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Do digital information technologies help unemployed job seekers find a job? Evidence from the broadband internet expansion in Germany



Nicole Gürtzgen^{a,b,*}, André Diegmann (né Nolte)^c, Laura Pohlan^d, Gerard J. van den Berg^e

^a IAB, University of Regensburg, ZEW, Germany

^b IAB Regensburger Str. 104, Nuremberg D-90478, Germany

^c IWH, ZEW, Germany

^d IAB, ZEW, Germany

^e University of Groningen, University Medical Center Groningen, IFAU, IZA, ZEW, CEPR and CESifo, Netherlands

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ABSTRACT

This paper studies effects of the introduction of a new digital mass medium on reemployment of unemployed job seekers. We combine data on broadband internet availability at the local level with German individual register data. We address endogeneity by exploiting technological peculiarities that affected the roll-out of broadband internet. Results show that broadband internet improves reemployment rates after the first months in unemployment for males. Complementary analyses with survey data suggest that internet access mainly changes male job seekers' search behavior by increasing online search and the number of job applications.

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1. Introduction

The emergence of the internet as a mass medium has led to a dramatic decline in the cost of acquiring and disseminating information. During the last two decades, this has brought about a significant reduction in all kinds of information frictions, such as in the areas of elections as well as insurance, goods, housing and labor markets. Against this background, there has been a surge of empirical studies dealing with the internet's impact on outcomes such as product market performance (Brynjolfsson and Smith, 2000, Brown and Goolsbee, 2002), voting behavior (Falck et al., 2014) and crime (Bhuller et al., 2013) amongst others. In the context of labor markets, one of the major features that are likely to be affected by the internet is the way how workers and employers search for each other and eventually form a match (Autor, 2001).

* Corresponding author at: IAB, University of Regensburg, ZEW, Germany.
E-mail address: nicole.guertzen@iab.de (N. Gürtzgen).

The goal of this study is to identify the effect of the emergence of the internet on job search outcomes in the German labor market. Germany provides an interesting case, as - even though access to the internet has been improving considerably over the recent decade - there is still substantial regional variation in households' access to high-speed internet. Closing the last remaining gaps in internet coverage especially in Germany's rural areas is therefore currently considered a major policy goal. Against this background, our study shall help to improve our understanding of whether and to what extent the spread of the internet may have facilitated job search among unemployed job seekers. To investigate the impact of the emergence of high-speed internet on job search outcomes, we explore the effect of the introduction of the digital subscriber line (DSL) technology on reemployment probabilities of unemployed job seekers. To do so, we will exploit variation in DSL availability at the regional level in Germany in order to quantify the net effect of an increase in regional internet availability on the fraction of unemployed individuals who experience a transition into employment.

In exploring the impact of the internet expansion on search outcomes, our study contributes to the (still small) literature that concentrates on different job search channels - especially searching via the internet - and their impact on labor market outcomes. [Kuhn and Skuterud \(2004\)](#) were the first to exploit individual variation in internet usage and to evaluate the impact of online job search on unemployment durations for the years 1998–2000 based on the Current Population Survey (CPS). Their results suggest that after controlling for observables, unemployed workers searching online do not become reemployed more quickly than their offline job-seeking counterparts. This leads the authors to conclude that either internet job search does not reduce unemployment durations or that workers who look for jobs online are negatively selected on unobservables. Based on the same data set, [Fountain \(2005\)](#) performs logistic regressions with a job finding indicator as the dependent variable. Her results provide evidence of a small internet advantage compared to offline job search in 1998. Moreover, she finds that internet searching advantages had disappeared by 2000. [Kuhn and Mansour \(2014\)](#) replicate the analysis by [Kuhn and Skuterud \(2004\)](#) combining information from the CPS with the National Longitudinal Survey of Youth (NLSY). Comparing the relationship between internet usage and unemployment durations in 1998/2000 and 2008/2009, the authors find that while internet usage was ineffective one decade ago, it was associated with a reduction in the duration of unemployment of about 25% in 2008/2009. Using the German Socio-Economic Panel (GSOEP), [Thomsen and Wittich \(2010\)](#) explore the effectiveness of various job search channels for the job finding probability among unemployed job seekers in Germany. The authors find that internet usage does not raise the reemployment probabilities for this group of job seekers. Finally, [Bhuller et al. \(2020\)](#) show that the expansion of broadband internet increased overall job finding rates in Norway.

By presenting new evidence on the internet's impact on search outcomes for Germany, our study makes several contributions to this literature: First, our empirical approach explicitly accounts for the endogeneity of job search channels. Finding exogenous variation in the availability and use of the internet is a key challenge, as individuals - as well as employers - are likely to self-select into different search channels. Moreover, when looking at regional variation in internet availability, regions with high-speed internet access are likely to differ from those with low-speed internet access along many dimensions. While much of the literature is not able to deal with these issues, our analysis exploits exogenous variation in the availability of high-speed internet access at the German municipality level. The source of this variation, as put forward by [Falck et al. \(2014\)](#), stems from technological restrictions in the roll-out of the first generation of DSL in the early 2000s in Germany. We concentrate on DSL availability as this is the dominant broadband technology in Germany. More specifically, the variation was caused by technological peculiarities of the traditional public switched telephone network (PSTN), through which the early generations of DSL had been implemented. As described by [Falck et al. \(2014\)](#), almost one-third of West German municipalities could not readily employ the new technology as early DSL availability relied on the copper wires between the household and the main distribution frame (MDF) of the regional PSTN. The crucial issue causing exogenous variation in DSL availability is that, while the length of the copper wires connecting households and MDFs - whose distribution was determined in the 1960s - did not matter for telephone services, it strongly affected the DSL connection. In particular, there exists a critical value of 4200m, with municipalities further away than this threshold from the MDF having no access to DSL. The only way to provide internet access was to replace copper wires with fiber wires, which took time and was costly. This exogenous variation in internet availability during the early DSL years allows us to use each municipality's distance to the next MDF as an instrument for DSL availability. This enables us to identify an intention to treatment effect (ITT) of an expansion in internet availability on the reemployment prospects of unemployed individuals for less agglomerated municipalities in West Germany.

A second contribution is that we explicitly aim at identifying the internet's effect on reemployment probabilities that arise from a reduction in search frictions rather than from an increase in labor demand. Thus, we will quantify the importance of demand-side factors that might also improve unemployed job seekers' employment prospects, without necessarily reducing search frictions.

Third, our analysis relies on administrative data sources. In particular, we use German register data, the universe of the Integrated Employment Biographies (IEB) of the Federal Employment Agency. The data provide an ideal basis for estimating the internet's impact on individual unemployment durations for several reasons: First, the data permit us to precisely measure the duration of different labor market states and transitions between them, most notably transitions between unemployment and employment. Second, due to their administrative nature, the IEB are less prone to panel attrition than comparable information from survey data. This is especially relevant as panel attrition has been recognized to give rise to biased estimates of the rates at which unemployed individuals become employed ([Van den Berg et al., 1994](#)). An additional advantage over survey data is the considerably larger number of observations. The latter allows us to construct an inflow

sample into unemployment, thereby avoiding the typical length bias that may arise in stock samples of unemployment durations.

Based on this empirical strategy, we document the following key results. Our IV estimates point to positive, but imprecisely estimated effects of the DSL expansion on the reemployment prospects of unemployed individuals for the pooled sample. Breaking down the analysis by socio-economic characteristics suggests that the internet's positive effect is particularly pronounced for males after about a quarter in unemployment. In terms of magnitude, assigning an individual from an "unlucky" municipality (i.e., one that could not readily be supplied with high-speed internet) to a "lucky" counterpart increases the reemployment probability for males by about 4% points.

To substantiate our findings we seek to provide more direct evidence on the relationship between an expansion in internet availability and job seekers' search behavior. Specifically, we investigate job search strategies at the individual level using survey data from the Panel Study on Labour Markets and Social Security (PASS). We address first-stage effects by looking at whether the availability of internet at home increases the incidence of online job search, i.e. the use of the internet as a job search channel. The results show that home internet access significantly increases online job search activities for all subgroups except for young job seekers. Our findings further indicate that males are overrepresented among the compliers, i.e. those who are induced to have home internet access because the household is located close to the next MDF, and that the predicted effect of home internet access on online job search among compliers is largest for this subgroup. At the same time, crowding out effects on offline job search channels play no major role for males. Moreover, male job seekers significantly increase the number of own-initiative applications. This suggests that the internet expansion led to better reemployment prospects especially for male job seekers by increasing overall search intensity.

Additional analyses at the municipality level support the view that the documented DSL effects on reemployment prospects are likely to be driven by changes in job seekers' search behavior. In particular, by analyzing demand-side effects we rule out that the main effects are driven by labor demand channels which may arise from, e.g., firms increasing online sales in response to the internet expansion. This is consistent with the idea that internet access constraints prior to the internet expansion were more binding for individuals than for employers. This is also consistent with the facts that the average municipality in our sample is small and that the vast majority of unemployed job seekers become reemployed in a municipality different from their home municipality. This implies that the reemployment prospects of unemployed job seekers are unlikely to be driven by broadband induced demand effects.¹ We further show that the broadband internet expansion leads to a higher inflow into unemployment, while at the same time leaving unemployment rates unchanged. Along with constant firm size, the increase in the unemployment inflow is consistent with a higher labor turnover due to lower labor adjustment costs. The composition of the inflow sample, however, remains constant, such that our documented DSL effects are unlikely to reflect a different composition of the unemployed. Finally, we also show that our results are not driven by selective mobility patterns or by potential agglomeration effects in municipalities that could be readily endowed with broadband access.

The remainder of the paper is structured as follows. The next section provides descriptive evidence for the diffusion of broadband internet at the individual and employer level and its importance for job search and recruiting behavior. Section 3 presents some theoretical considerations of how online job search may be expected to affect reemployment probabilities. While Section 4 deals with the sources of empirical identification, Section 5 lays out the overall empirical strategy. The data sources and the sample selection are described in Section 6. Section 7 shows descriptive statistics. Section 8 presents the empirical results, while Section 9 provides further empirical evidence on potential mechanisms underlying individuals' job search behavior. The final Section 10 concludes.

2. Broadband internet, online job search and recruiting

Broadband internet diffusion. The diffusion of high-speed internet in Germany started during the years 2000/01 and was based entirely on digital subscriber line technologies (DSL). The fraction of non-DSL broadband technologies such as hybrid fiber coax (HFC) cable or satellite was relatively low at 8% (Bundesnetzagentur, 2012). Prior to the introduction of broadband internet, internet access was only feasible via low-speed technologies such as modems or integrated services digital network (ISDN). DSL provides an access speed that is at least 6-times faster than the old technologies. The fraction of individuals using the internet increased within five years from about 37% at the beginning of the new century to 55% in 2005.

¹ Our study is related to the literature on the aggregate effects of the broadband internet expansion on regional labor market performance. Looking at city-level unemployment rates, Kroft and Pope (2014) exploit geographic and temporal variation in the availability of online search induced by the expansion of the U.S. website Craigslist. They do not find effects on local city-level unemployment rates. In a similar vein, Czernich (2014) points to absence of internet effects on regional unemployment rates in Germany. The author exploits regional variation in broadband internet availability and addresses the endogeneity of internet availability using a similar identification approach as in our study. However, her study is confined to unemployment stocks in the years 2002 and 2006 and does not take into account inflows and outflows into unemployment. Finally, a large body of empirical research analyzes the link between broadband internet and employment as well as economic growth. Examples include the studies by Crandall et al. (2007), Kolkko (2012) and Whitacre et al. (2014), who generally document a positive association between the expansion of broadband infrastructure and employment growth. Using municipality data from Germany, Fabritz (2013) finds a moderate positive association between broadband availability and employment. Exploiting regional variation in broadband availability in Norway, the study by Akerman et al. (2015) provides evidence for skill-biased labor demand effects of broadband internet.

Online job search and recruiting tools. The most important online job search and recruiting tools include (1) online job boards, which provide websites including searchable databases for job advertisements; (2) job postings on the companies' websites which may (but do not necessarily) solicit online applications as well as (3) networks such as LinkedIn or Xing permitting online search on behalf of employers or headhunters targeting suitable candidates via their online CVs. Online job boards in Germany are typically divided into private job boards such as Monster and StepStone and public job boards, such as that from the Federal Employment Agency. In terms of market shares, the Federal Employment Agency's job board was the most important one, with 325,000 jobs posted in February 2005, followed by JobScout24 and Monster with about 20,000 jobs (Grund, 2006).

Other than market shares, the efficiency of the (job board) technology is rather difficult to measure. In December 2003, the Federal Employment Agency implemented a new online job board, by aggregating 25 different single systems (*BA-Einzel-Börsen*) into one single portal, the "*Jobbörse*" (Bieber et al., 2005). By incorporating profile matching, this new system was explicitly designed to increase the efficiency of the match between job seekers and employers.² Still, there is some evidence that customers did not quickly adapt to the newly established *Jobbörse*, which may reflect initial limitations of its user-friendliness. As described by Bieber et al. (2005), this may have been due to fact that the new job board was too complex for a broad customer segment, in particular for simple jobs and tasks, such as cleaning staff or other low-wage occupations. Overall, these considerations point to a quite limited usability of the *Jobbörse* at the start of the DSL period.

Online search among employers. While online recruiting among employers was already widespread in the mid 2000s in Germany, its importance has continued to increase during the last decade.³ Recent evidence from the IAB Job Vacancy Survey (Brenzel et al., 2016) shows that in 2015, over 50% of all completed hires were preceded by job postings on the companies' websites and 41% by advertisements on online job boards.

The study also suggests that online recruiting channels and their success rates appear to play a larger role for high-skilled than medium- and low-skilled jobs. The question which jobs are posted online is important when assessing the internet's effectiveness in helping unemployed job seekers find a job. Clearly, the intensity with which employers use the internet for recruiting purposes is an important prerequisite for the internet's ability in improving job finding prospects. Based on the IAB Job Vacancy Survey⁴, Panel (A) of Fig. A.1 in Online Appendix A shows the overall fraction of jobs being posted online among all successful hirings. Panels (B) and (C) show the respective shares broken down by selected occupational categories. The graphs are shown for the years 2005–2008, which in most studies are considered to be the DSL period in Germany. Two noteworthy facts emerge from these graphs: First, the fraction of jobs posted online increased by about 15% points from 2005 to 2008 (Fig. A.1 Panel (A)). Second, the fraction of jobs being posted online is larger and increases over time for more skilled white-collar occupations (Fig. A.1 Panel (B)) as compared to less skilled or blue-collar occupations (Fig. A.1 Panel (C)).⁵

Online search among job seekers. According to a survey among individual job seekers, the share of individuals preferring online over print applications rose from 48% to 88% between 2003 and 2014 (Weitzel et al., 2015). Using data from the German Socio-Economic Panel (GSOEP), Grund (2006) and Thomsen and Wittich (2010) show that the incidence of online job search among unemployed job seekers is higher for younger and better qualified individuals. To date, there is few evidence as to what extent an expansion in internet availability has translated into an increase in online job search and has given rise to crowding out effects of other job search channels. Against this background, we will complement the empirical evidence by own empirical analyses based on the PASS survey data in Section 9.

3. Theoretical considerations

In job search theories, informational frictions (or search frictions) reflect the fact that it takes time and/or effort to find a suitable partner in the labor market (Mortensen, 1986). The process with which workers and firms meet is often described by a meeting function m which is a function of the measure of unemployed workers u in the market and the measure of vacancies v . At a given point in time with given u and v , the rate at which meetings occur in the market is then $m(u, v)$. Accordingly, a randomly chosen unemployed individual faces a meeting rate equal to $m(u, v)/u$. The function m can be parameterized as $m(u, v) = \alpha u^\beta v^\gamma$, with $\alpha, \beta, \gamma > 0$ and with the special case of $\beta + \gamma = 1$ leading to a constant returns

² Related to that, Belot et al. (2018) provide experimental evidence on the effects of online advice to job seekers by suggesting relevant occupations. Their results point to a larger number of job interviews, which may provide some evidence in favor of an improvement in the technology to match job seekers and employers.

³ At the employer level, evidence based on firm-level survey data indicates that about 94% of all firms already had access to the internet in 2002. In 2007 the fraction increased to 98%, of whom 93% had high-speed internet access, with 86% having access via DSL or dedicated lines (ZEW ICT-Survey, 2007). Overall, the diffusion of high-speed internet in Germany in the early years of the 2000s suggests that any restriction in internet access was likely to be more binding for individual job seekers than for employers.

⁴ The IAB Job Vacancy Survey is based on a repeated annual cross-section of German establishments, whose sampling frame encompasses all German establishments that employ at least one employee paying social security contributions. Apart from information on various establishment attributes, such as size, industry and regional affiliation, the surveyed establishments are asked to report information on their most recent (randomly determined) hiring process. In particular, the data contain information on employers' adopted search channels relating to the most recent hiring, such as social networks, newspaper ads, private and public employment agencies and most notably the use of companies' websites and online job boards.

⁵ Skilled white-collar occupations include managers, technicians, professionals and clerical support workers, whereas less skilled or blue-collar occupations include service and craft workers, plant and machine operators as well as agricultural jobs.

to scale function. If the flow of meetings increases for given u and v then this reflects that the meeting technology has improved. Hence, the parameter α is said to represent the meeting technology. In our context, the advent of the internet can be represented by an increase of α . Practical examples are that job boards facilitate the search for keywords and that they provide more information than comparable newspaper print advertisements. Furthermore, because job offers can be published on the internet without major time delays, they are also more up-to-date than comparable print offers.

If employers accept unemployed applicants upon meeting them then the meeting rate $m(u, v)/u$ (i.e., $\alpha u^{\beta-1} v^\gamma$) for an unemployed individual is equal to his or her job offer arrival rate which may be denoted by λ . In reality, unemployed job seekers have some leeway in influencing this quantity, by setting their job search effort level. This level is chosen with an eye on search costs. As a result, the parameter α may also depend on the unit search costs. The internet, if it reduces the flow of search costs, leads to an increase in search effort, and hence to an increase of α . Indeed, job boards allow for dissemination at a considerably lower cost than print advertisements. Also, application costs when applying on the internet may be much lower than the costs involved in sending a letter.

If unemployed individuals accept every vacancy they encounter then the probability of moving to employment within t units of time equals, in continuous time, $1 - \exp(-\lambda t) = 1 - \exp(-\alpha u^{\beta-1} v^\gamma t)$ which is increasing in α . Of course, in real life, individuals are selective with respect to job opportunities. Also, a market-wide increase of α may trigger equilibrium effects. In Online Appendix B we show that such generalizations are unlikely to lead to a reversal of the positive effect on reemployment probabilities.

Some other model extensions shed light on the timing of the effect. A small literature distinguishes between formal and informal search channels, which, for the moment, can be thought of as proxies for online versus other search methods. [Van den Berg and Van der Klaauw \(2006\)](#) build a model with two channels and derive relatively mild conditions under which an increase in the rate at which one of the channels generates offers (or a decrease in its cost of usage) increases the optimal usage of that channel and subsequently raises the reemployment rate. As argued in the literature (see, e.g., [Van den Berg and Van der Klaauw, 2019](#)), informal search channels are most important in the early stages of the unemployment spell. Effort along informal channels is cheap. However, the number of friends and relatives is finite, and most friends can only be meaningfully asked a limited number of times whether they know about possible vacancies. Thus, at some point the informal search channel dries up. Newly unemployed workers may therefore first exhaust their informal channel before turning to formal channels. Formal channels remain available because of the arrival of new vacancies over time. Indeed, because of the stock-flow sampling phenomenon, the quality of newly arriving vacancies may exceed the quality of vacancies available online at the moment of entry into unemployment. In sum, the effect of high-speed internet may be increasing with the elapsed duration of unemployment, at least until more drastic disadvantages of long-term unemployment such as stigmatization kick in.

An alternative explanation for a smaller effect at the onset of the unemployment spell concerns the existence of recall options to return to the previous employer. Such options usually disappear after some months in unemployment. Exercising a recall option does not require search effort, and hence fast internet is unlikely to affect this type of reemployment early. We investigate the importance of this in the empirical analysis.

4. Identification

Identifying the effects of internet availability on labor market outcomes suffers from several endogeneity issues. Regions (in our case: municipalities) with high-speed internet access are different compared to regions with lower speed. By simply comparing e.g. unemployed job seekers' reemployment propensities across municipalities with different high-speed internet levels, one would not be able to estimate the true causal effect. As a result, a simple regression of labor market outcomes on DSL availability at the municipality level would potentially be biased. The same is true when controlling for (municipality) observables, since the expansion of broadband internet might still be correlated with time-variant unobservables (see below).

To overcome potential endogeneity biases, we will make use of regional peculiarities of the West German traditional public switched telephone network (PSTN), which determined the capacity to provide DSL in certain municipalities. As described in [Falck et al. \(2014\)](#) and [Steinmetz and Elias \(1979\)](#), early DSL roll-out made use of the copper wires between households and the main distribution frames (MDFs). The distribution of MDFs was originally determined in the 1960s with the overall purpose to provide telephone services in West Germany. While municipalities with a high population density have at least one MDF, less agglomerated areas typically share one MDF. The reason is that hosting a MDF required the acquisition of lots and buildings. As the distance to the next MDF did not affect the quality of telephone services, the choice of MDF locations in less agglomerated areas was determined by the availability of such facilities. The crucial issue causing exogenous variation in DSL availability is that, while the length of the copper wires connecting households and the MDFs did not matter for telephone services, it strongly affected the DSL connection. In particular, there exists a critical value of 4200m, and municipalities situated beyond this distance from the MDF had no access to DSL. The only way to provide internet availability was to replace copper wires by fiber wires, which took time and was costly. These technical peculiarities provide a quasi-experimental setting for less agglomerated municipalities without an own MDF, for whom the distance to each municipality's regional centroid to the MDF can be used as an instrument for DSL availability. We exploit this quasi-experimental set-up for West German municipalities that are connected to a MDF located in another municipality

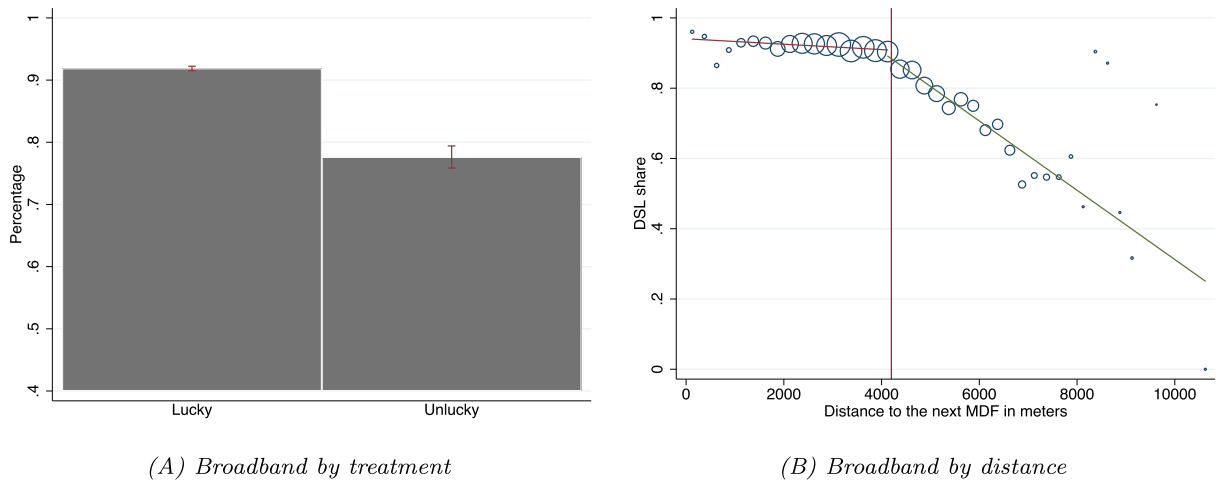


Fig. 1. Share of households with DSL availability.

Notes: The figures plot the fraction of households with broadband internet (DSL) availability for lucky and unlucky West German municipalities between 2007 and 2008. The left Panel (A) reports averages by treatment status (lucky and unlucky municipalities). 95% confidence intervals are reported at the top of each bar in Panel (A). Panel (B) plots the DSL shares against the distance to the next main distribution frame. The size of the circles in Panel (B) corresponds to the number of municipalities within 250 m bins. The figures are based on the German municipalities used in the empirical analysis.

and where no closer MDF is available.⁶ Because of the quasi-experimental setting spelled out above, we label municipalities with a distance below the threshold of 4200 m as *lucky* ones and municipalities with a distance above the threshold as *unlucky* ones.⁷ To illustrate DSL availability rates at the household level for both groups, Panel (A) of Fig. 1 plots the mean fraction of households having access to DSL from 2007 to 2008. Municipalities with relatively short distances to the next MDF (below 4200 m) exhibit a fraction of about 92% of households for whom DSL is available. The low confidence intervals at the top of the bars indicate only little variation across municipalities. Once the distance surpasses 4200 m, the share drops considerably to about 77% with a higher variation across municipalities as reflected by the higher confidence intervals.

Panel (B) plots the DSL shares against the distances to the next MDF for 250 m bins. The size of the circles corresponds to the number of municipalities. Lucky municipalities below the threshold exhibit a constant DSL share, whereas the DSL share decreases monotonically with higher distances among the unlucky municipalities. There are, however, some municipalities that exhibit a large distance to the next MDF, while simultaneously having relatively high DSL shares.⁸ Note that this might violate the exogeneity assumption. To address potential endogeneity concerns for these municipalities, we will later perform robustness checks by excluding these outliers. Moreover, we will also narrow the bandwidth around the threshold, which creates a set of municipalities that are likely to be more comparable in terms of their observables.

5. Empirical model

In our empirical analysis, we first compare changes in outcomes across municipalities i with different changes in DSL availabilities. Δ_t measures changes from the pre-DSL period to the DSL period, indexed by t .⁹ Thus, we regress the change in the outcome variable on the change of the share of households who technically have home internet access in municipality i and period t , ΔDSL_{it} , and a vector of covariates X_{it-1} measured in the pre-DSL period.

$$\Delta y_{itm} = \beta_{0m} + \beta_{1m} \cdot \Delta DSL_{it} + X'_{it-1} \cdot \beta_{2m} + MDF_i + \epsilon_{itm} \tag{1}$$

Given that DSL availability is zero in the pre-DSL period, Eq. (1) regresses the change in the outcome variable on the actual level of households with DSL availability, DSL_{it} . X_{it-1} is a vector of characteristics at the municipality level (see Table 1) and ϵ_{itm} is an idiosyncratic error term. Note that we need to control for municipality observables as DSL availability not only depends on technological restrictions, but also on the telecommunication companies' incentives to roll-out DSL. These incentives, in turn, are likely to be correlated with municipalities' socio-demographic or economic characteristics. We include pre-DSL levels of municipality characteristics rather than differences, as the latter might have been endogenous to the DSL expansion. Moreover, we introduce MDF-fixed effects (MDF_i), thus comparing two municipalities that are connected to the same MDF but differ in their distance to the MDF. Our outcomes are the *unconditional* probabilities of entering employment

⁶ Our analysis concentrates on West German municipalities because East Germany modernized the distribution frames after German unification.

⁷ Roughly one third of the municipalities used in our analysis are unlucky municipalities.

⁸ Likely reasons for the observed outliers are, for instance, special investment programs for 100 municipalities in Bavaria in 2006 ([Wissenschaftliche Dienste des Deutschen Bundestages, 2007](#)).

⁹ In our empirical analysis, each period covers two years, which will be pooled for either period.

within m months after entering unemployment, for various values of m . For a given m , this probability is quantified as the fraction of individuals entering employment within m months, among those who entered unemployment, where we actually use the Kaplan-Meier estimator to control for right-censoring of unemployment spells.¹⁰ As we estimate this equation separately by month m after the inflow into unemployment, the coefficients and the changes in the outcome variable are indexed by m as well.

The empirical model in Eq. (1) might be subject to endogeneity issues. Individuals in municipality i might acquire broadband internet in order to search for a job. Moreover, individuals' unobserved productivity attributes, such as the level of motivation and propensity to work, might be correlated with the willingness to pay for broadband internet, such that compositional changes at the regional level might also be correlated with the expansion in high-speed internet. To account for time-varying unobserved effects that are correlated with both, labor market performance and DSL availability at the municipality level, we follow an instrumental variable (IV) approach. As spelled out above, we use as an instrument the distance from each municipality's center (population-weighted) to the next MDF. The first-stage can thus be written as:

$$\Delta DSL_{it} = \gamma_0 + \gamma_1 \cdot PSTN_i + X'_{it-1} \cdot \gamma_2 + MDF_i + \Delta \psi_{it} \quad (2)$$

In the first stage, $PSTN_i$ is a dummy variable that takes on the value of 1 for unlucky (treated) municipalities.¹¹ This IV strategy identifies a local average treatment effect for the compliant municipalities.

6. Data and sample selection

Data. The data used in this study stem from different data sources. We measure high-speed internet availability by the share of households at the municipality level for whom DSL is potentially available. The original data stem from the broadband atlas (*Breitbandatlas Deutschland*) published by the Federal Ministry of Economics and Technology (2009). The telecommunication operators self-report covered households with a minimum data transfer rate of 384 kb/s. Hence, for these covered households a high-speed internet connection is technically available. The self-reported data is available for the universe of German municipalities from 2005 onwards. In this study, we use the territorial boundaries of the municipalities from the year 2008. In the literature, the DSL period is typically defined as covering the years from 2005 to 2008, whereas the pre-DSL period refers to the years 1996–1999 (Falck et al., 2014).

Even though we measure broadband availability at the household level, it might be conceivable that DSL effects capture some potential demand-side dynamics. Higher broadband internet availability might, e.g., alter the dynamics of firm entries and exits. If labor demand is affected by an increase in high-speed internet availability, unemployed individuals might experience different unemployment durations without necessarily searching online for a job. In our empirical analysis, we will also address potential demand-side effects in order to investigate whether our estimates are driven by labor demand effects rather than by changes in search behavior. Using data provided by the *Mannheim Enterprise Panel* (MUP), we retrieve information on the number of firm exits and entries at the municipality level.¹² We further include variables provided by the *Establishment History Panel* of the Federal Employment Agency. These include the total number of establishments and establishment size.

The main outcome variable in this study is a measure of unemployment duration. To measure unemployment durations and reemployment probabilities, we will use German register data, the Integrated Employment Biographies (IEB) of the Federal Employment Agency provided by the IAB (for detailed information of a sub-sample of this data set, see e.g. Oberschachtsiek et al., 2009 and Table C.2 in Online Appendix C for a description of all labor market states). This administrative data set covers the universe of all individuals who have at least one entry in their social security records from 1975 on in West Germany and starting from 1992 in East Germany. The data cover approximately 80% of the German workforce and provide longitudinal information on individual employment biographies. Self-employed workers, civil servants, and individuals doing their military service are not included in the data set. For our empirical analysis, we use the universe of unemployed individuals who experienced at least one unemployment spell in the above defined subset of municipalities during our time period of consideration (1996–2008).¹³

The data provide daily information on employment records subject to social security contributions, unemployment records with transfer receipt as well as periods of job search. This permits us to precisely measure the duration of different labor market states and transitions between them, most notably transitions between unemployment and employment. The data do not allow for a distinction between voluntary and involuntary unemployment, though. We therefore follow Lee and Wilke (2009) and define involuntary unemployment as periods of registered job search and/or transfer receipt without a parallel employment relationship. Further information on the definition of un- and non-employment can be found in Online Appendix C. As the IEB are based on employers' notifications to the social security authorities, they are less prone

¹⁰ See Section 6 for a precise definition of this variable.

¹¹ In a robustness check, we also use the distance as a continuous measure instead of a dummy variable as an instrument.

¹² The data set covers the universe of firms in Germany including a municipality identifier. The earliest available representative year is 2000. Thus, we use the year 2000 as the pre-DSL year.

¹³ When constructing the outcome variables as well as some control variables, we exploit the universe of individuals who experienced at least one unemployment spell in the above defined subset of municipalities during our time period of consideration as well as a random 50%-sample of employed individuals living in the above defined subset of municipalities.

to measurement error than comparable information from survey data, like e.g. the German Socio-Economic Panel (GSOEP). Additional advantages over survey data include the much lower extent of panel attrition and most notably the possibility to construct an inflow sample, which captures also shorter unemployment spells. To construct a measure of municipality-specific reemployment propensities, we link the universe of individuals with an employment to unemployment transition in every single year during the pre-DSL and DSL period (referred to as the *unemployment inflow sample*) with a municipality identifier at either the individual or establishment level. This allows us to merge the administrative data with information from other data sources (see Table C.1 in Online Appendix C).¹⁴ In our analysis, we concentrate on individuals who were at least three months employed before they became unemployed. In doing so, we exclude individuals with short employment spells who may display a different pattern of search activity over their spells of employment and unemployment.

Sample selection and main outcome variable. In our empirical analysis, the pre-DSL period covers the years 1998 and 1999, whereas the DSL period covers 2007 and 2008. We focus on these later DSL years for several reasons. First, as set out earlier, we will complement our analysis with individual-level survey data that are available from 2007 onwards. This restricts us in documenting first-stage effects starting from 2007 only. Second, there is evidence that the early DSL years may be considered as transition years towards a new technology equilibrium. This appears to be particularly true for the less agglomerated municipalities, which typically have no own MDF and hence form the basis for our empirical analysis. To support this notion, Fig. D.1 in Online Appendix D plots the distribution of DSL availability against time. Panel (A) of Fig. D.1 displays the development for agglomerated municipalities, whereas Panel (B) shows the distributions for less agglomerated municipalities. The graphs illustrate that the transition phase among less agglomerated municipalities took apparently longer as compared to urban regions. Third, online search and recruiting technologies appear to have become more elaborated over the course of time. Some evidence for this consideration was documented in Section 2, pointing to some inefficiencies of the Federal Employment Agency's job board technology during the early DSL period. Some further evidence for improvements of the underlying technologies is given by the increasing importance of online recruiting among employers (see Section 2 and Fig. A.1 in Online Appendix A).

As set out earlier, our main outcome variables of interest are the *unconditional* probabilities of entering employment within m months after entering unemployment, for various values of m . To account for right-censoring of unemployment spells, these probabilities are defined as the complement of the survival function, which is estimated by the non-parametric Kaplan-Meier estimator.¹⁵ In our sample, the median of individuals who were entering unemployment during the whole DSL period is 83 per municipality. The median over all pre-DSL years equals 77. To calculate meaningful averages at the municipality level, we further condition on observing at least five individuals per period and municipality in our final unemployment inflow sample. Due to this condition, the final sample of municipalities (3204) covers 96% of all available less agglomerated municipalities (3339) that fulfill the requirements described above.

7. Descriptive statistics

Given that our empirical strategy focuses on less agglomerated municipalities without an own main distribution frame (MDF), we provide descriptive statistics for the above defined set of 3204 municipalities.

Municipality-level variables. Panel A of Table 1 shows that lucky and unlucky municipalities differ significantly in DSL availability rates. In addition to broadband internet information, the table provides information on further regional characteristics at the municipality level for the pre-DSL and DSL years.¹⁶ Table 1 and Table D.1 in Online Appendix D show the main set of control variables differentiated by treatment status and period. According to Panel B of Table 1, the local sociodemographic and age composition is quite similar between lucky and unlucky municipalities and across time. We see a slight trend towards an aging population and higher qualifications.

Panel C of Table 1 shows firm-level characteristics. The figures indicate that the average number of establishments increased in West Germany, whereas average establishment size decreased slightly and amounted to about six. As to firm entries and exits, the table documents that less firms entered and more firms exited the market, while total sales increased. Panel D of Table 1 displays the main characteristics of the unemployment inflow sample. The average age is around 35 and exhibits a slight increase over time. The same pattern is observed for the share of females among those entering unemployment. Moreover, low-skilled individuals and foreigners tend to be disproportionately represented in the inflow sample as compared to the overall average skill level and the share of foreigners at the municipality level (see Panel C of Table D.1 for further inflow characteristics).

Column (7) provides difference-in-differences estimates conditional on MDF-fixed effects to get some hints on whether other municipality characteristics might have been affected by the DSL expansion. Results point to different developments

¹⁴ More specifically, the municipality identifier in the administrative data is based on individuals' place of residence. If the place of residence is missing, we use the municipality identifier of individual spells from the previous or subsequent five years or - in a final step - information on individuals' workplace (establishment) location.

¹⁵ Formally, the estimator is given by: $\hat{S}(m) = \prod_{i: m_i \leq m} (1 - \frac{d_i}{n_i})$, where d_i is the number of spells that transit into employment in month, m_i , and n_i is the total number of individuals at risk during the time interval $[m_i, m_{i+1}]$.

¹⁶ The descriptive statistics of the municipality characteristics shown in Panel B of Table 1 are based on re-weighted averages. As our sample consists of the universe of the unemployed and a 50% sample of employed individuals, we re-weight the averages to match the official unemployment rates. Some further regional characteristics for the pre-DSL and DSL years are also available from Falck et al. (2014) (see Table C.1 in Online Appendix C).

Table 1
Descriptive statistics for pre-DSL and DSL years.

	Pre-DSL years 1998/99			DSL years 2007/08			
	Unlucky (1)	Lucky (2)	Diff ^{a)} (3)	Unlucky (4)	Lucky (5)	Diff ^{a)} (6)	Diff-in-Diff (7)
<i>Panel A: Broadband availability</i>							
DSL	0.000	0.000	0.000	0.767	0.918	-0.126***	-
<i>Panel B: Municipality characteristics</i>							
Inflow unemployed	62.807	53.038	-0.261	61.754	55.950	-2.509	-2.248***
Population	1330.462	1210.939	-42.206	1339.175	1219.120	-46.009	-3.803
Female population share	0.499	0.501	-0.002*	0.501	0.502	-0.001	0.001
Population share aged 18–65	0.655	0.658	-0.004***	0.610	0.621	-0.001	0.003**
Population share > 65	0.163	0.164	0.003**	0.187	0.188	0.001	-0.002**
Net migration rate	0.004	0.005	-0.000	-0.002	-0.001	-0.001	-0.001
Unemployment rate	0.039	0.040	0.001	0.039	0.041	0.001	0.000
Average real daily wage	95.887	97.593	-0.451	96.568	98.433	-0.530	-0.080
Low-skilled	0.171	0.169	-0.002	0.149	0.153	-0.003*	-0.001
Medium-skilled	0.778	0.774	0.003*	0.783	0.774	0.005***	0.001
High-skilled	0.051	0.057	-0.002	0.068	0.073	-0.002**	-0.001
Foreign nationals	0.021	0.025	-0.002**	0.021	0.025	-0.002**	0.000
<i>Panel C: Demand-side characteristics</i>							
Number of establishments	28.772	26.587	-1.801	39.429	35.988	-1.918	-0.118
Establishment size	6.073	5.981	-0.434*	5.928	5.911	-0.451**	-0.017
Number of firm entries	2.647	2.415	-0.078	2.303	2.088	-0.040	0.037
Number of firm exits	1.700	1.664	-0.146	3.050	2.926	-0.066	0.080
Sales	17.039	31.376	-9.579	51.118	61.069	-22.489	-12.909
<i>Panel D: Inflow characteristics</i>							
Age	35.324	35.270	0.136	35.924	35.738	0.234*	0.098
Female share	0.352	0.376	-0.006	0.406	0.421	-0.002	0.005
Low-skilled	0.196	0.203	-0.009**	0.207	0.220	-0.008*	0.001
Medium-skilled	0.765	0.751	0.010**	0.744	0.725	0.012**	0.002
High-skilled	0.039	0.046	-0.001	0.049	0.055	-0.004	-0.003
Foreign nationals	0.033	0.038	-0.002	0.029	0.036	-0.006***	-0.004*
Number of individuals in unemployment inflow sample	70,740	109,637		69,757	115,711		
Number of municipalities	1069	2135		1069	2135		

Notes: The table reports municipality-level descriptive statistics for unlucky and lucky municipalities in West Germany. The pre-DSL period covers the years 1998 and 1999. The DSL period covers the years 2007 and 2008. ^{a)} Differences in means between unlucky and lucky municipalities are conditional on MDF-fixed effects (column (3) and (6)). Column (7) reports the differences in means between the DSL and pre-DSL period and between unlucky and lucky municipalities conditional on MDF-fixed effects. Panel A reports the DSL availability rate. Panel B reports municipality characteristics. Panel C reports demand-side variables. Panel D reports age, female, education and nationality structure for the unemployment inflow sample. Further control variables are reported in Table D.1 in Online Appendix D. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

of the number of individuals who became unemployed, the local age composition as well as the shares of production and service occupations (see Panel A of Table D.1). We will discuss changes of observables due to the broadband expansion in more detail in Section 9.3.

Overall, the descriptive figures provide some evidence for level differences in observable characteristics during the pre-DSL years between lucky and unlucky municipalities. Table D.2 in Online Appendix D shows the development of our set of observables for lucky and unlucky municipalities for the years 1995 and 1999. Except for three variables, we fail to detect any differential pre-trends. Turning to the few exceptions, the share of individuals in agricultural and production jobs evolved significantly differently between 1995 and 1999 in lucky and unlucky municipalities. The difference-in-differences estimates are -0.2% and -0.4% points, respectively. However, we consider these differences as economically small. A similar result holds true for public sector employment.

Cumulative reemployment probabilities. Based on the inflow sample at the municipality level, Panel (A) of Fig. 2 shows cumulative reemployment probabilities at the municipality level for month m , after the entry into unemployment, separately for the DSL (2007/08) and the pre-DSL years (1998/99). For example, the cumulative probability of having experienced a transition into employment by month 12 after entering unemployment was about 78% during the defined DSL years, whereas during the pre-DSL years the respective probability was about 75%. At the end of the second year, we observe that the cumulative reemployment probability increased further by 10% points. This indicates that much of the dynamics already occurs during the first 12 months of unemployment. For this reason, we concentrate in our empirical analysis on the first year of unemployment.¹⁷

¹⁷ A further reason is that after one year of unemployment, individuals are counted as long-term unemployed and experience different state-governed treatments, such as lower unemployment benefits and increased job search assistance.

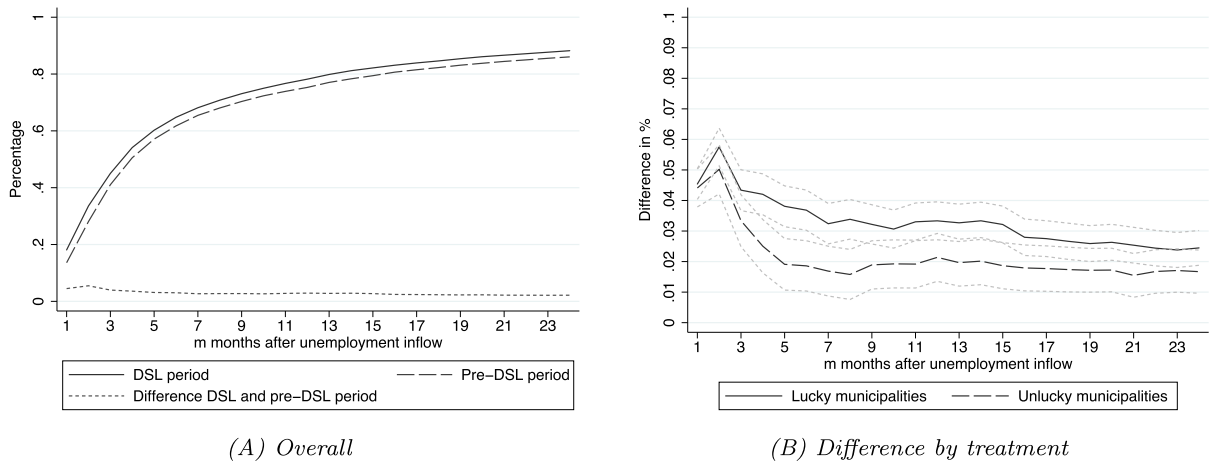


Fig. 2. Reemployment probability and difference between lucky and unlucky municipalities.

Notes: Panel (A) plots the cumulative probability of becoming reemployed within m months after an inflow into unemployment averaged at the municipality level, distinguishing between the DSL (2007/08) and the pre-DSL (1998/99) period. The bottom line plots the difference between the two upper lines against time. Panel (B) plots the same difference separately for lucky and unlucky municipalities. Grey dotted lines represent 95% confidence intervals.

The bottom line in Panel (A) of Fig. 2 plots the difference between the two upper lines. Overall, this line illustrates that during the DSL years the cumulative probability of experiencing a transition into employment is larger than in the pre-DSL period. Over the first 12 months, cumulative reemployment probabilities increased, on average, by 3.4% points.¹⁸ Panel (B) of Fig. 2 plots the difference in reemployment rates between the pre-DSL and DSL period by further distinguishing between lucky and unlucky municipalities. The graphs show that after the third month, lucky municipalities show higher cumulative reemployment probabilities than their unlucky counterparts. This indicates, on a descriptive basis, that municipalities with higher DSL availability experienced a larger increase in reemployment probabilities and, as a result, a larger decline in unemployment durations over the two defined periods.

8. Empirical results

8.1. Transitions from unemployment to employment

Baseline effects. We now turn to regression models in order to calculate standard errors and conduct hypothesis tests. We start our regression analysis by looking at differences in outcomes between the pre-DSL years (1998/99) and the DSL years (2007/08) over a constant time span. We cluster standard errors at the MDF level as the identifying variation is measured at this level. Fig. 3 displays the estimated effects of a 1% point increase in the municipality-specific share of households with DSL availability on the cumulative probability of reentering employment within m months after their inflow into unemployment.

The left figure shows the ordinary least squares (OLS) estimates of the first difference model controlling for observable characteristics measured during the pre-DSL period and MDF-fixed effects. The OLS coefficients are close to zero in the first year after inflow into unemployment. The right figure shows the IV estimates. The Kleibergen-Paap F -Statistics is 50.1 and the first-stage treatment coefficient is 0.061 and significant at the 1% level, indicating that unlucky municipalities have on average 6% points lower DSL rates. Therefore, weak identification issues do not apply here. In the IV model the point estimates become positive and partly significant at the 10% significance level after seven months in unemployment. In terms of magnitude, the coefficient amounts to 0.15 in month eight, which corresponds to up to 2.3% points higher cumulative reemployment probabilities after assigning an individual to a lucky instead of an unlucky municipality, where the unconditional difference in DSL rates (shown in Fig. 1) is roughly 15% points.¹⁹

Heterogeneous effects by socio-economic characteristics. The results from the pooled sample might mask heterogeneous effects across different subgroups. In particular, it might be conceivable that more skilled individuals or younger workers have greater exposure to the internet and thereby make more efficient use of online job search tools. We test this hypothesis

¹⁸ Throughout the analyses, we right-censor spells that are ongoing at the moment of the introduction of fast internet. An alternative approach would be to apply the methodology of Van den Berg et al. (2020) to identify and estimate average causal effects on hazard rates by exploiting such ongoing spells while taking dynamic selection due to unobserved heterogeneity into account. We do not pursue this approach because the fraction of such spells in the data is very low. Moreover, we suspect that the availability of DSL only leads to online search with a certain delay of which we do not know its length. Using ongoing spells while ignoring delays would severely underestimate the effects.

¹⁹ One may argue that these effects are small in magnitude. However, they have a similar order of magnitude as the effects of successful active labor market policies for the unemployed; see e.g. Crépon and Van den Berg (2016).

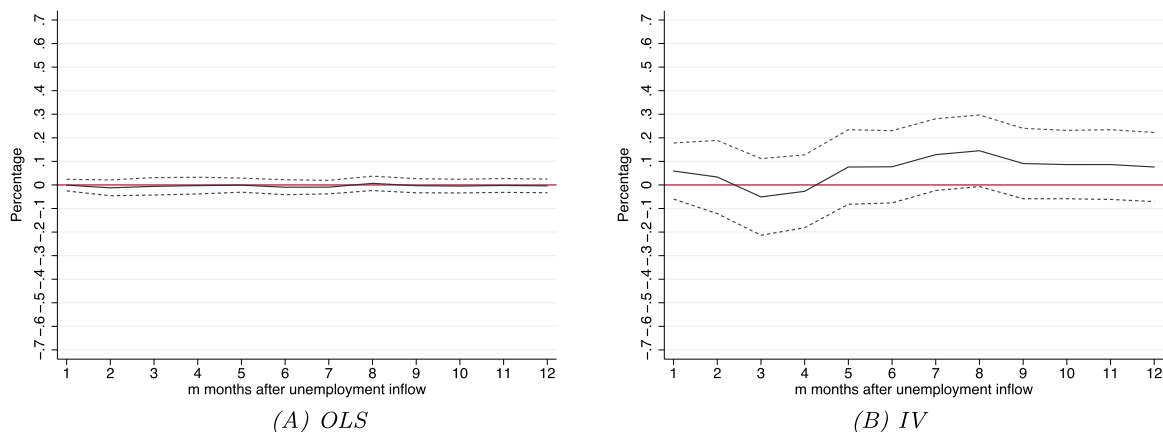


Fig. 3. IV regression results of DSL on unemployment-to-employment transitions.

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within m months for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure measured during the pre-DSL period (see Table C.1 in Online Appendix C). Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust and clustered at the MDF level. Panel (A) plots the effects using OLS. Panel (B) corresponds to the IV model, where the distance is measured from the municipalities population-weighted center to the next MDF. Regressions are based on 3204 municipalities and 866 MDFs. The Kleibergen-Paap F -Statistic for the first stage in Panel (B) is 50.1.

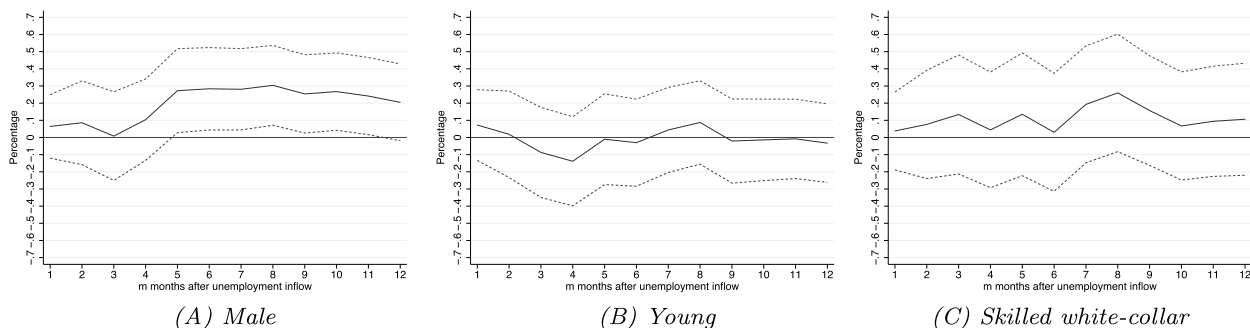


Fig. 4. IV regression results of DSL on unemployment-to-employment transitions by socio-economic characteristics.

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probability from unemployment to employment within m months for an inflow sample of individuals who entered unemployment between 1998/1999 and 2007/2008 separately for males, young individuals (≤ 35 years) and skilled white-collar individuals. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure measured during the pre-DSL period (see Table C.1 in Online Appendix C). Dotted lines present the 95% confidence interval. Standard errors are heteroskedasticity robust and clustered at the MDF level. The distance is measured from the municipalities population-weighted center to the next MDF. Regressions are based on 2951 municipalities and 842 MDFs for males, 2828 municipalities and 832 MDFs for young individuals and 2638 municipalities and 815 MDFs for skilled white-collar individuals. The Kleibergen-Paap F -Statistic for the first stage is 43.3, 40.3 and 36.6 for the three groups, respectively.

by estimating the regressions for different subgroups of the unemployment inflow sample. We first break down the sample by gender as well as age, by distinguishing young (≤ 35 years) and old workers (> 35 years).

We further test the hypothesis that the intensity with which employers use the internet for recruitment purposes may matter for its effectiveness in raising reemployment prospects for job seekers. Given that the descriptives from the IAB Job Vacancy Survey (see Section 2) suggested that vacancies for more skilled and white-collar occupations were more likely to be advertised online, we restrict our sample to these occupations. We do so by looking at skilled individuals (who have completed a vocational training or hold a university degree/technical school degree) entering unemployment from a *white-collar* job, with the latter comprising higher clerks, service, clerical or sales occupations. The first-stage coefficients for the sub-groups range between 0.05 and 0.07 and are significant at the 1% level for all groups. Fig. 4 plots the estimated coefficients along with their confidence intervals. Compared with the estimates from the pooled sample, Panel (A) of Fig. 4 point to a clearer picture for unemployed males, for whom the positive effect of higher DSL availability is particularly pronounced after month four. In terms of magnitude, assigning a male individual to a lucky instead of an unlucky municipality increases the cumulative reemployment probability by about 4% points on average after four months in unemployment. Given that the positive effects can only be established for males, but not for females (see Fig. F.1 in Online Appendix F), our findings argue against the view that the effects are driven by potential female labor supply effects, which may, e.g., arise from the internet facilitating work-family balance (see Dettling, 2017).

For skilled individuals who entered unemployment from white-collar jobs and young job seekers, we observe insignificant point estimates that are mostly close to zero.²⁰ Moreover, for males and young individuals the comparison of the IV and OLS estimates suggests that these groups appear to be negatively selected. Complementary analyses based upon the PASS data (see Section 9) show a negative correlation between internet access at home and work attitudes for both groups (see Table I.3 in Online Appendix I). A further explanation for a negative selection might stem from social networks. If the informal search channel dries up earlier due to weaker networks, individuals might use the internet more intensively and earlier within the unemployment spell.

The results so far suggest that the increase in DSL availability appears to raise the cumulative reemployment probabilities for males. Moreover, a further finding is that the positive effect on reemployment probabilities shows up or becomes significant only with a certain time delay after entering unemployment. In Section 9, we will turn to the underlying mechanisms and address the question to what extent this finding may be explained by heterogeneous changes in job search related outcomes across subgroups, such as job seekers' adopted search channels and their application behavior.

8.2. Robustness checks

Sample specification and weighting. In this subsection, we conduct several robustness checks. We start by providing regressions results for different sample specifications. First, we include all individuals in the inflow sample irrespectively of the length of their previous employment spell. Second, to address the issue that the results might be driven by small municipalities with few inflows into unemployment, we re-estimated our specifications by conditioning on municipalities with at least 500 inhabitants (in addition to conditioning on at least five individuals entering unemployment). Finally, we show the results without weighting the municipality-level variables by the number of inhabitants. The estimates shown in Fig. F.2 in Online Appendix F suggest that the overall pattern of results remains qualitatively unaltered. For males, including all individuals irrespectively of their previous tenure or non-weighting lead to point estimates that are smaller than in the baseline specification.

Recalls. A further concern could be that our estimates are affected by potential recalls, e.g. individuals who return to their pre-unemployment establishment.²¹ In particular, it might be conceivable that unemployed individuals who are reemployed by the same employer do not actively search for a new job. There is evidence that individuals with recalls experience shorter unemployment durations and lower search intensities as compared to unemployed job seekers entering a new job (Nekoei and Weber, 2015, Fujita and Moscarini, 2017). This could be a potential explanation for the non-positive DSL effect at the beginning of the unemployment spell. Due to the endogeneity of recalls, we refrain from conditioning on this outcome, but rather re-estimate our model after excluding industries with a priori high recall rates. These industries include agriculture, construction and passenger transport. Fig. F.3 in Online Appendix F presents the estimates. The figures suggest that the overall pattern of results remains unaltered.

Empirical specification. We further conduct robustness checks with respect to the empirical specification. In particular, we narrow the distance around the threshold and exclude outlier municipalities in terms of their distance to the threshold and their broadband availability shares. Panels (A) and (B) of Fig. F.4 in Online Appendix F give the results for the three socio-economic groups. The figures corroborate the pattern of results that has been found earlier. To explicitly compare municipalities with similar pre-trends in observables, we further include changes in observable characteristics between the pre-DSL years 1999 and 1995 as additional control variables. Fig. F.5 in Online Appendix F shows that the results are robust to including these controls.

The analysis so far has used a dichotomous treatment variable dividing municipalities into lucky and unlucky ones. Panel (B) of Fig. 1 shows that the treatment intensity increases with higher distances. As a further robustness check, the treatment dummy is interacted with the actual distance to the next MDF. Note that the observed distribution in distances does not allow for a regression kink design, as there is no policy rule that generates the distribution.²² Panel (C) of Fig. F.4 in Online Appendix F presents the results. The positive point estimate for males becomes slightly smaller but is still significant from month five onwards. The results for young individuals and skilled white-collar workers remain insignificant.

8.3. Regional mobility and agglomeration

One threat to our identification strategy may stem from selective mobility patterns and agglomeration forces. If, for example, the internet expansion induces especially job seekers in unlucky municipalities with a higher reemployment propensity to move to a high internet region or, alternatively, if individuals in lucky municipalities simply benefit from a closer proximity to an urban agglomeration or public spaces with internet availability, the DSL coefficient would be upward biased. A descriptive analysis of the spatial distribution of our sample municipalities shows that the latter are relatively small

²⁰ Fig. E.1 in Online Appendix E further plots the coefficients measuring the effects on monthly hazard rates rather than on cumulative probabilities. For males, the effects on monthly hazard rates exhibit a similar pattern as the effects on cumulative reemployment probabilities, as there are positive effects between 2-4% points after four months in unemployment.

²¹ In our sample, 23% of all individuals returned to their previous employer. The mean unemployment duration of recalls amounts to 98 days, with 75% of all spells being shorter than four months.

²² For this reason, Falck et al. (2014) argue that using the distance as an instrument may violate the exclusion restriction. The value of this sensitivity analysis is therefore not clear.

compared to their respective regional labor market and that the average distance between lucky and unlucky municipalities within the MDF region is about 6 km (see Section G.1 in Online Appendix G for a detailed description). Therefore, one may view our setting as having small units that are unlikely to change general conditions in their local labor market.

In a next step, we calculate from our administrative data the fraction of unemployed individuals who move to lucky municipalities. Panel B of Table G.2 in Online Appendix G shows that regional mobility into lucky municipalities during an unemployment spell is relatively small. The fraction of individuals moving to another lucky municipality is in fact higher for individuals who were initially located in lucky municipalities. Moreover, for both groups we observe an increase in mobility rates after the DSL expansion of a similar order of magnitude, such that differences in mobility rates remain largely constant. The internet, therefore, appears not to have changed mobility patterns among unemployed job seekers. To address potential agglomeration effects, Panel A of Table G.2 shows that the difference in the distance to the next urban center between lucky and unlucky municipalities is about 1.4 km (0.5 km with MDF-fixed effects). While this difference appears to be economically insignificant, individuals in lucky municipalities might still benefit from the closer proximity to an urban agglomeration. To test this channel, we analyze the DSL effect on the probability of finding a job in an urban agglomeration (operationalized by a city with at least 100,000 inhabitants). Table G.3 in Online Appendix G shows that the DSL expansion does not affect this probability. For young job-seekers, the effect is large, but insignificant at any conventional level.²³ The DSL effect on reemployment prospects is therefore unlikely to be biased by regional mobility or by different degrees of exposure to economic activity. Results are further unaltered when controlling for the distance to the next urban center as shown in Fig. G.1 in Online Appendix G.

8.4. Effects during the early DSL years

Online Appendix H presents the results for the early DSL years (2005/06), which have been shown to characterize a transition period towards a new technology equilibrium especially for the less agglomerated municipalities. Fig. H.1 presents the baseline results. The overall pattern that emerges from the baseline estimates is that higher DSL availability does not affect the cumulative reemployment probabilities for all defined subgroups. A potential explanation for these findings may be that employers and job seekers were still adapting to the new technology and that job search technologies, such as that from the Federal Employment Agency, were still characterized by inefficiencies during the early DSL period. Taken together, the comparison of the early and late DSL years leads us to conclude that the effectiveness of the internet appears to have considerably improved across these periods. Note that this is in line with the findings of [Kuhn and Mansour \(2014\)](#), who showed that the relationship between internet job search and unemployment durations became more efficient over time.

8.5. Placebo test

To test for the similarity or divergence in time trends across lucky and unlucky municipalities during the pre-DSL period, we further conduct a placebo test. In particular, we compute the differences in outcomes between 1995 and 1999 and regress the change in the fraction of unemployed entering employment during the first 12 months after entering unemployment on the treatment dummy (and further controls including MDF-fixed effects). Overall, the results in Fig. 5 show that the effect of the treatment dummy is insignificant and close to zero for each month after the inflow into unemployment. Hence, the placebo estimates point to similar pre-treatment trends across lucky and unlucky municipalities for all subgroups which suggests a causal interpretation of the DSL effect on cumulative reemployment probabilities.

9. Mechanisms

9.1. Individual-level job search strategies based on survey data

Given that our strategy thus far identified an ITT, the question of to what extent the established effects arise from changes in individuals' job search behavior remains unanswered at this stage. To provide evidence on the underlying mechanisms, we complement our analysis by exploiting survey data on job search channels among job seekers from the survey *Panel Study on Labour Markets and Social Security (PASS)*. A detailed description of the variables used in this study can be found in Online Appendix I (Table I.1). The survey started in 2007 as a panel, with the main purpose of surveying low-income households. We use the first waves of the data set, which correspond to the years 2007–2009 (see [Trappmann et al., 2010](#) for a detailed description of the data).²⁴ Due to sample size limitations, we cannot confine our sample to the subset of less agglomerated municipalities as in the analysis using the administrative data. We therefore make use of the full sample including all municipalities, i.e. also those with an own MDF. For each individual, we restrict the sample to the observation in the first

²³ Individuals might also benefit from a closer proximity to public spaces like libraries or internet cafés that potentially provide internet access. Based on Bureau van Dijk data (Orbis data pulled from Wharton Research Data Services Amadeus) for the years 2007/08, the average distance to the nearest library is 24 km, whereas the nearest café is 7 km away. Excluding municipalities close to these spots (within a 5 km radius) provides robust results. The estimates are available on request.

²⁴ The first wave is conducted mostly in 2007. 73% of all individuals used in our sample are interviewed in 2007. 23% are interviewed in 2008/09. The remaining 4% correspond to the year 2006.

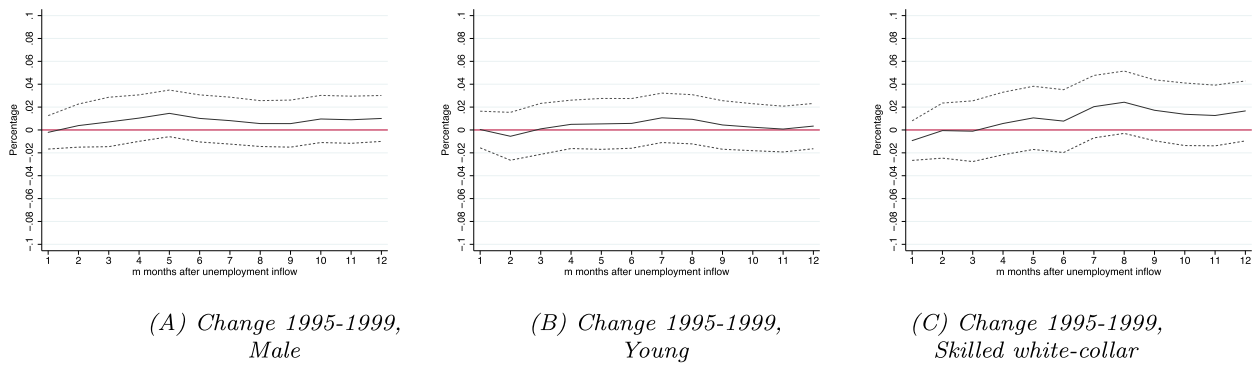


Fig. 5. Placebo results.

Notes: The figure shows the effects of the treatment dummy on the cumulative transition probability from unemployment to employment within m months for an inflow sample of individuals who entered unemployment in 1995 and 1999 separately for males, young individuals (≤ 35 years) and skilled white-collar individuals. The endogenous variable is the change between 1995 and 1999. The regressions are population-weighted and performed separately for each month. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure measured in 1999 (see Table C.1 in Online Appendix C). Dotted lines present the 95% confidence interval. Standard errors are heteroskedasticity robust and clustered at the MDF level. The distance is measured from the municipalities population-weighted center to the next MDF. Regressions are based on 2915 municipalities and 839 MDFs for males, 2794 municipalities and 827 MDFs for young individuals and 2563 municipalities and 803 MDFs for skilled white-collar individuals.

wave in which (s)he participated, and we ignore responses in subsequent waves. If respondents are looking for a job at the time of the relevant interview, they are asked to report their specific adopted job search channels. Possible categories include online job search, search via newspapers, friends/relatives, private brokers, the local employment agencies or further (non-specified) search channels. Moreover, the survey also asks whether a job seeker's household possesses a computer with an internet connection.²⁵ Table I.2 in Online Appendix I shows that home internet access is positively correlated with the incidence of online job search. Overall, the fraction of job seekers searching online is more than 25% points higher among job seekers with home internet access as compared to those with no home internet access.²⁶

In what follows, we explore whether home internet access increases the incidence of online job search and changes the use of other job search channels. These results are mostly descriptive as they ignore the fact that the PASS subsamples of unemployed individuals are so-called stock samples of the unemployed in which individuals with unfavorable unobserved characteristics are overrepresented.²⁷ Similar to our empirical strategy at the municipality level, we again make use of regional identifiers provided by the Federal Employment Agency. Apart from the municipality identifier, we are also able to take advantage of the postal codes provided by PASS. This is a particularly attractive feature of the data, as the combination of the municipality identifier and the postal code provides greater scope for variation in the treatment indicator that is needed for the IV regression (see Fig. I.1 in Online Appendix I for a graphical illustration).

Survey evidence on search channels. Table 2 reports the estimates of the effect of home internet access on the probability of searching online for a job. The F -Statistic in the full sample is at the benchmark value of 10. This value decreases when analyzing subsamples. While weak instruments in just-identified models are of no major concern as long as the first-stage coefficient differs from zero, they are associated with higher standard errors (Angrist and Pischke, 2008, Angrist and Pischke, 2009). Overall, the IV estimates suggest that the OLS estimates are downward biased. This downward bias has also been documented in the analysis using the administrative data. Home internet access is associated with a significant increase in the probability of online job search. Moreover, the results suggest that the point estimate for young individuals is insignificant and lower than that for the pooled sample.²⁸ Turning to our final subgroup, we are not able to condition on skilled individuals with white-collar occupations (if unemployed, in their previous job) due to sample size restrictions. For this reason, we provide separate estimations for skilled individuals and individuals whose (previous) occupation was a

²⁵ The survey does not specifically ask about broadband internet connection. This can induce misclassification of our explanatory variable. Depending on the extent of misclassification, IV estimates would therefore represent an upper bound.

²⁶ The analysis of the relationship between home internet access and online job search is based on both unemployed and employed job seekers. However, most individuals were unemployed at the time of the interview date (82%, see Table I.4 in Online Appendix I) while the employed were mostly in low-income precarious employment with high inflow rates into unemployment.

²⁷ Note that this is the main reason for why we do not pursue a 2SIV or 2S2SLS approach. Conceptually, the latter approaches allow for identification of the effect of the use of online search channels on reemployment probabilities, by combining first-stage estimates based on the PASS data with the ITT from the administrative data. Implementing such an approach requires comparable samples, though. The restriction of our ITT analysis with register data to less agglomerated municipalities and, in particular, to a flow sample renders this approach infeasible.

²⁸ A potential explanation for lower point estimates of home internet access on online job-search among the young might relate to time-consuming online activities other than job search. It has been shown that broadband internet leads to more entertainment consumption (Falck et al., 2014), music downloads and online shopping (Kolko, 2010), which is likely to be particularly relevant for younger individuals. There is also evidence that primarily young males spend a great deal of time playing computer games (e.g. first person shooter games) and fulfill their need for social interaction by participating in online networks (Jansz and Tanis, 2007, Frostling-Henningsson, 2009).

Table 2
Estimation results for home internet on online job search.

	Full sample OLS (1)	Full sample IV (2)	Male IV (3)	Young IV (4)	Skilled IV (5)	White-collar jobs IV (6)
<i>Panel A: Estimation results</i>						
Home internet access	0.273*** (0.018)	0.674** (0.317)	0.685** (0.346)	0.517 (0.530)	0.699* (0.391)	0.774* (0.426)
Threshold (first stage)		-0.118*** (0.037)	-0.160*** (0.053)	-0.123* (0.067)	-0.112*** (0.043)	-0.117** (0.048)
F-statistic		10.00	9.06	3.41	6.67	5.99
<i>Panel B: Compliance</i>						
$P[X = x]$		1.000	0.507	0.388	0.647	0.557
$P[X = x I_{ij} > I_{0i}]$		1.000	0.687	0.404	0.614	0.552
$\frac{P[X=x I_{ij}>I_{0i}]}{P[X=x]}$		1.000	1.355	1.041	0.949	0.991
Mean value of online search	0.731	0.731	0.742	0.777	0.764	0.751
Observations	2914	2914	1478	1133	1884	1624

Notes: Panel A reports regression results of home internet access on online job search for individuals in West Germany. The results are based on linear probability models. Home internet access is instrumented by a threshold dummy indicating whether the distance of the centroid of a person's home municipality to the next MDF is above 4200 m. The *F*-test of excluded instruments refers to the Kleibergen-Paap *F*-Statistic. Standard errors are heteroskedasticity robust and clustered at the household level. The list of control variables includes individual characteristics, household information, father's education and information on the labor market history (see Table I.1 in Online Appendix I). Tables I.2 and I.4 provide descriptive statistics. Panel B reports characteristics of compliers. This includes the distribution of the population $P[X = x]$ and the distribution of compliers $P[X = x|I_{ij} > I_{0i}]$ calculated as first-stage coefficient times population share divided by the overall first-stage coefficient. The last statistic shows the relative likelihood of an individual belonging to a subgroup, in the complier group compared to the overall subgroup population. The number of observations refers to the first observation of individuals during the first three waves. Thus, if we observe an individual multiple times during the first three waves, we use the first information only. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

white-collar job. The results show that the point estimate for skilled individuals is of the same order of magnitude as in the pooled sample and significant at the 10% level, whereas individuals whose last job was a white-collar job feature the highest point estimates.

The IV estimates should be interpreted as the LATE of home internet access on online job search for the sub-population of compliers induced to have home internet access because the household is located relatively close to the next MDF (Imbens and Angrist, 1994). Panel B of Table 2 provides information about the characteristics of the complier group. The proportion of the compliers ($P[X = x|I_{ij} > I_{0i}]$) is calculated as the ratio of the first-stage coefficient for the respective subgroup relative to the overall first-stage coefficient times the population share (Angrist and Pischke, 2009; Bhuller et al., 2013). The table shows that the share of males in the population is 50.7%, whereas 68.7% of the compliers are males. This result indicates that males are overrepresented among the compliers. The same result does not hold for the other subgroups with relative likelihoods being close to 1.

To obtain an economic meaning of the IV estimates, we calculate the counterfactual online job search probability that would have occurred without broadband internet. The counterfactual rate is given by the actual fraction of online job search minus the predicted effect of home internet on online job search among compliers. We calculate the predicted effect of home internet on online search by the fraction of individuals in lucky municipalities (0.937) times the first-stage coefficient times the LATE. Overall, the predicted effect of home internet on online job search is 7.4% points, suggesting a relatively small effect which fits to the rather small documented effect of broadband internet on reemployment obtained from the municipality-level analysis. The predicted effect for males is with 10.4% points 40% higher than that obtained from the full sample.

To address potential crowding out effects, Table I.5 in Online Appendix I reports estimated effects of home internet access on job seekers' use of other, more traditional job search channels (see Section I.1 in Online Appendix I for a detailed description). The results provide suggestive evidence for a (sizeable albeit insignificant) reduction in offline job search – as measured by the number of offline search channels – especially for skilled and white-collar jobs, whereas for men crowding out effects seem to play a minor role.

Survey evidence on application intensity. Apart from job search channels, the data set allows us to analyze the number of job applications as a measure of search intensity as well as the number of (realized) job interviews. While the number of applications may be considered as a further measure of search intensity, the number of job interviews is likely to be an important prerequisite of job offers and may therefore be viewed as a (weak) proxy for the arrival of job offers. Table 3 reports the estimation results of the effects of home internet access on these outcomes.²⁹

²⁹ More specifically, the survey asks respondents to report the number of *own-initiative* applications as well as the number of realized job interviews during the last four weeks.

Table 3
Estimation results for home internet on application intensity.

	Full sample OLS (1)	Full sample IV (2)	Male IV (3)	Young IV (4)	Skilled IV (5)	White-collar jobs IV (6)
<i>Panel A: # Own-initiative applications</i>						
Home internet access	−0.031 (0.229)	5.212 (3.781)	12.033** (4.805)	0.688 (7.279)	5.832 (4.805)	3.147 (4.376)
Mean value of applications	2.49	2.49	2.71	2.80	2.45	2.33
<i>Panel B: # Job interviews</i>						
Home internet access	0.007 (0.066)	0.040 (0.897)	−0.636 (1.170)	0.626 (1.364)	−0.126 (1.156)	−0.415 (1.147)
Mean value of interviews	0.60	0.60	0.67	0.64	0.59	0.60
Observations	2914	2914	1478	1133	1884	1624

Notes: The table reports regression results of home internet access on the number of applications and realized job interviews for individuals in West Germany. Own-initiative applications may result from using all available search channels. Home internet access is instrumented by a threshold dummy whether the distance of the centroid of a person's home municipality to the next MDF is above 4200 m. The *F*-test of excluded instruments refers to the Kleibergen-Paap *F*-Statistic and is the same as in Table 2. Standard errors are heteroskedasticity robust and clustered at the household level. The list of control variables includes individual characteristics, household information, father's education and information on the labor market history (see Table I.1 in Online Appendix I). Tables I.2 and I.4 provide descriptive statistics. The number of observations refers to the first observation of individuals during the first three waves. Thus, if we observe an individual multiple times during the first three waves, we use the first information only. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Comparing the point estimates from the IV specification with the OLS results in Panel A reveals that the OLS coefficients are downward biased. Turning to the subsamples, the figures show that especially males exhibit a positive home internet access effect on the number of applications. Based on the procedure spelled out above, the predicted home internet effect for males is 1.8 applications. This effect can be considered sizeable relative to the mean of 2.7 applications. For females, the coefficient on home internet access is −2.3 and insignificant (not reported). Transforming the outcome variable by taking the natural logarithm to account for outliers results in a predicted home internet effect for males of roughly 40%, whereas the predicted effect for females is −0.4%. Thus, other than females, males appear to use the new technology to apply more intensively. According to the estimates in Panel B, the substantial increase in search intensity among males does not translate into a larger number of realized job interviews, though.

9.2. Dynamics within individual unemployment spells

The results from the municipality analysis show that the positive effect on reemployment probabilities shows up or becomes significant only some time after entry into unemployment. Using the PASS data, we can now analyze the frequency of job interviews over the elapsed length of an unemployment spell. Due to data restrictions, we provide the analysis on a purely descriptive basis, by comparing the outcome of interest between individuals with different unemployment durations.³⁰

Fig. I.3 in Online Appendix I plots the difference in the fraction of unemployed individuals with job interviews by home internet access against different unemployment durations. Overall, the graphs illustrate that among those with home internet access the probability of job interviews is greater during the second to fourth quarter in unemployment as compared to their counterparts without home internet access. However, we wish to note that due to the small sample size these differences are estimated quite imprecisely. For males, home internet access raises the incidence of job interviews even more pronounced during the second to fourth quarter in unemployment – but again imprecisely estimated. The time gap is found to match that from the municipality-level estimations. These patterns are consistent with the internet expansion raising job offer arrival rates with a certain time delay of at least one quarter in unemployment.³¹

9.3. Demand-side effects and other margins of adjustments

It is not inconceivable that DSL effects capture some product and labor demand dynamics. As to product demand dynamics, an increase in broadband internet availability might affect labor demand by altering firm entries and exits, the size

³⁰ In Online Appendix I, we further analyze dynamics of online job search over the unemployment spell and document that the incidence of online job search increases during the first year of unemployment among individuals with home internet access. The relative increase is more pronounced among males after four months in unemployment. Among skilled white-collar workers, this increase starts after six months in unemployment (see Fig. I.2 in Online Appendix I).

³¹ Fig. I.3 in Online Appendix I also shows the corresponding graphs for the other three subgroups. The increase in the incidence of job interviews during the second quarter in unemployment is also visible for young, skilled and white-collar workers, but less pronounced than for males. Along with the insignificant overall online job search incidence as documented in Fig. I.2, this result may provide a rationale for the absence of a DSL effect documented in Section 8.

Table 4
DSL effect on demand-side variables & sample composition.

	# Firm entries (1)	# Firm exits (2)	Total sales (3)	Unempl. rate (4)	Unempl. inflow (5)	Size of establ. (6)
<i>Panel A: Demand-side effects</i>						
Δ DSL	-0.0057 (0.005)	-0.0048 (0.004)	2.0365 (6.030)	-0.0072 (0.015)	0.4663** (0.242)	0.0098 (0.025)
	Previous employment spell (1)	Previous wage (2)	Medium-skilled (3)	High-skilled (4)	Female (5)	Age (6)
<i>Panel B: Inflow sample compositional effects</i>						
Δ DSL	0.2683 (2.347)	-0.0006 (0.001)	-0.0770 (0.057)	0.0143 (0.029)	-0.0213 (0.066)	-0.0410** (0.018)
<i>Panels A & B</i>						
Threshold (first stage)	-6.142*** (0.867)	-6.142*** (0.867)	-6.142*** (0.867)	-6.142*** (0.867)	-6.142*** (0.867)	-6.142*** (0.867)
F-Statistic	50.13	50.13	50.13	50.13	50.13	50.13
Municipalities	3204	3204	3204	3204	3204	3204

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on demand-side variables (Panel A) and the composition of the unemployment inflow sample (Panel B). Sales are measured in million euro. The number of firm entries and firm exits are measured in logs. The pre-DSL year for firm entry, firm exit, and sales refers to the year 2000. The unemployment rate is defined as fraction of unemployed individuals over employed individuals based on information of the place of residence. Previous employment spell is measured in days. Previous wage is measured in logs. The regressions are population-weighted. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure measured during the pre-DSL period (see Table C.1 in Online Appendix C). The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust and clustered at the MDF level. The distance is measured from the municipalities population-weighted center to the next MDF. *** Significant at the 1 percent level. ** Significant at the 5% level. * Significant at the 10% level.

of firms or total sales. Given that online job postings are cheaper than traditional job advertisements, an increase in internet availability might also reduce labor adjustment costs. To the extent that such a reduction induces employers to lay-off workers more quickly, this might increase inflows into unemployment and arguably change the composition of the unemployed. Panel A of Table 4 indicates that there appears to be no significant DSL effect on firm entry, firm exits and sales. These findings therefore support the view that broadband internet does not affect the reemployment probability through a change in product demand. Note that this result is consistent with the average municipality in our sample being small. This implies that not only firms in lucky, but also those in unlucky municipalities should have benefitted from an increase in DSL availability, as firms' sales radius was likely to comprise unlucky as well as lucky municipalities in the pre-DSL period.³²

Columns (4) and (5) in Panel A provide results on the municipality-level unemployment rate as well as the inflow into unemployment. While the unemployment rate remains unaffected, broadband internet leads to a higher inflow into unemployment: a 15% point increase in DSL availability increases the number of unemployed by about seven individuals. At the same time, the size of the establishments does not change. These results indicate that despite an increase of the unemployment inflow, firms do not downsize. This finding is consistent with higher turnover due to lower labor adjustment costs.

To check whether our established effects on reemployment prospects are mediated through a change in the unemployment composition, Panel B of Table 4 provides results on the DSL effects on compositional changes of the inflow sample into unemployment. While we document a positive internet effect on the number of individuals entering unemployment, the composition of the inflow sample remains fairly constant.³³ Our results therefore indicate that while broadband internet affects two margins, inflows into and outflows out of unemployment, it leaves the composition of those entering unemployment largely unaltered. This leads us to conclude that the documented DSL effects on reemployment prospects are most likely to be driven by changes in individuals' search behavior.

The introduction of broadband internet could further affect other variables which could confound or mediate the effect on reemployment propensities. To test for this, we redo the above analysis for the remaining control variables.³⁴ None of the

³² This is also consistent with the fact that the vast majority (about 90%) of unemployed job seekers in our sample become reemployed in a municipality different from their home municipality. This implies that the reemployment prospects of unemployed job seekers are unlikely to be driven by potential broadband induced demand effects in their home municipality.

³³ If any, a 1% point increase in DSL leads to a slightly younger composition of unemployed (half a month). Moreover, we find no effect of DSL on the occupation of individuals who become unemployed.

³⁴ The results are available on request.

coefficients is significant, except for the share of medium-skilled individuals which decreases significantly by 0.05% points if DSL availability increases by 1% point. This effect is considered to be economically small and unlikely to explain our main estimates. This conclusion is supported by the finding that the baseline results remain unaltered after controlling for changes in all observable characteristics (see Fig. F.6 in Online Appendix F).

9.4. Search externalities

In this section, we address potential search externalities. A first source of spill-overs relates to interdependencies across lucky and unlucky municipalities. If the job finding prospects of unemployed job seekers located in lucky municipalities improve due to better online job search opportunities, this might, in turn, reduce the respective prospects of those located in unlucky municipalities.

The underlying notion is that job seekers in lucky and unlucky municipalities are likely to compete for jobs in the same local labor market, such that those benefiting from the internet expansion may impose a congestion externality on their “unlucky” counterparts. The quantitative relevance of such spill-overs is likely to depend on individuals’ and employers’ search radius and the extent to which this radius has been altered by the internet expansion. As long as interdependencies arise from employers’ behavioral changes, this should limit the scope for spill-overs. The reason is that employers’ search radius was likely to comprise unlucky as well as lucky municipalities already in the pre-DSL period. At the same time, however, there is evidence that the pre-DSL restrictions in internet access were more likely to be binding for workers than for firms (see Section 2). Thus, we would expect that potential spill-over effects primarily arise from the behavior of individual job seekers, whose search radius was likely to be affected by the internet. Even though our treated municipalities only account for a small fraction of their respective labor market regions (Section G.1 in Online Appendix G), such search externalities might still be conceivable. In that case, our estimated coefficients would have to be interpreted as effects inclusive of potential general equilibrium spill-overs.

In particular, the documented DSL effect would consist of a direct effect driven by a reduction in search frictions and an indirect effect that arises from reduced reemployment prospects for individuals in unlucky municipalities. Both effects would be consistent with a positive DSL coefficient. Note, however, that the indirect effect can only be relevant in the presence of any direct positive effect, as the broadband expansion itself is unlikely to reduce reemployment prospects among job seekers in unlucky municipalities.

Empirically, we are able to address externalities caused by a different group of job seekers, who are not included in our treatment and control group. As set out in Section 3, the internet expansion not only reduces search costs for the unemployed, but also for those searching on-the-job. To the extent that the internet also raises the job finding prospects of the employed, the resulting search externalities may mitigate or counteract the internet’s effect on unemployed job seekers’ job finding rates. To test this notion, we explore whether the expansion in broadband availability has led to an increase in job-to-job transitions among employed individuals. To rule out potential match quality effects, we confine our analysis to employment relationships that had already started prior to the DSL-period. To do so, we construct a stock sample of individuals who were employed at the cut-off date of 30th of June 2000 and who were still employed at the same employer at the start of 2007.

For this sample, we then calculate the fraction of job-to-job transitions at the municipality level during the DSL years 2007/08. To compare this outcome with the pre-DSL period, we construct an analogous sample and outcome variable for the pre-internet period, based on individuals who were employed at the cut-off date of 30th of June in 1991 and who were still employed at the same employer at the start of 1998 (see Table J.1 in Online Appendix J for basic descriptive statistics for both samples). This implies that we exclude individuals from our sample who experienced a transition from employment to unemployment or non-employment during the pre-DSL and DSL period, respectively. While this procedure allows us to rule out match quality effects, which – depending on the direction of the internet’s effect on match quality – are also likely to affect the extent of job-to-job transitions, it comes at the cost of restricting the analysis to very stable employment relationships.

Panel A of Table 5 shows the results for the above described stock sample of employed individuals with a tenure of at least seven years. Columns (1) and (2) present the OLS and IV estimates for the full sample. The coefficients are small and not significantly different from zero. An increase in DSL availability does not affect the probability of a direct job-to-job transition at the municipality level. A similar result holds if the regressions are performed separately by subgroups. Confining the estimates to employed individuals with a shorter tenure (see Panel B of Table 5) yields larger coefficients, which are still not significantly different from zero.

Overall, these findings argue against the view that increased competition from employed job seekers should have played a significant role for the internet’s effect on the job finding prospects of their unemployed counterparts. To the extent that employed individuals may have made use of their workplace internet access for job search, these results are consistent with the fact that the restrictions in internet access were more likely to be binding for private households than for employers during the DSL period. The results are also consistent with the observation that the size of our sample municipalities, either in terms of the population at large or the number of unemployed, compared to the respective labor market region is rather small.

Table 5
Spill-over estimation results, job-to-job transitions.

	Full sample		Male	Young	Skilled white-collar
	OLS (1)	IV (2)	IV (3)	IV (4)	IV (5)
<i>Panel A: Tenure of at least 7 years</i>					
Δ DSL	-0.0044 (0.012)	0.0146 (0.064)	-0.0148 (0.083)	0.0603 (0.125)	0.0103 (0.093)
Threshold (first stage)		-0.0628*** (0.009)	-0.0606*** (0.009)	-0.0548*** (0.009)	-0.0558*** (0.009)
F-Statistic		50.95	48.07	40.72	42.32
Municipalities	3187	3187	3045	2741	2856
<i>Panel B: Tenure of at least 3 years</i>					
Δ DSL	-0.0035 (0.011)	0.0376 (0.049)	0.0305 (0.061)	0.0782 (0.095)	0.1060 (0.065)
Threshold (first stage)		-0.0650*** (0.009)	-0.0642*** (0.009)	-0.0600*** (0.009)	-0.0628*** (0.009)
F-Statistic		54.05	52.96	47.49	52.10
Municipalities	3293	3293	3234	3054	3175

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on the probability of job-to-job transitions for a stock sample of employed individuals. The estimates in Panel (A) are based on a sample of individuals whose employment relationship started in 1991 and 2000, respectively (see Table J.1 in Online Appendix J). The estimates in Panel (B) are based on a sample of individuals whose employment relationship started in 1995 and 2004, respectively. Columns (3)–(5) show the results separately for males, young individuals (≤ 35 years) and skilled white-collar individuals. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1 in Online Appendix C). Standard errors are heteroskedasticity robust and clustered at the MDF level. The distance is measured from the municipalities population-weighted center to the next MDF. *** Significant at the 1 percent level. ** Significant at the 5% level. * Significant at the 10% level.

10. Discussion and conclusions

In this paper, we study the effects of the expansion in broadband internet (DSL) on reemployment probabilities among unemployed job seekers by exploiting regional peculiarities of the traditional public switched telephone network in West Germany. After accounting for the endogeneity in internet availability, our estimates for the pooled sample point to slightly positive, albeit insignificant internet advantages for unemployed job seekers with a certain time delay. Breaking down the analysis by socio-economic characteristics suggests that a significant positive effect of the internet is observed for male job seekers after spending four months in unemployment.

Additional analyses at the municipality level show that the main effects are not driven by labor demand channels which may arise from firms increasing online sales in response to the internet expansion. We also rule out that changes in labor adjustment costs drive our results. In particular, we document an increase in the inflow into unemployment that arises from higher labor turnover due to lower labor adjustment costs. At the same time, we show that the composition of the inflow sample remains constant. This implies that our documented DSL effects are unlikely to reflect compositional effects among the unemployed. Overall, these findings therefore support the view that the documented DSL effects on reemployment prospects are likely to be driven by changes in job seekers' search behavior.

Given that our analysis at the municipality level identifies an ITT, we address first-stage effects by retrieving information on the adoption of job search channels from the PASS survey data. Using these data, we explore whether the availability of internet at home raises job seekers' use of the internet as a search channel. The results, which are based on a similar IV strategy as in the municipality-level analysis, indicate that for all groups - except for young workers - home internet access is associated with an increase in online job search activities. Based on a complier analysis, we find that males are overrepresented among the compliers and that the predicted effect of home internet access on online job search among compliers is largest for this subgroup. The survey data further reveal that home internet access changes male job seekers' search behavior by raising the number of own-initiative applications, while crowding out effects on offline job search channels play no major role. Overall, these findings lead us to conclude that the expansion of internet availability led to better reemployment prospects especially for males via raising the intensity with which this group has made use of the internet to search for jobs, without at the same time reducing overall search effort.

A further finding is that the positive effect on reemployment probabilities shows up or becomes significant only with a certain time delay after entering unemployment. To provide empirical support for a delayed positive effect on job offers, we further explore whether the incidence of job interviews across those with and without home internet access varies over the duration of an unemployment spell. Our findings provide some tentative evidence that internet access appears to give rise to an increase in the incidence of job interviews with a certain time delay, which appears to match the delay found in the municipality-level analysis. These results also offer potential directions for future research. Given that the internet raises the number of potential jobs that need to be evaluated against each other, future research should examine in more detail the internet's effect on job quality, e.g. whether the internet helps job seekers find a better job.

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Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:[10.1016/j.eurocorev.2021.103657](https://doi.org/10.1016/j.eurocorev.2021.103657).

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