please note that the ATT and ATE estimators rest on different identifying assumptions, which are weaker for the ATT estimator. See Cameron and Trivedi (2005, p. 864).

are no omitted confounding covariates (see Sianesi, 2004). To ensure that causal effects of the main explanatory variables are measured, we address the omitted variable bias at the municipality level by including several time-variant control variables in our empirical analysis. The other main matching assumption, referred to as ‘overlap’ or ‘common support’, implies that there is overlap in the two groups, as for each treated municipality there is another matched control group with a similar set of covariates.

4.2 *The Impact of Broadband Deployment on Employment*

The second stage of our analysis focuses on the impact of broadband infrastructure on different employment-related outcome variables. Broadband infrastructure, however, is not deployed randomly, but likely to be correlated with economic prosperity in a certain region or municipality. Accordingly, broadband infrastructure might be correlated with the error term which yields biased and inconsistent estimates. To address this point, we make use of 2SLS/IV estimation with two-way fixed effects in which we use the interaction term $D_1 D_2$ as the external instrument for broadband coverage. This allows us to identify a causal effect of state aid on the outcome variable. This is true because the interaction term has no effect on the outcome variable. The only mechanism through which it is working is the broadband deployment. The granting of state-aid solely for the purposes of broadband deployment has no direct or indirect—other than through broadband deployment—impact on the outcome variable. Accordingly, the second stage model of our empirical analysis reads as follows:

$$Y_{it}^I = \delta + \gamma_1^{Iq} bb_hh_{it}^q + \mathbf{X}_{it}^{IV} \gamma_2^{Iq} + \theta_i^{IV} + \lambda_t^{IV} + \varphi_{it}^q \quad (3)$$

where Y_{it}^I is the relevant employment outcome (measured by indicator I) in municipality i in period t . The outcome variable of the first stage, $bb_hh_{it}^q$, is now the main explanatory variable of interest. Accordingly, the coefficient γ_1^{Iq} indicates the impact of broadband coverage on employment outcome variables. Note, however, that the estimated coefficients in

the employment equations represent the impact of broadband availability on employment outcomes but not the effect related to actual broadband usage: whereas the former measures the intention-to-treat effect, the latter directly impacts economic outcomes such as employment in particular and is a function of broadband availability. Accordingly, we estimate a reduced form where the estimated coefficients represent a proportional effect which is smaller than the effect via broadband usage (see Czernich, 2014). \mathbf{X}_{it}^{IV} includes the set of covariates used in the first stage as well as education as a major employment specific covariate. θ_i^{IV} and λ_t^{IV} represent the municipality fixed effects and period effects respectively, and φ_{it}^q is the additive error term. By multiplying the coefficient β_3^{sq} from the first stage with the coefficient γ_1^{Iq} from the second stage, we can assess the causal effect (reduced form) of the state aid programme on the respective outcome variable.

5 Data

Our empirical analysis makes use of several separate data sets: the GENESIS database¹¹ and the INKAR¹² database together provide most of our socio-structural, geo-structural, economic and political covariates; the ACXIOM¹³ database provides information on the number of freelancers; the Schnelles Internet für Bayern⁶ database and the German Breitbandatlas¹⁴ provide data on which municipalities received state aid as well as broadband coverage. All variable definitions and sources are summarised in Table A1 in the Appendix. Table A2 provides the summary statistics.

The time window for our analysis ranges from 2010 to 2014. The treatment—i.e. the approval of state aid for broadband deployment—took place between 2010 and 2011. In view of the

11 See <https://www.statistikdaten.bayern.de/genesis/online> (last accessed on 24 October 2016).

12 See <http://www.inkar.de/> (last accessed on 24 October 2016).

13 See <http://www.acxiom.de/> (last accessed on 24 October 2016).

14 See http://www.zukunft-breitband.de/Breitband/DE/Breitbandatlas/BreitbandVorOrt/breitband-vor-ort_node.html (last accessed on 24 October 2016).

institutional rigidities described in section 3, however, we consider the year 2010 as pre-treatment when controlling for initial conditions. We do not evaluate the outcome variables with respect to the year 2011, as it can neither be defined as pre- nor as post-treatment for aid-receiving municipalities. In contrast, the years from 2012 to 2014 unambiguously define our post-treatment period.

5.1 Broadband Availability in Bavarian Municipalities

Broadband availability in the years 2010 to 2014 is measured as the share of households in a municipality that have access to a particular bandwidth quality level. In our analysis, we measure standard broadband with three different levels of download speed— ≥ 2 , ≥ 6 and ≥ 16 Mbit/sec—denoted by HH_2MB, HH_6MB and HH_16MB. Since the state aid programme was predominantly designed to provide a basic supply of broadband infrastructure, we concentrate on these low to medium speed levels.

Figure 2 illustrates the development of bandwidth levels in our observation period. As shown in the left-hand graph, treated municipalities are developing faster than untreated ones and, in the year 2012, overtook the untreated municipalities. The right-hand graph shows the same trend for the observations in the control group (instead of all untreated municipalities). Interestingly, the difference between treated and untreated (control) municipalities in the year 2014 is even larger in the right-hand graph.

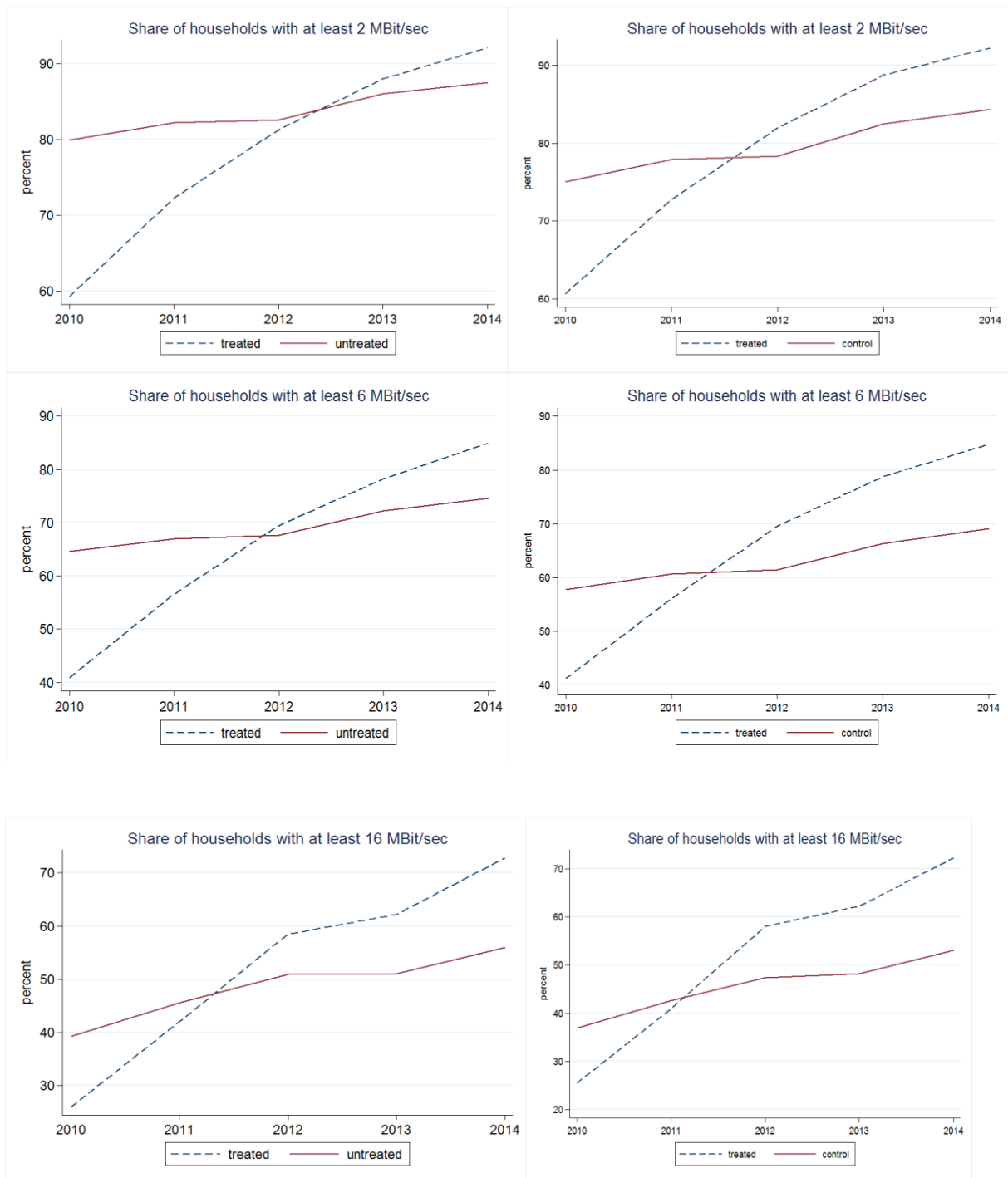


Figure 2: Development of Broadband Deployment in Bavaria

Data source: Federal Ministry of Transport and Digital Infrastructure (2016)

5.2 Employment in Bavarian Municipalities

We measure employment in a municipality by the number of employees with social insurance at place of residence (EMPL_RES) as well as place of work (EMPL_WORK) per 100

residents in 2010. So, we hold the denominator fixed to make interpretation easier. The number of employees with social insurance at place of residence measures the number of persons with a job living in a given municipality. The number of employees with social insurance at place of work measures the number of persons working in a given municipality. The variable EMPL_SELF measures the number of self-employed workers and freelancers per 100 residents in 2010. Finally, we are also able to examine the impact of basic broadband infrastructure on the average worker's annual gross wages (WAGE).

In Figure 3 we show the development of employees with social insurance per 100 inhabitants at place of work and at place of residence. Treated municipalities show fewer employees measured at place of work than untreated municipalities in 2010. However, in 2012 and 2013, treated municipalities are found to overtake the untreated ones. Compared with employees at place of work, the distance between treated and untreated (control) municipalities is substantially larger for employees at place of residence and is more pronounced in 2014 than in 2010. In Figure 4, we depict the development of self-employed and freelancers per 100 inhabitants and the annual gross wages. It appears that treated and untreated municipalities follow rather similar trends. In treated municipalities, we observe fewer self-employed and freelancers than in untreated municipalities. This gap does not close after the treatment period. A comparable development is found in the graph plotting annual gross wages.

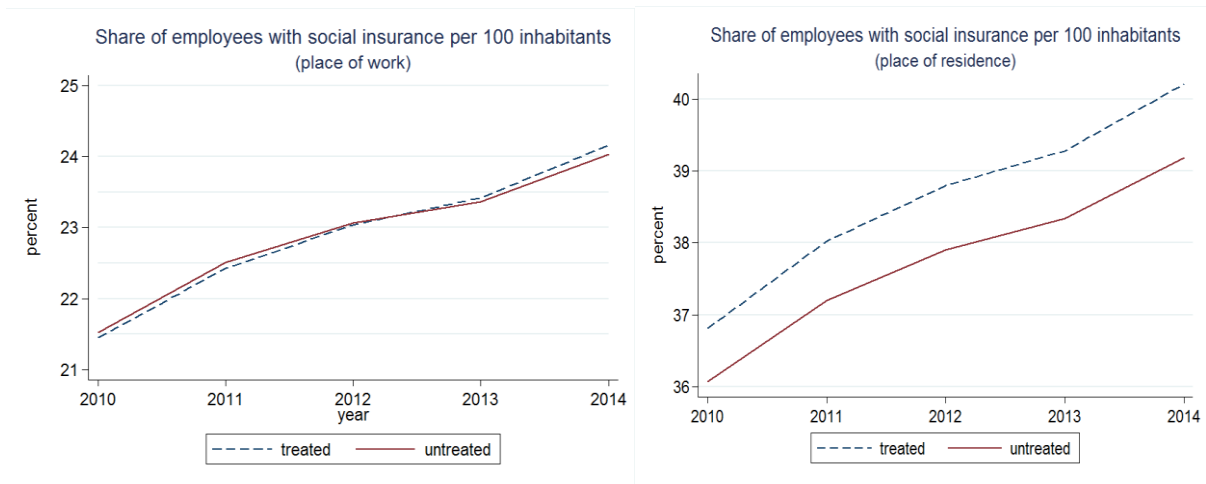


Figure 3: Development of Employees with Social Insurance at Place of Work (Left Panel) and Residence (Right Panel) in Bavaria

Data source: GENESIS database

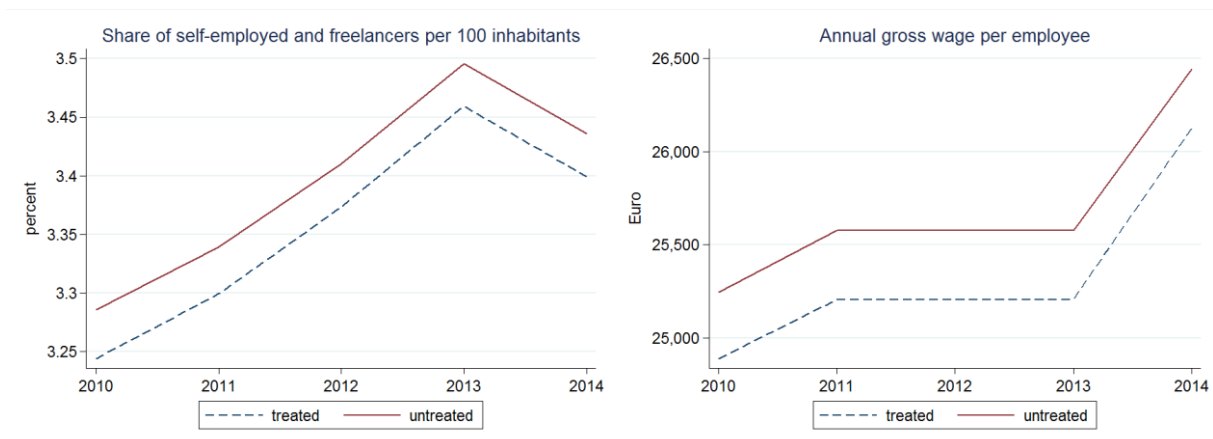


Figure 4: Development of Self-Employed (Left Panel) and Gross Annual Wages (Right Panel) in Bavaria

Data source: AXCIOM and GENESIS databases

5.3 Main Explanatory Variables and Further Covariates

Our main explanatory variable for the treatment analysis in the first stage is a binary indicator (D_1) which measures whether a municipality received state aid between 2010 and 2011. We expect that municipalities receiving state aid have increased access to broadband, which should become reinforced due to substantial adjustment costs in the subsequent years.

Regarding the second stage of our analysis, the main explanatory variable of interest is the available broadband infrastructure stock, as described in section 5.1. We expect that a higher broadband infrastructure stock induces positive externalities in major sectors of the economy, including local labour markets. Accordingly, we expect that broadband deployment comes with positive net employment effects in aided municipalities.

We distinguish five sets of covariates. Covariates controlling for the socio-structural dimension of a municipality are alongside the proportion of working people (WORKING_AGE) and the share of females (FEMALE) in a municipality. The age structure is measured by the share of people between 18 and 65.

The next set of covariates controls for the geo-structural characteristics of a municipality. We include a municipality's population density (POP_DENS), which captures average broadband deployment costs—characterised by so-called 'economies of density', leading to considerably higher costs in rural areas. Furthermore, densely populated areas tend to have thicker labour markets for relevant IT skill complementarities (see Forman *et al.*, 2012). Transport accessibility is measured as the average journey time (by car) in minutes to the next public accessibility point in terms of the next motorway (ACC_MOTOR) and the next regional metropolitan area (ACC_CITIES). Rural municipalities with good public accessibility might grow faster and attract funding more easily in view of a lower profitability gap. Very remote municipalities in, for example, alpine regions, however, most likely did not receive aid at all, as average deployment costs are simply too high to fall under the treatment scheme.

We further collected economic covariates along the following dimensions: the size and the business segment of the local companies. Specifically, we include the share of medium (FIRM_MED) and large firms (FIRM_BIG), as well as the gross value added (GVA) in the secondary (GVA_SEC) and tertiary sectors (GVA_TER)—i.e. we can control for how important the production and service sectors are in comparison to the primary sector.

Municipalities with a higher number of large firms and more pronounced secondary and tertiary sectors might have greater impact when claiming state aid. At the same time, local politicians might have an incentive to support economically underdeveloped areas in particular.

We further control for the political dimension by incorporating the outcome of the last communal election. In particular, we control whether municipalities led by one of the two main parties in Bavaria—the Christian Social Union (CSU) and the Social Democratic Party (SPD)—have been more successful in receiving aid. These variables can also be regarded as a proxy for how well a municipality is managed.

Our employment-specific control variable captures education (EDUC), measured as the share of school leavers with a higher education entry qualification in the total number of school leavers.

5.4 Covariates used in PSM

In PSM, we consider the initial conditions in a municipality in the period from 2007 to 2010. Firstly, we control for the average yearly growth rate of broadband coverage of 1 Mbit/sec between 2007 and 2009 (GR_1MB). Secondly, we include the share of households that had access to at least 2, 6 and 16 Mbit/sec in the year 2010 (HH_2MB, HH_6MB, HH_16MB). This ensures that pre-treatment units followed the same trend in the years directly preceding the granting of state aid and were, with respect to broadband availability, on the same initial level. Further covariates used for PSM are the number of households (HH), population density (POP_DENS), share of persons of working age (WORKING_AGE) and share of females (FEMALE), as well as the type of municipality (TYPE_X). These covariates are available for the year 2010. The latest outcome of the communal election in Bavaria before our funding period refers to the year 2008. Here, we include the shares of votes gained by the two most popular parties, CSU and SPD. Finally, we have information on the number of medium

(FIRM_MED) and large firms (FIRM_BIG), the share of gross value added in the secondary (GVA_SEC) and tertiary sectors (GVA_TER), the accessibility of motorways (ACC_MOTOR) and cities (ACC_CITIES), and the average annual growth in rents per square metre (GR_RENT) over the time period from 2007 to 2009.

6 Estimation Results

In this section, we present our main results of the treatment analysis based on DiD approaches. Section 6.1 first discusses the results of the PSM procedure, which serves as a prerequisite to delineate the sample for conditional DiD. Subsequently, we present the results of basic and conditional DiD in section 6.2 (effectivity question). In both DiD variants, we analyse the immediate (i.e. static) impact, as well as the dynamic effects of state aid policies. In section 6.3, we discuss the main IV estimation results with regard to the impact of broadband infrastructure on employment (efficiency question).

6.1 Propensity Score Matching

Before coming to our DiD and IV estimation, we aim at making treated and untreated municipalities as similar as possible so that the common trend assumption is likely to hold. We do this by propensity score matching (PSM). We apply nearest neighbours matching with two and three nearest neighbours.

Table *I* is a balancing table and shows the mean differences in broadband availability and covariates between treated and control municipalities before and after matching. We apply ordinary two-sample *t*-tests to check the balancing properties of our matching procedure. The tests examine whether the differences in means vary in treated and untreated groups before and after matching (H_0 : means are equal for both groups). For obvious reasons, matching is designed to ensure that for units with a similar propensity score, the assignment to treatment is random and independent of the covariates. If this is satisfied, then municipalities with a similar propensity score must have the same distribution of covariates independent of the treatment status. This balancing condition is testable through differences in means for each covariate. Table A3 reports the mean tests applied to the whole sample—i.e. before matching is conducted. As expected, almost all means of the covariates are significantly different between treated municipalities ($N=1129$) and untreated municipalities ($N=756$). In contrast, one can infer from

Table *I* that all covariates are well balanced after matching, which holds for both control groups (NB=2 and NB=3). Therefore, and in line with the above tests, we are confident that our matching procedure was successful in identifying valid counterfactuals for the group of treated (i.e. state aid-receiving) municipalities.

Table A4 reports the results of the probit regression of the PSM approach: assessments of the quality of the matching procedure refer to the performance measures of the probit model. The Pseudo R^2 measures the explanatory power of the covariates and should be significantly lower after the matching procedure (see Sianesi, 2004). Indeed, comparing the Pseudo R^2 in Table A4 (0.158) with the respective value of the probit regression after matching based on the sample of treated units and counterfactuals (0.003 for NB=2 and 0.003 for NB=3)¹⁵ indicates that the systematic differences between the two groups decreased substantially after controlling for covariates. Similarly, one can compare likelihood ratio tests on the joint significance of all covariates in the probit model before and after matching. As required, the null hypothesis (all covariates are jointly insignificant) is rejected before ($p=0.000$) but not after matching ($p=0.917$ for NB=2 and $p = 0.875$ for NB=3).

¹⁵ The underlying probit regressions are available from the authors upon request.

Table 1: Mean Difference Tests after Matching with Unequal Variances

	Aid-receiving municipalities		Selected control group (2NB)		Selected control group (3NB)		Results of <i>t</i> -test on mean differences	
	N=1,077		N=2,154 (547)		N=3,231 (616)		2 NB	3NB
	mean	S.D.	Mean	S.D.	Mean	S.D.		
Covariates								
HH_2MB_2010	60.94	32.11	60.70	35.94	59.64	35.89		
HH_6MB_2010	41.65	33.33	41.00	36.87	41.35	36.68		
HH_16MB_2010	25.81	28.99	26.82	32.29	26.68	31.51		
GR_RENT	0.06	0.03	0.06	0.03	0.06	0.04		
GR_1MB	3.22	3.97	5.50	5.41	3.37	3.40		
HH	2.39	8.53	2.06	3.80	2.19	5.59		
WORKING_AGE	62.92	1.94	62.89	1.98	62.89	2.04		
FEMALE	50.02	1.49	50.04	1.41	50.00	1.51		
POP_DENS	160.64	189.61	161.93	194.32	161.10	219.97		
TYPE_1	0.00	0.04	0.00	0.02	0.00	0.04		
TYPE_2	0.03	0.17	0.03	0.17	0.03	0.16		
TYPE_3	0.08	0.27	0.07	0.26	0.08	0.27		
TYPE_4	0.27	0.44	0.29	0.45	0.26	0.44		
TYPE_5	0.62	0.49	0.61	0.49	0.63	0.48		
FIRM_MED	14.85	3.64	15.06	3.85	14.85	3.84		
FIRM_BIG	2.40	0.89	2.43	0.89	2.39	0.91		
GVA_SEC	37.46	9.34	37.32	10.49	37.63	10.89		
GVA_TER	60.75	9.42	60.90	10.51	60.56	10.87		
CSU	0.24	0.20	0.24	0.21	0.24	0.22		
SPD	0.10	0.12	0.10	0.13	0.10	0.13		
ACC_MOTOR	15.29	11.09	15.38	9.91	15.27	10.09		
ACC_CITIES	31.53	14.79	30.83	14.42	31.01	14.61		
Outcome var.								
HH_2MB_2012	81.90	24.34	65.99	33.70	65.35	34.15	***	***
HH_2MB_2013	88.63	18.56	71.70	30.90	71.05	31.32	***	***
HH_2MB_2014	92.22	14.07	75.12	29.44	74.21	30.09	***	***
HH_6MB_2012	69.63	30.24	47.03	36.45	46.93	36.58	***	***
HH_6MB_2013	78.69	25.92	52.56	35.98	52.19	36.01	***	***
HH_6MB_2014	84.74	20.66	57.05	36.42	56.46	36.56	***	***
HH_16MB_2012	58.01	31.30	36.25	32.83	36.22	32.84	***	***
HH_16MB_2013	62.13	30.09	38.76	34.15	38.37	33.94	***	***
HH_16MB_2014	72.16	26.89	44.79	36.23	44.31	36.03	***	***

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. H_0 : equal means for both groups. As the nearest neighbour matching procedure is performed with replacement, we impose Lechner's variance approximation (Lechner, 2001). Due to the lack of common support, 52 municipalities had to be dropped, resulting in 1,077 treated municipalities. With two and three nearest neighbours, this corresponds to 2,154 or 3,231 observations in the control group, respectively. The numbers in parentheses in the heading of

Table *I* indicate the number of real municipalities used in PSM. As we have fewer untreated than treated municipalities, we reuse municipalities in the control group for several treated municipalities.

6.2 First Stage: The Impact of State Aid on Broadband Deployment

Table 2: shows the estimation results of the static and dynamic DiD models on the basis of the whole (columns 1, 3 and 5) and the matched sample of treated and untreated municipalities (columns 2, 4 and 6). In order to take into account the fact that several non-treated municipalities are overrepresented in the matched sample due to replacement, we also applied weights in the DiD estimation. If a municipality from the control group was a neighbour for several treated municipalities, it accordingly received a proportionally higher weight in the DiD estimation. Table 2: reports the average treatment effect (ATE), which averages across all municipalities (whole sample), and the average treatment effect on the treated (ATT) for the matched sample.

In the static model, the treatment effect is averaged over the years following the treatment. For instance, the ATT in column 2 is ~16.76, meaning that within the treatment group, the fraction of households which had access to at least 2 Mbit/sec increased by 16.76 percentage points after the treatment. Regarding the ATT for 6 and 16 Mbit/sec, the ATT is even larger, with about 20 percentage points. Generally, the treatment effects are higher for 6 and 16 Mbit/sec quality levels in all specifications. These findings appear to be reasonable, as bandwidth of ≥ 2 Mbit/sec represented a very elementary quality level in the post-treatment period. Hence, it appears likely that funding was used to realise higher broadband levels, since in 2010, broadband with 2 Mbit/sec bandwidth had already been widely dispersed, even in treated municipalities.

The dynamic model reports the ATE/ATT for each year after treatment individually. All coefficient estimates indicate that there is a highly significant and positive treatment effect underlying all quality levels of broadband infrastructure. However, since some municipalities received treatment later—i.e. in 2011 instead of 2010—and in view of the gradual infrastructure deployment process, it appears unlikely that corresponding treatment effects

have already been materialised completely within the first year of policy assessment—i.e. in 2012. Due to adjustment costs, potential impacts are rather expected in the following years 2013 to 2014. In line with these expectations, the treatment effect strictly increases over the years for each broadband quality level and for both specifications (based on the whole and matched sample only). For instance, for the speed of 6 Mbit/sec, the treatment effect for the matched sample (column 4) increases from ~18.75 percentage points in 2012 to ~23.52 percentage points in 2013, and to ~27.21 percentage points in 2014.

Table 2: First Stage DiD Estimation Results

Dep. var.: Sample:	(1) HH_2MB Whole	(2) HH_2MB Matched & Weighted	(3) HH_6MB Whole	(4) HH_6MB Matched & Weighted	(5) HH_16MB Whole	(6) HH_16MB Matched & Weighted
Static Model						
ATE/ATT	17.1569*** (23.39)	16.7563*** (22.04)	23.7151*** (27.82)	23.2355*** (25.71)	20.9038*** (23.76)	20.2103*** (19.88)
Dynamic Model						
ATE/ATT 2012	13.8541*** (19.63)	13.6111*** (18.56)	18.9725*** (22.29)	18.7469*** (21.36)	16.1000*** (17.53)	15.4308*** (14.48)
ATE/ATT 2013	17.4633*** (21.47)	17.2047*** (20.79)	23.8157*** (24.84)	23.5227*** (23.33)	20.5130*** (20.41)	19.9361*** (17.25)
ATE/ATT 2014	20.1909*** (22.54)	19.3537*** (21.05)	28.3151*** (26.56)	27.2138*** (24.16)	26.4073*** (22.62)	25.2467*** (19.24)
# Obs.	9,425	8,120	9,425	8,120	9,425	8,120

t-Statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the municipality level and robust to heteroscedasticity. In the matched case, we used the Stata 13 *pweight* option. The complete estimation results for the matched and weighted sample can be found in Table A5.

6.2.1 Assessing the Parallel Trend Assumption

The key identifying assumption underlying the DiD estimator is the parallel trend assumption. Although the latter is not directly testable, the assumption can be investigated using pre-treatment data for both groups of treated and untreated municipalities. Figure 5 provides strong visual evidence of a common underlying trend for the pre-treatment period from 2005 to 2009 where data are available for 1 Mbit/sec broadband coverage. Moreover, if we compare the left-hand and right-hand graphs, we can infer that the remaining differences between treated and untreated municipalities are even further reduced if we focus on matched

municipalities only (right-hand graph). This provides a reasonable justification for our preference of the conditional DiD approach on the basis of the matched sample.

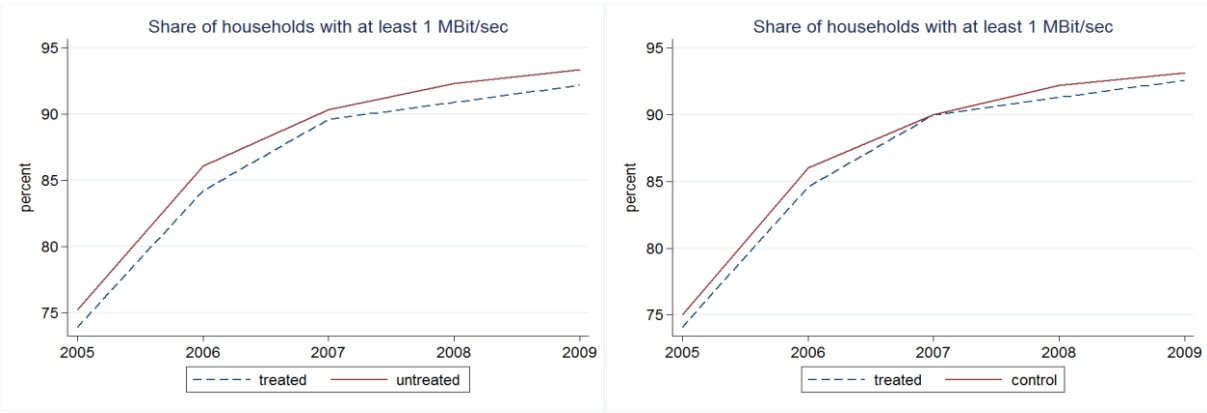


Figure 5: Trends in Years Preceding Treatment for all Municipalities (Left Panel) and Matched Municipalities only (Right Panel)

Data source: Federal Ministry of Transport and Digital Infrastructure (2016)

From Figure 5 we also infer that there is no apparent evidence of Ashenfelter’s pre-treatment dip (see Ashenfelter, 1978) for aid-receiving municipalities, which might have occurred in view of expectations related to the institutional design and gradual development of the funding schemes in Bavaria. However, on the basis of the visual evidence in Figure 5, we can conclude that potential crowding-out effects and overestimation bias should be of secondary importance (if relevant at all). However, at the beginning of the pre-treatment period (2010), the observed substantial differences in higher bandwidth levels (Figure 2) suggest some differences in pre-treatment trends. Accordingly, we also control for higher bandwidth levels in 2010 in constructing the control group in PSM.

6.2.2 Assessing the Treatment Effect with PSM

We further investigate whether the treatment effects are similar on the basis of a pure PSM procedure which rests upon a different key identifying assumption. Whereas PSM does not

require parallel trends, it assumes that, conditional on pre-treatment outcomes, confounding unobservables are irrelevant (selection is on observables).

The lower part of

Table 1 reports the broadband outcome values for aid-receiving municipalities and different control groups with two (2NB) and three (3NB) nearest neighbours. The differences in average outcome variables of the treated municipalities are statistically significant according to two-sided t -tests throughout all definitions and for both counterfactual groups. For instance, if we compare the 2 Mbit/sec broadband coverage of the treated municipalities with the counterfactual group with two nearest neighbours in the year 2012, the treatment effect is $81.90 - 65.99 = 15.91$. The respective value is quite similar to the respective DiD coefficient reported in Table 2: (13.85 for the whole and 13.61 for the matched and weighted sample), which also applies to the other bandwidth quality levels for both control groups: DiD and PSM based point estimates range from 13.85 to 28.31 (Table 2:) and from 12.19 to 28.28 (Table A6) respectively. From Table A6, we also see that estimated ATE and ATT again becomes more effective if we allow for a longer post-treatment period and for higher bandwidth levels.

Overall, treatment analysis on the basis of PSM points to rather similar estimates in terms of magnitude, dynamics and bandwidth levels, providing further arguments for the robustness of our first stage estimation results.

6.3 *Second Stage: The Impact of Broadband Deployment on Employment*

The second stage of our empirical analysis examines the impact of broadband deployment on employment-related outcome variables, which are reported in Table 3 in columns 1a–4c.¹⁶ The employment variables in columns 1a–3c are measured as a fraction of 100 residents measured in 2010, whereas average workers' gross wages in columns 4a–4c are measured in € per year. Similar to section 6.2, we report the estimation results in Table 3 on the basis of whole as well as matched and weighted samples.

¹⁶ The estimation results for the full models are available from the authors upon request. For columns 1a–1c, the full specifications can be found in Table A7.

The broadband coverage in our model is endogenous with respect to the share of employees at place of residence (EMPL_RES) and for the share of employees at place of work (EMPL_WORK), as the Durbin-Wu-Hausman (DWH) tests reject the null hypothesis of broadband coverage being an exogenous variable in columns 1a–2c for all broadband quality levels. In turn, this is not the case for the other outcome variables (EMP_SELF and WAGE). OLS estimates for the full specifications of all employment outcomes are reported in Tables A8 and A9. To deal with endogeneity underlying the specifications in columns 1a–2c, we employ the interaction term D_1D_2 of the first stage as our source of exogenous variation. First stage F -statistics of excluded instruments and the Cragg-Donald Wald (CDW) F -statistic, which clearly exceeds the IV critical value by Stock and Yogo (2005) for all dependent variables and quality levels, suggest that our instrument is strong and explains the broadband coverage very well.

As revealed in Table 3, with the exception of the employees measured at the place of residence variable (EMPL_RES), we do not find any significantly positive impact of broadband coverage on employment outcome variables.¹⁷ This finding is at least partly in line with existing empirical evidence, which also struggles to find supportive evidence for overall significant labour market effects.

Our results suggest that the benefit of increasing broadband coverage in rural areas is visible only with respect to employment measured at place of residence. Taking, for example, the coefficient of HH_6MB (0.0063) in column 1b and multiplying it with the coefficient from the first stage (23.2355 in Table 3) yields an overall value of 0.146, suggesting that the number of employees with social insurance at place of residence increased by about 0.14 percentage points. Multiplied with the average size of treated municipalities in the pre-treatment year

¹⁷ Table A8 in the Appendix also reports significant and positive coefficient estimates for self-employment (EMPL_SELF) and gross annual wages (WAGE) in OLS specifications; however, the magnitudes of related marginal effects are negligible.

2010 (50.0420), this results in 7.3253, meaning on average about seven additional persons with jobs with social insurance in each municipality that received the state aid. Multiplying this result with the total number of municipalities that received aid (1,129) results in 8,270 additional jobs at place of residence. This key result suggests that households and the respective individuals remain in (or move to) rural areas to live (but not to work) there if basic broadband coverage is present, a finding well in line with recent evidence reported in Ahlfeldt *et al.* (2016). There, the authors estimate consumers' valuation of broadband speed via house prices and find an elasticity of property prices with respect to internet speed of about 3%. Their data covers similar ranges of basic broadband connection and underlines the residential importance of broadband (although this effect is more relevant in urban areas).

Furthermore, there is weak evidence for an increased number of jobs with social insurance at place of work as well. This means that some jobs have been created, too. The coefficient is even larger than the coefficient discussed in the preceding section. Given the short time span after the state aid the somewhat weaker significance can be explained by the fact that it needs time for extensions of companies and the creation of new jobs, whereas private person with their housing decision might react in a timelier manner.

Last but not least, the fact that increased broadband coverage in treated municipalities did not induce additional jobs measured at place of work but only jobs at place of residence further suggests that a basic internet infrastructure makes activities such as tele-working or commuting to other (more urban) municipalities more attractive for some of the working age people living in rural areas. In other words, it can be concluded that although we do not find evidence consistent with the hypothesis that state aid has a significant impact on closing the economic divide, it is found to have a significantly positive effect on the closing of the digital divide between urban and rural areas in the German state of Bavaria. Furthermore, if an additional political aim of broadband state aid was seen to be the avoidance of further

depopulation of rural areas, our results provide evidence that state aid has significantly contributed to reaching this aim.

Table 3: Second Stage IV Estimation Results

Dep. var.	1a EMPL_ RES	1b EMPL_ RES	1c EMPL_ RES	2a EMPL_ WORK	2b EMPL_ WORK	2c EMPL_ WORK	3a EMPL_ SELF	3b EMPL_ SELF	3c EMPL_ SELF	4a WAGE	4b WAGE	4c WAGE
Matched & Weighted												
HH_2MB	0.0087 ^{***} (2.97)			0.0157 [*] (1.92)			0.0003 (0.91)			1.7615 (0.99)		
HH_6MB		0.0063 ^{***} (2.97)			0.0114 [*] (1.92)			0.0002 (0.91)			1.2726 (0.99)	
HH_16MB			0.0072 ^{***} (2.96)			0.0130 [*] (1.92)			0.0002 (0.91)			1.4554 (0.99)
# Obs.	8,120	8,120	8,120	8,120	8,120	8,120	8,120	8,120	8,120	8,120	8,120	8,120
# Groups	1,624	1,624	1,624	1,624	1,624	1,624	1,624	1,624	1,624	1,624	1,624	1,624
<i>F</i> (excl. instr.)	758.17	1043.25	634.75	758.17	1043.25	634.75	755.81	1041.29	633.63	755.81	1041.29	633.63
<i>F</i>	589.36	590.41	584.18	55.67	55.94	55.77	274.86	274.82	274.48	158.99	158.64	158.48
<i>CDWF</i>	370.760	482.437	355.819	370.760	482.437	355.819	368.881	480.138	354.151	524.32	660.93	84.20
<i>DWH</i>	0.0083	0.0034	0.0011	0.0073	0.0054	0.0109	0.9028	0.8941	0.5302	0.8110	0.8294	0.6799
<i>R</i> ²	0.702	0.702	0.697	0.141	0.144	0.142	0.431	0.431	0.430	0.402	0.401	0.401
Whole sample												
HH_2MB	0.0079 ^{***} (3.29)			0.0084 (1.39)			0.0003 (1.27)			0.8400 (0.49)		
HH_6MB		0.0057 ^{***} (3.29)			0.0061 (1.39)			0.0002 (1.27)			0.6085 (0.49)	
HH_16MB			0.0065 ^{***} (3.29)			0.0069 (1.39)			0.0003 (1.27)			0.6867 (0.49)
# Obs.	9,425	9,425	9,425	9,425	9,425	9,425	9,425	9,425	9,425	9,425	9,425	9,425
# Groups	1,885	1,885	1,885	1,885	1,885	1,885	1,885	1,885	1,885	1,885	1,885	1,885
<i>F</i> excl. instr.	897.96	1236.28	884.43	897.96	1236.28	884.43	897.48	1235.33	882.85	897.48	1235.33	882.85
<i>F</i>	652.98	653.69	649.55	71.11	71.26	71.26	331.25	331.01	330.54	172.44	172.42	172.42
<i>CDWF</i>	749.354	990.947	727.784	749.354	990.947	727.784	748.021	989.249	726.298	748.02	989.25	726.30
<i>DWH</i>	0.0056	0.0023	0.0009	0.0140	0.0141	0.0725	0.8378	0.7796	0.3756	0.9263	0.7366	0.8419
<i>R</i> ²	0.651	0.651	0.649	0.153	0.155	0.155	0.412	0.411	0.410	0.350	0.350	0.350

t-Statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the municipality level and robust to heteroscedasticity. All regressions include fixed effects for year, municipality and controls of equation 30, as well as a covariate measuring education (EDUC).

7 Summary and Conclusions

In May 2012, the European Commission announced its State Aid Modernisation (SAM) reform aiming at fostering growth in the internal market through streamlined rules and faster decisions. ‘State aid control should more effectively target sustainable growth-enhancing policies while encouraging budgetary consolidation, limiting distortions of competition and keeping the single market open’ (European Commission, 2012, p. 4). Interestingly, in working towards these goals, the Commission’s strategy not only envisages the identification of common principles for assessing the compatibility of aid with the internal market—in combination with the creation or revision of guidelines and frameworks—but explicitly includes an ex-post evaluation programme as a key tool to ensure an effective EU state aid policy (see European Commission, 2014).

In this paper, we provide such an ex-post evaluation for the example of a major state aid programme for broadband deployment in rural areas of the German state of Bavaria. Using a unique micro panel dataset, we evaluate the causal effect of state aid that was granted in the period from 2010 to 2011 to a total of 1,300 municipalities. Our post-treatment period refers to the years from 2012 to 2014. Using a difference-in-differences estimation strategy on the basis of a matched sample of 1,845 aided and non-aided rural municipalities, we first examined—from an effectivity perspective—the question of whether the granting of state aid had the desired direct effect. Our treatment analysis revealed that state aid indeed had an impact on the municipalities treated, as they have significantly higher broadband coverage than comparable non-aided municipalities. In particular, we found that the aided municipalities have, depending on broadband quality, between 16.8% and 23.2% higher broadband coverage than non-aided municipalities. Our results further suggest that the effect of state aid is more pronounced for medium (6 Mbit/sec) bandwidth levels and that it gains strength over the years after treatment.

Secondly—from an efficiency perspective—we examined whether the additional broadband coverage also carried over to socially desirable indirect effects in terms of creating new jobs. We found that state aid-induced higher broadband coverage generated significantly positive employment effects with respect to the number of employees at place of residence only. Based on an average bandwidth level (6 Mbit/sec), we found that in sum 8,139 additional individuals with social insurance were induced by the broadband state aid programme in the years 2010 to 2011 to live in the treated Bavarian municipalities. Furthermore, our empirical results suggest that more people decided to move into treated municipalities than left these rural areas, indicating that improved broadband coverage makes these municipalities more valuable places to live. In that sense, the funding programme successfully served as a means of preventing rural municipalities from depopulation; however, it does not impose a measurable effect on the closing of the economic divide in the form of creating new jobs in these municipalities.

Coming back to the main policy-related question raised in the title of the article—namely, whether state aid can help in bridging the digital and economic divide—our empirical results for the state of Bavaria support the conclusion that state aid programmes can be an effective instrument to foster broadband deployment, particularly in rural areas. When it comes to the subsequent efficiency question of such investments, our result of a total of 8,139 additional individuals with social insurance being induced to live in the treated Bavarian municipalities may, on the surface, appear modest compared to the total amount of €107.6m of state aid provided by the public authorities to the respective municipalities.

However, a closer look reveals that such a simple comparison of benefits and costs would be superficial for at least two reasons. On the one hand, it ignores the further positive knock-on effects of improved broadband coverage on, for example, innovation or economic growth that are expected to be generated in the longer run. On the other hand, such a simple comparison

ignores the additional (counterfactual) costs that would have been created by an accelerating digital and economic divide between urban and rural areas that might have occurred in the absence of the respective state aid programme. Such aspects need to be investigated as part of future research to work towards well-founded conclusions on effective and efficient ways to bridge the digital and economic divide.

Appendix

Table A1: Description of Variables and Sources

Variable	Description	Source
Outcome variables stage 1		
HH_XMB	Share of households with at least XMB fixed-line broadband connections. X can take the values 2, 6 and 16, municipality level (2010–2014)	Breitbandatlas/ TÜV Rheinland
Outcome variables stage 2		
EMPL_RES	Number of employees with social insurance, municipality level at place of residence per 100 residents in 2010 (2010–2014)	GENESIS
EMPL_WORK	Number of employees with social insurance, municipality level at place of work per 100 residents in 2010 (2010–2014)	GENESIS
EMPL_SELF	Number of self-employed workers and freelancers, municipality level per 100 residents in 2010 (2010–2014)	ACXIOM
WAGE	Annual gross wage in € per employee in a municipality (2010–2014)	GENESIS
Treatment variable		
D ₁	Dummy on whether a municipality received state aid between 2010 and 2011, municipality level	Schnelles Internet für Bayern
Control variables		
HH	Number of households, municipality level (2010)	Micro Census 2011
GR_RENT	Annual growth in rents, municipality level (2007–2009)	IDN Immo Daten GmbH
GR_1MB	Average yearly growth rate in the share of households with access to 1MB, municipality level (2007–2009)	Breitbandatlas/ TÜV Rheinland
TYPE_X	Municipality type, indicator of how rural a municipality is (2010)	INKAR
FEMALE	Share of female inhabitants, municipality level (2010–2014)	GENESIS
WORKING_AGE	Share of people of working age (i.e. 18 to 65 years), municipality level (2010–2014)	INKAR
POP_DENS	Population density per square kilometre, municipality level (2010–2013)**	GENESIS
GVA_SEC	Share of gross value added in secondary sector, county level (2010–2013)**	INKAR
GVA_TER	Share of gross value added in tertiary sector, county level (2010–2013)**	INKAR
FIRM_MED	Share of firms with 50 to 250 employees, county level (2010–2013)**	INKAR
FIRM_BIG	Share of firms with more than 250 employees, county level (2010–2013)**	INKAR
CSU	Share of the CSU party in the municipal election (2008–2014)	GENESIS
SPD	Share of the SPD party in the municipal election (2008–2014)	GENESIS
ACC_MOTOR	Average journey time (car) in minutes to the next motorway, municipality level (2010, 2012–2014)*	INKAR
ACC_CITIES	Average journey time (car) in minutes to the next regional metropolitan area, municipality level (2010, 2012–2014)*	INKAR
EDUC	Percentage share of school leavers with a higher education entry qualification in the total number of school leavers, county level (2010–2013)**	INKAR

Notes: * Missing values for 2011 were calculated using linear interpolation. ** In case control variables were only available up to the year 2013, we have extrapolated them to the year 2014.

Table A2: Summary Statistics

	# Obs.	Mean	Std. Dev.	Min.	Max.
HH_2MB	9,425	80.351	26.727	0.000	100.000
HH_6MB	9,425	66.734	32.788	0.000	100.000
HH_16MB	9,425	50.150	34.103	0.000	100.000
EMPL_RES	9,425	38.234	3.517	21.463	52.522
EMPL_WORK	9,425	22.759	18.457	0.930	200.573
EMPL_SELF	9,425	3.369	1.147	0.558	8.150
WAGE	9,425	25520.433	4318.374	13662.086	83865.617
D ₁	9,425	0.599	0.490	0.000	1.000
EDUC	9,425	25.965	9.796	8.900	70.300
WORKING_AGE	9,425	63.289	2.135	52.800	70.700
GR_RENT	9,425	2.288	2.803	0.000	5.885
GR_1MB	9,425	1.363	5.465	-42.855	50.000
TYPE_1	9,425	0.003	0.056	0.000	1.000
TYPE_2	9,425	0.029	0.168	0.000	1.000
TYPE_3	9,425	0.077	0.266	0.000	1.000
TYPE_4	9,425	0.266	0.442	0.000	1.000
TYPE_5	9,425	0.624	0.484	0.000	1.000
HH	9,425	2.760	18.552	0.088	732.793
FEMALE	9,425	50.111	1.418	36.100	56.700
POP_DENS	9,425	189.626	292.660	6.000	4531.200
FIRM_MED	9,425	15.325	3.754	8.200	40.200
FIRM_BIG	9,425	2.570	0.941	0.480	7.470
GVA_SEC	9,425	37.908	9.601	12.800	71.700
GVA_TER	9,425	60.227	9.710	26.800	86.500
CSU	9,425	0.246	0.205	0.000	1.000
SPD	9,425	0.109	0.130	0.000	0.663
ACC_MOTOR	9,425	14.662	10.790	0.000	69.000
ACC_CITIES	9,425	29.029	14.172	0.000	82.300

Table A3: Descriptive Statistics of the Used Samples / Sample Means before Matching

	Control group		Aid-receiving municipalities		Results of <i>t</i> -test on mean differences	
	N=756		N=1,129		Diff	<i>t</i> -value
	Mean	S.D.	mean	S.D.		
Covariates						
HH_2MB_2010	79.96	26.54	58.94	33.02	***	(15.26)
HH_6MB_2010	64.60	31.80	39.75	33.68	***	(16.23)
HH_16MB_2010	39.25	32.89	24.62	28.83	***	(9.94)
GR_RENT	0.05	0.02	0.05	0.02		(-1.29)
GR_1MB	5.43	5.65	5.27	4.91		(0.66)
HH	3.44	27.47	2.31	8.34		(1.10)
WORKING_AGE	62.52	2.21	62.93	1.91	***	(-4.22)
FEMALE	50.35	1.49	49.97	1.53	***	(5.32)
POP_DENS	239.50	396.52	157.27	186.10	***	(5.32)
TYPE_1	0.01	0.07	0.00	0.04		(1.20)
TYPE_2	0.03	0.17	0.03	0.17		(0.26)
TYPE_3	0.07	0.26	0.08	0.27		(-0.56)
TYPE_4	0.23	0.42	0.29	0.46	***	(-3.29)
TYPE_5	0.67	0.47	0.60	0.49	***	(3.03)
FIRM_MED	14.70	3.81	14.89	3.62		(-1.11)
FIRM_BIG	2.38	0.89	2.43	0.91		(-1.25)
GVA_SEC	36.42	10.34	37.74	9.36	***	(-2.83)
GVA_TER	61.80	10.47	60.48	9.43	***	(2.79)
CSU	0.26	0.20	0.24	0.21	***	(2.83)
SPD	0.13	0.14	0.10	0.12	***	(4.35)
ACC_MOTOR	12.98	9.21	15.84	11.55	***	(-5.95)
ACC_CITIES	28.77	14.65	31.97	14.92	***	(-4.62)
Outcome variables						
HH_2MB_2012	82.58	24.36	80.55	26.04	*	(1.72)
HH_2MB_2013	86.07	21.64	87.82	19.97	*	(-1.77)
HH_2MB_2014	87.54	20.51	92.09	14.44	***	(-5.28)
HH_6MB_2012	67.63	30.67	68.23	31.67		(-0.41)
HH_6MB_2013	72.20	29.39	77.82	27.17	***	(-4.19)
HH_6MB_2014	74.58	28.70	84.76	20.93	***	(-8.38)
HH_16MB_2012	50.90	30.80	57.09	32.25	***	(-4.20)
HH_16MB_2013	50.98	32.51	61.70	30.76	***	(-7.17)
HH_16MB_2014	55.91	32.56	72.60	27.05	***	(-11.66)
# Obs.	756		1129			1885

Table A4: Probit Regression Results

Dep. var. ($Pr(D_j = 1)$)	Coefficient	Standard error
HH_2MB_2010	-0.0034	(-1.68)
HH_6MB_2010	-0.0163***	(-6.98)
HH_16MB_2010	0.0054**	(3.08)
GR_RENT	0.7224	(0.64)
GR_1MB	-0.0152	(-1.90)
HH	0.0025	(0.96)
WORKING_AGE	0.0686***	(4.08)
FEMALE	-0.0221	(-0.91)
POP_DENS	-0.0008***	(-4.13)
TYPE_1	0.8892	(1.14)
TYPE_2	1.1595***	(5.26)
TYPE_3	0.9295***	(6.79)
TYPE_4	0.6179***	(7.81)
TYPE_5	0.0000	(.)
FIRM_MED	0.0015	(0.12)
FIRM_BIG	0.0114	(0.21)
GVA_SEC	0.1517**	(3.16)
GVA_TER	0.1481**	(3.08)
CSU	0.0558	(0.32)
SPD	-0.2006	(-0.68)
ACC_MOTOR	0.0110**	(2.89)
ACC_CITIES	0.0028	(1.02)
Constant	-17.1135***	(-3.34)
# Obs.		1,885
Pseudo R^2		0.158
p -value (Prob > χ^2)		0.000

Table A5: Static and Dynamic DiD Models with Controls Based on Matched and Weighted Sample

Dep. var.:	(1) HH_2MB	(2) HH_2MB	(3) HH_6MB	(4) HH_6MB	(5) HH_16MB	(6) HH_16MB
ATE	16.7563 ^{***} (22.04)		23.2355 ^{***} (25.71)		20.2103 ^{***} (19.88)	
ATE 1 year post		13.6111 ^{***} (18.56)		18.7469 ^{***} (21.36)		15.4308 ^{***} (14.48)
ATE 2 year post		17.2047 ^{***} (20.79)		23.5227 ^{***} (23.33)		19.9361 ^{***} (17.25)
ATE 3 year post		19.3537 ^{***} (21.05)		27.2138 ^{***} (24.16)		25.2467 ^{***} (19.24)
WORKING_AGE	4.3977 ^{***} (7.24)	3.7082 ^{***} (5.81)	4.6779 ^{***} (6.72)	3.3789 ^{***} (4.71)	3.4330 ^{***} (5.05)	2.7544 ^{***} (3.91)
FEMALE	0.5471 (0.77)	0.8411 (1.18)	0.6550 (0.83)	1.1871 (1.50)	0.6518 (0.89)	1.1986 (1.63)
POP_DENS	-0.0799 ^{**} (-2.08)	-0.0883 ^{**} (-2.16)	-0.0914 ^{**} (-1.98)	-0.1100 ^{**} (-2.16)	-0.0458 (-1.07)	-0.0475 (-1.09)
FIRM_MED	2.3316 ^{***} (5.46)	1.6017 ^{***} (3.32)	3.0448 ^{***} (6.00)	1.6284 ^{***} (2.89)	2.4527 ^{***} (4.92)	1.8246 ^{***} (3.30)
FIRM_BIG	6.9912 ^{***} (4.13)	5.4900 ^{***} (3.06)	8.9559 ^{***} (4.46)	5.9642 ^{***} (2.82)	8.0731 ^{***} (3.97)	6.8768 ^{***} (3.19)
L.GVA_SEC	-4.2449 ^{***} (-2.87)	-3.0847 ^{**} (-2.00)	-7.5940 ^{***} (-4.35)	-5.1696 ^{***} (-2.84)	-5.5065 ^{***} (-3.14)	-5.7733 ^{***} (-3.17)
L.GVA_TER	-5.0014 ^{***} (-3.35)	-3.8309 ^{**} (-2.47)	-8.6717 ^{***} (-4.92)	-6.2355 ^{***} (-3.39)	-6.8736 ^{***} (-3.87)	-7.0870 ^{***} (-3.84)
CSU	-1.9837 (-0.35)	-0.6329 (-0.11)	-6.7633 (-1.05)	-4.4937 (-0.71)	-8.8715 (-1.36)	-6.0305 (-0.95)
SPD	8.2660 (1.27)	15.2402 ^{**} (2.25)	0.1855 (0.02)	12.5843 (1.40)	0.3622 (0.03)	16.0559 (1.52)
ACC_MOTOR	-0.1248 (-0.41)	-0.0507 (-0.18)	-0.2615 (-0.54)	-0.0983 (-0.21)	0.0641 (0.13)	0.0918 (0.19)
ACC_CITIES	-0.0339 (-0.86)	0.0180 (0.43)	-0.0746 (-1.32)	0.0283 (0.46)	-0.0594 (-0.97)	-0.0174 (-0.26)
Post treatment period	-3.5423 ^{***} (-6.44)		-4.1776 ^{***} (-6.48)		1.8005 ^{**} (2.26)	
YEAR 2012		-1.6398 ^{***} (-3.88)		-1.7626 ^{***} (-3.83)		5.0874 ^{***} (6.78)
YEAR 2013		-3.0763 ^{***} (-3.27)		-2.3610 ^{**} (-2.12)		-0.3720 (-0.31)
YEAR 2014		-1.4917 (-1.50)		0.1637 (0.14)		4.5042 ^{***} (3.42)
Constant	194.3866 (1.14)	120.6963 (0.70)	495.0074 ^{**} (2.53)	335.4202 [*] (1.69)	367.5307 [*] (1.89)	414.2523 ^{**} (2.10)
# Obs.	8,120	8,120	8,120	8,120	8,120	8,120
R ² within	0.2673	0.3370	0.2949	0.3957	0.3151	0.4170
F-Test	88.44	74.24	121.90	102.86	150.81	130.06

t-Statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the municipality level. All regressions include municipality fixed effects. The share of GVA in the secondary and tertiary sectors was lagged (L.GVA) to avoid reverse causality.

Table A6: Immediate and Long-Term Treatment Effects for PSM

Dep. var.:	(2NB)			(3NB)		
	HH_2MB	HH_6MB	HH_16MB	HH_2MB	HH_6MB	HH_16MB
ATE 2012	12.19*** (9.58)	18.29*** (14.65)	18.16*** (14.37)	12.56*** (11.35)	18.30*** (16.37)	18.26*** (16.01)
ATT 2012	15.91*** (8.29)	22.60*** (12.73)	21.77*** (12.88)	16.56*** (10.14)	22.70*** (14.68)	21.79*** (14.73)
ATE 2013	12.96*** (11.43)	21.08*** (17.02)	20.07*** (15.38)	13.47*** (13.30)	21.45*** (19.42)	20.52*** (17.55)
ATT 2013	16.92*** (9.88)	26.13*** (14.76)	23.37*** (13.26)	17.58*** (11.74)	26.50*** (17.25)	23.76*** (15.52)
ATE 2014	13.24*** (13.76)	22.37*** (19.94)	23.54*** (17.81)	13.86*** (13.76)	22.82*** (19.76)	23.86*** (17.94)
ATT 2014	17.11*** (12.04)	27.70*** (18.07)	27.37*** (16.44)	18.01*** (12.02)	28.28*** (17.62)	27.85*** (16.31)
# Obs.	1,833	1,833	1,833	1,833	1,833	1,833

t-Statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the municipality level and robust to heteroscedasticity.

Table A7: Second Stage Full IV/2SLS Model with Matched and Weighted Sample

Dep. var.:	(1)	(2)	(3)
	EMPL_RES	EMPL_RES	EMPL_RES
HH_2MB	0.0086^{***} (3.15)		
HH_6MB		0.0062^{***} (3.15)	
HH_16MB			0.0071^{***} (3.13)
WORKING_AGE	-0.0670 ^{**} (-2.09)	-0.0569 [*] (-1.82)	-0.0538 [*] (-1.73)
FEMALE	0.1012 ^{**} (2.20)	0.1007 ^{**} (2.17)	0.1003 ^{**} (2.15)
POP_DENS	-0.0226 ^{***} (-3.40)	-0.0227 ^{***} (-3.40)	-0.0231 ^{***} (-3.44)
FIRM_MED	-0.0245 (-1.01)	-0.0223 (-0.92)	-0.0217 (-0.89)
FIRM_BIG	0.1330 [*] (1.82)	0.1394 [*] (1.92)	0.1376 [*] (1.88)
L.GVA_SEC	0.1699 ^{**} (2.01)	0.1782 ^{**} (2.11)	0.1805 ^{**} (2.13)
L.GVA_TER	0.1690 ^{**} (1.97)	0.1781 ^{**} (2.08)	0.1815 ^{**} (2.11)
CSU	0.3580 (0.75)	0.3813 (0.81)	0.3967 (0.84)
SPD	-0.4596 (-0.97)	-0.4095 (-0.85)	-0.4480 (-0.92)
ACC_MOTOR	-0.0032 (-0.24)	-0.0029 (-0.23)	-0.0045 (-0.35)
ACC_CITIES	0.0041 ^{**} (1.99)	0.0042 ^{**} (2.01)	0.0043 ^{**} (2.08)
EDUC	-0.0014 (-0.25)	-0.0011 (-0.20)	-0.0010 (-0.17)
# Obs.	8,120	8,120	8,120
R ²	0.702	0.702	0.697
F	634.48	634.27	626.20
F-Test of excl. instr.	755.81	1041.29	633.63
DWH	0.0103	0.0047	0.0011

t-Statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the municipality level. All regressions include fixed effects for year, municipality and controls of equation (1) as well as education (EDUC). The share of GVA in the secondary and tertiary sectors was lagged (L.GVA) to avoid reverse causality.

Table A8: Second Stage OLS Estimates for Self-Employment and Gross Wages

Dep. var.: Sample:	(1) SELF Matched & Weighted	(2) SELF Matched & Weighted	(3) SELF Matched & Weighted	(4) WAGE Matched & Weighted	(5) WAGE Matched & Weighted	(6) WAGE Matched & Weighted
HH_2MB	0.0003** (2.22)			1.3524** (2.52)		
HH_6MB		0.0002* (1.91)			1.0131** (2.33)	
HH_16MB			0.0001 (0.90)			0.8739** (2.16)
WORKING_AGE	-0.0088* (-1.88)	-0.0085* (-1.81)	-0.0081* (-1.73)	-36.1891* (-1.78)	-34.6957* (-1.70)	-33.5371 (-1.65)
FEMALE	0.0126** (2.57)	0.0126** (2.57)	0.0127*** (2.59)	35.0609 (1.31)	34.9465 (1.31)	35.1765 (1.32)
POP_DENS	-0.0033*** (-3.37)	-0.0033*** (-3.37)	-0.0033*** (-3.37)	1.5518 (0.58)	1.5495 (0.58)	1.4703 (0.56)
FIRM_MED	0.0075* (1.72)	0.0075* (1.73)	0.0074* (1.71)	1.8661 (0.14)	2.2388 (0.17)	2.0960 (0.16)
FIRM_BIG	0.0229** (2.01)	0.0231** (2.03)	0.0230** (2.02)	-24.9602 (-0.50)	-23.9392 (-0.48)	-24.2326 (-0.49)
L.GVA_SEC	0.0238* (1.94)	0.0240* (1.96)	0.0240* (1.96)	-133.3653*** (-2.70)	-132.0566*** (-2.68)	-131.8256*** (-2.67)
L.GVA_TER	0.0247** (1.98)	0.0250** (2.00)	0.0250** (2.00)	-143.9100*** (-2.82)	-142.4625*** (-2.79)	-142.1500*** (-2.79)
CSU	-0.0476 (-1.15)	-0.0469 (-1.14)	-0.0474 (-1.15)	204.9707 (0.62)	208.8540 (0.63)	209.3233 (0.63)
SPD	-0.1385** (-2.07)	-0.1370** (-2.05)	-0.1353** (-2.03)	491.4961 (1.00)	498.7508 (1.01)	498.5980 (1.01)
ACC_MOTOR	-0.0045** (-2.37)	-0.0045** (-2.36)	-0.0045** (-2.35)	3.1267 (0.45)	3.1553 (0.45)	3.0204 (0.44)
ACC_CITIES	-0.0014*** (-3.01)	-0.0014*** (-3.02)	-0.0014*** (-2.98)	-2.8229** (-2.10)	-2.8252** (-2.08)	-2.7786** (-2.04)
EDUC	-0.0011 (-1.57)	-0.0011 (-1.55)	-0.0011 (-1.47)	-4.7151 (-1.61)	-4.6833 (-1.60)	-4.5510 (-1.55)
Constant	1.3255 (1.03)	1.2869 (0.99)	1.2662 (0.98)	39044.33*** (7.41)	38849.2784*** (7.38)	38773.618*** (7.36)
# Obs.	8,120	8,120	8,120	8,120	8,120	8,120
R ²	0.431	0.431	0.430	0.402	0.401	0.401
F	201.83	202.41	202.79	155.27	156.03	155.25

t-Statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the municipality level and robust to heteroscedasticity. The share of GVA in the secondary and tertiary sectors was lagged (L.GVA) to avoid reverse causality.

Table A9: Second Stage OLS Estimates for Employees at Place of Residence and Place of Work

Dep. var.:	(1) EMPL_ RES	(2) EMPL_ RES	(3) EMPL_ RES	(4) EMPL_ WORK	(5) EMPL_ WORK	(6) EMPL_ WORK
Sample:	Matched & Weighted	Matched & Weighted	Matched & Weighted	Matched & Weighted	Matched & Weighted	Matched & Weighted
HH_2MB	0.0020** (2.18)			-0.0055** (-2.29)		
HH_6MB		0.0010 (1.37)			-0.0045** (-2.26)	
HH_16MB			0.0000 (0.01)			-0.0040** (-1.99)
WORKING_AGE	-0.0443 (-1.29)	-0.0405 (-1.17)	-0.0375 (-1.08)	-0.3002*** (-3.29)	-0.3051*** (-3.35)	-0.3100*** (-3.41)
FEMALE	0.1062** (1.99)	0.1066** (1.99)	0.1077** (2.01)	0.1109 (0.80)	0.1117 (0.80)	0.1108 (0.80)
POP_DENS	-0.0231*** (-3.03)	-0.0232*** (-3.02)	-0.0233*** (-3.02)	-0.0114 (-0.86)	-0.0114 (-0.86)	-0.0110 (-0.83)
FIRM_MED	-0.0272 (-0.96)	-0.0270 (-0.96)	-0.0278 (-0.99)	0.0554 (0.59)	0.0535 (0.57)	0.0538 (0.57)
FIRM_BIG	0.1363 (1.57)	0.1376 (1.59)	0.1373 (1.59)	0.4546* (1.88)	0.4503* (1.86)	0.4516* (1.86)
L.GVA_SEC	0.1745* (1.81)	0.1762* (1.83)	0.1759* (1.82)	0.1102 (0.41)	0.1046 (0.39)	0.1034 (0.39)
L.GVA_TER	0.1729* (1.76)	0.1748* (1.78)	0.1742* (1.77)	0.1068 (0.40)	0.1005 (0.38)	0.0989 (0.37)
CSU	0.3491 (0.68)	0.3517 (0.69)	0.3463 (0.68)	-1.0773 (-0.87)	-1.0950 (-0.89)	-1.0976 (-0.89)
SPD	-0.3333 (-0.68)	-0.3132 (-0.63)	-0.2956 (-0.60)	-0.0223 (-0.01)	-0.0450 (-0.03)	-0.0423 (-0.03)
ACC_MOTOR	-0.0020 (-0.13)	-0.0019 (-0.12)	-0.0017 (-0.11)	0.0855* (1.84)	0.0854* (1.84)	0.0860* (1.86)
ACC_CITIES	0.0046* (1.87)	0.0047* (1.90)	0.0047* (1.93)	-0.0017 (-0.24)	-0.0017 (-0.23)	-0.0019 (-0.26)
EDUC	0.0020 (0.26)	0.0022 (0.29)	0.0026 (0.35)	0.0197 (0.87)	0.0201 (0.88)	0.0201 (0.87)
Constant	20.6965** (2.01)	20.3369** (1.97)	20.2021* (1.96)	23.5553 (0.77)	24.3033 (0.80)	24.6329 (0.81)
# Obs.	8,120	8,120	8,120	8,120	8,120	8,120
R ²	0.706	0.706	0.706	0.157	0.157	0.157
F	404.11	402.40	400.14	38.77	38.80	38.83

t-Statistics in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the municipality level and robust to heteroscedasticity. The share of GVA in the secondary and tertiary sectors was lagged (L.GVA) to avoid reverse causality.

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