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Does Online Search Improve the Match Quality of New Hires?

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Does Online Search Improve the Match Quality of New Hires?*

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Abstract

This paper studies the effects of the high-speed internet expansion on the match quality of new hires. We combine data on internet availability at the local level with German individual register and vacancy data. Results show that internet availability has no major impact on the stability of new matches and their wages. We confirm these findings using vacancy data, by explicitly comparing match outcomes of online and non-online recruits. Further results show that online recruiting not only raises the number of applicants and the share of unsuitable candidates per vacancy, but also induces employers to post more vacancies.

JEL Classification: J64, H40, L96, C26
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1 Introduction

The advent of the internet has fundamentally changed the way how workers and employers search for each other and eventually form a match (Autor, 2001). This trend is mirrored by the increased use of online search and recruiting tools by individual job seekers and employers (DeVaro and Gürtler, 2018). Online search tools go along with a considerable decline in search frictions arising from imperfect information and transaction costs. Given these developments, a question of particular interest is whether search via the internet along with the improved market information help to establish better matches in the labor market.

So far, the empirical literature has primarily focussed on the impact of online job search on job finding probabilities. Studies dealing with the relationship between the internet and unemployment outcomes have remained inconclusive, though, as the effects range from zero to positive.[1] While the established effects on job finding probabilities may reflect a change in matching efficiency, they tell us nothing about the internet's impact on the quality of matches arising from online search. The present study aims to fill this gap, by exploring the effect of the expansion of high-speed internet on match quality outcomes of newly hired workers. To do so, we exploit a quasi-experimental setting created by the expansion of high-speed internet (DSL) at the regional level in Germany. Using a combination of administrative and vacancy-level data, we merge employment histories of newly hired workers with information on their recruitment processes. Compared to previous studies, which often rely on job seekers' search channels, this allows us to explicitly compare outcomes of online and non-online recruits.

There are several mechanisms through which online job search and recruiting tools may improve the match quality of newly hired workers. First, employers may use the internet for online screening, e.g., by restricting online applications only to those candidates possessing the required qualifications. Second, by way of identifying and notifying plausible matches based on matching algorithms, online recruiting software may speed up the process in which employers and individuals find one another. As a result, online tools are likely to raise the number of meetings between employers and individual job seekers per time unit. Theory predicts that a potential increase in matching efficiency will give rise to an increase in the reservation match quality (Krueger, 2000), which will in turn raise productivity, workers' earnings, and firms' profits.

Yet, another hypothesis is that the internet may take a role in reducing the quality of matching due to adverse selection: The underlying rationale is that the dramatic decline

[1] See, e.g., Fountain (2005) and Kuhn and Mansour (2014) for studies at the individual level and Kroft and Pope (2014) and Czernich (2014) at the regional level. Gürtzgen et al. (2018) study the effects of the expansion of high-speed internet on reemployment probabilities of unemployed job seekers in Germany. The authors provide evidence of modest positive effects of the internet on re-employment probabilities, with the effects being most pronounced for male workers after the first four months in unemployment.

in search and application costs may entail excess applications, with many workers applying to considerably more jobs compared with more traditional search channels. Excess applications are likely to be especially relevant for poor candidates, who would not have applied if the submission of their applications had been more costly. This may create additional costs of screening, as employers need to spend more time and effort in establishing the required qualifications of their applicants (Autor, 2001). Overall, these considerations make clear that the direction of the internet's effect on the match quality is generally ambiguous.

Despite a key role in theory, only a small number of studies have empirically analyzed this issue.[2] Moreover, a limitation of the existing studies is that they rely on information on individual job seekers' search strategies. This is problematic as the use of the internet for individual job search does not necessarily imply that individuals were actually hired through online recruiting. The reason is that job seekers may use multiple job search channels.[3]

Based on a combination of administrative and vacancy-level data, our study contributes to this sparse literature by explicitly comparing new matches that were hired through online search and recruiting to those that were formed via different recruitment channels. An additional novelty of our study is that we are able to address the phenomenon of excess applications that may arise with online search, as our data allow us to retrieve information on the number (and fraction) of suitable applicants. A further contribution of our analysis is that we exploit a quasi-experimental setting created by the expansion of high-speed internet (DSL) at the regional level in Germany. The source of variation - described in detail by Falck et al. (2014) - stems from technological peculiarities of the traditional public switched telephone network (PSTN), through which the early generations of DSL have been implemented. The crucial issue causing exogenous variation in DSL availability is that, while the length of the copper wires connecting households and the main distribution frames (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 4,200 meters, such that for municipalities with distances to the next MDF that lie above this threshold no access to DSL was possible. The only way to provide

[2]Using the NLSY from 2005 to 2008, Kuhn and Mansour (2014) find weak evidence of a positive effect of internet job search on wages in the new job of unemployed job seekers. Using the same data source, Prakash (2014) investigates the association between internet job search and tenure and finds that exit rates out of employment are reduced at least by 28% when the internet is used as job search channel. Based on the German Socio-Economic Panel (GSOEP), Mang (2012) addresses potential selection biases by exploiting individual variation in internet usage over time, thereby comparing two matching outcomes of the same worker. Overall, his findings point to higher subjective match quality outcomes when adopting online job search.

[3]The only study that has looked at the match quality effects of the usage of online recruitment strategies by employers has been conducted by Hadass (2004). Using personnel data from a large manufacturing firm, the author finds that internet recruits have shorter job durations than comparable workers hired through employee referrals but similar durations to those hired through print advertising.

internet access was to replace copper wires by fiber wires, which took time and was costly.

Using this exogenous variation in internet availability during the early DSL years, we start by estimating the internet effects on characteristics of job matches at the municipality level. We instrument DSL availability at the municipality level by the distance to the next MDF and measure characteristics of jobs in the flow of newly hired workers coming out of unemployment. In this group, the impact of the internet on outcomes such as employment stability is more likely to capture effects that arise from a change in search channels than in the group of workers searching on-the-job. The reason is, as we will argue below, that the restriction in internet availability prior to the DSL expansion was more binding for unemployed than for employed job seekers. This is especially relevant as estimation at the municipality level only allows for identification of intention to treat (ITT) effects. To explore more direct effects of the *usage* of online recruiting, we additionally link the employment outcomes of newly hired workers to vacancy-level data. Though this comes at the expense of a much smaller sample size, it enables us to obtain a more comprehensive view on the effects. First, employment histories after (online) recruitings are informative on effects on conventional job characteristics and outcomes such as employment stability and wages. Secondly, the vacancy-level data are informative on applications and vacancy creation. This enables us to explore concessions that employers make when choosing a candidate, which may be considered as a measure of job-worker mismatch. For ease of exposition, we refer to the joint set of outcome measures as match quality indicators, where it should be kept in mind that some of these (notably conventional job characteristics) may just reflect job quality rather than match quality.

Based on this empirical strategy, we document the following key findings. The results from the municipality-level analysis suggest that for the full sample the increase in internet availability affects neither employment stability nor wage outcomes. If anything, internet availability reduces employment-to-unemployment transitions for formerly unemployed male and white-collar workers. For these subgroups the effects are found to be non-negligible, but the precision of the estimates is low. The analysis based on vacancy-level data confirms the absence of any effects of online recruiting on wages and provides evidence of a slight increase in employment stability, with the effect being driven by reduced employment-to-employment transitions. Looking at potential mechanisms underlying the absence of major effects, we find that online recruiting leads to a dramatic increase in the number of applications and raises the share of unsuitable candidates, while at the same time inducing employers to post more vacancies. We also find some weak evidence that adverse selection with online recruiting is more relevant for females.

The remainder of the paper is laid out 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. While Section 3 deals with the sources of empirical identification, Section 4 lays out the overall empirical strategy. Section 5 contains the description of the data sources and the sample selection. Section 6 shows descriptive statistics, and Sections 7 and 8 present the empirical results. The final Section 9 concludes.

2 Broadband Internet and Online Search

Broadband internet diffusion. In this study, we exploit the quasi-experimental setting created by the high-speed internet explansion in Germany. The diffusion of high-speed internet in Germany started during the years 2000/01 and was based entirely on digital subscriber line (DSL) technologies (Bundesnetzagentur, 2012). Prior to the high-speed internet expansion, internet access was restricted to low-speed technologies such as modems or integrated services digital network (ISDN). By providing an access speed that is at least six times faster than the old technologies, the expansion of DSL led to a considerable reduction in waiting times for loading websites and downloading files.

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. As of 2005, there existed around 800 online job boards in Germany (Crosswaters, 2005). Among these job boards, the Federal Employment Agency's job board was the most important one, with about 325,000 jobs posted in February 2005, followed by JobScout24 and Monster with about 20,000 jobs. Regarding page views, it was also most frequently used by job seekers, with about 201 million views per month in 2005 compared to 41 million clicks at Monster and 9.2 million clicks at JobScout24 (Grund, 2006). In December 2003, the Federal Employment Agency implemented a new online job board with the main purpose of 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.[4]

Online search among employers. At the employer level, survey results from firm-level

[4] Yet, there is evidence that the new technology was characterized by a couple of inefficiencies at the start of the DSL period. E.g., customers used to stick to the traditional Federal Employment Agency's search engine and 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. Overall, these considerations point to a quite limited usability of the *Jobbörse* at the start of the DSL period.

data show 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 2000s suggests that any restriction in internet access was less binding for employers than for individual job seekers. As a result, the usage of online recruiting tools among employers was already widespread in the mid 2000s in Germany.[5] Still, there is evidence that its importance has continued to increase during the expansion of high-speed internet. Based on the IAB Job Vacancy Survey, Panel (a) of Figure A.1 in Appendix A shows the overall fraction of jobs being posted online among all successful hirings. Panel (b) and (c) show the respective shares broken down by selected occupational categories.[6] The graphs refer to the years 2005 to 2008, which in most studies are considered to be the DSL period in Germany. Three noteworthy facts emerge from these graphs: First, the fraction of jobs posted online increased by about 15% points from 2005 to 2008 (Figure A.1 Panel (a)). Second, in terms of levels, the fraction of jobs being posted online is larger for more skilled white-collar occupations (Figure A.1 Panel (b)) than less skilled or blue-collar occupations (Figure A.1 Panel (c)).[7] Third, the graphs also illustrate that the first group of occupations experienced an increasing trend in online recruiting during this time period, whereas the relevance of online recruiting for the latter group rather remained constant. Overall, these figures provide some first evidence on an important selection issue, namely the type of jobs being posted online. This is of particular relevance, as the jobs individuals search for online might systematically differ from those job seekers search for via alternative search channels, which, in turn, might be correlated with our match quality outcomes of interest.

Online search among job seekers. Evidence from a survey among individual job seekers shows that the share of individuals preferring online over print applications rose from 48 to 88% between 2003 and 2014 in Germany (Weitzel et al., 2015). Using information from the GSOEP, Thomsen and Wittich (2010) document an increase in the share of unemployed job seekers searching online from 37% in 2003 to 53% in 2007. Based upon the same data set, Mang (2012) shows that the fraction of job changers who found a new job via the internet was in the year 2007 six times as high as in 2000. Adopting a similar IV approach as in this paper, Gürtzgen et al. (2018) explicitly address the question whether an increase in internet availability at home increases the use of the internet for job search. Using survey data from the PASS Survey (Panel of Labour Markets and Social Security),

[5] According to a survey among 1,000 large German employers, the fraction of vacancies that were advertised on the surveyed companies' websites (via job boards) amounted to 85% (52%) in 2005 and rose to 90% (70%) in 2014, respectively. Moreover, among the surveyed companies the fraction of hires that resulted from online recruiting was 50% in 2005 and rose to over 70% in 2014 (König et al., 2005, Weitzel et al., 2015).

[6] For a description of the IAB Job Vacancy Survey, see Section 5.1.

[7] 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 wells as agricultural jobs.

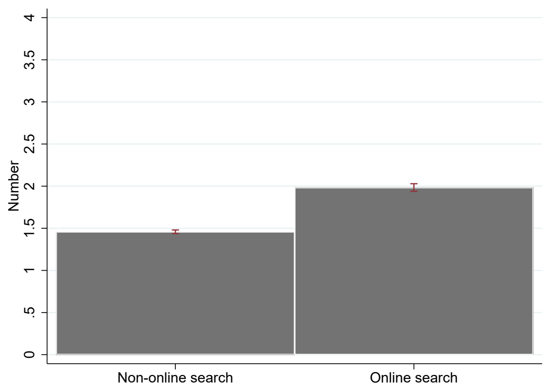
the authors show that internet access at home raises the incidence of online job search by about 67% points.

Search and application intensity. The dramatic decline in search and application costs brought about by the internet may alter job seekers' and employers' search intensity, either by potentially crowding out or by raising the use of other (non-online) search channels. For individual job seekers, Gürtzgen et al. (2018) show, based on the PASS data, that online search does not crowd out other search channels and raises the number of search channels for certain socio-demographic groups. A similar result emerges for employers. Based on the IAB Job Vacancy Survey, Panel (a) of Figure 1 illustrates that searching online for candidates does not crowd other search channels. The figures show that online search is associated with a significantly larger number of non-online search channels. An increase in search intensity does not necessarily imply, though, that employers receive more suitable applications than with non-online search channels. Panel (b) and (c) of Figure 1 show the number of applications and the share of unsuitable applications that employers receive with different recruitment channels. The recruitment channel is defined as the successful search channel, as employers may in general adopt multiple search channels when looking for a candidate. Two noteworthy facts emerge from these figures: First, online recruitment is associated with a significantly larger number of applications and, second, with a higher share of unsuitable candidates. While these findings provide evidence of higher search intensity among employers and, to some extent, also among job seekers with online as compared to non-online search, they also highlight the potential relevance of excess applications that may arise when searching online. In Section 8.2.1, we will turn to a multivariate and causal approach, when assessing the relevance of this phenomenon.

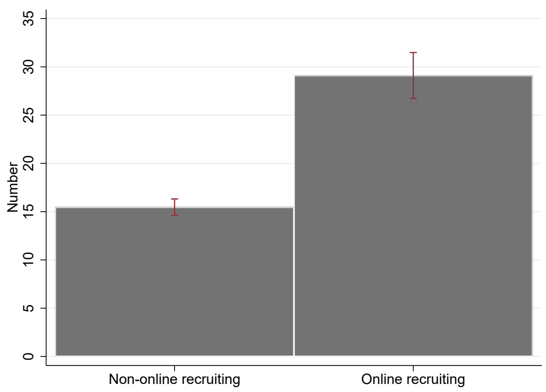
3 Identification

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 (in our case: municipalities) with high-speed internet access are likely to differ from those with low-speed internet access along many dimensions. For instance, 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 differences at the regional level are likely to be correlated with the expansion in high-speed internet.

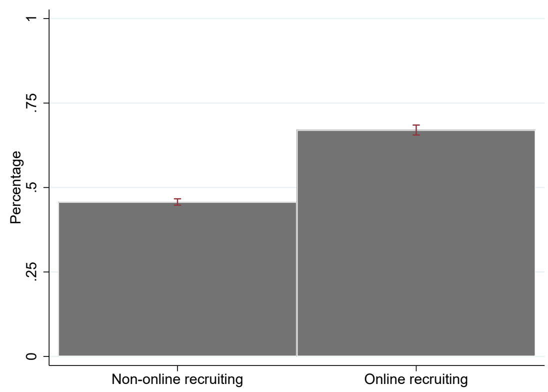
To address these concerns, we exploit exogenous variation in broadband internet availability at the municipality level, as suggested by Falck et al. (2014). This variation stems from technological peculiarities of the traditional public switched telephone net-



(a) Number of non-online search channels



(b) Number of applicants



(c) Share of unsuitable applicants

Notes: For a description of the IAB Job Vacancy Survey, see Section 5.1. The data collect information on establishments' last successful hiring process, in particular information on search and recruitment channels, the overall number of applications and the number of suitable applications. Whether applications are judged as being suitable for the position to be filled, is based on a self-assessment by the survey's respondents. Search channels refer to the channels that employers adopted when looking for a candidate, whereas recruitment channels refer to the successful search channel.

Figure 1: Search and application intensity

work, through which the early generations of DSL have been implemented. As described in Falck et al. (2014) and Steinmetz and Elias (1979), early DSL availability relied on the use of the copper wires that connected households with MDFs. The latter were originally built in the 1960s to provide voice-telephony services, which implies that the choice of their location was made long before the DSL expansion took place. While agglomerated municipalities usually own at least one MDF, less agglomerated municipalities often share one MDF. Hosting a MDF required the acquisition of lots and buildings, such that the spatial distribution of MDFs in rural areas was mainly determined by the availability of such facilities. The critical issue for identification is that the distance between the next MDF and the individual household did not influence the quality of the telephone connection, whereas it matters for the DSL connection since the length of the copper wires affects the available bandwidth. In particular, if the distance exceeds a critical value of 4,200 meters, no access to DSL was possible and the only way to provide high-speed internet was to replace copper wires by fiber wires (Falck et al., 2014).[8] Since the copper wires ran underground, such a replacement took time and came along with high costs. In what follows, we use the exogenous variation in DSL availability created by the spatial distribution of MDFs in order to explore the effects of the broadband internet expansion on labor market outcomes for municipalities without an own MDF. In this way and by controlling for MDF-fixed-effects, we ensure that we compare similar municipalities which are "by chance" close enough (lucky municipality) or too far away (unlucky municipality) to have, on average, access to DSL. In particular, we concentrate on West German municipalities that are connected to a MDF located in one of their neighboring municipalities and where no closer MDF is available.[9]

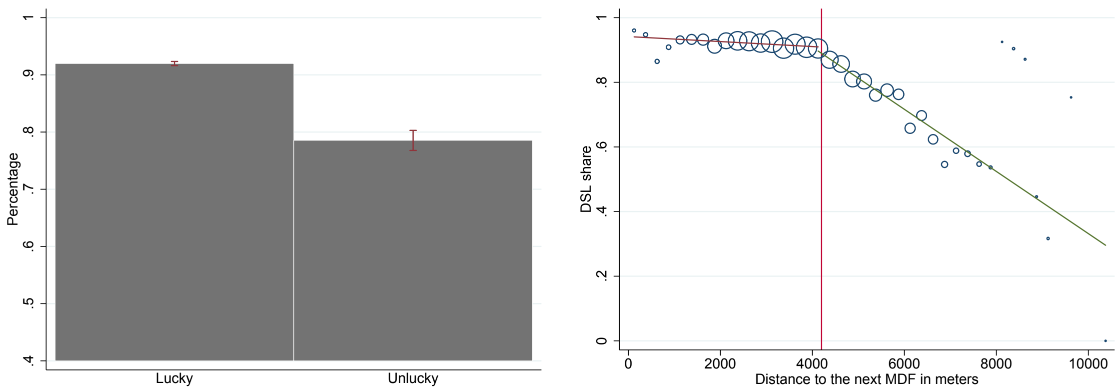
To predict DSL availability, we construct an instrumental variable based on the distance from each municipality's regional centroid to the location of the next MDF. We label municipalities with a distance below the threshold of 4,200 meters as *lucky* ones and municipalities with a distance above the threshold as *unlucky* ones.[10] To illustrate the difference in DSL availability rates among lucky and unlucky municipalities, Figure 2 Panel (a) plots the mean fraction of households having access to DSL in 2007 and 2008. The Figure shows that in municipalities with distances to the next MDF of less than 4,200 meters the share of households for whom DSL is available amounts to about 92%, while the share drops considerably to about 78% in municipalities with larger distances. The latter group of municipalities exhibits larger variation in broadband internet availability, which is indicated by larger confidence intervals.

Figure 2 Panel (b) shows the fraction of households with DSL availability plotted against the distances to the next MDF. The size of the circles corresponds to the number of municipalities within 250 meters bins. Lucky municipalities exhibit a fairly constant DSL share, while the share declines monotonically with higher distances once the threshold distance is surpassed. Note that the observed distribution does not allow for a regression kink design because there is no policy rule that generates the distribution. From a technical perspective, only the 4,200-meters threshold determines overall DSL availability while the distance determines the available bandwidth (Prieger and Hu, 2008). The actual distance might, however, be correlated with unobserved municipality characteristics that are correlated with match quality outcomes. For that reason, using the distance as an instrument may violate the exclusion restriction (see also argumentation in Falck et al.,

[8] The authors argue that the threshold distance is determined by the lowest downstream data transfer rate of 384 kb/s at which German telecommunication providers (Deutsche Telekom) market DSL subscriptions and the maximum allowed line loss of 55 dB to reach this minimum data transfer rate. With a line loss of 14dB/km, the threshold distance is reached at 4,200 meters.

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

[10] Roughly one third of the municipalities used in our analysis are unlucky municipalities.



(a) Broadband by treatment *(b) Broadband by distance*

Notes: The figures plot the fraction of households with broadband internet (DSL) availability for lucky and unlucky West German municipalities between 2007 and 2008. 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 (MDF). The size of the circles in Panel (b) corresponds to the number of municipalities within 250 meters bins. The figures are based on the German municipalities used in the empirical analysis.

Figure 2: Share of households with DSL availability

2014). Some indication for such a violation is provided by some municipalities with a large distance to the next MDF, which at the same time feature relatively high DSL shares.[11] To address potential violations of our identification strategy for these municipalities, we perform robustness checks by excluding these outliers.

4 Empirical Model

In our empirical analysis, we start by estimating the effects of broadband internet availability on match quality outcomes at the municipality level. To do so, we use an inflow sample of new hires out of an unemployment spell which started in period t (see Section 5) and regress the change in outcome variables on the change in DSL availability and control variables in municipality i. The estimation equation is given by the following expression:

$$\Delta y_{it} = \beta_0 + \beta_1 \cdot \Delta DSL_{it} + \Delta X'_{it} \cdot \beta_2 + MDF_i + \Delta\epsilon_{it} \tag{1}$$

In this specification Δ_t denotes changes from a defined pre-DSL period to a defined DSL period. In our empirical analysis, each period, t, covers two years (pre-DSL: 1998/1999; DSL: 2007/2008), which will be pooled for either period. The change in outcomes is given by Δy_{it}, in DSL availability by ΔDSL_{it} and in a vector of covariates by ΔX_{it}. ϵ_{it} is an idiosyncratic error term. Moreover, we introduce MDF-fixed effects (MDF_i), thus comparing two municipalities that are connected to the same MDF but that differ

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

in their distance to the MDF. Given that DSL availability is zero in the pre-DSL period, eq. (1) regresses the change in the corresponding outcome variable on the actual level of households with DSL availability (DSL_i).

As discussed earlier, the empirical model in eq. (1) might be subject to some endogeneity issues. To account for time-varying unobserved effects that are correlated with both labor market outcomes and DSL availability at the municipality level, we adopt an instrumental variable (IV) strategy and identify a local average treatment effect for the compliant municipalities. We use as a (time-constant) instrument the distance from each municipality's center (population-weighted) to the next MDF. The first-stage can be written as:

$$\Delta DSL_{it} = \gamma_0 + \gamma_1 \cdot PSTN_i + \Delta X'_{it} \cdot \gamma_2 + MDF_i + \Delta\psi_{it}, \tag{2}$$

with $PSTN_i$ denoting a dummy variable that takes on the value of unity for unlucky (treated) municipalities, i.e. for municipalities with distances above the threshold of 4,200 meters.

In the second part of our empirical analysis, we proceed by merging the employment histories with information of our vacancy-level data. As set out earlier, this combination allows us to track the employment histories of new hires along with information on their recruitment process. We use this information to compare various outcomes of new workers who were hired through online search and recruiting to the outcomes of those who were hired via different (non-online) recruitment channels. To do so, we estimate the following equation:

$$y_j = \beta_0 + \beta_1 \cdot Online_j + X'_j \cdot \beta_2 + \epsilon_j, \tag{3}$$

with y_j denoting various outcomes of the recruitment process and match quality outcomes and X_j denoting a vector of establishment-, vacancy- and employee-specific characteristics. $Online_j$ is an indicator variable taking on the value of unity if a new employee was hired through online-recruiting in the recruitment process j. We instrument this indicator again by the indicator variable $PSTN_j$. Given that there is evidence that the lack in internet availability provided a larger restriction for individuals than for employers, the indicator refers to the distance of the centroid of the hired person's home municipality to the next MDF. While this specification allows us to directly estimate an effect of the usage of online recruiting on our outcomes, y_j, it comes at the cost of a considerably smaller sample size. Moreover, as we are not able to exploit the survey data for the pre-DSL period, this approach does not allow us - other than in the municipality-level analysis - to net out differences in outcomes between matches of employers in lucky and unlucky municipalities that already existed prior to the DSL expansion.

Note that the identifying assumption for using $PSTN_j$ as an instrument for online recruiting presumes that our outcomes are affected by the distance to the next MDF only through the impact of $PSTN_j$ on online recruiting. Especially for our match quality outcomes, this assumption may be violated for two reasons. First, the distance to the next MDF may directly affect employment stability by reducing on-the-job search costs and thereby increasing job-to-job mobility. Second, the distance may directly affect wages and the probability of terminating a job by altering the value of unemployment. As to job-to-job mobility, Gürtzgen et al. (2018) show that the increase in internet availability had no effect on the probability of job-to-job mobility at the municipality level.[12] This finding is consistent with the fact that a wide majority of employers had already internet access prior to the DSL expansion (see Section 2). This implies that pre-DSL many employed individuals could access the internet via their workplace, such that the distance to the next MDF was unlikely to substantially alter their on-the-job search costs.

The second potential threat to the validity of the instrument is that the distance to the next MDF directly affects employment stability and wages, by raising the value of unemployment. Earlier evidence from Gürtzgen et al. (2018) suggests that increased internet availability has positive effects on re-employment prospects among male unemployed job seekers. However, the extent to which a higher value of unemployment affects our outcomes of interest depends on whether the latter is internalized by unemployed job seekers. If this were the case, home internet access due to a closer proximity to a MDF should lead to larger reservation wages. In Appendix B, we provide supplementary evidence based on individual-level survey data, suggesting that internet access does not lead to higher individual reservation wages. Overall, these findings provide support for the assumption that the exclusion restriction is fulfilled.

5 Data and Sample Selection

5.1 Data

Our empirical analysis makes use of different data sources. The information on high-speed internet availability at the municipality level stems from the broadband atlas (*Breitbandatlas Deutschland*), an annual survey published by the Federal Ministry of Economics and Technology. In this survey, telecommunication operators self-report covered households with a minimum data transfer rate of 384 kb/s. For these households a digital subscriber line technology (DSL) connection is technically available. The municipality-specific DSL share is measured as the percentage of households in a municipality for whom DSL is available. The survey data is available for the universe of German municipalities (in

[12]The authors address this issue in the context of spillover effects of employed versus unemployed job seekers. To isolate mobility effects and to rule out potential match quality effects of the internet, the authors confine their analysis to a stock sample of employment relationships that had already started prior to the DSL-period. This is important, because with internet availability being the main source of variation, job-to-job transitions may be affected via the internet's effect on mobility, i.e. the effect of the internet on finding a subsequent better job, and via the internet's effect on match quality.

2008 territorial boundaries) from 2005 to 2008, which was the main introduction phase of broadband technology in Germany (Falck et al., 2014).

Although broadband availability is measured at the household level, it might be conceivable that DSL effects capture some potential demand-side dynamics. For instance, higher DSL availability might affect the dynamics of firm entries and exits. A rise in labor demand induced by high-speed internet availability might change the employment outcomes, such as wages and employment stability, of individuals independent of online search. In our empirical analysis, we therefore include demand-side controls in order to isolate the effect of online job search from potential demand-side effects. Using data provided by the *Mannheim Enterprise Panel* (MUP), we retrieve information on total sales and on the number of firm exits and entries at the municipality level.[13] We further include establishment information such as the total number of establishments and establishment size provided by the *Establishment History Panel* of the Federal Employment Agency.

The main match quality outcomes used in this study are based on German register data. The Integrated Employment Biographies (IEB) of the Federal Employment Agency provided by the IAB provide an ideal data basis for analyzing the internet effects on wages and employment stability (for detailed information of a sub-sample of this data set, see e.g. Oberschachtsiek et al. (2009)). The IEB are based on employer notifications to the social security system and are available for 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. This represents about 80% of the German workforce. Periods of self-employment, civil service and military service are not included. This administrative data set provides detailed information on individual employment histories including spells of employment subject to social security contributions, of unemployment with transfer receipt and of job search on a daily basis. This allows us to construct precise measures of employment and unemployment durations and transitions between labor market states. To define periods of (involuntary) unemployment, we follow Lee and Wilke (2009) and only consider periods of registered job search and/or transfer receipt without a parallel employment relationship. Further information on the definition of different labor market states can be found in Table C.2 in Appendix C.

As a final data source, we use the IAB Job Vacancy Survey. This data set 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. The data are available from 1989 onwards, with the most recent waves covering about 15,000 establishments. Starting from 2009, it is possible to merge the data with a municipality identifier from the administrative employment history panel.

[13] The data set covers the universe of firms in Germany including a municipality identifier starting in the year 2000. Thus, we use the year 2000 as the pre-DSL year.

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 hiring process. This information includes individual characteristics of the hired employee and characteristics of the specific position to be filled. The data also 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.

5.2 Sample Selection

We conduct the first part of our empirical analysis at the municipality level, with the availability of broadband internet being the main explanatory variable of interest. As set out earlier, our estimation at the municipality level allows us to identify only an intention to treatment effect (ITT). Without having explicit knowledge about whether the internet was actually used for online recruiting of the new hires, the internet's effect on employment stability may theoretically reflect both, match quality as well as mobility effects. Note, however, that Section 4 argues that such mobility effects are unlikely to play a major role. One important argument is that the DSL expansion was less likely to substantially alter employed job seekers' on-the-job search costs, as the lack in internet availability provided a larger restriction for unemployed than for employed individuals. In what follows, we therefore focus on formerly unemployed new hires in the municipality analysis in order to focus on those job seekers for whom the lack in internet availability was most binding.

To do so, we draw on the universe of unemployed individuals who experienced at least one unemployment spell in the above defined subset of municipalities during 1998 and 2008. The data allow us to follow these individuals until 2014.[14] The estimation sample then consists of the universe of individuals entering a new employment relationship after an episode of unemployment that started in each single year during the pre-DSL and DSL period, indexed by t. To avoid the overlapping of the pre-DSL and the DSL period, we only look at individuals with an unemployment-to-employment transition up to the end of the year 2002 for the pre-DSL period and 2011 for the DSL period. In doing so, we ensure that the period during which unemployed job seekers were looking for a job referred either to the pre-DSL or the DSL period. In what follows, this sample will be referred to as the *hires inflow sample*.

As a further sample restriction, we drop individuals in industries with a-priori high recall rates (larger than 50 per cent) from our sample.[15] The reason is that recall options typically involve the return to the previous employer, which does not require search effort, such that fast internet is unlikely to affect reemployment and match quality outcomes

[14] In addition, we draw on a random 50%-sample of employed individuals living in the above defined subset of municipalities.

[15] Due to the endogeneity of recalls, we refrain from conditioning on this outcome at the individual level.

among recall workers. We further exclude individuals who were less than three months employed before they became unemployed in order to make sure that individuals do not systematically switch from unemployment to employment and vice versa. This might be the case for workers in seasonal jobs, in jobs with high recall rates or for temporary workers. These workers might be less likely to search actively for a job. To calculate meaningful averages at the municipality level, we further condition the sample on observing at least five individuals per year and municipality in our final hires inflow sample.[16] Due to this condition, the final sample of municipalities (3,054) covers 91% of all available municipalities (3,339) without an own MDF. Next, we use a municipality identifier to link administrative information to information from other data sources (see Table C.1 in Appendix C) to complete our set of outcome and control variables.[17]

In our empirical specification, the years 1998 and 1999 are defined as pre-DSL years and the years 2007 and 2008 as DSL years. We concentrate on these later DSL years as in particular less agglomerated municipalities were still in a transition phase with regard to the adoption of these new digital information technologies.[18] Moreover, online search and recruiting technologies appear to have become more efficient as time evolved. For instance, there is some evidence for a quite limited usability of the job board of the Federal Employment Agency at the start of the DSL period (Bieber et al., 2005). Some further evidence for improvements of the underlying technologies is given by the increasing importance of online recruiting among employers. Figures from the IAB Vacancy Survey show that the fraction of hirings that were preceded by online recruiting increased from about 45% to over 60% between 2005 and 2008 (see Figure A.1 in Appendix A).

To estimate eq. (3) in the second part of our empirical analysis, we combine information on the most recent hiring process of the IAB Job Vacancy Survey with information from the IEB. Tracking the most recent hire's employment history after their recruitment, we retrieve information on the same match quality outcomes at the individual level as described in the next section as well as several recruitment outcomes as described in Section 8 (see also Appendix G). From the IAB Job Vacancy Survey, we use the waves 2009 and 2010, since these are the earliest years for which a record linkage with the administrative employment biographies is available.

[16] In our estimation sample, we observe for 93% of the unemployed individuals a unemployment-to-employment transition in the DSL period and for 91% in the pre-DSL period. These figures are sufficiently high to rule out that results are strongly affected by selectivity due to interaction effects between internet availability and unobserved systematic determinants of the transition to work, on the occurrence of employment before the end of the observation window. Notice that the occurrence of subsequent events before the end of this window may be susceptible to such selection issues (see, e.g., Abbring and van den Berg (2005), for details).

[17] 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.

[18] See Gürtzgen et al. (2018) for more details on why we concentrate on this DSL period.

5.3 Municipality-Level Outcome Variables

To estimate the effects of broadband internet on match quality outcomes, we consider a variety of outcomes. First, we compute employment stability measures as the municipality-specific shares of individuals reentering unemployment or changing the employer within m months after the start of the employment spell. The state that we call "nonparticipation" (see Table C.1 in Appendix C) is the third (and residual) exit destination. We calculate these fractions relative to the number of individuals at risk, i.e. those who are still employed. These probabilities are defined as the complement of the survival function, which is estimated by the non-parametric Kaplan-Meier estimator.[19]

Second, we analyze effects on wages. In particular, we consider the municipality-specific mean entry wage that new hires earn in their new job following an unemployment spell, the mean difference in wages before and after the unemployment spell and the mean wage growth after one year of tenure.

6 Descriptive Statistics

Municipality-level variables. Table 1 shows descriptive statistics for the subset of municipalities without an own MDF for the pre-DSL and the DSL period, separately for lucky and unlucky municipalities. The figures indicate that during the years 2007 and 2008 the share of households who had in principle access to DSL was 78% in unlucky municipalities and 92% in lucky municipalities. In addition to broadband internet information, Table 1 and Table D.1 in Appendix D show the main set of control variables used in the empirical analysis.[20]

Panel B of Table 1 documents some significant differences in observable characteristics between unlucky and lucky municipalities for the pre-DSL years, such as the population share aged 18-65, the average daily wage level, the skill structure and the share of foreign nationals. However, these differences are also visible for the DSL years. Given that our empirical analysis is based on a difference-in-differences approach, we also display differences by treatment status and period conditional on MDF-fixed effects (column (7)). Here, the findings point to different developments of the local age composition, the share of foreign nationals in the hires inflow sample and the share of production workers and employees in the construction sector (see Panel A and B of Table D.1 in Appendix D).

[19]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 with a transition in month m_i and n_i is the total number of individuals at risk during the time interval $[m_i, m_{i+1}]$.

[20]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 those entering employment after an unemployment spell 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 Appendix C).

Table 1: Descriptive statistics

	pre-DSL years 1998/99			DSL years 2007/08			Diff-in-Diff
	Unlucky	Lucky	Diff[a)]	Unlucky	Lucky	Diff[a)]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Broadband availability							
	0.000	0.000	0.000	0.777	0.919	-0.118***	-
Panel B: Municipality characteristics							
Population	1,371.554	1,261.935	-48.790	1,380.760	1,270.442	-52.209	-3.419
Female population share	0.499	0.501	-0.002*	0.501	0.502	-0.001	0.001
Population share aged 18-65	0.656	0.659	-0.004***	0.610	0.621	-0.001	0.003**
Population share > 65	0.162	0.163	0.002*	0.186	0.188	0.000	-0.002**
Net migration rate	0.004	0.005	0.000	-0.002	-0.001	-0.001	-0.001
Unemployment rate	0.039	0.041	0.000	0.039	0.041	0.001	0.000
Average real daily wage	95.865	97.778	-0.609**	96.667	98.662	-0.520	0.090
Low-skilled	0.171	0.169	-0.002	0.149	0.153	-0.002	-0.001
Medium-skilled	0.778	0.774	0.004**	0.783	0.773	0.005***	0.001
High-skilled	0.051	0.057	-0.002**	0.068	0.074	-0.003**	-0.001
Foreign nationals	0.022	0.026	-0.002**	0.021	0.025	-0.002**	0.000
Panel C: Demand-side characteristics							
Number of establishments	29.685	27.767	-2.032	40.662	37.551	-2.137	-0.105
Establishment size	6.159	6.074	-0.448*	5.984	6.015	-0.508**	-0.059
Number of firm entries	2.734	2.512	-0.085	2.288	2.073	-0.021	0.065
Number of firm exits	1.775	1.761	-0.153	3.319	3.208	-0.005	0.148
Sales	17.877	33.370	-10.093	54.712	55.739	-11.017	-0.924
Panel D: Hires inflow characteristics							
Age	33.839	33.817	0.046	34.908	34.889	0.047	0.002
Female share	0.361	0.385	-0.002	0.446	0.451	0.002	0.004
Low-skilled	0.190	0.197	-0.005	0.215	0.224	-0.003	0.002
Medium-skilled	0.775	0.761	0.006	0.739	0.725	0.008	0.001
High-skilled	0.035	0.042	-0.002	0.046	0.051	-0.005*	-0.003
Foreign nationals	0.034	0.038	0.001	0.028	0.036	-0.007***	-0.007***
Number of individuals in hires inflow sample	51,976	79,518		46,864	80,329		
Number of municipalities	1,030	2,024		1,030	2,024		

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 Appendix D. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Panel C of Table 1 shows our measures of demand-side 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 figures show that less firms entered and more firms exited the market, while total sales increased over time. Importantly, the development in lucky and unlucky municipalities was similar as shown by column (7). Panel D of Table 1 displays the main characteristics of the hires inflow sample. The average age is around 34 in the pre-DSL period and exhibits a slight increase over time. The same pattern is observed for the share of females among those entering employment after a period of unemployment. Moreover, as expected, low-skilled individuals and foreigners tend to be disproportionately represented in the hires 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 hires inflow characteristics). However, the share of foreigners tends to fall over time, with the decrease being more pronounced in unlucky municipalities.

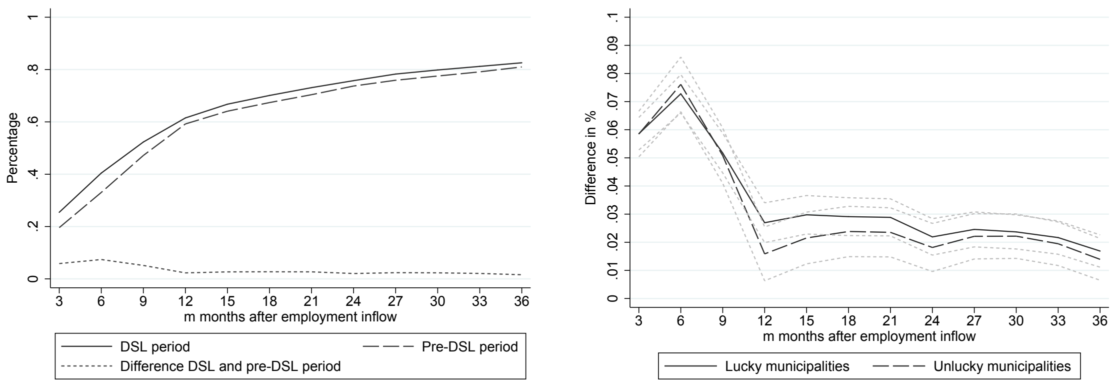
Match quality outcomes. Figure 3 shows the raw differences in the probabilities of leaving a new employment relationship. Panel (a) of Figure 3 shows the differences across the pre-DSL and the DSL period. The bottom line plots the difference between the two upper graphs against time. Overall, this line illustrates that during the DSL years the cumulative probability of leaving a new employment relationship became slightly larger than during the pre-DSL period. The difference is largest during the first nine months, where the cumulative probabilities of leaving employment increased, on average, by 5% points.[21]

Turning to the differences between lucky and unlucky municipalities, Panel (b) of Figure 3 shows that the increase in the probability of leaving employment during the first ten months is fairly similar in both groups. After about one year, individuals in lucky municipalities exhibit a slightly larger increase in the cumulative probability of leaving a new employment relationship than those in their unlucky counterparts, with the differences being statistically insignificant.

7 Empirical Results

Baseline effects. We start our regression analysis by looking at differences in outcomes between the pre-DSL years (1998/99) and the DSL years (2007/08). Figure 4 displays

[21] To make sure that employment spells of the pre-DSL hires inflow sample do not overlap with the DSL period, we do not follow spells that are ongoing at the introduction of high-speed internet. Given that our latest pre-DSL (DSL) inflows into employment occur in 2002 (2011), the maximum duration for which we can follow all job inflows is three years. To avoid relying on small subsamples, we do not follow employment spells beyond the first three years.



(a) Overall *(b) Difference by treatment*

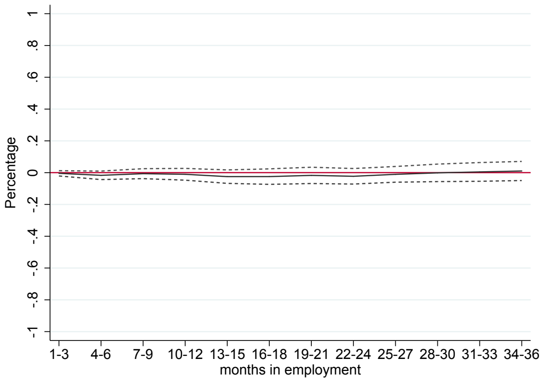
Notes: Panel (a) plots the cumulative probability of leaving a new employment relationship within m months for an inflow sample of formerly unemployed individuals who entered a new employment relationship averaged at the municipality level, distinguishing between the DSL (2007/08) and the pre-DSL (1998/99) 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.

Figure 3: Probability of leaving a new employment relationship and difference between lucky and unlucky municipalities

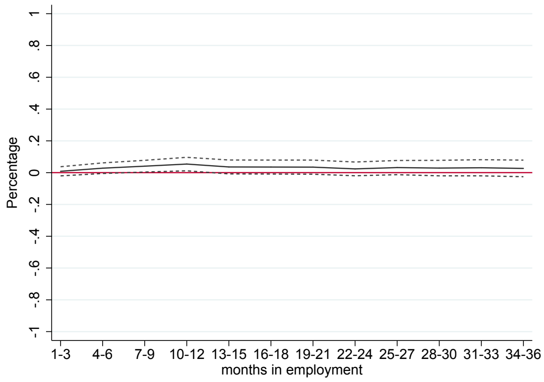
the estimated effects of a 1% point increase in the municipality-specific share of households with DSL availability on the cumulative probability of leaving employment within m months after entering a new employment relationship. The upper graphs show the ordinary least squares (OLS) estimates of the first difference model controlling for observable characteristics and MDF-fixed effects. Overall, the OLS coefficients are close to zero and not significant at any conventional levels during the first three years of tenure. Looking at the employment-to-unemployment transitions, the DSL effects are significantly positive during month 7 to 12. The lower graphs show the IV-estimates. The Kleibergen-Paap F-Statistic is 52 and the first stage treatment coefficient indicates that unlucky municipalities have, on average, 6% points lower DSL rates. The IV-estimates are characterized by larger standard errors and point to negative but insignificant point estimates particularly for the employment-to-employment transitions after the first nine months after entering employment.

Table 2 shows the estimated effects on wage outcomes for the full sample. While the IV-DSL effects on entry wages and wage growth compared to the previous job are positive, the estimated coefficients are not significant at conventional levels. Individuals in lucky municipalities have a 2.4% larger entry wage and experience a 3% larger wage growth compared to their unlucky counterparts.[22] The estimated effects on wage growth for those working full-time are even smaller, suggesting that much of the effect on wage growth (Column (2)) is driven by individuals changing working time. Overall, the estimates in

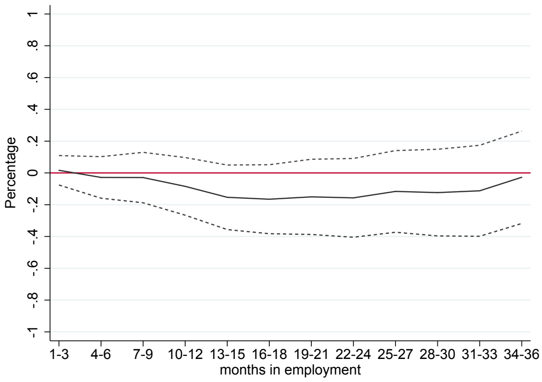
[22]Because the unconditional difference in DSL rates between lucky and unlucky municipalities (shown in Figure 2) is roughly 14% points, the estimates need to be multiplied by 14 in order to arrive at an interpretation that reflects the difference in DSL rates between lucky and unlucky municipalities.



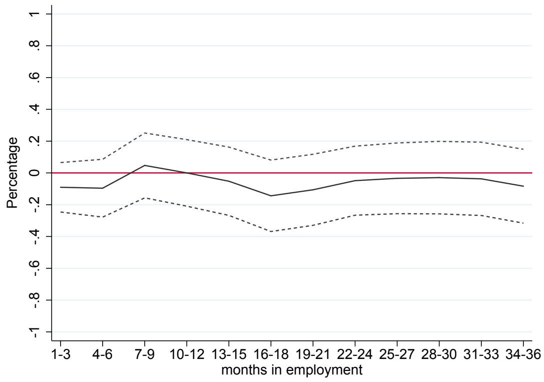
(a) *Employment-to-employment, OLS*



(b) *Employment-to-unemployment, OLS*



(c) *Employment-to-employment, IV*



(d) *Employment-to-unemployment, IV*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probabilities from employment to unemployment and from employment to employment within m months for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. Figures (a) and (b) plot the OLS coefficients. Figures (c) and (d) show coefficients from the IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1 in Appendix C). The regressions are population-weighted and performed separately for each month. Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust and clustered at the MDF level. Regressions are based on 3,054 municipalities and 853 MDFs. The Kleibergen-Paap F-Statistic for the first stage in Figures (c) and (d) is 52.24.

Figure 4: Regression results of DSL on leaving new employment relationships

Table 2 indicate that in terms of wages there appear to be no discernible effects of the DSL expansion on the match quality of new hires.

Table 2: Regression results of DSL on wage outcomes

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Panel A: OLS				
Δ DSL	-0.0001	0.0001	0.0002	0.0000
	(0.0003)	(0.0003)	(0.0002)	(0.0003)
Panel B: IV				
Δ DSL	0.0017	0.0022	0.0006	0.0012
	(0.002)	(0.002)	(0.001)	(0.001)
Threshold (first stage)	-5.998***	-5.998***	-6.001***	-5.942***
	(0.830)	(0.830)	(0.830)	(0.833)
F-Statistic	52.24	52.24	52.30	50.91
Number of municipalities	3,054	3,054	3,048	3,033
Number of MDF's	853	853	851	852

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. Panel A shows the OLS coefficients. Panel B shows the coefficients from the IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1 in Appendix C). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

7.1 Heterogeneous Effects

In this subsection, we test whether the internet expansion has heterogeneous match quality effects on subgroups of workers. Previous evidence for Germany addressing search outcomes of the unemployed suggests that the internet expansion mainly benefits male workers, who experienced the most pronounced increase in their job finding prospects (Gürtzgen et al., 2018). We break down the sample by gender and age, by distinguishing young ($\leq$ 35 years) and old workers ($>$ 35 years). In light of the descriptive evidence presented in Section 2, suggesting that vacancies for more skilled and white-collar occupations were more likely to be advertised online, we also look at heterogeneous effects for these occupations.[23]

Turning first to our employment stability outcomes, the OLS-estimates presented in Figure E.1 in Appendix E overall suggest zero to negative DSL effects on employment-to-employment transitions and zero to positive effects on employment-to-unemployment transitions. For males and skilled white-collar workers the IV-estimates on the employment-to-employment transitions tend to be negative throughout the majority of months after

[23]For the sake of expositional brevity, we show the subgroup effects for males, young workers as well as skilled white-collar workers and suppress the results for their counterparts. Skilled white-collar workers are skilled individuals (who have completed a vocational training or hold a university degree/technical school degree) with a previous *white-collar* job, with the latter comprising higher clerks, service, clerical or sales occupations.

entering employment but are not significantly different from zero at conventional levels (see Figures E.2 (a) and (e) in Appendix E). Figure E.2 (b) shows that compared with the estimates from the pooled sample, male workers experience a more pronounced decline in the cumulative probability of an employment-to-unemployment transition, with the IV-estimates being significant at the 10% level during the first 18 months of tenure. In terms of magnitude, assigning a male worker from an unlucky to a lucky municipality reduces his cumulative probability of entering unemployment by 5% points (0.36 $\times$ 14) on average after a tenure of 15-18 months.[24] A similar picture emerges for young workers, with the estimated coefficients being statistically significant at the 10% level during month 13-18 after the inflow into employment (Figure E.2 (d)). For skilled white-collar workers, we obtain larger negative coefficients of about 0.6% points from month 16 onwards, which are significant at the 5% level. However, we find no significant effects of DSL on the overall exit probability comprising exits to unemployment, employment and nonparticipation for the whole sample and the defined subgroups.[25]

Turning to the wage effects, Table E.1, E.2 and E.3 in Appendix E show the results after breaking down the specifications by gender, age and skill groups. Similar to the estimates for the pooled sample, there appear to be no discernible DSL effects on wage outcomes among all subgroups with the exception of the effects on wage growth after one year for skilled white-collar workers: the results in Table E.3 suggest that wage growth after one year is 7.7% higher in lucky than in unlucky municipalities.

7.2 Robustness Checks and Placebo Tests

In this section, we conduct some robustness checks. Recall that for our baseline estimates we excluded industries with a-priori high recall rates from our sample. The reason is that recall options typically involve a (pre-determined) return to the previous employer. Such a return does not require search effort, such that fast internet is unlikely to affect reemployment and the match quality outcomes among recall workers. To check the sensitivity of our results with respect to this exclusion, we repeated all specifications after including these industries in our sample. Turning first to our employment stability outcomes, Figure F.1 in Appendix F shows that for males and skilled white-collar workers the DSL effects are slightly smaller in absolute terms but similar to the baseline results. Table F.1, F.2 and F.3 in Appendix F show the estimates of the wage effects, broken down by gender, age and skill groups. Similar to our baseline estimates, the results indicate that there appear to be no discernible effects of the DSL expansion on wage outcomes of new hires.

As a second robustness check, we re-estimated our specifications by excluding municipalities, which are characterized by relatively large distances to the next MDF and, at

[24] As spelled out above, the unconditional difference in DSL rates between lucky and unlucky municipalities (shown in Figure 2) is roughly 14% points.

[25] The results are available on request.

the same time, exhibit large DSL rates (see Figure 2). The estimates on the transitions are shown in Figure F.2 and the estimates on wages in Tables F.4 to F.6 in Appendix F. The results suggest that the overall pattern of results remains unaltered, while the DSL effects on wage growth after one year become slightly larger.

A further threat to our identification strategy may stem from agglomeration forces. For instance, locations further apart from a MDF might belong to smaller labor markets. However, 86% of our treated and control municipalities connected to the same MDF share the same labor market as defined by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR). The average distance between all municipalities in our estimation sample within the respective MDF area is about 5.2 kilometers. Moreover, the population of our sample municipalities accounts for 0.2% of the population of their labor market regions. Hence, one may view our setting as having small units that are unlikely to change general conditions in their local labor market. As a further robustness check, we further control for the proximity to the next urban center. The main findings are unaltered (see Figure F.3 and Tables F.7 - F.9 in Appendix F).

Our estimation results at the municipality level are further robust to different empirical and sample specifications, which are available upon request.[26] For instance, our results remain unaltered after excluding demand-side controls (i.e. firm entries and exits, the size of firms and total sales) or including the levels of control variables in the pre-DSL period.

Finally, we conduct placebo tests to compare time trends across lucky and unlucky municipalities during the pre-DSL period. To do so, we analyze the effect of the treatment dummy on the change in employment durations and wages between 1995 and 1999. The results in Figure F.4 in Appendix F show that for males and young workers, the treatment dummy is, in general, insignificant and close to zero for each month after the inflow into employment. For skilled white-collar workers, the effects are also insignificant, but negative for the employment-to-unemployment transitions. This implies that during the pre-DSL period skilled white-collar workers in unlucky municipalities exhibited smaller increases in cumulative probabilities of reentering unemployment as compared to their lucky counterparts. Overall, this difference in trends during the pre-DSL years might lead to an upward bias of the estimated DSL coefficients.

Turning to wage outcomes, the results in Tables F.10 to F.12 in Appendix F indicate no differences in pre-treatment trends between the years 1995 and 1999, again with the exception of skilled white-collar workers. For this group, the results point to a larger wage growth after one year in unlucky municipalities as compared to lucky municipalities. Again, this difference in trends might cause a downward bias in the estimated DSL coefficients.

[26] For a detailed description of the sensitivity analyses see Gürtzgen et al. (2018).

Overall, the placebo estimates for males and young workers point to a similar pre-treatment trend across lucky and unlucky municipalities, which suggests that both groups performed similarly during the pre-DSL years. Taken together, this analysis points to a causal interpretation of the DSL effect on match quality measures for these groups and a underestimation of the DSL effect for skilled white-collar workers.

8 Evidence from Vacancy Data

8.1 Online Recruitment and Match Quality Outcomes

Our strategy thus far identified an ITT, which makes it difficult to assess the magnitude of the effects on individuals' search and employers' recruitment behavior. Earlier evidence from individual-level data suggests that an increase in internet availability at home is indeed associated with an increase use of the internet for job search activities (Gürtzgen et al., 2018). However, as emphasized earlier, the use of the internet for individual job search does not necessarily imply that individuals were actually hired through online recruiting, as job seekers (as well as employers) may use multiple search channels.

To address these issues, we seek to provide more direct evidence on the relationship between employers' recruitment behavior and new hires' match quality outcomes by exploiting data from the IAB Job Vacancy Survey. Next to information on a variety of establishment characteristics, employers are asked to report information on their most recent successful hiring process during the last 12 months prior to the interview date. Apart from individual characteristics of the hired employee, this information includes characteristics of the specific position to be filled as well as characteristics of the recruitment process, such as employers' adopted search and recruitment channels. To complement this information with match quality outcomes of the most recently hired worker, we merge the survey information with the individual hire's employment history from the Integrated Employment Biographies.

We use these matched vacancy-administrative data to explicitly compare outcomes of new matches that were hired through online search and recruiting to those that were formed via different recruitment channels. This allows for a more direct interpretation of the estimated effects as match quality effects that arise from the recruitment channel, thereby enabling us to also include formerly employed new hires into our analysis. A drawback of this empirical exercise is that it comes at the expense of two important limitations that basically arise from the survey's small sample size. The first caveat is that we have to perform the analysis on the set of all West German municipalities, which does not allow us to homogenize our sample of municipalities as in Section 7. A second limitation concerns the lack of statistical power when breaking down the analysis by different subgroups.

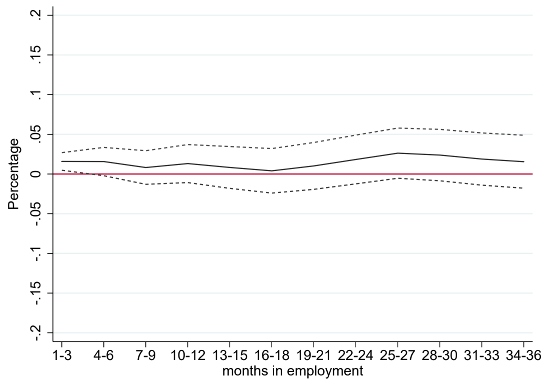
From the vacancy data, we use the waves 2009 and 2010, as it is possible to merge the survey data with administrative data only from 2009 onwards. Pooling the years 2009 and 2010 yields information on 8,406 hiring processes, out of which 4,796 can be uniquely merged to the hired individuals' administrative employment records.[27] Table G.1 in Appendix G displays descriptive statistics of establishment, position-specific attributes and individual characteristics of the most recent hiring process broken down by online and non-online recruits. The figures show that online recruiting is more prevalent in larger establishments and for more skilled positions, which also require more frequently special skills, such as a long work experience and leadership competencies. For the subsample of 4,796 hires whose employment histories can be identified, Table G.2 shows differences in employment stability and wage outcomes across online and non-online recruits.

In what follows, we explore the relationship between employers' use of online recruiting channels and newly hired workers' match quality outcomes, by adopting an IV estimation strategy similar to that in Section 7. In particular, we again make use of the municipality identifier and the information on the municipalities' (geographic centers') distance to the next MDF in order to estimate eq. (3). This equation includes an indicator variable taking on the value of unity if the most recent hired worker was recruited online as the main explanatory variable of interest, which is instrumented by the distance indicator variable $PSTN$. Given that there is evidence that the lack in internet availability provided a larger restriction for individuals than for employers, the indicator refers to distance of the centroid of the hired person's home municipality to the next MDF.

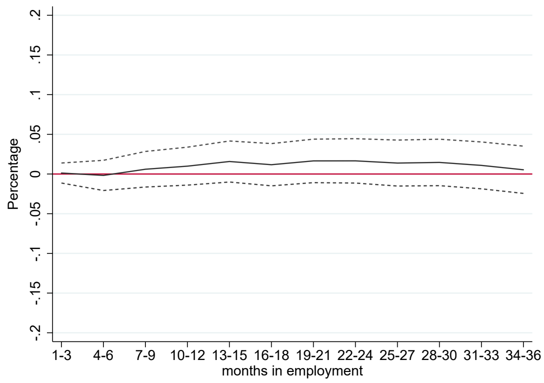
Figure 5 shows the estimates of the effects of the incidence of online recruiting on the cumulative transition probabilities (a) from employment to unemployment and (b) from employment to employment within m months after entering a new employment relationship. Overall, the OLS-estimates tend to be higher than the IV-estimates. With respect to the IV regression, the Kleibergen-Paap F-Statistic is 9.07 and the significant first stage treatment coefficient equals -0.052, indicating that employers in unlucky municipalities exhibit, on average, a 5% points lower probability of online recruiting. While the F-statistic does not reach its critical value of about 10, the first stage coefficient is of a similar order of magnitude compared to the first-stage coefficient from the municipality-level analysis, indicating that lucky municipalities have a 5% points higher DSL rate. Contrary to the municipality-level analyses, the IV-estimates now point to significantly negative point estimates particularly for the employment-to-employment transitions during month four to nine after entering the new employment relationship. The point estimates for the employment-to-unemployment transitions are found to be close to zero and insignificant at any conventional level. In terms of magnitude, online recruits exhibit a 1% point lower cumulative probability of an employment-to-employment transition between month four

[27] As the vacancy data do not contain an individual identifier of the most recent hire, the linkage of both data sets requires an algorithm based on observables that are available in both data sources. For a more detailed description of this algorithm, see Lochner (2019).

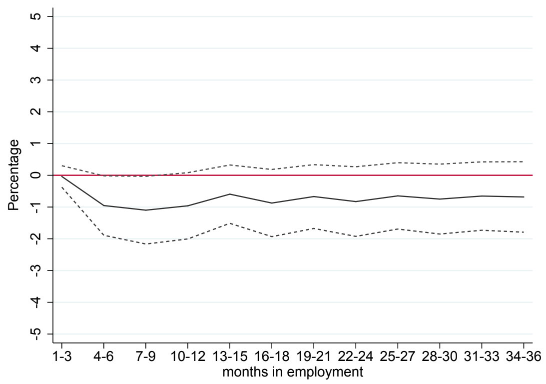
to nine after entering the new employment relationship as compared to new hires that were recruited by non-online channels.



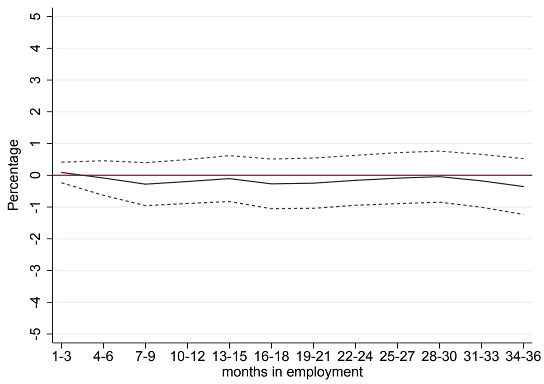
(a) *Employment-to-employment, OLS*



(b) *Employment-to-unemployment, OLS*



(c) *Employment-to-employment, IV*



(d) *Employment-to-unemployment, IV*

Notes: The figure shows regression results of online recruiting on cumulative transition probabilities from employment to unemployment and from employment to employment within m months of the most recent hire by establishments in West Germany in 2009 and 2010. Figures (a) and (b) plot the OLS coefficients. Figures (c) and (d) show coefficients from the IV model, where online recruiting is instrumented by a threshold dummy indicating whether the distance of the centroid of the hired person's home municipality to the next MDF is above 4,200 meters. The list of control variables includes establishment and predetermined job characteristics and individual characteristics of the last hired person (see Table G.1 in Appendix G). Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust. Regressions are based on 4,796 establishments. The Kleibergen-Paap F-Statistic for the first stage is 9.07.

Figure 5: Regression results of online recruiting on leaving new employment relationships

Breaking down the estimates by subgroups, the IV-estimates in Figure G.2 (a) and (c) in Appendix G show that the negative effects on employment-to-employment transitions are of the same order of magnitude across gender as well as age groups, as the estimates for male and young workers are found to be fairly similar to those for the full sample. Overall, the estimates for the employment stability outcomes indicate that online recruiting appears to reduce employment-to-employment transitions rather than employment-to-unemployment transitions. While this result seems to be at odds with our municipality-level analyses, a plausible explanation relates to the fact that the vacancy-level data analysis is based on a sample that also includes new hires who were formerly employed. A natural test of this explanation would involve a break down of the survey sample by new hires' former employment status, but due to sample size limitations this exercise yields very imprecise estimates.[28]

Table 3 shows the estimated effects on wage outcomes for the full sample.[29] For the change in log (full-time) wages, the IV-coefficient on online recruiting is 0.94 and significant at the 10% level. As this estimate cannot be directly compared with the IV-estimate from the municipality-level analysis, it is useful to calculate the reduced-form coefficient, which may be obtained from multiplying the estimated coefficient with the first-stage coefficient. The vacancy-level analysis yields a reduced-form coefficient of about 0.06 (0.944×0.068), which is considerably larger than the reduced-form coefficient arising from Column (3) in Table 2, whose order of magnitude is about 0.004. Again, it has to be kept in mind that the results from Table 3 also capture wage changes of new hires with a job-to-job transition, whose wage gain after such a transition may be expected to be considerably larger than that of formerly unemployed hires.

Table 3: Regression results of online recruiting on wage outcomes

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Panel A: OLS				
Online recruiting	0.035*	-0.009	-0.009	-0.025
	(0.021)	(0.037)	(0.023)	(0.016)
Panel B: IV				
Online recruiting	-0.102	0.525	0.944*	-0.205
	(0.502)	(0.952)	(0.539)	(0.300)
Threshold (first stage)	-0.052***	-0.056***	-0.068***	-0.066***
	(0.017)	(0.017)	(0.022)	(0.019)
F-Statistic	9.04	10.36	10.03	11.73
Observations	4,790	4,679	2,805	3,580

Notes: The table shows regression results of online recruiting on wage outcomes of the most recent hire by establishments in West Germany in 2009 and 2010. Panel A shows the OLS coefficients. Panel B shows the coefficients from the IV model, where online recruiting is instrumented by a threshold dummy indicating whether the distance of the centroid of the hired person's home municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust. The list of control variables includes establishment and predetermined job characteristics and individual characteristics of the last hired person (see Table G.1 in Appendix G). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

8.2 Mechanisms

Our results thus far suggest that the effects of the DSL expansion and the effects of online recruiting on employment stability and wage outcomes are quite moderate, and this is

[28] Breaking down the sample by new hires' former employment status yields point estimates for the effects on employment-to-unemployment transitions that are more negative for formerly unemployed individuals than those for the full sample, but not significantly so.

[29] The results for the subgroups are reported in Tables G.3 to G.3 in Appendix G.

true for both the results from the municipality-level and the vacancy-data analysis. While both the estimated ITT from the municipality-level analysis as well the direct effect from the vacancy-level analysis pointed to some positive effects on employment stability, the municipality-level estimates in Table 2 suggest that there are no discernible wage effects of the DSL expansion. With the exception of the estimates for (full-time) wage growth, a similar result holds for the vacancy-data estimates from the previous section.

8.2.1 Adverse Selection

In what follows, we seek to substantiate these findings by explicitly addressing adverse selection issues. As emphasized earlier, online recruiting may have ambiguous effects on the match quality of new hires due to adverse selection: The underlying rationale is that the decline in search and application costs may entail excess applications, with many workers applying to many more jobs compared with more traditional search channels. Excess applications are likely to be especially relevant for unsuitable candidates, who would not have applied if the submission of their applications had been more costly.

To address this issue, we next explore whether the descriptive findings from Section 2 can be confirmed based on our IV approach. We use the information for the last hiring process from the IAB Job Vacancy Survey to construct several outcomes: (1) the number of applications, (2) the share of unsuitable applications and (3) the share of unsuitable applications broken down by gender. The latter shares are calculated as the number of unsuitable applications from males (females) over all applications from male (female) individuals. Table G.7 in Appendix G displays the descriptives of these variables broken down by non-online and online recruiting.

Table 4: IV regression results for online recruiting on applicants

	(1) Log number of applicants	(2) Share of unsuitable applicants	(3) Share of unsuitable male applicants	(4) Share of unsuitable female applicants
Online recruiting	3.176***	1.302**	2.171**	2.578**
	(1.130)	(0.541)	(0.889)	(1.056)
Threshold (first stage)	-0.059***	-0.058***	-0.053***	-0.054***
	(0.016)	(0.016)	(0.017)	(0.016)
F-Statistic	14.55	13.61	10.97	11.46
Observations	6,826	6,669	6,315	6,404

Notes: The table shows IV regression results of online recruiting on applicants for establishments in West Germany in 2009 and 2010. Specifications (2) - (4) are based on Tobit estimations which account for left-censoring at zero. Online recruiting is instrumented by a threshold dummy indicating whether the distance of the centroid of an establishment's municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust. The list of control variables includes establishment and predetermined job characteristics (see Table G.6 in Appendix G). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 4 shows IV-estimates of the effects of online recruiting on application outcomes. As a non-negligible fraction of the calculated shares takes on the value of zero, we provide estimates based on Tobit-IV regressions, where online recruiting is again instrumented by the distance indicator variable. Because these outcomes relate to an establishment's recruitment process and given that we not have information on applicants' home municipalities, the distance now refers to the distance between the geographic center of the hiring establishment's municipality and the next MDF. The estimate in Column (1) shows that for hiring processes via online-recruiting employers receive considerably more applications than for hirings that occur through other recruitment channels. The coefficient equals about 3.18 and is highly significant, suggesting that online recruiting roughly quadruples the number of applicants. Evaluated at the mean value of the number of applications of 18, online-recruiting raises the number of applications by 57. According to Column (2), the larger number of applications goes along with a larger share of unsuitable applications. Online recruiting raises the fraction of unsuitable applications by 1.3% points, which is significant at the 5% level. However, compared to the overall mean of the share of unsuitable applications of about 50%, this increase is rather small. Column (3) and (4) show the estimated effects on the fraction of unsuitable applications by gender. The point estimate for females is 2.6 and that for males is 2.2. Even though the difference between the coefficients is not significantly different from zero, the estimates provide some weak evidence that online recruiting raises the share of unsuitable applications for female applicants to a slightly greater extent than for their male counterparts. Overall, these estimates lend some support to the excess application hypothesis. Given that excess applications may be expected to create additional costs of screening, employers need to spend more time and effort in establishing the required qualifications of their applicants. With asymmetric information on candidates' true productivity, this may arguably counteract potential positive effects of online recruiting on new hires' match quality. To provide some further support for potential adverse selection effects, we also explore the effects of online recruiting on the incidence of employers' concessions with respect to the required attributes of the candidate.[30] Given that concessions reflect the extent of mismatch in terms of observables, they may be considered as an alternative measure of match quality. Table G.9 in Appendix G shows that online recruiting gives rise to a (weakly significant) 60% points higher probability of making such a concession.

In a final step, we also explore whether online-search channels crowd out other search channels, such as newspapers, referrals or the employment agency. The results in Table G.10 in Appendix G indicate that online search does not significantly reduce the use of other search channels. We find a positive effect of online search on the sum of non-online search channels. However, other than the descriptive difference from Section 2, the difference is insignificant. Overall, these findings do not provide any evidence for crowding out effects.

[30]These concessions may refer to the required qualification, years of experience, applicants' age as well as to the salary.

8.2.2 Selectivity of Successful Hirings

One caveat of the analysis from Section 8.1 and 8.2.1 is that the results are based on successful hiring processes. This raises a selectivity concern as employers' online search might be systematically related to the outcome of whether a suitable candidate can be found to fill the vacancy. Assume that unobservables determining a recruitment failure and the fraction of unsuitable applications are positively correlated. Then, if online search had a positive effect on the incidence of a recruitment failure, the estimated coefficients reported in Column (2) to (4) in Table 4 would be downward biased. In contrast, the estimated coefficients from Section 8.1 concerning match quality outcomes would be upward biased.

To investigate this issue further, we next exploit information on unsuccessful recruitment attempts. In the IAB Job Vacancy Survey, employers are not only asked to report information on their most recent successful hiring process, but also on their most recent recruitment failure during the last 12 months prior to the interview. We exploit this information to pool all (successful and unsuccessful) search processes for the year 2011. We use this wave instead of the years 2009 and 2010, as information on employers' adopted search channels for recruitment failures is available only from 2011 onwards. For the year 2011, 1,236 employers report both, a successful hiring process and a recruitment failure. 4,564 employers report only a successful hiring, whereas 104 employers report only a recruitment failure. For those employers who report information on both processes, we randomly select either the successful or unsuccessful search process so as to match the establishment-specific overall fraction of recruitment failures. We adopt this procedure in order to avoid a large fraction of employers with both processes in our sample, as the latter would not allow us to identify the coefficients on establishment-specific control variables into our specification. Conditioning further on non-missing information of the relevant covariates of interest, this procedure yields a sample of 5,661 search processes.

Table G.11 in Appendix G reports the results from a linear probability model regressing an indicator variable for a recruitment failure on a set of controls and an indicator variable taking on the value of unity if an employer used online search channels (possibly amongst other search channels) for both specifications. The coefficient of the online search indicator is about 0.05, indicating that online search increases the probability of a recruitment failure by about 5% points. However, the estimate is not significant at conventional levels. Moreover, the F-statistic is found to be fairly small. Taken together, these findings indicate that there is no clear evidence on whether online search either increases or decreases the incidence of recruitment failures.

8.2.3 Number of Vacancies

According to the predictions of the standard search and matching model, the decline in search and recruitment costs associated with the use of online search channels may be expected to induce employers to post more vacancies. An increase in the number of posted vacancies may exacerbate the increase in the number of applications per vacancy and may therefore provide an additional explanation for potential countervailing internet effects on the match quality of new hires. Such negative effects may be relevant if an increase in the number of vacancies per employer is not accompanied by an expansion of employers' capacities for personnel issues, such as the amount of staff responsible for recruitment. While we do not have information on employers' recruitment-related expenses per vacancy, the IAB Job Vacancy Survey allows us to retrieve information on the number of posted vacancies at the employer level (at the time of the interview). In what follows, we therefore explore whether the decline in search costs associated with the DSL expansion induces employers to post more vacancies. As information on the use of online search and recruitment channels is only available for the most recent hiring process, we do not have information on search channels for all open vacancies reported by an employer. For this reason, we regress the number of vacancies on our measure of DSL availability at the (establishment's) municipality level and again instrument the DSL share by our distance indicator.

Table 5: IV regression results for DSL on the number of vacancies

	Open vacancies
DSL	0.038**
	(0.017)
Threshold (first stage)	-5.796***
	(1.026)
F-Statistic	31.91
Observations	3,513

Notes: The table shows IV regression results of a 1% point increase in the share of households with DSL availability on the number of posted vacancies for establishments in West Germany in 2009. DSL is instrumented by a threshold dummy indicating whether the distance of the centroid of an establishment's municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust and clustered on the municipality level. The list of control variables includes establishment characteristics (see Table G.6 in Appendix G) and the growth rate in the number of employees. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 5 shows the results. The coefficient on the DSL rate suggests that a 1% point in DSL availability at the municipality level increases the number of posted vacancies by about 0.038. In terms of the difference between employers in lucky and unlucky municipalities, where the difference in DSL rates amounts to 14% points, this translates into an increase of 0.53 vacancies per employer. Compared to the mean of 0.519 vacancies posted per employer, this effect is economically meaningful.[31]

[31]Note that this finding is consistent with our earlier established result that the increase in broadband availability had no impact on labor demand (see Gürtzgen et al., 2018). As we control for employment growth, the broadband effect on vacancies merely stems from an effect on excess turnover. As a robustness check, we performed the same regression, after restricting the number of vacancies to those that merely

9 Conclusions

The aim of this study is to explore the effect of the expansion of high-speed internet on match quality outcomes of newly hired workers. To do so, we exploit a quasi-experimental setting created by the expansion of high-speed internet (DSL) at the regional level in Germany. The source of variation stems from technological peculiarities of the traditional public switched telephone network (PSTN), through which the early generations of DSL have been implemented.

Using this exogenous variation in internet availability during the early DSL years, we start by estimating the internet's effect on match quality outcomes at the municipality level based on an inflow sample of newly hired workers who found a new job after a spell of unemployment. We focus on formerly unemployed new hires, since the restriction in internet availability prior to the DSL expansion was more binding for unemployed than for employed job seekers. As a result, for formerly unemployed new hires the impact of the internet on outcomes such as employment stability is more likely to capture match quality effects that arise from a change in search channels as compared with those who were searching on-the-job. To further investigate more direct effects of the *usage* of online recruiting, we next link the administrative employment histories of newly hired workers with vacancy data. While these data come at the expense of a much smaller sample size, they enable us to directly track employment histories after online recruiting. This, in turn, involves a more direct interpretation of the estimated effects as pure match quality effects, thereby allowing us to also include formerly employed new hires into our analysis.

Based on this empirical strategy, our municipality-level analysis suggests that the availability of broadband internet reduces especially employment-to-unemployment transitions among formerly unemployed males and white-collar workers but has no impact on wages. Our analysis based on vacancy-level data suggests that for the pooled sample online recruiting has no impact on wages, but leads to a moderate increase in employment stability, with the effect now being driven by reduced employment-to-employment transitions.

Looking at potential mechanisms underlying the moderate match quality effects, the survey data reveal that online recruiting leads to a dramatic increase in the number of applications and raises the share of unsuitable candidates. We also find some weak evidence that online recruiting raises the share of unsuitable candidates to a slightly larger extent for females than for males. The results provide also some weak evidence for an increase in employer concessions with respect to the required experience and/or qualification of the position to be filled. Overall, these findings lead us to conclude that consistent with the adverse selection explanation, online recruiting gives rise to the phenomenon of excess applications, which creates additional screening costs for employers and may give rise to

arise from new job creation. With this outcome, the DSL coefficient is found to be small and insignificant at any conventional level.

potential countervailing negative effects of online recruiting on match quality outcomes. The increase in applications per vacancy is exacerbated by an internet induced increase in the number of vacancies posted per employer. As long as this increase is not accompanied by an appropriate increase in recruitment expenses, this may provide a further explanation for countervailing negative effects on match quality outcomes.

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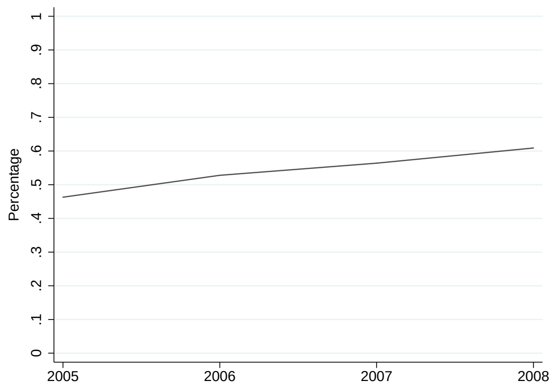
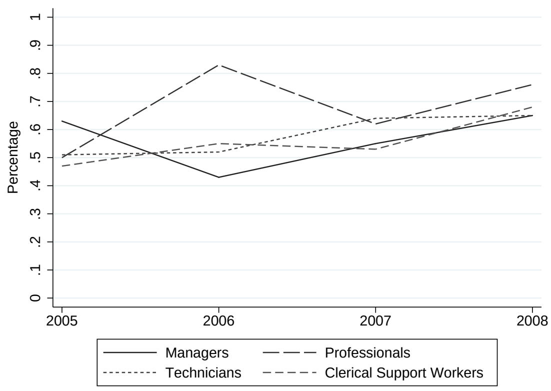
Appendix

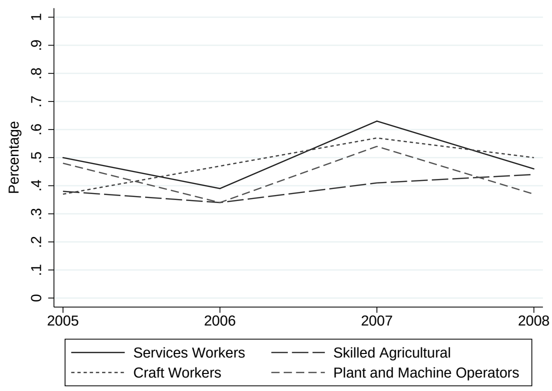
A Evolution of Online Recruiting



(a) Overall online recruiting



(b) Online recruiting by occupation - I



(c) Online recruiting by occupation - II

Notes: The plots show the fraction of vacancies being posted online among all successful hirings. Panel (a) shows the overall time trend. Panel (b) and Panel (c) show the trend by different occupational categories.

Figure A.1: Evolution of online recruiting

B Reservation Wages

To investigate whether internet access has and impact on reservation wages, we make use of the PASS data 2007-2009 (for a description see Gürtzgen et al., 2018). These household and individual-level survey data provide information on home internet access as well as reservation wages. The survey question in the PASS data is as follows: "How much would the net monthly wage at least have to be, so that you would be willing to work for it? And how many hours per week would you have to work to earn this wage?" To explore whether job seekers internalize a higher value of unemployment due to internet access via higher reservation wages, we regress reservation wages on home internet access and instrument the latter by our distance instrumental variable $PSTN$. Table B.1 presents the OLS and IV results. OLS generates close to zero point estimates, while the IV model is accompanied by rather large standard errors and small F-statistics for the subgroups. While this limits the interpretation of the IV results, these findings overall suggest that home internet access does not give rise to higher reservation wages.

Table B.1: Home internet on log reservation wages, OLS and IV

	Full sample	Male	Young	Skilled	White-collar jobs
Panel A: OLS					
Home internet	0.008	-0.024	-0.016	0.026	0.029
	(0.020)	(0.021)	(0.031)	(0.022)	(0.024)
Panel B: IV					
Home internet	-0.083	-0.29	0.832	0.098	-0.693
	(0.344)	(0.430)	(1.381)	(0.473)	(0.596)
F-Statistic	5.11	2.67	0.61	2.82	3.16
Observations	2,047	1,066	808	1,336	1,169

Notes: Panel A reports OLS regression results of home internet access on the log reservation wage for individuals in West Germany. Panel B reports IV-estimates, whereby 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 4,200 meters. 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 controls in Gürtzgen et al., 2018). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

C Administrative Data Addendum

Table C.1: Definition of variables

Labor market variables	Description
Exit probability	Exit probabilities are based on a yearly inflow sample of new hires. Exit probabilities are estimated at the municipality level as the share of individuals with a transition into unemployment or to another job.
	Source: IEB, Federal Employment Agency
Wage outcomes	Mean wages in the new job, mean difference in wages before and after unemployment and mean wage growth after one year at the municipality level based on a yearly inflow sample of new hires.
	Source: IEB, Federal Employment Agency
Internet variables	
Broadband internet	Fraction of households in municipality i at year t with a subscription to DSL defined by an access speed of 384 kb/s or above. Documented numbers start in 2005.
	Source: Breitbandatlas Deutschland
Treatment	Equals 1 for municipalities in West Germany with a distance of more than 4,200 meters to the next main distribution frame (MDF). The distance is calculated using the geographic centroid weighted by the location of the population.
	Source: Falck et al. (2014)
Control variables	
Population	Number of inhabitants in municipality i at year t.
	Source: Falck et al. (2014)
Female population share	Fraction of females in municipality i at year t. The female share is also measured for the inflow-specific sample.
	Source: Falck et al. (2014) and IEB, Federal Employment Agency
Population aged 18-65	Fraction of the population aged between 18 and 65 years in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Population aged > 65	Fraction of the population aged above 65 years in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Net migration	Net migration rate in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Unemployment rate	Unemployment rate in municipality i at year t. The pre-DSL fraction refers to the year 2001.
	Source: Falck et al. (2014)
Foreign nationals	Fraction of foreigners in municipality i at year t. The nationality is also measured for the inflow-specific sample.
	Source: IEB, Federal Employment Agency
Occupation	Occupational shares in municipality i at year t calculated for the categories agriculture, production, salary, sale, clerical and service (ref. service sector). The occupation is also measured for the inflow-specific sample.
	Source: IEB, Federal Employment Agency

Table C.1: Definition of variables *(continued)*

Control variables	Description
Industry	Industry shares in municipality i at year t calculated for the categories agriculture/energy/mining, production, steel/metal/machinery, vehicle construction/apparatus engineering, consumer goods, food, construction, finishing trade, wholesale trade, retail trade, transport and communication, business services, household services, education/helth, organizations, public sector, else. The industry is also measured for the inflow-specific sample. Source: IEB, Federal Employment Agency
Skill level	Skill level in municipality i at year t. *Low skilled*: No degree/ highschool degree *Medium skilled*: Vocational training *High skilled*: Technical college degree or university degree. Skill level is also measured for the inflow-specific sample. Missing and inconsistent data on education are corrected according to the imputation procedure described in Fitzenberger et al. (2006). This procedure relies on the assumption that individuals cannot lose their educational degrees. Source: IEB, Federal Employment Agency
Real daily wage	Average real daily wage in municipality i at year t calculated among full-time employees. Gross daily wages are right-censored due to the upper social security contribution limit. To address this problem, we construct cells based on gender and year. For each cell, a Tobit regression is estimated with log daily wages as the dependent variable and age, tenure, age squared, tenure squared, full-time dummy, two skill dummies, occupational, sectoral as well as regional (Federal State) dummies as explanatory variables. As described in Gartner (2005), right-censored observations are replaced by log wages randomly drawn from a truncated normal distribution whose moments are constructed by the predicted values from the Tobit regressions and whose (lower) truncation point is given by the contribution limit to the social security system. After this imputation procedure, nominal wages are deflated by the CPI of the Federal Statistical Office Germany normalized to 1 in 2010. Source: IEB, Federal Employment Agency
Number of establishments	Number of establishments in municipality i at year t. Source: IEB, Federal Employment Agency
Size of establishments	Number of employees per establishment in municipality i at year t. Source: IEB, Federal Employment Agency
Number of females & low-qualified employees	Number of female and low-qualified employees per establishment in municipality i at year t. Source: IEB, Federal Employment Agency
Median establishment wage/age	Median wage/age at the establishment level based on employee information in municipality i at year t. Source: IEB, Federal Employment Agency
Number of firm entries	Number of firms entering the market in municipality i at year t. The pre-DSL fraction refers to the year 2000. Source: Mannheim Enterprise Panel
Number of firm exits	Number of firms exiting the market in municipality i at year t. The pre-DSL fraction refers to the year 2000. Source: Mannheim Enterprise Panel
Total sales	Total sales based on firm information in municipality i at year t. The pre-DSL fraction refers to the year 2001. Source: Mannheim Enterprise Panel

Table C.2: Description of labor market states

Definition of labor market states
Employment: Employment spells include continuous periods of employment (allowing gaps of up to one month) subject to social security contributions and (after 1998) marginal employment. For parallel spells of employment and unemployment (e.g. for those individuals who in addition to their earnings receive supplementary benefits), we treat employment as the dominant labor market state.
Unemployment Unemployment spells include periods of job search as well as periods with transfer receipt. Prior to 2005, the latter include benefits such as unemployment insurance and means-tested unemployment assistance benefits. Those (employable) individuals who were not entitled to unemployment insurance or assistance benefits could claim means-tested social assistance benefits. However, prior to 2005, spells with social assistance receipt may be observed in the data only if the job seekers' history records social assistance recipients as searching for a job. After 2004, means-tested unemployment and social assistance benefits were merged into one unified benefit, also known as 'unemployment benefit II' (ALG II). Unemployment spells with receipt of ALG II are recorded in the data from 2007 onwards, such that the data provide a consistent definition of unemployment only for the period 2007-2010.
Distinction between unemployment and nonparticipation Involuntary unemployment is defined as comprising all continuous periods of registered job search and/or transfer receipt. Gaps between such unemployment periods or gaps between transfer receipt or job search and a new employment spell may not exceed one month, otherwise these periods are considered as nonparticipation spells (involving voluntary unemployment or an exit out of the social security labor force). Similarly, gaps between periods of employment and transfer receipt or job search are treated as involuntary unemployment as long as the gap does not exceed six weeks, otherwise the gap is treated as nonparticipation.

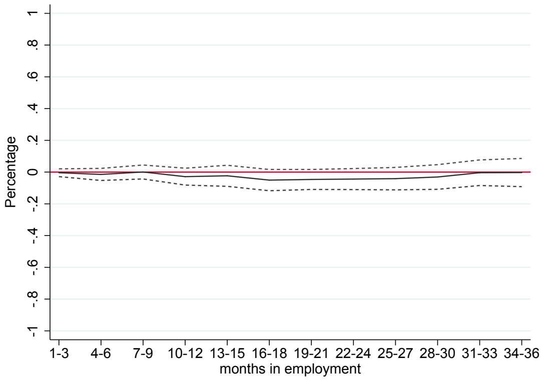
D Descriptive Statistics

Table D.1: Further descriptive statistics

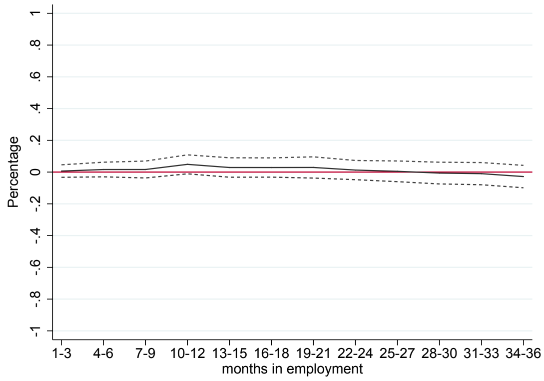
	pre-DSL years 1998/99			DSL years 2007/08			
	Unlucky	Lucky	Diff[a)]	Unlucky	Lucky	Diff[a)]	Diff-in-Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Regional occupational structure							
Agriculture	0.028	0.024	0.002**	0.027	0.024	0.002***	0.000
Production	0.373	0.356	0.010***	0.303	0.295	0.003	-0.007***
Salary	0.103	0.111	-0.003**	0.112	0.117	-0.001	0.002
Sale	0.063	0.067	-0.003***	0.069	0.072	-0.002**	0.001
Clerical	0.197	0.207	-0.003*	0.204	0.213	-0.002	0.001
Service	0.228	0.226	-0.002	0.276	0.269	0.001	0.004
Panel B: Sector composition							
Agriculture/Energy/Mining	0.037	0.033	0.002**	0.035	0.032	0.003***	0.001
Production	0.061	0.066	-0.001	0.047	0.050	-0.002**	-0.001
Steel/Metal/Machinery	0.091	0.091	0.004**	0.086	0.085	0.003**	-0.001
Vehicle construction/ Apparatus engineering	0.040	0.041	-0.001	0.038	0.037	0.000	0.001
Consumer goods	0.055	0.054	-0.002	0.041	0.041	-0.001	0.000
Food	0.037	0.034	0.000	0.034	0.032	-0.001	-0.001
Construction	0.076	0.065	0.005***	0.046	0.040	0.003***	-0.003**
Finishing trade	0.051	0.048	0.002*	0.038	0.036	0.001*	-0.000
Wholesale trade	0.049	0.052	-0.002**	0.047	0.050	-0.003***	-0.001
Retail trade	0.089	0.094	-0.003**	0.097	0.099	-0.001	0.002
Transport and communication	0.046	0.047	-0.000	0.055	0.054	0.002*	0.002*
Business services	0.082	0.085	-0.001	0.102	0.106	-0.002	-0.001
Household services	0.067	0.066	0.000	0.080	0.082	-0.001	-0.001
Education/Health	0.119	0.121	-0.003	0.133	0.138	-0.005***	-0.002
Organizations	0.017	0.018	-0.001	0.020	0.022	-0.000	0.000
Public sector	0.057	0.057	0.000	0.056	0.057	0.001	0.001
Else	0.021	0.022	-0.001	0.031	0.033	-0.001	-0.000
Panel C: Hires inflow characteristics							
Occupation							
Agriculture	0.036	0.030	0.004*	0.038	0.031	0.005**	0.001
Production	0.491	0.468	-0.005	0.372	0.360	0.005	0.009
Salary	0.065	0.072	-0.001	0.071	0.070	0.004	0.004
Sale	0.058	0.063	-0.001	0.078	0.080	-0.004	-0.003
Clerical	0.126	0.139	0.002	0.140	0.149	-0.006	-0.008
Service	0.224	0.228	0.001	0.301	0.309	-0.004	-0.005

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 occupational structure. Panel B report the sector structure. Panel C reports the occupational structure for the unemployment inflow sample. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

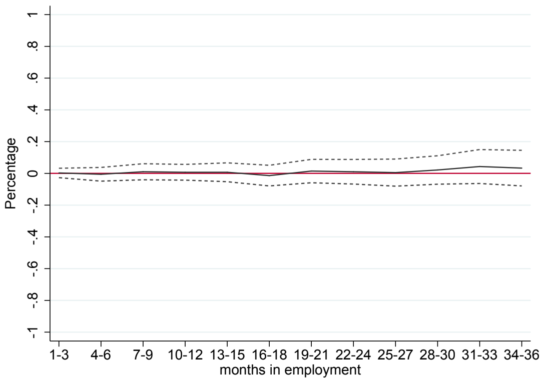
E Heterogeneous Effects



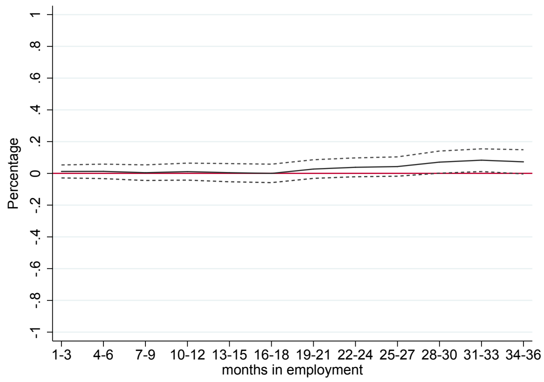
(a) *Employment-to-employment, males*



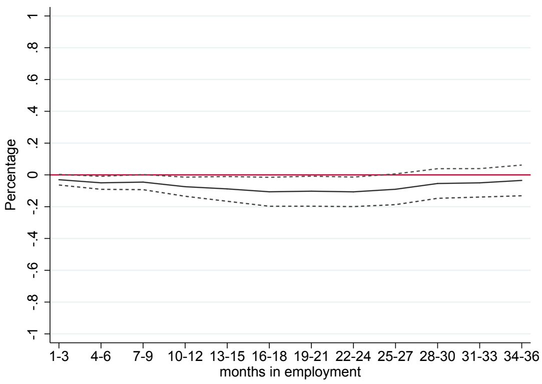
(b) *Employment-to-unemployment, males*



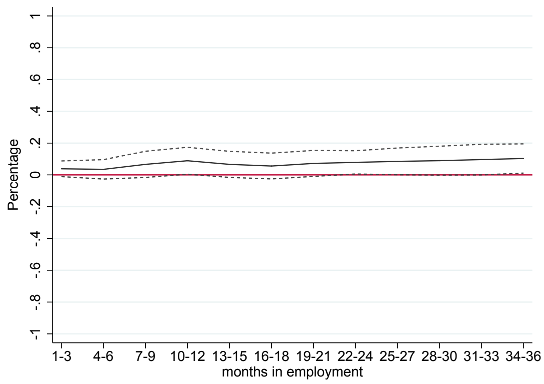
(c) *Employment-to-employment, young*



(d) *Employment-to-unemployment, young*



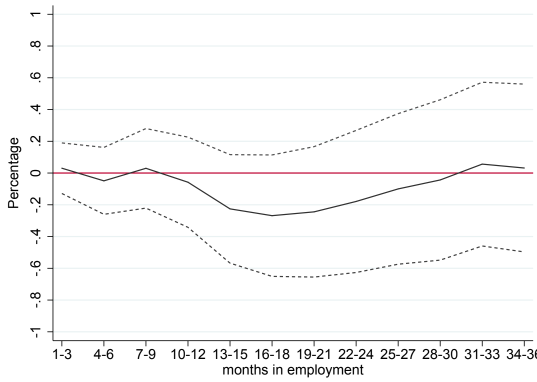
(e) *Employment-to-employment, skilled white-collar*



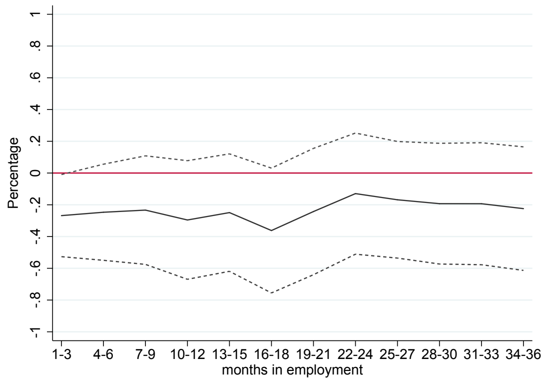
(f) *Employment-to-unemployment, skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probabilities from employment to unemployment and from employment to employment within m months for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The coefficients are based on OLS regressions. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted and performed separately for each month. Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust and clustered at the MDF level. Regressions are based on 2,606 municipalities and 807 MDFs for males, on 2,566 municipalities and 802 MDFs for young workers and on 2,280 municipalities and 750 MDFs for skilled white-collar workers.

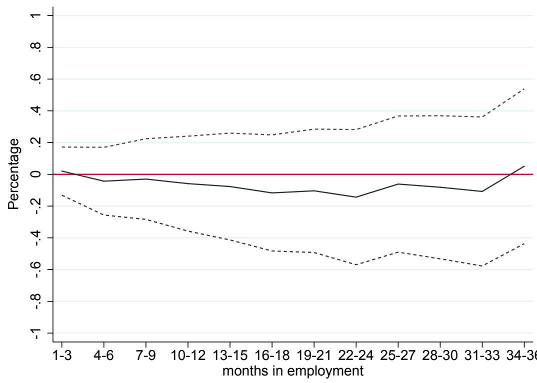
Figure E.1: OLS regression results of DSL on leaving new employment relationships



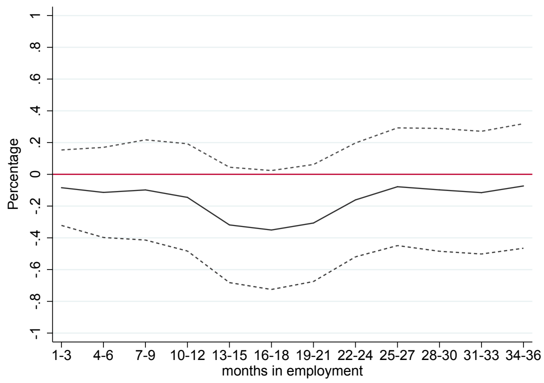
(a) *Employment-to-employment, males*



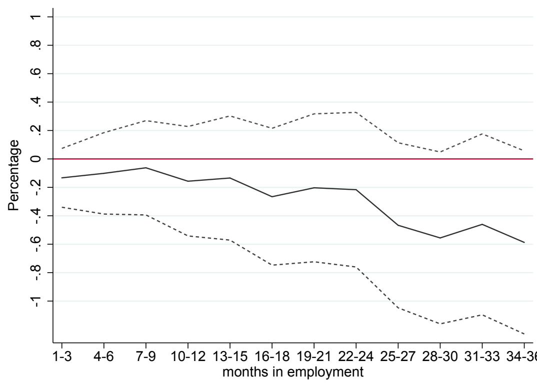
(b) *Employment-to-unemployment, males*



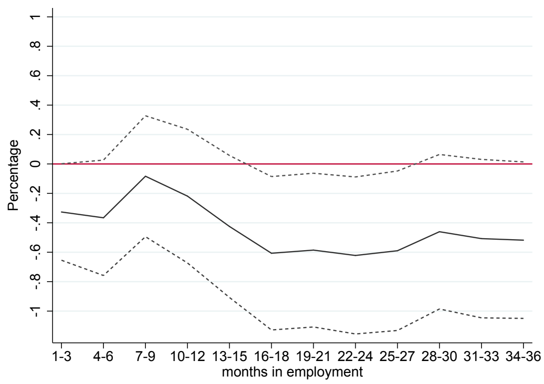
(c) *Employment-to-employment, young*



(d) *Employment-to-unemployment, young*



(e) *Employment-to-employment, skilled white-collar*



(f) *Employment-to-unemployment, skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probabilities from employment to unemployment and from employment to employment within m months for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted and performed separately for each month. Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust and clustered at the MDF level. Regressions are based on 2,606 municipalities and 807 MDFs for males, on 2,566 municipalities and 802 MDFs for young workers and on 2,280 municipalities and 750 MDFs for skilled white-collar workers. The Kleibergen-Paap F-Statistic for the first stage is 39.01, 39.37 and 33.34, respectively.

Figure E.2: IV regression results of DSL on leaving new employment relationships

Table E.1: Regression results of DSL on wage outcomes, males

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Panel A: OLS				
Δ DSL	-0.0004	0.0000	-0.0001	0.0001
	(0.0004)	(0.0004)	(0.0003)	(0.0004)
Panel B: IV				
Δ DSL	0.0006	0.0034	0.0018	0.0021
	(0.003)	(0.003)	(0.002)	(0.002)
Threshold (first stage)	-5.034***	-5.034***	-5.035***	-5.056***
	(0.806)	(0.806)	(0.806)	(0.802)
F-Statistic	39.01	39.01	39.03	39.69
Number of municipalities	2,606	2,606	2,605	2,541
Number of MDF's	807	807	807	798

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. Panel A shows the OLS coefficients. Panel B shows the coefficients from the IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table E.2: Regression results of DSL on wage outcomes, young

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Panel A: OLS				
Δ DSL	-0.0002	0.0000	0.0003	0.0002
	(0.0004)	(0.0005)	(0.0003)	(0.0003)
Panel B: IV				
Δ DSL	0.0002	0.0019	0.0003	0.0030
	(0.002)	(0.003)	(0.002)	(0.002)
Threshold (first stage)	-5.374***	-5.374***	-5.331***	-5.293***
	(0.856)	(0.856)	(0.847)	(0.855)
F-Statistic	39.37	39.37	39.62	38.36
Number of municipalities	2,566	2,566	2,544	2,519
Number of MDF's	802	802	801	796

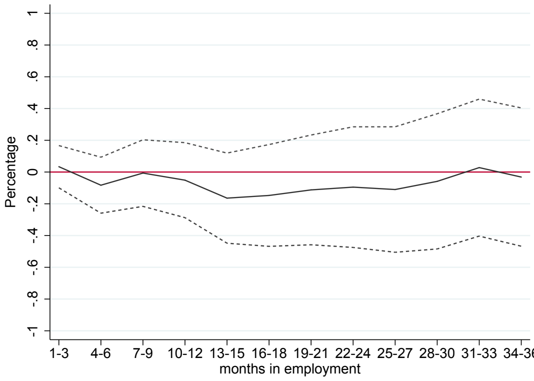
Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. Panel A shows the OLS coefficients. Panel B shows the coefficients from the IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table E.3: Regression results of DSL on wage outcomes, skilled white-collar

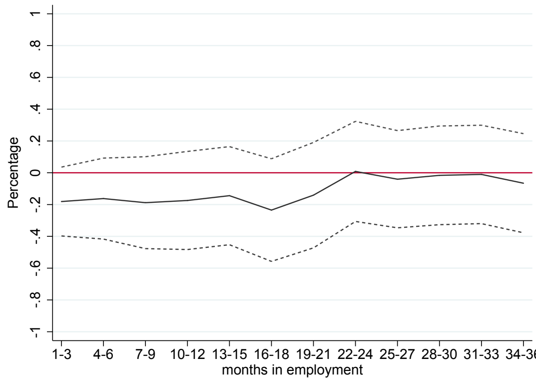
	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Panel A: OLS				
Δ DSL	0.0011*	0.0014**	0.0005	0.0003
	(0.0006)	(0.0006)	(0.0005)	(0.0004)
Panel B: IV				
Δ DSL	0.0018	0.0002	-0.0013	0.0055**
	(0.003)	(0.004)	(0.003)	(0.003)
Threshold (first stage)	-4.664***	-4.664***	-4.558***	-4.595***
	(0.808)	(0.808)	(0.803)	(0.794)
F-Statistic	33.34	33.34	32.21	33.53
Number of municipalities	2,280	2,280	2,265	2,239
Number of MDF's	750	750	747	742

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. Panel A shows the OLS coefficients. Panel B shows the coefficients from the IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

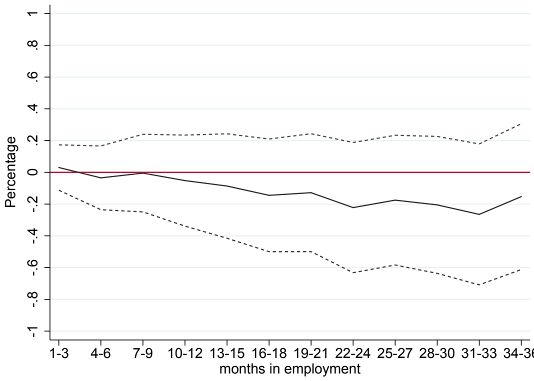
F Robustness Checks and Placebo Tests



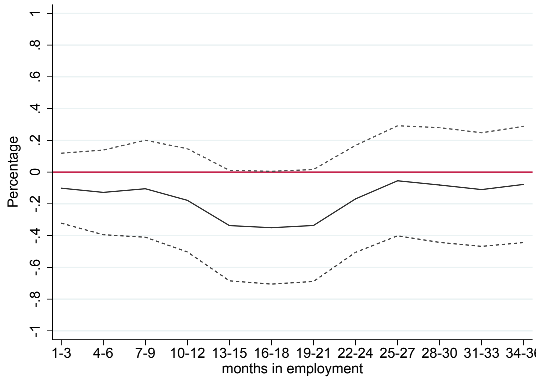
(a) *Employment-to-employment, males*



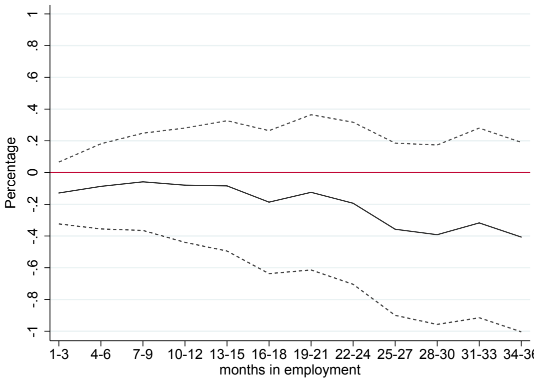
(b) *Employment-to-unemployment, males*



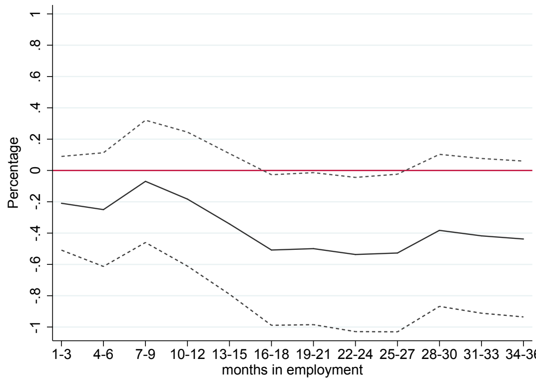
(c) *Employment-to-employment, young*



(d) *Employment-to-unemployment, young*



(e) *Employment-to-employment, skilled white-collar*



(f) *Employment-to-unemployment, skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probabilities from employment to unemployment and from employment to employment within m months for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The regressions include individuals entering unemployment from sectors with a priori high recall rates (e.g. agriculture, construction and passenger transport). The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted and performed separately for each month. Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust and clustered at the MDF level. Regressions are based on 2,764 municipalities and 823 MDFs for males, on 2,642 municipalities and 810 MDFs for young workers and on 2,332 municipalities and 764 MDFs for skilled white-collar workers. The Kleibergen-Paap F-Statistic for the first stage is 44.73, 40.93 and 35.37, respectively.

Figure F.1: IV regression results of DSL on leaving new employment relationships - with recall industries

Table F.1: IV regression results of DSL on wage outcomes, with recall industries, males

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	0.0018	0.0031	0.0003	0.0008
	(0.002)	(0.002)	(0.001)	(0.002)
Threshold (first stage)	-5.496***	-5.496***	-5.450***	-5.363***
	(0.822)	(0.822)	(0.820)	(0.816)
F-Statistic	44.73	44.73	44.12	43.23
Number of municipalities	2,764	2,764	2,761	2,710
Number of MDF's	823	823	823	819

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The regressions include individuals entering unemployment from sectors with a priori high recall rates (e.g. agriculture, construction and passenger transport). The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table F.2: IV regression results of DSL on wage outcomes, with recall industries, young

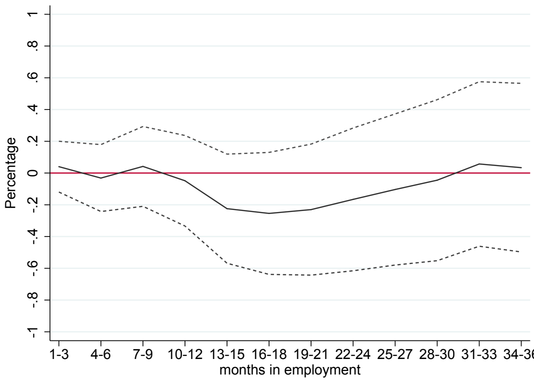
	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	0.0005	0.0015	-0.0008	0.0026
	(0.002)	(0.002)	(0.002)	(0.002)
Threshold (first stage)	-5.400***	-5.400***	-5.384***	-5.261***
	(0.844)	(0.844)	(0.842)	(0.836)
F-Statistic	40.93	40.93	40.87	39.60
Number of municipalities	2,642	2,642	2,623	2,598
Number of MDF's	810	810	809	805

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The regressions include individuals entering unemployment from sectors with a priori high recall rates (e.g. agriculture, construction and passenger transport). The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

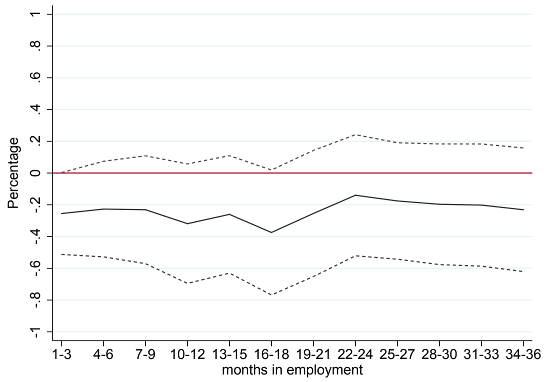
Table F.3: IV regression results of DSL on wage outcomes, with recall industries, skilled white-collar

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	0.0007	-0.0007	-0.0020	0.0047**
	(0.003)	(0.003)	(0.002)	(0.002)
Threshold (first stage)	-4.789***	-4.789***	-4.771***	-4.703***
	(0.805)	(0.805)	(0.804)	(0.793)
F-Statistic	35.37	35.37	35.26	35.16
Number of municipalities	2,332	2,332	2,321	2,285
Number of MDF's	764	764	762	754

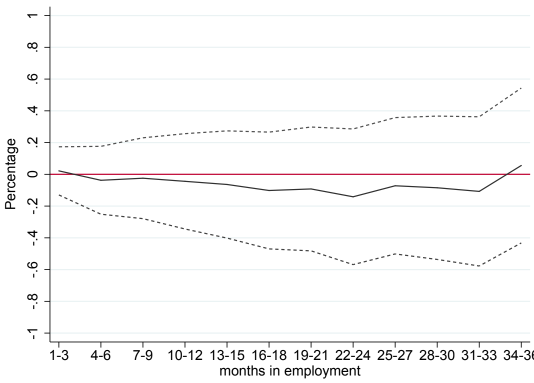
Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The regressions include individuals entering unemployment from sectors with a priori high recall rates (e.g. agriculture, construction and passenger transport). The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.



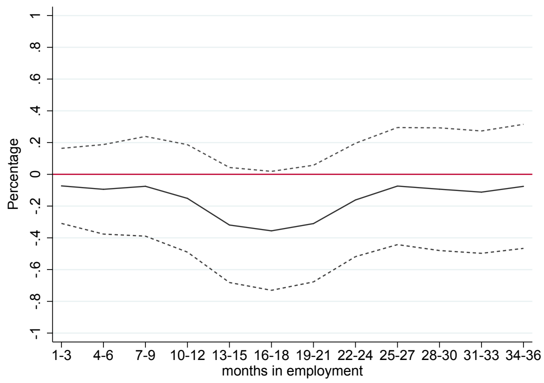
(a) *Employment-to-employment, males*



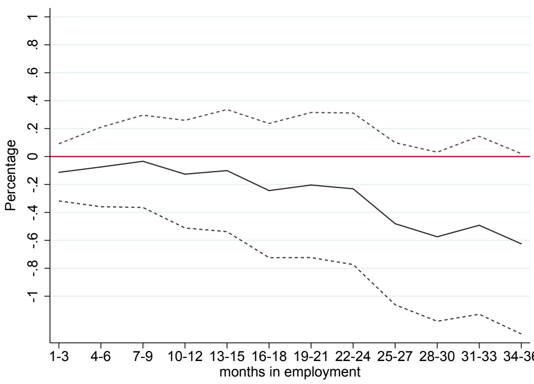
(b) *Employment-to-unemployment, males*



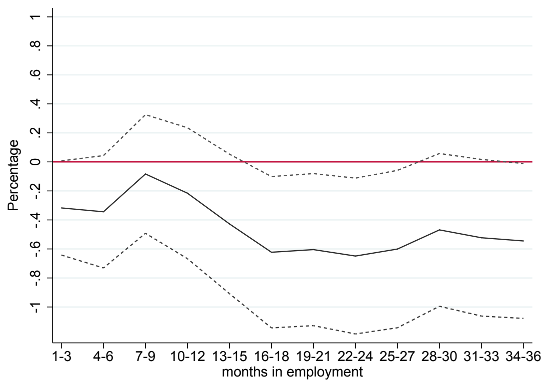
(c) *Employment-to-employment, young*



(d) *Employment-to-unemployment, young*



(e) *Employment-to-employment, skilled white-collar*



(f) *Employment-to-unemployment, skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probabilities from employment to unemployment and from employment to employment within m months for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The regressions exclude outlier municipalities defined by distances to the next MDF of above 8 km and DSL rates of above 60%. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted and performed separately for each month. Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust and clustered at the MDF level. Regressions are based on 2,594 municipalities and 804 MDFs for males, on 2,554 municipalities and 795 MDFs for young workers and on 2,268 municipalities and 747 MDFs for skilled white-collar workers. The Kleibergen-Paap F-Statistic for the first stage is 39.03, 39.41 and 33.53, respectively.

Figure F.2: IV regression results of DSL on leaving new employment relationships - excluding outlier municipalities

Table F.4: IV regression results of DSL on wage outcomes, excluding outlier municipalities, males

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	0.0008	0.0035	0.0018	0.0021
	(0.003)	(0.003)	(0.002)	(0.002)
Threshold (first stage)	-5.068***	-5.068***	-5.069***	-5.087***
	(0.811)	(0.811)	(0.811)	(0.807)
F-Statistic	39.03	39.03	39.05	39.68
Number of municipalities	2,594	2,594	2,593	2,529
Number of MDF's	804	804	804	795

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The regressions exclude outlier municipalities defined by distances to the next MDF of above 8 km and DSL rates of above 60%. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table F.5: IV regression results of DSL on wage outcomes, excluding outlier municipalities, young

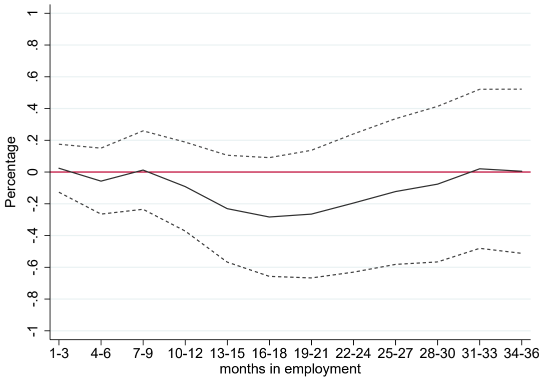
	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	0.0000	0.0018	0.0002	0.0030*
	(0.002)	(0.002)	(0.002)	(0.002)
Threshold (first stage)	-5.421***	-5.421***	-5.382***	-5.345***
	(0.864)	(0.864)	(0.854)	(0.862)
F-Statistic	39.41	39.41	39.72	38.43
Number of municipalities	2,554	2,554	2,532	2,507
Number of MDF's	799	799	798	793

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The regressions exclude outlier municipalities defined by distances to the next MDF of above 8 km and DSL rates of above 60%. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

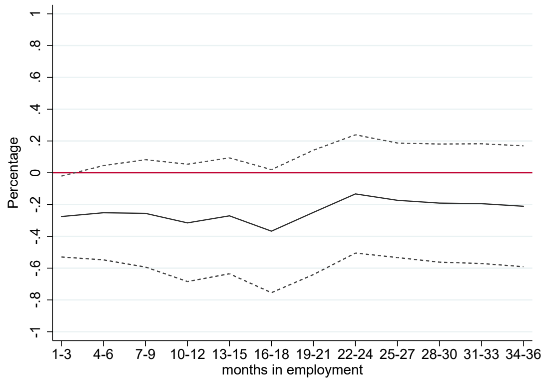
Table F.6: IV regression results of DSL on wage outcomes, excluding outlier municipalities, skilled white-collar

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	0.0011	0.0002	-0.0010	0.0061**
	(0.003)	(0.004)	(0.003)	(0.003)
Threshold (first stage)	-4.711***	-4.711***	-4.602***	-4.637***
	(0.813)	(0.813)	(0.809)	(0.799)
F-Statistic	33.53	33.53	32.38	33.70
Number of municipalities	2,268	2,268	2,253	2,227
Number of MDF's	747	747	744	739

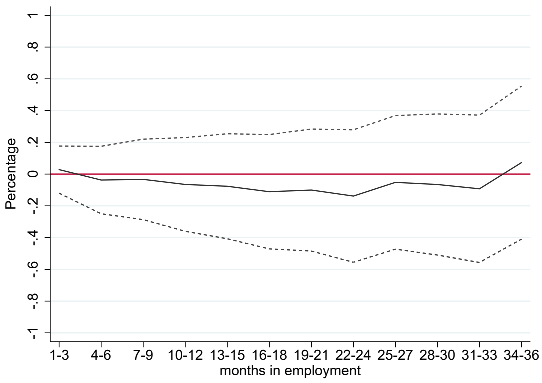
Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The regressions exclude outlier municipalities defined by distances to the next MDF of above 8 km and DSL rates of above 60%. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.



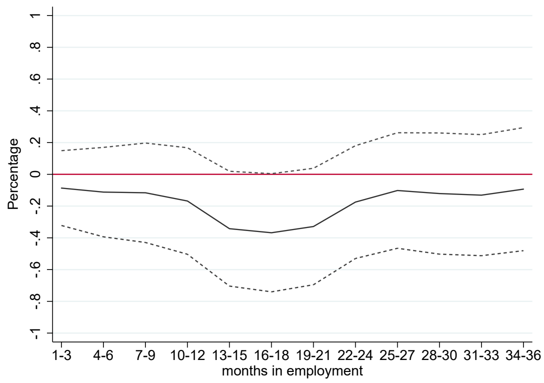
(a) *Employment-to-employment, males*



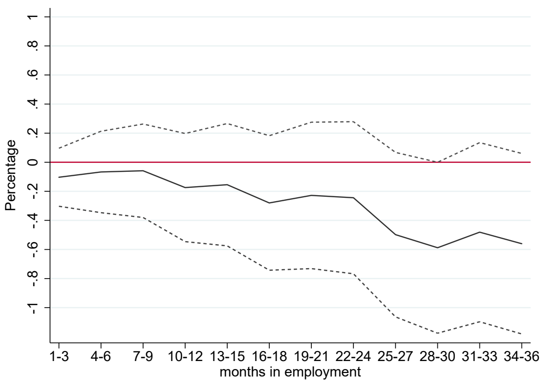
(b) *Employment-to-unemployment, males*



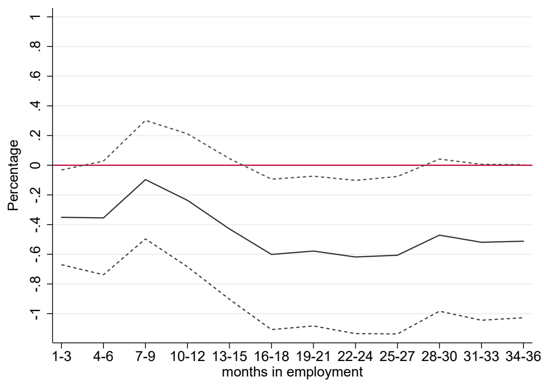
(c) *Employment-to-employment, young*



(d) *Employment-to-unemployment, young*



(e) *Employment-to-employment, skilled white-collar*



(f) *Employment-to-unemployment, skilled white-collar*

Notes: The figure shows the effects of a 1% point increase in the share of households with DSL availability on the cumulative transition probabilities from employment to unemployment and from employment to employment within m months for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions include the distance to the next urban center (source: Falck et al., 2014). The regressions are population-weighted and performed separately for each month. Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust and clustered at the MDF level. Regressions are based on 2,606 municipalities and 807 MDFs for males, on 2,566 municipalities and 802 MDFs for young workers and on 2,280 municipalities and 750 MDFs for skilled white-collar workers. The Kleibergen-Paap F-Statistic for the first stage is 38.41, 38.50 and 32.91, respectively.

Figure F.3: IV regression results of DSL on leaving new employment relationships - controlling for distance to next urban center

Table F.7: IV regression results of DSL on wage outcomes, controlling for distance to next urban center, males

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	0.0000	0.0036	0.0019	0.0024
	(0.003)	(0.003)	(0.002)	(0.002)
Threshold (first stage)	-5.209***	-5.209***	-5.209***	-5.230***
	(0.840)	(0.840)	(0.840)	(0.834)
F-Statistic	38.41	38.41	38.41	39.36
Number of municipalities	2,606	2,606	2,605	2,541
Number of MDF's	807	807	807	798

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions include the distance to the next urban center (source: Falck et al., 2014). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table F.8: IV regression results of DSL on wage outcomes, controlling for distance to next urban center, young

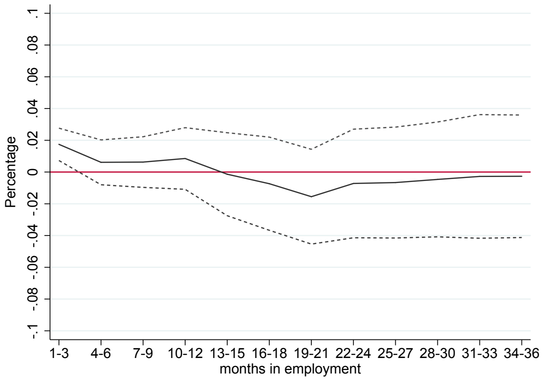
	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	-0.0002	0.0016	0.00025	0.0031*
	(0.002)	(0.002)	(0.002)	(0.002)
Threshold (first stage)	-5.540***	-5.540***	-5.497***	-5.440***
	(0.893)	(0.893)	(0.884)	(0.887)
F-Statistic	38.50	38.50	38.67	37.62
Number of municipalities	2,566	2,566	2,544	2,519
Number of MDF's	802	802	801	796

Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions include the distance to the next urban center (source: Falck et al., 2014). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

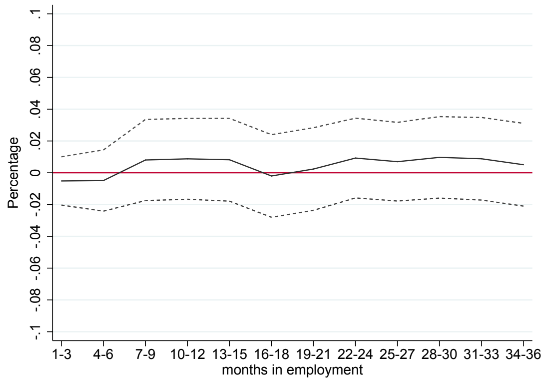
Table F.9: IV regression results of DSL on wage outcomes, controlling for distance to next urban center, skilled white-collar

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Δ DSL	0.0024	0.0010	-0.0013	0.0048**
	(0.003)	(0.003)	(0.003)	(0.002)
Threshold (first stage)	-4.891***	-4.891***	-4.775***	-4.816***
	(0.853)	(0.853)	(0.847)	(0.837)
F-Statistic	32.91	32.91	31.77	33.11
Number of municipalities	2,280	2,280	2,265	2,239
Number of MDF's	750	750	747	742

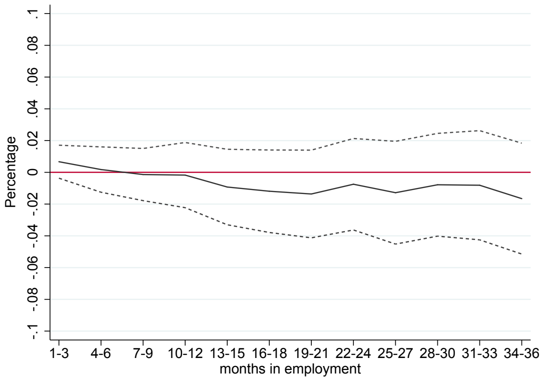
Notes: The table shows the effects of a 1% point increase in the share of households with DSL availability on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1998/1999 and 2007/2008. The coefficients are based on an IV model, where the distance is measured from the geographic centroid to the next MDF and weighted by the location of the population. The list of control variables includes the population structure, employment structure, occupational shares, industry shares and firm structure (see Table C.1). The regressions include the distance to the next urban center (source: Falck et al., 2014). The regressions are population-weighted. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.



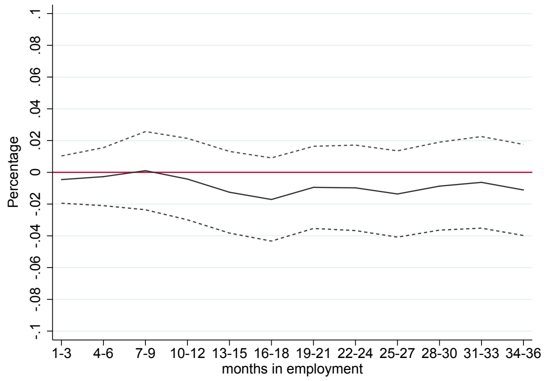
(a) *Employment-to-employment, males*



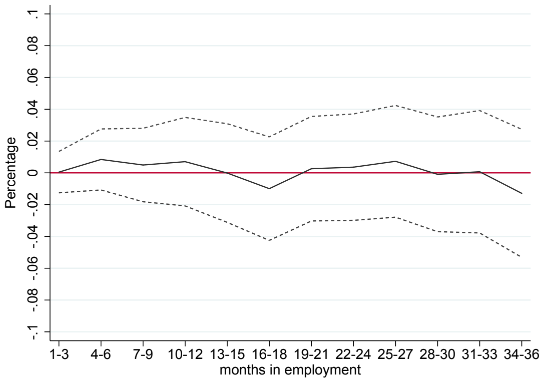
(b) *Employment-to-unemployment, males*



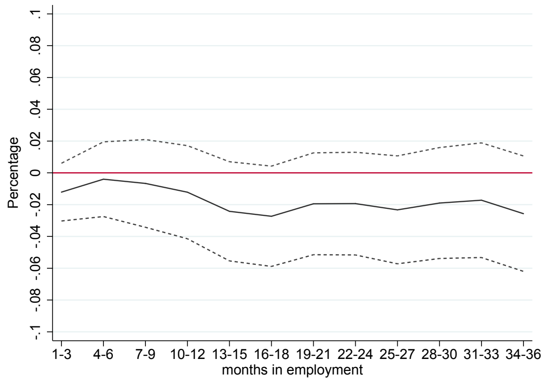
(c) *Employment-to-employment, young*



(d) *Employment-to-unemployment, young*



(e) *Employment-to-employment, skilled white-collar*



(f) *Employment-to-unemployment, skilled white-collar*

Notes: The figure shows the effects of the treatment dummy (PSTN) on the cumulative transition probabilities from employment to unemployment and from employment to employment within m months for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1995 and 1999. The endogenous variable is the change between 1999 and 1995. The list of control variables includes the employment structure, occupational shares and industry shares (see Table C.1). Due to data availability constraints we cannot control for firm dynamics, total population and age structure. The regressions are population-weighted and performed separately for each month. Dotted lines present the 95% confidence interval. Standard errors are heteroskedasticity robust and clustered at the MDF level.
Number of municipalities: Male: (A): 2,606; Young: (B): 2,566; Skilled white-collar: (C): 2,280.

Figure F.4: Regression results of the instrument on leaving new employment relationships - placebo tests

Table F.10: Regression results of the instrument on wage outcomes, placebo tests, males

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
PSTN	0.0172	0.0218	0.0010	0.0004
	(0.0119)	(0.0135)	(0.0084)	(0.0086)
Number of municipalities	2,606	2,606	2,605	2,541
Number of MDF's	807	807	807	798

Notes: The table shows the effects of the treatment dummy (PSTN) on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1995 and 1999. The endogenous variable is the change between 1999 and 1995. The list of control variables includes the employment structure, occupational shares and industry shares (see Table C.1). Due to data availability constraints we cannot control for firm dynamics, total population and age structure. The regressions are population-weighted. Dotted lines present the 95% confidence interval. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table F.11: Regression results of the instrument on wage outcomes, placebo tests, young

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
PSTN	0.0178	0.0135	-0.0032	0.0081
	(0.0121)	(0.0130)	(0.0105)	(0.0077)
Number of municipalities	2,566	2,566	2,544	2,519
Number of MDF's	802	802	801	796

Notes: The table shows the effects of the treatment dummy (PSTN) on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1995 and 1999. The endogenous variable is the change between 1999 and 1995. The regressions are population-weighted and performed separately for each month. The list of control variables includes the employment structure, occupational shares and industry shares (see Table C.1). Due to data availability constraints we cannot control for firm dynamics, total population and age structure. The regressions are population-weighted. Dotted lines present the 95% confidence interval. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table F.12: Regression results of the instrument on wage outcomes, placebo tests, skilled white-collar

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
PSTN	0.0055	-0.0136	-0.0091	0.0237**
	(0.0175)	(0.0180)	(0.0130)	(0.0093)
Number of municipalities	2,280	2,280	2,265	2,239
Number of MDF's	750	750	747	742

Notes: The table shows the effects of the treatment dummy (PSTN) on wage outcomes for an inflow sample of formerly unemployed individuals who entered a new employment relationship between 1995 and 1999. The endogenous variable is the change between 1999 and 1995. The regressions are population-weighted and performed separately for each month. The list of control variables includes the employment structure, occupational shares and industry shares (see Table C.1). Due to data availability constraints we cannot control for firm dynamics, total population and age structure. The regressions are population-weighted. Dotted lines present the 95% confidence interval. Standard errors are heteroskedasticity robust and clustered at the MDF level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

G Vacancy data

Table G.1: Descriptive statistics of the most recent hire's sample

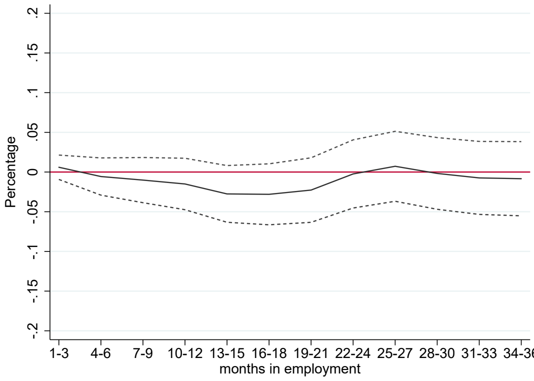
	All	SD	Non-online recruiting	Online recruiting	p-value
Establishment characteristics					
Number of employees	184.326	494.028	163.830	285.337	0.000
Share of female employees	0.444	0.276	0.440	0.466	0.014
Share of marginally employed	0.122	0.161	0.128	0.092	0.000
Age	41.351	5.368	41.589	40.179	0.000
Agriculture	0.019	0.135	0.021	0.005	0.002
Manufacturing	0.239	0.426	0.245	0.208	0.024
Energy, mining	0.072	0.259	0.078	0.047	0.002
Construction	0.045	0.208	0.051	0.016	0.000
Trade, retail	0.044	0.205	0.043	0.047	0.627
Hospitality	0.038	0.192	0.030	0.078	0.000
Transport, communication	0.095	0.293	0.090	0.120	0.008
Finance	0.043	0.203	0.042	0.048	0.419
Commercial services	0.111	0.314	0.107	0.129	0.076
Public administration	0.076	0.265	0.076	0.073	0.727
Education, health, social services	0.126	0.332	0.126	0.130	0.747
Other public and private services	0.092	0.289	0.091	0.100	0.391
Predetermined job characteristics					
Qualification: no vocational training	0.101	0.301	0.114	0.036	0.000
Qualification: vocational training	0.522	0.500	0.540	0.434	0.000
Qualification: advanced vocational training	0.124	0.329	0.123	0.126	0.817
Qualification: academic degree	0.228	0.420	0.195	0.391	0.000
Working hours per week	36.718	6.774	36.610	37.246	0.015
Working hours missing	0.010	0.100	0.011	0.005	0.113
Hard working conditions	0.129	0.335	0.140	0.074	0.000
Working time: weekend	0.505	0.500	0.514	0.456	0.002
Working time: shift	0.236	0.424	0.244	0.197	0.004
Working time: overtime	0.750	0.433	0.743	0.784	0.016
Working time: irregular	0.606	0.489	0.611	0.578	0.082
Special skills: long work experience	0.516	0.500	0.510	0.545	0.066
Special skills: training	0.196	0.397	0.186	0.248	0.000
Special skills: language	0.151	0.358	0.121	0.300	0.000
Special skills: social	0.573	0.495	0.550	0.689	0.000
Special skills: leadership	0.114	0.318	0.110	0.137	0.024
Most recent hire's characteristics					
Female	0.448	0.497	0.440	0.485	0.021
Low-skilled	0.094	0.292	0.098	0.078	0.080
Medium-skilled	0.669	0.471	0.691	0.560	0.000
High-skilled	0.237	0.425	0.211	0.362	0.000
Age	34.640	10.288	34.940	33.161	0.000
Employed before	0.511	0.500	0.517	0.485	0.096
N	4,796		3,987	809	

Notes: SD: standard deviation. The number of observations (N) refers to the number of hiring processes observed in 2009 and 2010 for whom the hired individual's employment history can be uniquely identified in the administrative records.

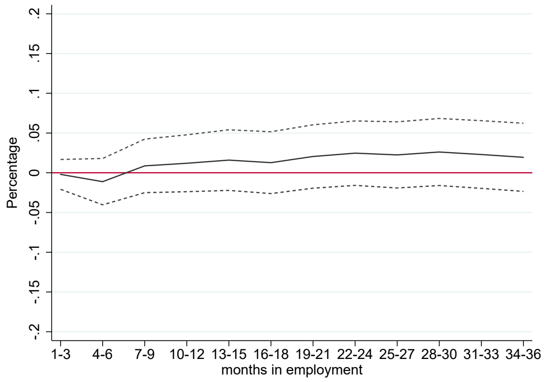
Table G.2: Wages and employment stability of the most recent hire

	N	All	SD	Non-online recruiting	Online recruiting	p-value
Log wage	4,790	4.185	0.677	4.158	4.316	0.000
Change log wage	4,679	0.416	0.933	0.411	0.440	0.439
Change log wage full-time	2,805	0.097	0.456	0.097	0.098	0.938
Wage growth after 1 year	3,580	0.084	0.337	0.089	0.063	0.088
Job duration	4,796	46.147	40.236	46.564	44.090	0.111
Job duration with etou transition	1,078	19.918	21.255	19.948	19.760	0.916
Job duration with etoe transition	1,745	31.568	29.503	31.750	30.735	0.582

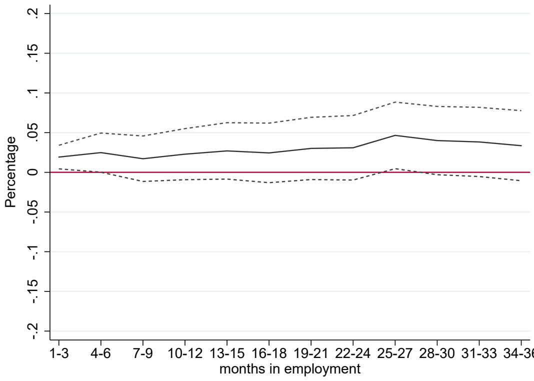
Notes: SD: standard deviation. The number of observations (N) refers to the number of hiring processes observed in 2009 and 2010 for whom the hired individual's employment history can be uniquely identified in the administrative records.



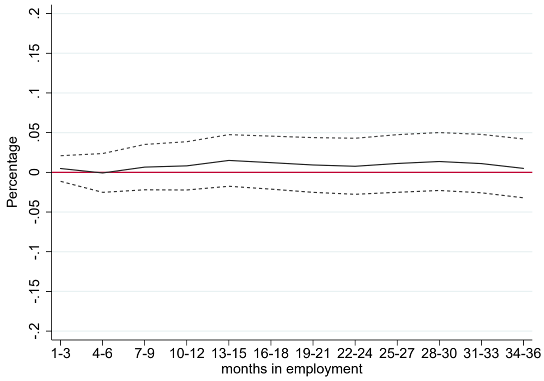
(a) *Employment-to-employment, males*



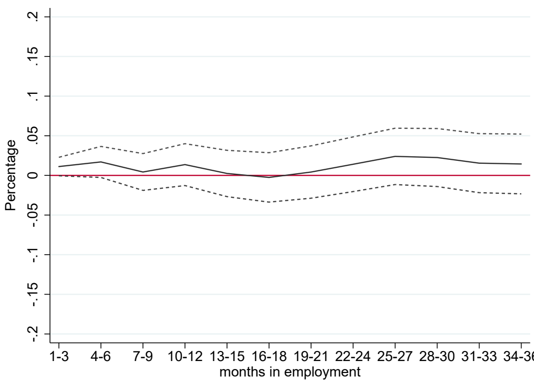
(b) *Employment-to-unemployment, males*



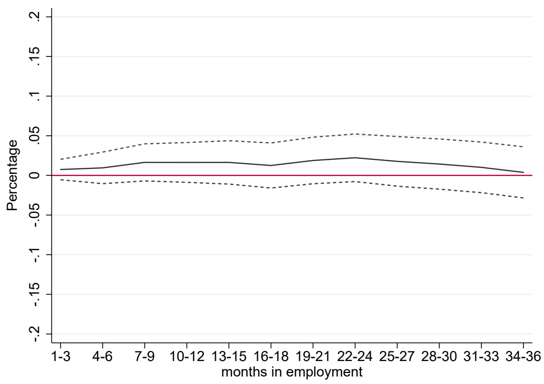
(c) *Employment-to-employment, young*



(d) *Employment-to-unemployment, young*



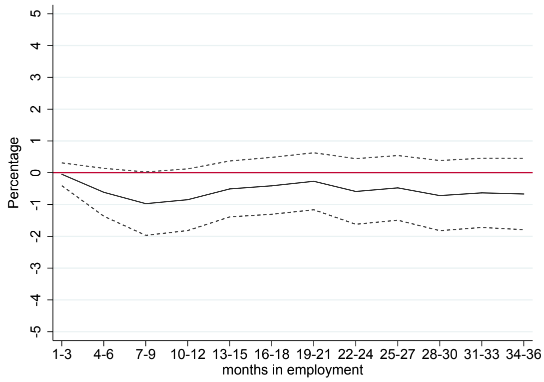
(e) *Employment-to-employment, skilled white-collar*



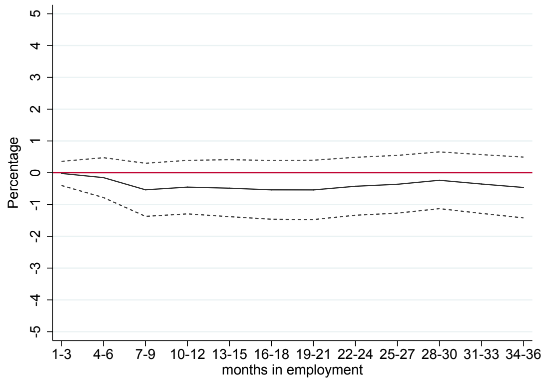
(f) *Employment-to-unemployment, skilled white-collar*

Notes: The figure shows OLS regression results of online recruiting on cumulative transition probabilities from employment to unemployment and from employment to employment within m months of the most recent hire by establishments in West Germany in 2009 and 2010. The list of control variables includes establishment and predetermined job characteristics and individual characteristics of the last hired person (see Table G.1). Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust. Regressions are based on 2,648 establishments for males, 2,703 establishments for young workers and 3,312 establishments for skilled white-collar workers.

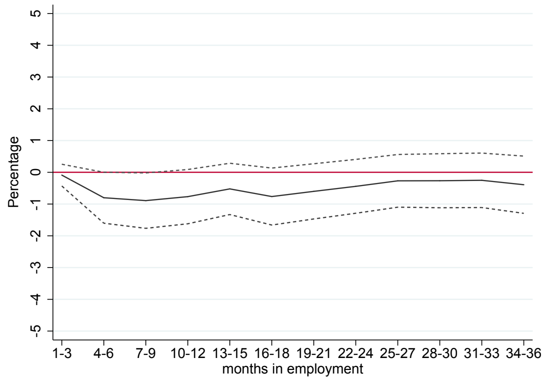
Figure G.1: OLS regression results of online recruiting on leaving new employment relationships



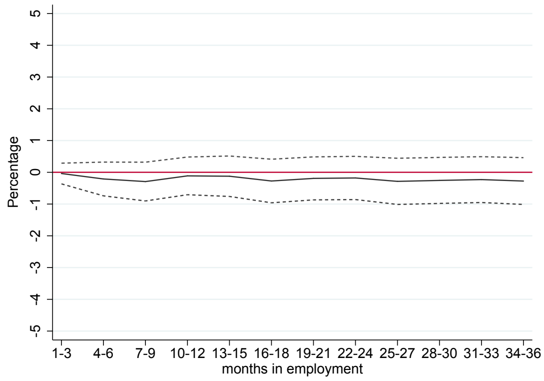
(a) *Employment-to-employment, males*



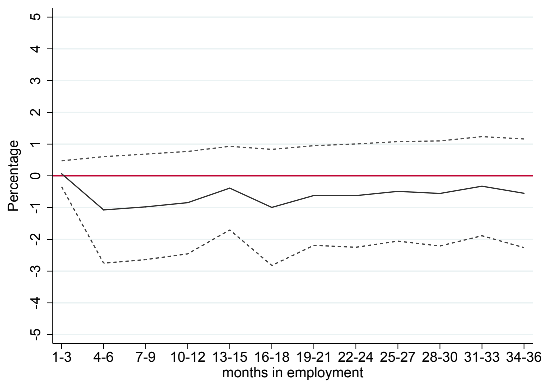
(b) *Employment-to-unemployment, males*



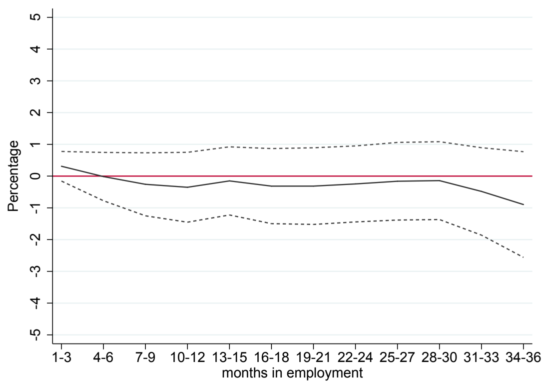
(c) *Employment-to-employment, young*



(d) *Employment-to-unemployment, young*



(e) *Employment-to-employment, skilled white-collar*



(f) *Employment-to-unemployment, skilled white-collar*

Notes: The figure shows IV regression results of online recruiting on cumulative transition probabilities from employment to unemployment and from employment to employment within m months of the most recent hire by establishments in West Germany in 2009 and 2010. Online recruiting is instrumented by a threshold dummy indicating whether the distance of the centroid of the hired person's home municipality to the next MDF is above 4,200 meters. The list of control variables includes establishment and predetermined job characteristics and individual characteristics of the last hired person (see Table G.1). Dotted lines present the 95% confidence intervals. Standard errors are heteroskedasticity robust. Regressions are based on 2,648 establishments for males, 2,703 establishments for young workers and 3,312 establishments for skilled white-collar workers. The Kleibergen-Paap F-Statistic for the first stage is 10.77, 13.71 and 2.71, respectively.

Figure G.2: IV regression results of online recruiting on leaving new employment relationships

Table G.3: Regression results of online recruiting on wage outcomes, males

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Panel A: OLS				
Online recruiting	0.036	-0.011	-0.016	-0.017
	(0.028)	(0.051)	(0.028)	(0.021)
Panel B: IV				
Online recruiting	-0.424	1.311	0.853	-0.359
	(0.509)	(0.955)	(0.529)	(0.352)
Threshold (first stage)	-0.068***	-0.069***	-0.080***	-0.089***
	(0.021)	(0.021)	(0.022)	(0.023)
F-Statistic	10.77	10.81	13.03	14.49
Observations	2,648	2,607	1,906	1,958

Notes: The table shows regression results of online recruiting on wage outcomes of the most recent hire by establishments in West Germany in 2009 and 2010. Panel A shows the OLS coefficients. Panel B shows the coefficients from the IV model, where online recruiting is instrumented by a threshold dummy indicating whether the distance of the centroid of the hired person's home municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust. The list of control variables includes establishment, predetermined job characteristics and individual characteristics of the last hired person (see Table G.1). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table G.4: Regression results of online recruiting on wage outcomes, young

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Panel A: OLS				
Online recruiting	0.053**	0.050	0.030	-0.027
	(0.026)	(0.048)	(0.032)	(0.019)
Panel B: IV				
Online recruiting	-0.537	0.449	0.780**	0.060
	(0.403)	(0.715)	(0.349)	(0.239)
Threshold (first stage)	-0.084***	-0.094***	-0.108***	-0.098***
	(0.023)	(0.022)	(0.027)	(0.026)
F-Statistic	13.61	17.87	15.69	13.94
Observations	2,699	2,629	1,511	1,973

Notes: The table shows regression results of online recruiting on wage outcomes of the most recent hire by establishments in West Germany in 2009 and 2010. Panel A shows the OLS coefficients. Panel B shows the coefficients from the IV model, where online recruiting is instrumented by a threshold dummy indicating whether the distance of the centroid of the hired person's home municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust. The list of control variables includes establishment, predetermined job characteristics and individual characteristics of the last hired person (see Table G.1). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table G.5: Regression results of online recruiting on wage outcomes, skilled white-collar

	Log wage	Change log wage	Change log wage full-time	Wage growth after 1 year
Panel A: OLS				
Online recruiting	0.028	-0.036	-0.007	-0.020
	(0.023)	(0.042)	(0.028)	(0.017)
Panel B: IV				
Online recruiting	-0.122	-0.288	0.635	-0.299
	(0.777)	(1.460)	(0.708)	(0.421)
Threshold (first stage)	-0.041*	-0.045*	-0.064*	-0.063**
	(0.025)	(0.025)	(0.033)	(0.026)
F-Statistic	2.71	3.35	3.72	5.78
Observations	3,306	3,228	1,872	2,560

Notes: The table shows regression results of online recruiting on wage outcomes of the most recent hire by establishments in West Germany in 2009 and 2010. Panel A shows the OLS coefficients. Panel B shows the coefficients from the IV model, where online recruiting is instrumented by a threshold dummy indicating whether the distance of the centroid of the hired person's home municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust. The list of control variables includes establishment, predetermined job characteristics and individual characteristics of the last hired person (see Table G.1). Skilled white-collar jobs include managers, professionals, technicans und clerical support workers. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table G.6: Descriptive statistics of establishment and job characteristics

	All	SD	Non-online recruiting	Online recruiting	p-value
Establishment characteristics					
Number of employees	150.341	437.974	134.237	237.810	0.000
Share of female employees	0.452	0.286	0.449	0.472	0.005
Share of marginally employed	0.132	0.171	0.139	0.092	0.000
Age	41.462	5.725	41.668	40.342	0.000
Agriculture	0.020	0.142	0.023	0.007	0.000
Manufacturing	0.219	0.413	0.223	0.197	0.035
Energy, mining	0.073	0.261	0.077	0.053	0.002
Construction	0.046	0.209	0.050	0.024	0.000
Trade, retail	0.044	0.204	0.043	0.048	0.369
Hospitality	0.042	0.200	0.037	0.069	0.000
Transport, communication	0.090	0.286	0.086	0.114	0.001
Finance	0.043	0.203	0.042	0.048	0.308
Commercial services	0.114	0.317	0.111	0.127	0.100
Public administration	0.078	0.267	0.080	0.066	0.084
Education, health, social services	0.136	0.343	0.134	0.148	0.153
Other public and private services	0.096	0.294	0.095	0.099	0.621
Predetermined job characteristics					
Qualification: no vocational training	0.106	0.307	0.118	0.040	0.000
Qualification: vocational training	0.504	0.500	0.519	0.424	0.000
Qualification: advanced vocational training	0.129	0.335	0.127	0.140	0.194
Qualification: academic degree	0.217	0.412	0.188	0.374	0.000
Working hours per week	36.493	7.147	36.326	37.381	0.000
Working hours missing	0.026	0.158	0.029	0.007	0.000
Hard working conditions	0.130	0.337	0.140	0.080	0.000
Working time: weekend	0.505	0.500	0.511	0.474	0.016
Working time: shift	0.231	0.422	0.238	0.196	0.001
Working time: overtime	0.730	0.444	0.722	0.775	0.000
Working time: irregular	0.598	0.490	0.602	0.574	0.057
Special skills: long work experience	0.502	0.500	0.495	0.544	0.001
Special skills: training	0.189	0.391	0.180	0.238	0.000
Special skills: language	0.137	0.344	0.114	0.264	0.000
Special skills: social	0.554	0.497	0.529	0.689	0.000
Special skills: leadership	0.115	0.319	0.110	0.137	0.006
N	8,406		7,099	1,307	

Notes: SD: standard deviation. The number of observations (N) refers to the number of hiring processes observed in 2009 and 2010.

Table G.7: Online recruiting, applicants and compromises

	N	All	SD	Non-online recruiting	Online recruiting	p-value
Number of applicants	6,826	17.840	34.696	15.466	29.109	0.000
Share of unsuitable applicants	6,669	0.495	0.347	0.457	0.670	0.000
Share of unsuitable male applicants	6,315	0.446	0.396	0.405	0.639	0.000
Share of unsuitable female applicants	6,405	0.359	0.396	0.321	0.541	0.000
Compromises in filling vacancy	8,208	0.165	0.372	0.161	0.187	0.021

Notes: SD: standard deviation. The number of observations (N) refers to establishments observed in 2009 and 2010.

Table G.8: Online search, non-online search, recruitment failure and open vacancies

	N	All	SD	Non-online search	Online search	p-value
Non-online search channels						
Newspapers	8,406	0.366	0.482	0.273	0.517	0.000
Referral	8,406	0.361	0.480	0.422	0.262	0.000
Empl. Agency	8,406	0.241	0.428	0.151	0.387	0.000
Own-initiative	8,406	0.227	0.419	0.221	0.237	0.092
Sum non-online	8,406	1.658	1.043	1.459	1.984	0.000
Indicator recruitment failure (random draw)	5,661	0.072	0.258	0.034	0.107	0.000
Open vacancies	3,513	0.519	6.492			

Notes: SD: standard deviation. For the indicator variable for a recruitment failure N refers to establishments observed in 2011. For the number of open vacancies N refers to establishments observed in 2009.

Table G.9: IV regression results for online recruiting on compromises in filling vacancy

	Full sample	Skilled white-collar	Other jobs
Online recruiting	0.596*	0.488	0.882
	(0.317)	(0.416)	(0.537)
Threshold (first stage)	-0.059***	-0.066***	-0.052***
	(0.013)	(0.023)	(0.015)
F-Statistic	21.28	8.11	11.88
Observations	8,208	4,665	3,095

Notes: The table shows IV regression results of online recruiting on compromises by employers when filling the vacancy for establishments in West Germany in 2009 and 2010. The results are based on linear probability models. Online recruiting is instrumented by a threshold dummy indicating whether the distance of the centroid of an establishment's municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust. The list of control variables includes establishment and predetermined job characteristics (see Table G.6). Skilled white-collar jobs include managers, professionals, technicans und clerical support workers. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table G.10: IV regression results for online search on non-online search channels

	Newspapers	Referral	Empl. Agency	Own-initiative	Sum non-online
Online search	-0.119	-0.364	0.049	0.018	0.392
	(0.254)	(0.255)	(0.218)	(0.209)	(0.479)
Threshold (first stage)	-0.097***	-0.097***	-0.097***	-0.097***	-0.097***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
F-Statistic	21.73	21.73	21.73	21.73	21.73
Observations	8,406	8,406	8,406	8,406	8,406

Notes: The table shows IV regression results of online search on non-online search channels for establishments in West Germany in 2009 and 2010. The results are based on linear probability models. Online search is instrumented by a threshold dummy indicating whether the distance of the centroid of an establishment's municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust. The list of control variables includes establishment and predetermined job characteristics (see Table G.6). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table G.11: IV regression results for online search on recruitment failure

	Indicator recruitment failure Random draw
Online search	0.045
	(0.287)
Threshold (first stage)	-0.052*
	(0.030)
F-Statistic	3.058
Observations	5,661

Notes: The table shows IV regression results of online search on the probability of a recruitment failure for establishments in West Germany in 2011. The results are based on linear probability models. Online search is instrumented by a threshold dummy indicating whether the distance of the centroid of an establishment's municipality to the next MDF is above 4,200 meters. The F-test of excluded instruments refers to the Kleibergen-Paap F-Statistic. Standard errors are heteroskedasticity robust. The list of control variables includes establishment and predetermined job characteristics (see Table G.6). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

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