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Conference Paper

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Beiträge zur Jahrestagung des Vereins für Socialpolitik 2015: Ökonomische Entwicklung - Theorie und Politik - Session: Labor 3, No. C17-V2

Provided in Cooperation with:
Verein für Socialpolitik / German Economic Association

Suggested Citation: Moczall, Andreas (2015) : The effect of hiring subsidies on regular wages, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2015: Ökonomische Entwicklung - Theorie und Politik - Session: Labor 3, No. C17-V2, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft

This Version is available at:
https://hdl.handle.net/10419/113225

The effect of hiring subsidies on regular wages

Abstract

What happens to the wages of regular workers in establishments subsidized with hiring subsidies? Does hiring programme participants result in windfalls that are distributed among regular workers? Do these reduce their wage demands to avoid being substituted by subsidized workers? Using linked employer-employee data from Germany, I estimate the effects of subsidizing an establishment on regular workers' wages using spell fixed effects regression. I find that hiring subsidy schemes do increase the daily wages of regular workers by up to almost one per cent in the manufacturing sector. These effects are limited to large establishments and above-median local unemployment rates. They occur within the establishment itself and are not merely the result of varying regional exposure to ALMP programmes. I conclude that hiring subsidies have a notable impact on regular workers beyond mere substitution.

JEL classification: J38, J68, H25, C23

Keywords: Wages, hiring subsidies, wage premium effect, linked employer-employee data

1 Introduction and Motivation

Research on the effect of active labour market policy (ALMP) programmes has traditionally focused on two effects: on the participating individuals, and on the economy as a whole. There has been little research on its effect on establishments. There are two reasons for this. First is that there are far fewer data sets of sufficient size and richness to study programme effects on the establishment level. Many high-quality establishment-level data sets have been created over the past fifteen years both from surveys and administrative sources in several countries. However, very few of them include information on subsidized employment. Even if they include it, the number of subsidized establishments is very small and provides few details on the characteristics of the subsidized individuals and positions. Second, the purpose of active labour market policy itself for the most part is not to improve the performance of establishments. Its purposes lie on the individual level --- to raise the chances of the unemployed of finding work --- and on the macroeconomic level --- to reduce overall unemployment. Both individual and macroeconomic outcomes however are necessarily the result of employer-employee interactions. Therefore, the investigation of employer behaviour is a necessary component in understanding why programmes do or do not work as intended.

The establishment-level perspective has then mostly been used to study unwanted side-effects of programmes. In particular, the comparative losses of regular employment in subsidized establishments due to deadweight loss and substitution effects (see e.g. Maré 2005; Calmfors/Forslund/Hemström 2001) have been investigated. In this line of research, papers attempting to provide isolated substitution effects using employer surveys tend to find comparatively high levels of regular employment losses (for an overview see Welters/Muysken 2008, 2006; Welters 2005). This stands in contrast to econometric studies that model counterfactual employment levels of subsidized establishments to measure the total effect on regular employment. This type of study tends to find fewer regular employment losses and even some regular employment gains (Crichton/Maré 2013; Moczall 2014, 2013; Hohendanner 2011; Rotger/Arendt 2010). Unless all these econometric studies are systematically biased, this indicates that either substitution effects are overstated in employer surveys or are counteracted by regular employment gains through scale effects. They may be overstated because they are conducted at a time when the employer has observed the subsidized workers' true productivity, while the actual hiring decision would have to be made without such knowledge. Scale effects by nature are not covered in employer surveys focusing only on the subsidized jobs themselves.

However, hiring subsidies may affect regular non-subsidized workers in that establishment in ways other than their employment prospects. Regular workers may be affected in their wages. Calmfors (1994) mentions that active labour market policy may have “competition effects for insiders”. Programmes may increase the competitiveness of outsiders (here: long-term unemployed). This potentially reduces the

wages of insiders (here: the employed and short-term unemployed) “by increasing their productivity, by substituting participation in labour market programmes for regular work experience when employers screen job applicants, or by encouraging more active search behavior” (Calmfors 1994: 16 f). Calmfors & Lang (1995) present a union wage-setting model to predict macroeconomic effects of ALMP programmes on wages (not just regular wages). Programmes reduce wages for outsiders as they prevent the labour supply from decreasing by maintaining labour force participation. They increase wages for insiders as they make it less damaging for insiders to risk being replaced, and thus becoming unemployed, from excessive wage demands. The more programmes are targeted to the long-term unemployed, the more a redistribution from insiders to outsiders occurs.

There has been very little study of the question how hiring subsidies affect the wages of regular workers in the same establishment. I am aware of only two papers that are loosely related to this topic. Kangasharju (2007) estimates the effect of a Finnish hiring subsidy on subsidized establishments' total wage sum using tax register data from 1995 to 2002 (about 30,000 establishments). He finds that the total effect on payroll is as high as would be expected in the absence of deadweight loss, substitution and displacement effects. He thus concludes that wage subsidies have the intended employment effect in magnitude. He however does not distinguish the wage sum (or the average wages) of regular versus subsidized workers. Lechner et al. (2013) estimate the effect of ten different programmes on over 40 different indicators of establishment performance using linked employer-employee data from Germany. They measure the effects from 2004 to 2008 from varying regional (not firm-level) exposure to programmes in 2001 to 2003. The “treatment” is a “high” versus “low” exposure to regional treatment intensities of each programme type. While the paper provides results for “subsidized employment”, this category combines both the kind of hiring subsidy studied in this paper as well as Job Creation Schemes. As with the Kangasharju paper, the effect on wages is not separated by regular and subsidized workers, and is not investigated in the actual econometric analyses. Descriptive tabulations show that an establishment receiving “high” regional exposure to “subsidized employment” is associated with lower average monthly earnings than those receiving “low” exposure (2,790 versus 2,700 euros).

This paper therefore is the first to study the effect of hiring subsidies on the wages, rather than the employment prospects, of regular workers in subsidized establishments. It makes use of a large linked employer-employee dataset and achieves identification by way of a spell fixed effects model. Section 2 briefly explains the hiring subsidy scheme being analysed and presents the research questions. Section 3 describes the linked employer-employee data set and the spell fixed effects model being used for the analyses. Section 4 presents the results. Section 5 discusses these results, while Section 6 concludes.

2 Background and research hypotheses

2.1 Institutional background of hiring subsidies in Germany

Hiring subsidies ("Eingliederungszuschüsse") are available to employers when hiring a person whose placement is impeded by personal attributes. They are of a limited duration based on the assumption that any productivity deficit that prevents those job-seekers from finding an unsubsidized job will diminish with work experience (Orszag/Snower 2003). Employers apply to the local employment agency or job centre for a hiring subsidy if they are seriously considering hiring a particular qualifying applicant. Job-seekers are encouraged by their placement manager to mention to any prospective employer that a hiring subsidy may be available for them. The actual reimbursement details are decided by the case worker and are to be based both on the subsidized individual's productivity deficits and the requirements of the job, not on the prospective employer's firm performance. If the subsidy is approved, employers receive a reimbursement of up to 50 % of wage costs for up to one year, and have to employ the person for which the subsidy was received for the same period afterwards, otherwise they may have to repay some part of the subsidy.[1] Special hiring subsidy types exist for disabled job-seekers and those below the age of 25 and the age of 50 and above, some with longer durations and higher reimbursement amounts. See Schünemann, Lechner & Wunsch (2013), Brussig & Schwarzkopf (2011) and Jaenichen & Stephan (2011) for a more detailed description and evaluations of the programme on its effects on participants.

2.2 Research questions

What is the effect of subsidizing an establishment on regular workers' wages? How does the effect differ across tenure?

In a simple static model of establishment labour demand with no adjustment costs (see Hujer/Caliendo/Radic 2001; Hamermesh1993), receiving a subsidy has two effects. First, the relative wage costs between subsidized and non-subsidized worker groups change so that demand for subsidized worker groups increases while demand for regular worker groups decreases. This is called a substitution effect. Second, because the subsidy lowers the cost of labour, the price of output decreases, resulting in an increase in the demand for output and thus an increase in demand for regular workers. This is called a scale effect.

This simple model assumes that regular workers' wages are exogenous and do not react to an establishment being subsidized. In practice several reactions of regular workers are possible. Regular workers who are at risk of being substituted in favor of subsidized workers could lower their wage demands to avoid being made redundant, for example, by not asking for wage increases or promotions. The presence of programme participants in the establishment serves as a signal that such substitu-

[1] It is not known how strictly the latter is actually enforced.

tion may be imminent or possible in the future and therefore depresses the wages of those regular workers that can easily be substituted. I call this a “threat effect” of ALMP programmes on regular wages. Evidence for this scenario of threat effect would be a negative effect on regular wages among workers who have been in the establishment for some time.

There is another scenario for this threat effect. The presence of subsidized workers is not directly known to job-seekers but merely indicates (to the researcher) that the employer is one who uses hiring subsidies as part of his personnel policy. If he can choose between workers for whom hiring subsidies are available and those for whom they are not, the latter will have to accept lower wages to be on par with the former in their chances at getting a job with this employer. As the former group's advantage will disappear once the subsidy is no longer paid, evidence for this scenario of threat effect would be a negative effect on regular wages only among entrants. Given that entrants have less firm-specific human capital than persons who have been with this employer for some time and are thus easier to substitute, this scenario should be the more likely of the two.

Both scenarios of threat effect will find their limitations in collective or sector-wide wage agreements which mean that employees cannot readily adjust wages downward even if they want to. The threat effect should therefore occur solely, or at least be greater, in establishments without a wage agreement or in which employers voluntarily pay above what is specified in a collective agreement. Furthermore, efficiency wage theory would predict that depressing wages in such a manner may negatively affect productivity by increasing shirking, by increasing the feeling of receiving an unfair wage, and by attracting less qualified workers.

On the other hand, hiring subsidies could also increase the wages of regular workers, at least for some of them. Workers with a long tenure (insiders) are highly dissimilar to job-seekers who would qualify for a subsidy, putting them at a low risk of substitution. When bargaining for wages, they could argue (rightly or not) that the employer enjoys a windfall due to the subsidy and thus demand higher wages than without subsidization. As they are not easily replaceable, they may be able to realize such demands. This adjustment may take place on short notice even with wage agreements in place in the form of bonuses that are dependent on an establishment's financial situation. Another reason why insiders' daily wages may rise is that because subsidized workers arguably are less productive than regular workers, insiders need to work extra hours to train them on-the-job. Insiders may also regard subsidized workers as unwelcome competition and demand a wage premium for cooperating with them. For all these reasons, subsidizing an establishment with hiring subsidies should increase insiders' wages, which I call a “wage premium effect”[1].

How does the effect differ between establishments, workers and labour markets?

When only daily, rather than hourly wages, can be observed, it cannot be measured directly whether any wage premium effect is due to windfalls, cooperation premiums or additional working hours. At least windfalls versus working hours may be distinguished however by finding situations in which one explanation is far more plausible rather than the other. For example, Welters & Muysken (2006) theorized and found empirically that large firms will produce more deadweight loss from hiring subsidies than small firms because they face lower screening costs due to economies of scale. They will therefore consider more unemployed job-seekers for assessment and therefore would have hired more of them even without a subsidy. Because deadweight loss from a policy perspective would result in a windfall from an employer perspective, if a daily wage premium effect is found primarily among large establishments, it will plausibly result from an increase in average hourly wages. Regular workers needing to work more hours to train and supervise unproductive subsidized hires would not affect the entire workforce, but only a few persons who directly work with them. The share of those regular workers among the entire workforce will necessarily be larger in small establishments. Therefore, if a daily wage premium effect is found primarily among small establishments, plausibility would instead point to the additional hours explanation.

It is considered standard and necessary in the evaluation of German labour market programmes to distinguish effects between East and West Germany (Lechner/Wunsch 2009). This is motivated by the two regions being highly dissimilar in their labour markets. East German firms are on average smaller, less innovative; their workforce is older, and the region exhibits negative population growth (Heimpold/Titze 2014). A dearth of regular job opportunities results in labour market programmes being granted on a larger scale with looser standards (Jacobi/Kluve 2007). For this reason, I expect the results of subsidization on regular wages to be different in West and East Germany. Because women in East Germany have a much greater labour market attachment however, adequately identifying effect differences between the two regions requires simultaneously separating the sample by gender as well.

Apart from this coarse separation, there will be comparatively good and bad labour markets within each region, good in terms of a low unemployment rate. Case managers facing subsidy granting decisions in bad labour markets will be judged by how much they can reduce unemployment. They will be much more willing to grant hiring subsidies, in higher or longer amounts. Therefore, in bad labour markets, subsidies are more likely to be overpaid in terms of whether granted at all, reimbursement amount and duration. A hiring subsidy is overpaid if it reimburses more than the jobseeker's productivity deficit as perceived by the employer, yielding windfalls for the

subsidized employer.[2] Therefore, if a positive effect of hiring subsidies on regular wages is observed primarily in high-unemployment labour markets, it would be evidence for increased average hourly wages from windfalls due to overly generous subsidy granting decisions.

Does the establishment-level effect change when including the regional programme intensity?

A full macroeconometric analysis of the relationship between the local programme intensity and regular wages is not within the scope of this paper. Nevertheless, controlling for local programme intensity should answer the question whether the establishment-level effect is actually a manifestation of an effect that occurs on the entire local labour market. This would be the case if the establishment-level effect changes significantly between a model specification with and without the local labour market's programme intensity.

3 Data and method

3.1 The LIAB with extensions

To identify a causal effect of an establishment receiving hiring subsidies on regular workers' individual wage levels, it is necessary to control both for individual-level and establishment-level characteristics. This requires linked employer-employee data. For German establishments, such a linked dataset is available as the LIAB (Heining/Scholz/Seth 2013). LIAB combines the survey data from the IAB Establishment Panel (Fischer et al. 2008) with administrative data on individuals from the German social security administration and from the Federal Employment Agency (BA). The administrative data were originally generated by employers reporting employment contracts to the social security administration or by individuals while applying for unemployment and for welfare benefits. They have been anonymized and made available as a scientific use file by the Research Data Centre (FDZ) at the German Federal Employment Agency. The IAB Establishment Panel is a yearly representative survey of German businesses across all sectors and establishment sizes; its net sample amounts to roughly 16,000 observations each year. The individual data combine records from the social security administration with unemployment, welfare benefit and job search information from the Federal Employment Agency. For every year that an establishment is in the IAB Establishment Panel, LIAB includes individual characteristics for all individuals employed in that establishment on June 30th measured on that date. Among these characteristics are, apart from sociodemographic and job characteristics, daily wages, the days in that job, in the labour market and the duration of previous unemployment periods.

[2] Note that any observed hiring subsidy can only be “just right” or overpaid. Underpaid hiring subsidies are not observed because the employer simply would not hire the person in question.

LIAB allows for longitudinal analyses of establishments and the individuals they employ. I will make use of ten waves, covering the period from 2001 to 2010. I exclude persons who are not employed full-time, are in an apprenticeship, or are minor-employed (earning less than 400 euros a month). As German social security record data do not include the precise working time (beyond the full-time/part-time distinction), I can only observe daily wages, not hourly wages. Furthermore, German social security record data is right-censored at the contribution limit. For analyses involving the distribution of wages, it is customary in literature using German data to use the method described in Gartner (2005) to statistically impute wages above the the contribution limit. However, as Blien et al. (2013), Schank, Schnabel & Wagner (2007) point out, imputation only has a negligible effect in Mincer-type models. As it adds an additional layer of uncertainty to the data, I perform the main analysis using non-imputed data, comparing against imputed data only for robustness purposes.

LIAB itself has no information on programme participation. Since this information is needed to separate regular from subsidized workers, programme participation spells from the Federal Employment Agency's process data are added to the data set on both the individual and the establishment level. For the programme intensity at the district level as well as local labour market characteristics, the Federal Employment Agency provides data on NUTS level 3 (districts). Among these data are the stock of employed, unemployed, long-term unemployed (one year or longer) persons as well as the stock of participants in various programmes.

3.2 Identification

To study an effect on wages, the usual starting point is a Mincer-type earnings function. The logarithm of individual i's wage $y_{i,t}$ at time t is explained by individual i's characteristics $x_{i,t}$ and the characteristics $z_{j(i,t)t}$ of the establishment j that employs i at time t. Part of $x_{i,t}$ are years of schooling and labour market experience. Part of $z_{j(i,t)}$ is an indicator of whether an establishment employs participants of ALMP programmes of different types.

$$y_{it} = \beta x_{it} + \gamma z_{j(i,t)t} + \theta_i + \psi_{j(i,t)} + \varepsilon_{it} \quad (1)$$

In practice, not all relevant individual-level and establishment-level covariates are observed. Fixed effects of unobservables can be grouped into an individual fixed effect θ_i and an establishment fixed effect ψ_j (Andrews/Schank/Upward 2006; Abowd/Kramarz/Margolis 1999). There are several STATA routines available for the estimation of such a two-way fixed effects model (Mittag 2012; Guimaraes/Portugal 2010; Cornelissen 2008). Since I am not interested in estimates of the time-invariant person and establishment effects θ_i or ψ_j but only in the coefficients of certain time-varying establishment characteristics $z_{j(i,t)t}$, I use a spell fixed effects model (Andrews/Schank/Upward 2006) that includes only one combined fixed effect: that of a worker-establishment combination (a “spell”). This kind of model controls for the effects of all sources of time-invariant unobserved heterogeneity of establishments

and individuals, which will include most sources of sample selection bias (Vella 1998).

The spell fixed effects model also nicely accounts for the fact that the base wage level is not negotiated anew from year to year. Including it in the spell fixed effect means that only the change in wages within that worker-firm combination needs to be explained. As Andini (2013a, 2013b) shows, earnings persistence matters greatly when explaining returns to schooling; not taking it into account would result in “persistence bias”. His structural model, a dynamic wage model with a lagged regressand, is derived from a simple wage-bargaining model using unemployment benefit level as an outside option. The worker's bargaining power is the coefficient of the lagged wage regressand, so that the regular Mincer model becomes a special case for the situation in which the worker's bargaining power is zero. It could be argued that the dynamic wage model therefore is the “correct” model because it takes earnings persistence into account even in its structural form. In the spell fixed effects model on the other hand, the base wage level is merely differenced out as a by-product of the statistical procedure. Its advantage lies in taking earnings persistence into account in a far simpler and more robust manner, as no additional endogenous regressor is introduced that would be expected to be highly correlated with the variables of interest. The disadvantage of course is that no effect for time-invariant characteristics can be estimated, making it unsuitable for estimating returns to schooling. Because I am only interested in the effect of an establishment characteristic that changes over time, I do not need the additional complexity of a dynamic model to estimate the effect of interest in this paper. Hence, the spell fixed effects model is the most suitable for the given application.

Table 1: Establishment growth rates by subsidization status

	one-year growth rate (in %)	
year	non-HSE	HSE
2001	-2.8	5.3
2002	-3.6	2.6
2003	-5.1	2.6
2004	-2.2	5.8
2005	-4.1	2.8
2006	0.1	10.6
2007	1.1	11.3
2008	0.5	10.0
2009	-0.5	6.7
2010	0.9	10.2

Values are 1-year growth rates of each establishment's entire employment stock averaged over non-weighted establishments. HSE: Establishment receiving hiring subsidies. Source: own calculations based on augmented LIAB data, 2001–2013.

Remaining endogeneity biases only require attention for variables whose coefficients are of interest with regards to the research question. This applies to an establishment's use of hiring subsidies. In principle, it could be argued that an employer's use of them is endogenous in the sense that exogenous wage increases may prompt them to seek subsidization in order to be able to pay those higher wages. The coefficient of hiring subsidy usage would be biased upwards, as higher wages would then cause more programmes to be used. In practice, this is less of an issue as establishments' programme applications are granted or denied by a case worker in the local employment agency or job centre, who is typically unaware of an establishment's overall performance. She may be aware of a large number of layoffs or hires recently, or that this employer has suddenly greatly increased its number of subsidy applications. She however does not have access to the business' true operational indicators. Therefore, exogeneity of hiring subsidy programme use is achieved by controlling for the establishment and local labour market characteristics that past studies have identified to be relevant selection factors (Moczall 2014, 2013; Bellmann/Stephan 2014, 2012; Hohendanner 2011; Hartmann 2004; Hujer/Caliendo/Radic 2001). I include them to the extent that they are not already subtracted out by the spell fixed effects model. Among these are in particular the use of other ALMP programmes as well as growth and churning rates. Because hiring subsidies are only paid for new hires, they are mainly used by growing establishments (see Table 1 and Bellmann/Stephan 2014, 2012).

All regressions are performed without sampling weights, as all variables used to construct them are either included in the model specification (Winship/Radbill 1994) or the spell-level fixed effect. The substitution of regular workers through subsidization is implicitly controlled for by simultaneously including the growth and churning

rates in addition to the subsidization information. Standard errors are clustered by establishment identificator. Because I use linked employer-employee data, I am able to include both individual and employer characteristics to explain wages. Due to the spell fixed effects model, covariates that do not change over time (except due to measurement error) or just increase by one from year to year are not included. This affects age, labour market experience, sex, nationality, and years of schooling on the individual level. It also affects sector, legal structure, bargaining regime, region and establishment age on the establishment level. I include tenure as it pertains to my research questions in the form of categorical dummies. I also include one dummy for each year of observation (minus one for the starting year 2001) as a form of time fixed effects to control for changes in the business cycle. See Appendix Table 19 and Table 20 for a complete list of covariates.

3.3 Descriptives

Table 2 shows how the initial full LIAB sample is reduced by one third for various reasons, the overwhelming majority related to the substance of the research question: that only full-time employed workers are analysed and therefore, persons working in part-time, in apprenticeships and minor employment are removed.[3] To make sure that I only look at regular workers, I remove all person-year observations from worker-firm combinations in which a person was subsidized at least once. This means that a person will not be considered when hired with a subsidy even after the subsidy has expired. However, as the subsidization information only goes back to the year 2000, some regular workers that were initially hired as subsidized workers will remain in the sample, as their initial subsidization status cannot be observed.

[3] A worker is a part-time worker if his/her "occupational position" ("Stellung im Beruf") contains the values 8 ("part-time employment without unemployment insurance") or 9 ("part-time employment with unemployment insurance").

Table 2: Sample exclusions

	# person-year observations
initial full LIAB 2001-2010	22,741,637
excluded because…	
employed with a subsidy anytime within spell	290,581
inconsistent stock and flow indicators	15,189
churning rate above 10	4,011
employed as apprentice	1,070,470
employed in part-time	5,210,346
minor employment (in full-time)	115,819
wage missing or zero	105,594
missing district labour market data	18,759
information on bargaining regime missing	250,681
no contributory employment in establishment	465
welfare administered locally	1,137,082
subsidized only between measurement points	981,790
net sample	**13,540,850**

Source: own calculations based on augmented LIAB data, 2001–2013.

Few establishments have to be removed for data quality reasons. The most important of these is starting in 2005, establishments have to be removed from the sample because they reside in districts in which welfare recipients are administered solely by the local communities instead of jointly by the Federal Employment Agency and the local community. In these districts, subsidization information is missing, so establishments in them have to be removed to prevent misidentifying subsidized as regular employment. These exclusions reduce the number of employer observations per year from about 16,000 to about 13,000 before 2005 and about 10,000 in 2005 and later.

Table 3: Number of changers in subsidization status within worker-firm combination

	establishments		person-year observations	
# of changers	#	%	#	%
0	28,684	84.5	8,586,841	63.4
1	2,654	7.8	1,603,454	11.8
2	1,876	5.5	2,407,102	17.8
3	419	1.2	616,294	4.6
4	252	0.7	258,310	1.9
5	53	0.2	49,141	0.4
6	17	0.1	11,845	0.1
7	5	0.0	7,863	0.1
total	33,960	100.0	13,540,850	100.0
≥ 1	5,276	15.5	4,954,009	36.6

Numbers show non-weighted frequencies of establishments and person-year observations. Source: own calculations based on augmented LIAB data, 2001–2013.

In a spell fixed effects model, a person moving from one establishment to another begins a new worker-firm combination and therefore a new fixed effect that is differenced out from all variables within such a model. Identifying the effect of changes in establishment characteristics therefore does not rely on the number of individuals who move between establishments (movers). Instead, it relies on the number of establishments with changes in that particular characteristic (changers) during the spell. In this case, this is the status of receiving a hiring subsidy. Table 3 shows the number of changers in terms of the number of establishments and the number of person-year observations. There are about 5,300 (16 % of all) establishments in the sample which between 2001 and 2010 change subsidization status, about half of which change more than once. Because subsidized establishments tend to be larger, about 37 % of all person-year observations are from spells in which subsidization status changes at least once. Unlike research designs that rely on a very small numbers of movers between establishments to identify particular effects, this approach relies upon a quite substantial part of the sample to identify the effect of interest.

Table 4: Wages of regular workers by sector and establishment subsidization status

sector	avg. monthly wage		% of regular workers in HSE	# of person-year obs.
	non-HSE	HSE		
agriculture/forestry/mining	2,985	2,326	8.9	174,383
manufacturing	3,708	3,247	11.1	6,766,433
energy/utilities/waste management	3,614	3,238	14.3	318,793
construction	2,938	2,784	18.2	324,074
retail/hospitality	3,049	2,919	17.0	1,341,614
IT/communication	4,182	3,354	8.3	132,206
financial services/insurance/real estate	3,959	3,770	4.6	846,576
professionals/scientists/technicians	3,919	3,402	10.7	406,113
temporary employment agencies	1,706	1,980	64.9	134,665
other services	2,958	2,683	26.6	977,826
administration/education/training	3,099	3,000	17.2	1,136,723
hospitals/medical practices	3,228	3,215	32.1	618,633
other medical services	2,643	2,577	34.4	165,277
arts/entertainment/sport	3,511	2,889	6.6	80,715
churches/associations/unions	3,235	2,735	15.4	100,037
miscellaneous	2,866	2,417	10.6	16,782
all sectors	3,506	3,024	14.9	13,540,850

Wages are monthly wages in EUR, deflated by the consumer price index, in prices of the year 2010 averaged over non-weighted person-year observations. HSE: Establishment receiving hiring subsidies. Source: own calculations based on augmented LIAB data, 2001–2013.

Table 4 columns 1 and 2 show, divided by sector, the monthly real wages of regular workers in establishments that receive no hiring subsidies compared with establishments that receive hiring subsidies for at least one (therefore non-regular) worker. Column 3 shows what percentage of the regular workers in that sector are in establishments that receive hiring subsidies for at least one subsidized worker. Column 4 shows how many cases of person-year combinations are in each sector in the sample.

Working as a regular worker in an establishment that receives hiring subsidies is negatively associated with wages in most sectors, most notably in agriculture/forestry/mining and IT/communication. One notable exception is the sector temporary employment agencies, where wages of regular workers are higher in subsidized than in non-subsidized establishments. This is also the sector in which almost two thirds of workers are in establishments (referring to the temporary employment agency itself, not the establishment into which they are sent) for which some other worker receives a hiring subsidy.

Appendix Table 18 shows descriptive statistics for the covariates used in the spell fixed effects models.

4 Results

Table 5 and Table 6 show the estimation results for the regressors of interest; Appendix Table 19 and Table 20 show estimates for the entirety of the models' covariates.

Table 5: Spell fixed-effects regression results of main sample, models 1–3

	(1)		(2)		(3)	
	b	SE	b	SE	b	SE
uses EGZ in current year	0.0035*	(0.0020)			0.0036*	(0.0021)
used EGZ in previous year			0.0007	(0.0015)	0.0011	(0.0016)
# observations	13,540,849		13,540,849		13,540,849	
# groups (spells)	4,463,891		4,463,891		4,463,891	
# clusters (establishments)	33,960		33,960		33,960	
# obs. per group (min)	1		1		1	
# obs. per group (avg)	3.033		3.033		3.033	
# obs. per group (max)	10		10		10	
R^2 within	0.045		0.045		0.045	
R^2 between	0.255		0.256		0.254	
R^2 overall	0.282		0.283		0.281	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

Table 6: Spell fixed-effects regression results of main sample, models 4–6

	(4)		(5)		(6)	
	b	SE	b	SE	b	SE
tenure: 0-6 months	-0.0762***	(0.0019)	-0.0753***	(0.0020)	-0.0753***	(0.0028)
tenure: 7-12 months	-0.0632***	(0.0018)	-0.0637***	(0.0019)	-0.0636***	(0.0024)
tenure: 12-36 months	-0.0264***	(0.0009)	-0.0265***	(0.0010)	-0.0265***	(0.0013)
tenure: more than 36 months (ref.)						
tenure: 0-6 months * EGZ			-0.0046	(0.0035)	-0.0048	(0.0040)
tenure: 7-12 months * EGZ			0.0027	(0.0030)	0.0025	(0.0031)
tenure: 12-36 months * EGZ			0.0006	(0.0020)	0.0005	(0.0021)
uses EGZ in current year	0.0039*	(0.0020)	0.0039*	(0.0022)	0.0039*	(0.0023)
EGZ intensity in establishment	-0.0004***	(0.0001)	-0.0004***	(0.0001)	-0.0004***	(0.0001)
EGZ intensity in district included?	no		no		Yes	
# observations	13,540,849		13,540,849		13,524,299	
# groups (spells)	4,463,891		4,463,891		4,463,891	
# clusters (establishments)	33,960		33,960			
# clusters (districts)					412	
# obs. per group (min)	1		1		1	
# obs. per group (avg)	3.033		3.033		3.030	
# obs. per group (max)	10		10		10	
R^2 within	0.045		0.045		0.045	
R^2 between	0.255		0.256		0.251	
R^2 overall	0.282		0.283		0.278	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

The first regressor of interest is a dummy variable indicating whether an establishment makes use of hiring subsidies in the same period when the wage is measured (labelled “uses EGZ in current year”). In the context of the fixed effects model, the effect of a change in the subsidization status on regular wages is identified. This is shown by column 1 in Table 5. The switch to receiving hiring subsidies by an establishment increases the wages of regular workers on average by 0.35 %, an effect that is statistically significant only on the 10 % level. Reporting significance levels on the 10 % level is not superfluous with over 13 million observations because these are clustered in only about 34,000 establishments.

The effect on regular wages may not occur simultaneously with the subsidization that causes it. The windfall that may be caused by a hiring subsidy may not be distributed among regular workers until the following year, for example.[4] I therefore replace the dummy indicating subsidization in the current year with a lagged dummy indicating use of hiring subsidies in the previous year (labelled “used EGZ in previous year”). This results in no effect whatsoever, as column 2 in Table 5 shows. In-

[4] German social security data internally stores wages as the amount earned within a calendar year divided by the number of days. Within the LIAB data set, a wage premium paid out to regular workers in December for a subsidy paid to an employer in June will therefore be measured at the same time because they occured within the same calendar year.

cluding both dummies in one model specification does not change these estimates much, as column 3 in Table 5 shows.

These results indicate that there is some kind of effect on regular workers' wages, but that the effect may be clouded by the model specification, or may only exist in certain subgroups of establishments or workers. Next I add the information into the model of how many subsidized workers are in the establishment as a measure of subsidization intensity. More subsidized workers may mean a higher reimbursement of the establishment's wage sum, and therefore potentially more windfall to distribute, in case the wage premium effect described in Section 2.2 applies. It could also indicate a different strategy of using subsidized workers compared to establishments with fewer of them.

The variable “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Without subsidization, both the dummy and the intensity will be zero. For an intensity of one subsidized worker, the subsidization dummy is one and the intensity variable is zero. For an intensity of more than one subsidized worker, the subsidization dummy is one while the intensity variable is above zero. This way of coding subsidization intensity means that the dummy coefficient shows the effect for a subsidization intensity of one worker, which is the median intensity in all (sub-) samples (see Table 7). The intensity coefficient then shows how the effect differs for every additional subsidized worker. This functional form implies a linear relationship between subsidization intensity and the natural logarithm of wages. Alternatively coding subsidization intensity as a natural logarithm does not improve the model fit. The intensity variable is not only highly significant in itself; because a higher subsidization intensity reduces the effect on regular wages, the coefficient of the subsidization dummy now slightly rises. Subsidization with exactly one worker raises regular wages by almost 0.4 %, while subsidization with two workers by only 0.35 %. The effect on regular wages would cross the zero line at a subsidization intensity of 0.0039/0.0004+1=10.75 workers, which affects only a negligibly small number of establishments (see Table 7). Measuring intensity alternatively as the number of subsidized workers *relative* to the total number of workers does not yield any significant effect. Since it also drastically worsens the precision of the subsidization dummy, it is not shown in the tables.

Next I separate the effects of a switch in subsidization status by the tenure of regular workers. As mentioned earlier, workers with a short tenure are easier to replace, and thus have lower negotiating power, than those with a long tenure and therefore a great amount of firm-specific human capital. I therefore expect effects to be different with different durations of belonging to the same establishment. In particular, I expect the wage premium effect among workers with a longer tenure, and a threat effect among those with a short tenure. All models already include dummies for a worker's tenure, with the longest tenure (more than 36 months) chosen as the reference category as it is the most common. Unsurprisingly, longer tenure is associated with higher wages. I interact the subsidization dummy with the tenure dummies. In

such a model specification, the previously-described coefficient of the bare subsidization dummy now represents the effect for the excluded (longest) tenure category. The interaction dummies show how the effect of a change in subsidization differs for regular workers with a shorter tenure.[5] Note that because the fixed effect is differenced out after the interaction dummies are created, the interpretation of the interaction dummies is "a change in the establishment's subsidization status given a particular tenure of the worker", and not "a change in the establishment's subsidization status given a change in tenure".[6] Table 6 column 5 shows the results for this interacted model. None of the tenure dummies are statistically significantly different from zero. Their inclusion does not change the previous base estimate, which is now the estimate for the longest tenure category, either when compared to Table 6 column 4. They do inflate the standard errors however. This indicates that at least in the main sample, there is no evidence for a threat effect of subsidization on regular wages. Expanding the model by interacting subsidization intensity with tenure produces no statistically significant interaction terms either.

In the last model (Table 6 column 6), I add the regional intensity of hiring subsidies in the establishment's NUTS 3 administrative district. Regional intensity is defined as the stock of hiring subsidy participants divided by the stock of unemployed persons within the NUTS 3 region. By including this variable, I aim to find out whether any effect I find on the establishment level really occurs at the establishment level or whether it is actually happening in the entire region. If that is the case, including the regional intensity should substantially change the establishment-level subsidization coefficient. Note that I am not aiming at identifying the causal effect of regional programme intensity changes itself[7] which is why I show its coefficient only in the full model in Table 20 in the appendix. The coefficient of the regional programme intensity is statistically significantly different (on the 10 % level) from zero and is associated with higher wages. Its inclusion however does not change the effect of the establishment subsidization dummy. This means that the establishment-level effect of subsidization on regular wages is a unique effect within the establishment and not merely the extension of a regional effect. Because I now include a regional variable of interest, the standard errors are clustered at the district level rather than the establishment level in this one model only.

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[5] See Brambor/Clark/Golder (2006); Braumoeller (2004), Bedeian/Mossholder (1994) for a discussion of the proper interpretation of interaction terms.

[6] Also note that because this is a fixed effects and not a first-differences model, it is entirely possible to speak of a "change" in subsidization status even for the first observation in each spell. By definition, all persons with a tenure of six months or less will have that tenure dummy at one in the first observation of their worker-firm spell. But not all first observations in a worker-firm spell are entrants, because most were in the establishment for a long time when the establishment was included in the IAB Establishment Panel for the first time.

[7] The identification of *causal* effects on the regional level is beyond the scope of this paper, as it is famously fraught with severe issues of reverse causality and simultaneity (Hujer/Rodrigues/Wolf 2009; Hagen 2004, 2003; Hujer et al. 2002; Calmfors/Forslund/Hemström 2001).

Table 7: Distribution of subsidization intensity in the presented subsamples

subsample	min	quantiles					max	mean	sd
		25%	50%	75%	90%	95%			
entire sample	1	1	1	2	5	7	102	2.59	5.80
only manufacturing	1	1	1	2	3	5	102	2.28	7.23
without manufacturing	1	1	1	3	6	8	62	2.78	4.73
without manufacturing or public sector	1	1	1	3	6	10	62	3.01	5.23
men in West Germany	1	1	1	2	4	7	38	2.34	3.99
women in West Germany	1	1	1	2	4	6	38	2.05	2.78
men in East Germany	1	1	1	3	6	9	102	3.59	9.75
women in East Germany	1	1	1	2	5	7	102	2.63	5.44
below/at-median district unemployment	1	1	1	2	4	7	102	2.68	6.29
above-median district unemployment	1	1	1	2	5	8	96	2.50	5.21
low-skilled workers	1	1	1	2	4	7	102	2.41	4.34
not low-skilled workers	1	1	1	2	4	7	102	2.60	6.03
small establishments (< 100 workers)	1	1	1	2	4	6	32	1.91	2.33
large establishments (≥ 100 workers)	1	1	1	2	5	8	102	2.68	6.09
with wage discretion	1	1	1	2	4	6	102	2.54	7.36
without wage discretion	1	1	1	2	5	8	62	2.81	5.12

Numbers show averages calculated over non-weighted person-year observations. Source: own calculations based on augmented LIAB data, 2001–2013.

I proceed by looking at the effects within selected subgroups of workers and establishments. Effect heterogeneity will help to explain why there is a positive effect on regular wages, in particular, whether a windfall or an increase in working hours is the more likely explanation. It will also help to determine under which conditions a threat effect or a wage premium effect occurs, allowing to formulate policy advice on how to prevent them from occuring, if so desired. For these subgroups, I only present the results for a model similar to Table 6 column 4 or 5, which includes the establishment-level subsidization intensity but omits the regional treatment intensity because it has shown itself to be irrelevant to my research question. Separate effects by tenure similar to Table 6 column 5 are shown when those exhibit robust significant effects for particular tenure groups. Otherwise I do not use the interacted model because the interaction terms increase the standard errors of all other variables, as we have seen in Table 6.

Table 8: Spell fixed-effects regression results in West Germany

	men		women	
	b	SE	b	SE
tenure: 0-6 months	-0.0742***	(0.0027)	-0.0945***	(0.0025)
tenure: 7-12 months	-0.0635***	(0.0028)	-0.0774***	(0.0023)
tenure: 12-36 months	-0.0262***	(0.0013)	-0.0343***	(0.0013)
tenure: more than 36 months (ref.)				
tenure: 0-6 months * EGZ	-0.0153***	(0.0050)	0.0031	(0.0048)
tenure: 7-12 months * EGZ	-0.0045	(0.0042)	0.0069	(0.0044)
tenure: 12-36 months * EGZ	-0.0014	(0.0028)	0.0027	(0.0020)
uses EGZ in current year	0.0067**	(0.0033)	0.0013	(0.0019)
EGZ intensity in establishment	-0.0008***	(0.0002)	-0.0006**	(0.0003)
# observations	7,906,738		2,668,064	
# groups (spells)	2,459,931		1,001,014	
# clusters (establishments)	19,811		19,534	
# obs. per group (min)	1		1	
# obs. per group (avg)	3.214		2.665	
# obs. per group (max)	10		10	
R^2 within	0.054		0.036	
R^2 between	0.257		0.225	
R^2 overall	0.279		0.240	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

Table 9: Spell fixed-effects regression results in East Germany

	men		women	
	b	SE	b	SE
uses EGZ in current year	0.0016	(0.0022)	0.0018	(0.0019)
EGZ intensity in establishment	-0.0004**	(0.0002)	0.0000	(0.0002)
# observations	1,712,515		1,253,532	
# groups (spells)	569,567		433,467	
# clusters (establishments)	9,913		9,721	
# obs. per group (min)	1		1	
# obs. per group (avg)	3.007		2.892	
# obs. per group (max)	10		10	
R^2 within	0.057		0.044	
R^2 between	0.248		0.132	
R^2 overall	0.287		0.127	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

First, I separate the sample both by region (West versus East Germany) and gender, for the reasons explained in Section 2.2. Table 8 shows results separately for men and women in West Germany, Table 9 the same in East Germany. Because I

only consider full-time workers even as women are far more likely to work part-time, the male sample is by far larger than the female sample in West Germany. This is not the case in East Germany, as East German women are much more likely to work full-time due to greater labour market attachment and greater availability of child care. I only find statistically significant effects of hiring subsidy use on regular workers among West German men. For them, the effect size is almost 0.7 %, twice as large as in the main sample. The coefficient of subsidization intensity is twice as large as well, so as in the main sample, the point at which the positive effect of subsidization becomes zero because of the effect of intensity is again at 0.0067/0.0008+1=9.375 workers, which affects only a very small part of the sample. More interesting is the fact that entrants with a tenure of up to six months find their wages depressed because of subsidization by an amount 1.5 % compared to workers with a tenure longer than 36 months, or 0.9 % for one subsidized worker (0.0067-0.0153=-0.0086) compared to non-subsidization of the establishment. This constitutes evidence for the first scenario of a threat effect described in Section 2.2. I find no effect on West German women or anyone in East Germany. While the effect of subsidization intensity is statistically different from zero on the 5 % level for West German women and East German men, it would take at least three (West German women) and five (East German men) subsidized workers to even neutralize the non-significant positive effects of the subsidization dummy. This is only achieved by less than a quarter in these subsamples, as Table 7 shows. The lack of significant effects is not merely the result of a large standard error due to the smaller sample size there, but instead, the effect coefficient is much smaller as well.

Table 10: Spell fixed-effects regression results by bargaining regime

	with wage discretion		without wage discretion	
	b	SE	b	SE
uses EGZ in current year	0.0060	(0.0040)	0.0020	(0.0015)
EGZ intensity in establishment	-0.0006***	(0.0001)	0.0000	(0.0002)
# observations	5,483,454		4,812,590	
# groups (spells)	2,088,437		1,723,055	
# clusters (establishments)	24,450		13,197	
# obs. per group (min)	1		1	
# obs. per group (avg)	2.626		2.793	
# obs. per group (max)	10		10	
R^2 within	0.043		0.056	
R^2 between	0.270		0.181	
R^2 overall	0.304		0.210	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

Another characteristic of establishments that demands attention is the bargaining regime. It can be argued that if subsidization has an effect on regular workers' wages, then it should be more pronounced (or even occur solely) when there is “wage

discretion” in the establishment. By this I mean that wages are not completely fixed by a sector-wide or establishment-level wage agreement but that there either is no wage agreement at all or the employer voluntarily pays wages above the level set in the sector-wide wage agreement. Table 10 shows results for these two types of establishments. Although the positive effect sizes are greater in establishments with wage discretion, the smaller sample sizes inflate standard errors to an extent that precludes substantive interpretation. What is significant is the negative effect of subsidization intensity, meaning that as before, more subsidized workers mean less of a wage premium. Eleven subsidized workers are necessary to cancel out the (non-significant) positive effect of one subsidized worker. To avoid distortions from establishments that change their bargaining regime, I only include observations from spells in which the bargaining regime remains constant throughout the worker-firm combination.

Table 11: Spell fixed-effects regression results in sectors other than manufacturing

	with public sector		without public sector	
	b	SE	b	SE
uses EGZ in current year	-0.0004	(0.0011)	-0.0012	(0.0014)
EGZ intensity in establishment	-0.0001	(0.0002)	-0.0001	(0.0002)
# observations	6,774,416		5,372,379	
# groups (spells)	2,505,927		2,169,866	
# clusters (establishments)	26,271		24,055	
# obs. per group (min)	1		1	
# obs. per group (avg)	2.703		2.476	
# obs. per group (max)	10		10	
R^2 within	0.040		0.034	
R^2 between	0.211		0.250	
R^2 overall	0.211		0.253	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

Table 12: Spell fixed-effects regression results in manufacturing, three different model specifications

	(1)		(2)		(3)	
	b	SE	b	SE	b	SE
tenure: 0-6 months	-0.0823***	(0.0025)	-0.0823***	(0.0024)	-0.0805***	(0.0026)
tenure: 7-12 months	-0.0658***	(0.0027)	-0.0659***	(0.0028)	-0.0652***	(0.0029)
tenure: 12-36 months	-0.0272***	(0.0012)	-0.0273***	(0.0012)	-0.0265***	(0.0014)
tenure: more than 36 months (ref.)						
tenure: 0-6 months * EGZ					-0.0113**	(0.0051)
tenure: 7-12 months * EGZ					-0.0040	(0.0046)
tenure: 12-36 months * EGZ					-0.0058**	(0.0026)
uses EGZ in current year	0.0084***	(0.0031)			0.0095***	(0.0034)
EGZ intensity in establishment	-0.0004***	(0.0001)	-0.0003**	(0.0001)	-0.0003***	(0.0001)
EGZ use begins in current year			0.0098**	(0.0038)		
EGZ use ends in current year			0.0046	(0.0034)		
# observations	6,766,433		6,766,433		6,766,433	
# groups (spells)	1,979,682		1,979,682		1,979,682	
# clusters (establishments)	7,991		7,991		7,991	
# obs. per group (min)	1		1		1	
# obs. per group (avg)	3.418		3.418		3.418	
# obs. per group (max)	10		10		10	
R^2 within	0.056		0.056		0.056	
R^2 between	0.359		0.360		0.360	
R^2 overall	0.368		0.370		0.369	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

Next, I separate the sample by industry. This is done for two reasons. First, the various sectors of economic activity differ by how labour intensive they are and the

speed of their labour adjustment. In labour-intensive sectors, labour costs make up a large share of production cost. A temporary drop in labour costs will therefore lower the cost of production substantially, lowering output price thus increasing output demand and thus hire more workers to satisfy that increased output demand, resulting in a scale effect. Capital-intensive sectors, such as manufacturing, cannot, as a small temporary drop in the price of labour does little to lower production costs. Instead, the windfall might get distributed among the existing workers. Second, the differences found between men and women in West and East Germany may merely be the result of them sorting themselves into different sectors. The first column of Table 12 shows the effects separately for the manufacturing sector, Table 11 for all other sectors. A highly significant positive effect of about 0.84 % is found in manufacturing (Table 12 column 1), whereas no effect at all exists in all other sectors combined, as the first column of Table 11 shows. This is true even when removing the public sector, where wages are typically more fixed than in the private sector, as the second column of Table 11 demonstrates.

All previous models implicitly assumed the effect of a change in subsidization status to be symmetrical. In other words, they included both the effect of an establishment starting to receive hiring subsidies as well as the opposite effect from stopping to receive them. Column 2 in Table 12 shows that the effect is only driven from starting to receive the subsidy.

Separating these effects in the manufacturing sector by tenure in Table 12 column 3 indicates that a tenure of six months or less results in lower regular wages from subsidization compared to a tenure of more than 36 months, which is the reference category. Given the standard errors, this negative effect is just large enough to cancel out the positive effect of the reference category and thus does not constitute evidence for a threat effect. Instead it restricts the wage premium effect in the manufacturing sector to only workers with a tenure of more than six months. Column 3 in Table 12 also shows a smaller negative effect in the tenure category of 12-36 months, which serves to reduce the positive effect in the reference tenure category. The effect of a subsidization intensity above one subsidized worker is highly significant and negative as well, but too small to lower the positive effect on regular wages of the subsidization dummy at almost all intensities that occur in the sample (Table 7).

Table 13: Spell fixed-effects regression results by skill level

	low-skilled		not low-skilled	
	b	SE	b	SE
tenure: 0-6 months	-0.0680***	(0.0040)	-0.0743***	(0.0018)
tenure: 7-12 months	-0.0576***	(0.0046)	-0.0633***	(0.0016)
tenure: 12-36 months	-0.0228***	(0.0021)	-0.0273***	(0.0010)
tenure: more than 36 months (ref.)				
tenure: 0-6 months * EGZ	-0.0173***	(0.0055)	-0.0054	(0.0034)
tenure: 7-12 months * EGZ	0.0043	(0.0061)	0.0007	(0.0028)
tenure: 12-36 months * EGZ	0.0046	(0.0039)	0.0006	(0.0021)
uses EGZ in current year	0.0046*	(0.0028)	0.0040*	(0.0023)
EGZ intensity in establishment	-0.0002	(0.0003)	-0.0004***	(0.0001)
# observations	1,620,934		11,255,304	
# groups (spells)	599,725		3,633,467	
# clusters (establishments)	17,267		30,773	
# obs. per group (min)	1		1	
# obs. per group (avg)	2.703		3.098	
# obs. per group (max)	10		10	
R^2 within	0.022		0.047	
R^2 between	0.207		0.249	
R^2 overall	0.221		0.269	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

I next divide the sample by the skill level of the regular workers. As mentioned in Section 2.2, I expect that a negative threat effect on regular wages will mostly be seen among low-skilled workers, possibly with a short tenure, because they can be easily replaced. “Low-skilled” in this context means having no vocational or university degree. For both subsamples, switching to hiring subsidies increases wages of regular wages with a tenure of more than 36 months by about 0.4–0.5 % (Table 13), as seen previously. The size of this effect is not statistically different between the two subsamples. There is a difference among workers who are at most six months in the establishment, however. Among them, low-skilled workers suffer a loss of 1.7 % in their wages compared to low-skilled workers in the longest tenure category, or 1.3 % in general. This is not seen among better-skilled workers. Therefore, I find that an establishment beginning to use hiring subsidies depresses the wages of regular low-skilled entrants but not of better-skilled ones. This is evidence for the second scenario of threat effect described in Section 2.2. Apart from low-skilled entrants, the wage premium effect applies. The negative effect of an increasing number of subsidized workers is only seen among better-skilled workers.

Table 14: Spell fixed-effects regression results by district unemployment rate

	below/at year's median		above year's median	
	b	SE	b	SE
tenure: 0-6 months	-0.0746***	(0.0024)	-0.0761***	(0.0032)
tenure: 7-12 months	-0.0629***	(0.0028)	-0.0643***	(0.0025)
tenure: 12-36 months	-0.0263***	(0.0012)	-0.0268***	(0.0017)
tenure: more than 36 months (ref.)				
tenure: 0-6 months * EGZ	-0.0078*	(0.0041)	0.0002	(0.0053)
tenure: 7-12 months * EGZ	0.0010	(0.0039)	0.0061	(0.0047)
tenure: 12-36 months * EGZ	0.0013	(0.0023)	0.0011	(0.0032)
uses EGZ in current year	0.0004	(0.0012)	0.0071*	(0.0042)
EGZ intensity in establishment	-0.0002*	(0.0001)	-0.0006***	(0.0002)
# observations	7,115,367		6,425,482	
# groups (spells)	2,612,407		2,373,227	
# clusters (establishments)	20,330		18,096	
# obs. per group (min)	1		1	
# obs. per group (avg)	2.724		2.707	
# obs. per group (max)	10		10	
R^2 within	0.045		0.044	
R^2 between	0.253		0.261	
R^2 overall	0.278		0.289	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

In Table 14, I separate the sample by the local labour market situation. As described in Section 2.2, I expect subsidies to be granted more generously in districts with a bad labour market. I therefore expect more potential for windfall that can be distributed among regular workers in these districts. A labour market is "bad” if the unemployment rate is above the median district unemployment rate in that particular year and region (West/East). As expected, I only find a wage premium effect of hiring subsidy use on regular wages in districts with above-median unemployment rates. While the effect is not measured very precisely (only significant on the 10 % level), it is positive and at 0.8 % larger than in the main sample. Conversely, I find a threat effect of subsidization against regular entrants only in districts with below-median unemployment rates, depressing their wages by 0.8 % in the case of one subsidized worker. The threat effect is strengthened while the wage premium effect is moderated by an increasing number of subsidized workers.

Table 15: Spell fixed-effects regression results by establishment size

	< 100 workers		≥ 100 workers	
	b	SE	b	SE
uses EGZ in current year	-0.0002	(0.0017)	0.0050**	(0.0024)
EGZ intensity in establishment	-0.0001	(0.0007)	-0.0005***	(0.0001)
# observations	1,521,898		10,616,914	
# groups (spells)	609,804		3,518,220	
# clusters (establishments)	24,755		6,992	
# obs. per group (min)	1		1	
# obs. per group (avg)	2.496		3.018	
# obs. per group (max)	10		10	
R^2 within	0.017		0.049	
R^2 between	0.186		0.243	
R^2 overall	0.194		0.266	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

Next, I separate effects by the size of the establishment. Because subsidized workers on average will make up a larger part of the entire wage sum in smaller establishments, one could expect larger effects among them. On the other hand, smaller establishments are less likely to use hiring subsidies (Bellmann/Stephan 2014), and the literature shows larger establishments to cause more deadweight loss (Welters/Muysken 2006) and thus higher windfalls. I therefore expect larger establishments to show more positive effects of subsidization on regular wages. This is borne out by Table 15, wherein I find no effect on regular wages in establishments with less than 100 workers (of any type), whereas larger establishments show a slightly larger effect size as the main sample in Table 6. The number of subsidized workers at which the positive effect of subsidization becomes zero because of the intensity variable is 0.0050/0.0005+1=11 workers, a rare occurence, as Table 7 shows.

Table 16: Spell fixed-effects regression results in manufacturing with imputed wages

	non-imputed wages		imputed wages	
	b	SE	b	SE
tenure: 0-6 months	-0.0799***	(0.0025)	-0.0799***	(0.0025)
tenure: 7-12 months	-0.0651***	(0.0029)	-0.0654***	(0.0029)
tenure: 12-36 months	-0.0265***	(0.0014)	-0.0264***	(0.0014)
tenure: more than 36 months (ref.)				
tenure: 0-6 months * EGZ	-0.0087*	(0.0051)	-0.0098*	(0.0054)
tenure: 7-12 months * EGZ	-0.0047	(0.0046)	-0.0056	(0.0048)
tenure: 12-36 months * EGZ	-0.0072***	(0.0028)	-0.0082***	(0.0031)
uses EGZ in current year	0.0099***	(0.0038)	0.0104**	(0.0043)
EGZ intensity in establishment	-0.0003***	(0.0001)	-0.0003***	(0.0001)
# observations	5,792,918		5,792,918	
# groups (spells)	1,729,144		1,729,144	
# clusters (establishments)	7,023		7,023	
# obs. per group (min)	1		1	
# obs. per group (avg)	3.350		3.350	
# obs. per group (max)	9		9	
R^2 within	0.049		0.048	
R^2 between	0.356		0.355	
R^2 overall	0.359		0.356	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

Finally, I compare whether effects change significantly if I replace the original wages, which are right-censored at the social security contribution limit, with values imputed above that limit using the method in Gartner (2005). Because the wage premium effect is the greatest in the manufacturing sector and thus would be most affected there, I choose this subsample for comparing the imputed and non-imputed samples. Because the imputation procedure requires the individual education information, which is missing in many cases, the number of observations in Table 16 is lower than in Table 12. While the effect of subsidization is slightly larger with the imputed outcome variable, it is not statistically different when considering its standard error. Using non-imputed data therefore does not result in bias of a magnitude worthy of consideration.

5 Discussion

The descriptive statistics seemed to suggest that subsidization is associated with overall lower earnings of regular workers (similar to the descriptive statistics in Lechner/Wunsch/Scioch 2013). Once I controlled for selectivity and unobserved heterogeneity on both the individual and establishment levels using a spell fixed effects model, the opposite effect emerged. Subsidizing an establishment with hiring subsidies slightly increased the wages of regular workers on average by 0.4 % in the case of one subsidized worker. More subsidized workers per establishment reduce this positive effect by about 0.04 % per subsidized worker. But very few estab-

lishments employ so many subsidized workers (more than ten) that the wage premium effect becomes zero or turns negative.

To get a sense of whether these effect sizes should be considered high or low, one needs to look at the share of the subsidized wage sum. This information is not available directly, but one may produce a rule-of-thumb estimate by looking at the average share of subsidized among all workers in contributory employment and taking into account that at most 50 % of wage costs may be reimbursed. Table 17 shows both the average number and the average share of subsidized workers in their respective establishments. Although the number in most sectors varies between 1 and 2 workers, sector-specific differences in establishment size make for varying relative shares. In the manufacturing sector subsidized workers make up on average 4.6 % of the total workforce in contributory employment. Roughly speaking, this means that at most 2.3 % of the wage sum can be reimbursed. The actual share will be lower, as subsidized workers should be expected to be on the lower end of the productivity scale and therefore the establishment's wage distribution. With this in mind, an average wage increase of over 0.8 % in manufacturing (Table 12) can be considered quite a substantial effect, indicating that at least in some establishments, any windfall enjoyed by the employer through the subsidy is to a large part absorbed by insiders.

Table 17: Intensity of subsidization with hiring subsidies by sector

	subsidized workers	
sector	#	%
agriculture/forestry/mining	1.87	8.0
manufacturing	1.83	4.6
energy/utilities/waste management	1.88	3.7
construction	1.45	9.5
retail/hospitality	1.53	8.6
IT/communication	1.45	8.3
financial services/insurance/real estate	1.49	8.3
professionals/scientists/technicians	1.78	10.6
temporary employment agencies	3.84	2.8
other services	2.23	3.3
administration/education/training	1.60	1.6
hospitals/medical practices	1.67	2.2
other medical services	1.92	2.2
arts/entertainment/sport	1.85	7.9
churches/associations/unions	1.71	4.4
miscellaneous	2.54	4.3
all sectors	1.87	5.2

Numbers show non-weighted averages calculated over subsidized establishments. Source: own calculations based on augmented LIAB data, 2001–2013.

Starting to receive hiring subsidies therefore increases the *daily* wages of regular workers in the same establishment. The interesting question is whether this is due to

a change in (average) *hourly* wages, or whether regular workers simply work more hours (within the full-time category). One limitation that is inherent in almost all German data is the fact that only daily wages are observed on an individual level, not hourly wages. The data set does include variables (which are included as controls) indicating both the weekly number of hours of full-time employees as well as whether overtime hours occurred in the past year. The number of overtime hours however is not observed.

An increase in the average hourly wage would indicate that the wage premium effect primarily comes from a windfall that is distributed among regular workers, for example through bonuses. This would indicate that the subsidized workers are productive enough to actually produce a windfall for their employer. An increase in the number of hours worked on the part of regular workers on the other hand would indicate that subsidized workers are not very productive, which is why regular workers need additional hours to train and supervise them. While I can neither observe the actual number of hours worked nor actual productivities of individual workers, this question can be answered on plausibility grounds by taking effect heterogeneity into account.

The fact that positive effects are only seen in the manufacturing sector might tempt one to conclude that because of its greater and more firm-specific human capital requirements, the effect on regular wages must primarily come from additional hours worked to train subsidized hires. However, there are many other sectors with high productivity requirements such as financial services which do not exhibit these positive effects at all, not even on a lower scale. What separates manufacturing instead from most of the other sectors is that manufacturing is much more capital-intensive.[8] In a capital-intensive sector, an employer will not be able to exploit a temporary drop in labour costs by expanding economic activity, as labour is not the limiting factor. The slight drop in labour costs will also not be large enough to expand the capital stock substantially enough to expand economic activity. Instead, any such windfall will either be paid out as profit dividends, or distributed among the existing workers. In a labour-intensive sector on the other hand, where the cost of labour is the limiting factor, a subsidy-induced drop in labour costs can immediately be used to (temporarily) expand economic activity and thus produce scale effects. Hence, any such windfall would not be distributed among the regular workforce. Thus, the heterogeneity in terms of sector point to positive effects on regular wages being caused by increased hourly wages rather than additional hours worked.

Another aspect to consider is that positive effects on regular wages are only seen in labour markets with above-median unemployment rates (Table 14). When unemployment is low, subsidies will only be granted to those job-seekers who absolutely need them, whose personal attributes make it extremely difficult for them to find

[8] "IT and communication" should also be considered a capital-intensive sector. However, given the low number of observations in that sector (see Table 4), its contribution to the overall effect should be expected to be tiny.

work even though the labour market is favourable. When unemployment is high and thus the labour market is slack, subsidies tend to be granted much more generously in terms of whether they are granted as well as how long and how high (Welters/Muysken 2008, 2006). They will thus be much more likely to be overpaid, that is, to reimburse more than the job-seeker's actual productivity deficit. High unemployment and slack labour markets therefore will produce more subsidy-related windfalls. If the positive effect of hiring subsidies on daily regular wages came from additional hours worked, it should occur when unemployment is low, as only unproductive job-seekers in need of training would receive subsidies. If the positive effect on daily regular wages came from a windfall, it should occur in slack labour markets, as this is where windfall occurs. Since positive effects only occur in the latter case, I conclude that the positive effect must come from windfall and therefore from an increase in the average hourly wages, not from additional hours worked.

A similar argument can be made from establishment size: as Table 15 showed, positive effects only occur in larger establishments. Welters/Muysken (2006) pointed out that it is there where windfall-inducing deadweight loss occurs. If positive effects on wages instead came from additional hours worked, they would show up in smaller establishments, for it is there that additional hours of the few regular workers tasked with training and supervising the subsidized hires would have a noticeable effect on the wage sum. In larger establishments with a far greater workforce, such an effect would hardly be perceptible. This provides further evidence that the positive effects come from windfall and thus increases of the average hourly wages, not working hours.

Separating the effect of subsidization on regular wages by tenure showed a threat effect in the sub-group of low-skilled regular workers and in labour markets with a below-median unemployment rate. Low-skilled entrants' wages are depressed by the presence of regular workers quite substantially, substantial in the sense that their negative effect is over twice as large as the positive effect in the entire sample. This can be seen as evidence for the second scenario of threat effect described in Section 2.2: low-skilled workers hired without a subsidy have to accept lower wage offers because otherwise they would not have been hired at all in favour of other workers for whom a subsidy was available. The presence of other workers for whom hiring subsidies are paid in this scenario indicates (to the researcher) that subsidies are indeed part of the employer's recruiting strategy; it is not necessary in this scenario that the low-skilled entrants are conscious of this process.

It is interesting to note that this threat effect is only visible in low-unemployment labour markets, while the wage premium effect only occurs when unemployment is high. If an employer cannot profit from the subsidized workers themselves as he can when unemployment is high and subsidies are more likely to be overpaid, he can at least try to use the subsidy to exert wage pressure on easily replaceable workers, in particular entrants. As the potential for substitution of this kind is rather low (hence the rather imprecise measurement), employers do not do this when they can far

more easily profit from overpayment of a subsidy in high-unemployment labour markets.

The bargaining regime was not of particular relevance when explaining regular wage changes from hiring subsidy use. It was not included in the spell fixed effects models because it does not change much in time, thus being included in the fixed effect. Separating the main sample by bargaining regime shows that as expected, effect sizes are higher, although measured less precisely, when there is wage discretion.

It is important to stress that the effect of subsidization on regular wages occurs while the subsidy is being paid, not one year later. Furthermore, it seems to be a unique effect on the establishment level that does not change much even when including the intensity of hiring subsidies in the establishments' NUTS 3 region (district level). This finding highlights the value of doing establishment-level, rather than just individual-level and macro-level, studies of labour market policy effects.

6 Conclusion

Using linked employer-employee data for Germany from the years 2001 to 2010, I estimated the effect of subsidizing an establishment with hiring subsidies on the wages of regular workers in the subsidized establishments. The descriptive statistics seemed to suggest that subsidization is associated with overall lower earnings of regular workers (similar to the descriptive statistics in Lechner/Wunsch/Scioch 2013). Once I controlled for selectivity and unobserved heterogeneity on both the individual and establishment levels using a spell fixed effects model, the opposite effect emerged: subsidizing an establishment with hiring subsidies slightly increased the wages of regular workers on average by 0.4 %. This was mostly driven by the manufacturing sector, where the wage increase amounts to almost 1 %. Although I could only measure daily rather than hourly wages, the heterogeneity of the effects among different types of establishments and labour markets made it much more plausible that the rise in daily wages comes from a rise on average hourly wages, not an increase in working hours. I also found some evidence for a threat effect in the form of lower wages for low-skilled regular entrants in low-unemployment districts.

In a way, these results can be seen as replicating those of Welters/Muysken (2006) from the Netherlands. Windfalls in the form of a positive effect on regular workers' wages caused by subsidization occur in slack labour markets and larger establishments. The first conclusion is thus similar: case workers should be particularly wary about granting hiring subsidies in these conditions. However, an additional insight from these results is that wage premiums only seem to occur in the manufacturing sector. Welters/Muysken (2006) only distinguished the for-profit from the non-profit sector, and as Table 11 showed, this is not the decisive factor. Rather, it appeared that only in capital-intensive sectors such as manufacturing did subsidization increase the wages of regular workers. This does not necessarily mean that subsidization produces no windfalls in labour-intensive sectors, but that in those sectors, at

least some part of the windfall can indirectly create additional employment through scale effects. Depending on how competitive a particular sector and market is, this may or may not occur at the expense of non-subsidized employers in the medium-to-long run, which would then be called a displacement effect.

Further research should make use of the intensity of subsidization by establishing a functional relationship between the reimbursement amount and the size of the effect on regular wages. It should also take into account the heterogeneity in the characteristics of the subsidized individuals vis-a-vis the distribution of these characteristics in the subsidized establishment. And of course, a replication of these results with a data set that includes precisely-measured hourly wages would be highly desirable.

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Appendix

Table 18: Descriptive statistics of model covariates

	min	quantiles			max	mean	sd
		25%	50%	75%			
tenure (months)	0	38	98	178	425	120.367	99.910
occupation: managers (ref.)	0	0	0	0	1	0.039	0.193
occupation: professionals	0	0	0	0	1	0.179	0.383
occupation: technicians/associate professionals	0	0	0	0	1	0.158	0.365
occupation: clerical support workers	0	0	0	0	1	0.152	0.359
occupation: service and sales workers	0	0	0	0	1	0.045	0.207
occupation: skilled agricultural/forestry/fishery	0	0	0	0	1	0.005	0.073
occupation: craft and related trades workers	0	0	0	0	1	0.185	0.388
occupation: plant/machine operators/assemblers	0	0	0	0	1	0.151	0.358
occupation: elementary occupations	0	0	0	0	1	0.080	0.271
occupation: jobs for the disabled	0	0	0	0	1	0.000	0.003
occupation: early retirees	0	0	0	0	1	0.000	0.005
occupation: (missing or unknown)	0	0	0	0	1	0.007	0.082
share of low-skilled workers (ref.)	0	0.045	0.102	0.190	1	0.140	0.135
share of mid-skilled workers	0	0.591	0.707	0.787	1	0.671	0.182
share of high-skilled workers	0	0.039	0.098	0.185	1	0.133	0.133
share of workers with unknown skill level	0	0.000	0.005	0.031	1	0.056	0.150
share of workers with tenure 0-6 months (ref.)	0	0.010	0.026	0.054	1	0.047	0.071
share of workers with tenure 7-12 months	0	0.020	0.034	0.056	1	0.047	0.052
share of workers with tenure 12-24 months	0	0.031	0.054	0.090	1	0.079	0.104
share of workers with tenure 25-60 months	0	0.086	0.134	0.200	1	0.176	0.162
share of workers with tenure >60 months	0	0.574	0.723	0.823	1	0.651	0.245
share of female workers	0	0.130	0.250	0.536	1	0.338	0.248
share of minor-employed workers	0	0.000	0.002	0.018	1	0.025	0.065
share of part-time workers	0	0.080	0.140	0.268	1	0.197	0.160
share of foreign workers	0	0.011	0.044	0.097	1	0.069	0.083
share of workers aged 18-24 (ref.)	0	0.057	0.081	0.115	1	0.094	0.069
share of workers aged 25-49	0	0.599	0.661	0.707	1	0.650	0.099
share of workers aged 50+	0	0.197	0.247	0.307	1	0.256	0.099
establishment did not exist in previous year	0	0.000	0	0.000	1	0.000	0.010
growth rate	-1.999	-0.041	-0.006	0.027	2.000	-0.005	0.137
churning rate	0.000	0.171	0.230	0.342	10.000	0.320	0.377
total # of workers	1	280	870	3,277	53,405	5,019	10890
overtime hours in past year: (missing/unknown)	0	0	0	0	1	0.190	0.392
overtime hours in past year: yes	0	0	1	1	1	0.716	0.451
overtime hours in past year: no (ref.)	0	0	0	0	1	0.093	0.291
profits in past year: unknown	0	0	0	0	1	0.043	0.202
profits in past year: very good (ref.)	0	0	0	0	1	0.069	0.254
profits in past year: good	0	0	0	1	1	0.266	0.442
profits in past year: satisfactory	0	0	0	0	1	0.240	0.427
profits in past year: average	0	0	0	0	1	0.111	0.314
profits in past year: poor	0	0	0	0	1	0.087	0.282
profits in past year: (not applicable)	0	0	0	0	1	0.185	0.388
works council: (missing/unknown)	0	0	0	0	1	0.013	0.112
works council: yes	0	1	1	1	1	0.876	0.330
works council: no (ref.)	0	0	0	0	1	0.111	0.315
uses One-Euro-Jobs: (unknown/not possible)	0	0	0	1	1	0.482	0.500
uses One-Euro-Jobs: yes	0	0	0	0	1	0.030	0.172
uses One-Euro-Jobs: no (ref.)	0	0	0	1	1	0.488	0.500
uses Job Creation Schemes	0	0	0	0	1	0.053	0.223
used Job Creation Schemes in previous year	0	0	0	0	1	0.068	0.251

	min	quantiles			max	mean	sd
		25%	50%	75%			
(non-missing) hours per week of full-time workers	4	35	38	39	98	37.500	2.410
hours per week of full-time workers missing	0	0	0	1	1	0.304	0.460
district unemployment rate	0.014	0.069	0.095	0.132	0.297	0.102	0.045
district tightness	0.009	0.086	0.142	0.215	3.577	0.172	0.156
uses hiring subsidies in current year	0	0	0	0	1	0.149	0.356
used hiring subsidies in previous year	0	0	0	0	1	0.141	0.348
# person-year observations							13,540,849

Source: own calculations based on augmented LIAB data, 2001–2013.

Table 19: Spell fixed-effects regression results of main sample, full models 1–3

	(1)		(2)		(3)	
	b	SE	b	SE	b	SE
tenure: 0--6 months	-0.0762***	(0.0019)	-0.0763***	(0.0019)	-0.0762***	(0.0019)
tenure: 7--12 months	-0.0632***	(0.0018)	-0.0632***	(0.0018)	-0.0632***	(0.0018)
tenure: 12--36 months	-0.0265***	(0.0009)	-0.0265***	(0.0009)	-0.0265***	(0.0009)
tenure: >36 months (ref.)						
occ.: managers (ref.)						
occ.: professionals	0.0467***	(0.0048)	0.0465***	(0.0049)	0.0467***	(0.0048)
occ.: technicians/assc. profess.	0.0305***	(0.0061)	0.0304***	(0.0061)	0.0305***	(0.0061)
occ.: clerical support	0.0043	(0.0038)	0.0044	(0.0038)	0.0043	(0.0038)
occ.: service and sales	-0.0204***	(0.0064)	-0.0204***	(0.0064)	-0.0204***	(0.0064)
occ.: agriculture/forestry/fishery	-0.0373***	(0.0097)	-0.0373***	(0.0097)	-0.0373***	(0.0097)
occ.: craft etc. trades	-0.0374***	(0.0068)	-0.0374***	(0.0067)	-0.0374***	(0.0067)
occ.: plant/operators/assemblers	-0.0429***	(0.0066)	-0.0429***	(0.0066)	-0.0429***	(0.0066)
occ.: elementary occupations	-0.0556***	(0.0084)	-0.0556***	(0.0084)	-0.0556***	(0.0084)
occ.: disabled	0.0871	(0.0834)	0.0880	(0.0837)	0.0870	(0.0833)
occ.: early retirees	-0.0944	(0.0608)	-0.0945	(0.0609)	-0.0943	(0.0609)
occ.: (missing or unknown)	-0.1881***	(0.0220)	-0.1882***	(0.0220)	-0.1881***	(0.0220)
% low-skilled workers (ref.)						
% mid-skilled workers	0.0111	(0.0474)	0.0108	(0.0475)	0.0111	(0.0474)
% high-skilled workers	0.1024*	(0.0605)	0.1016*	(0.0604)	0.1020*	(0.0604)
% workers with unknown skill level	0.0164	(0.0377)	0.0157	(0.0377)	0.0162	(0.0376)
% workers with tenure 0-6 months (ref.)						
% workers with tenure 7-12 months	-0.0420***	(0.0103)	-0.0420***	(0.0104)	-0.0422***	(0.0104)
% workers with tenure 12-24 months	-0.0541***	(0.0079)	-0.0558***	(0.0079)	-0.0543***	(0.0079)
% workers with tenure 25-60 months	-0.0739***	(0.0074)	-0.0755***	(0.0074)	-0.0739***	(0.0074)
% workers with tenure >60 months	-0.0818***	(0.0083)	-0.0836***	(0.0082)	-0.0817***	(0.0083)
% female workers	0.0162	(0.0188)	0.0167	(0.0189)	0.0162	(0.0188)
% minor-employed workers	-0.0764***	(0.0156)	-0.0760***	(0.0155)	-0.0762***	(0.0155)
% part-time workers	-0.0079	(0.0163)	-0.0079	(0.0163)	-0.0080	(0.0163)
% foreign workers	0.0344	(0.0402)	0.0331	(0.0400)	0.0343	(0.0400)
% workers aged 18-24 (ref.)						
% workers aged 25-49	-0.0492**	(0.0209)	-0.0488**	(0.0210)	-0.0491**	(0.0208)
% workers aged 50+	-0.1120***	(0.0234)	-0.1111***	(0.0235)	-0.1121***	(0.0234)
new establishment	0.0162	(0.0148)	0.0161	(0.0148)	0.0162	(0.0148)
growth rate	-0.0082**	(0.0034)	-0.0078**	(0.0034)	-0.0081**	(0.0034)
churning rate	0.0046**	(0.0018)	0.0047***	(0.0018)	0.0046**	(0.0018)
total # of workers (ln)	0.0264***	(0.0045)	0.0264***	(0.0045)	0.0263***	(0.0045)
overtime hours: (missing)	0.0083	(0.0068)	0.0082	(0.0069)	0.0083	(0.0068)
overtime hours: yes	-0.0002	(0.0011)	-0.0002	(0.0011)	-0.0002	(0.0011)
overtime hours: no (ref.)						
profits: unknown	-0.0014	(0.0038)	-0.0015	(0.0038)	-0.0013	(0.0038)
profits: very good (ref.)						
profits: good	-0.0024	(0.0027)	-0.0024	(0.0027)	-0.0023	(0.0027)
profits: satisfactory	-0.0084***	(0.0025)	-0.0085***	(0.0025)	-0.0083***	(0.0026)
profits: average	-0.0138***	(0.0028)	-0.0139***	(0.0028)	-0.0138***	(0.0028)

	(1)		(2)		(3)	
	b	SE	b	SE	b	SE
profits: poor	-0.0174***	(0.0035)	-0.0175***	(0.0035)	-0.0173***	(0.0035)
profits: (not applicable)	-0.0068**	(0.0034)	-0.0070**	(0.0034)	-0.0067*	(0.0035)
works council: (missing)	0.0078***	(0.0025)	0.0076***	(0.0025)	0.0077***	(0.0025)
works council: yes	0.0051***	(0.0015)	0.0051***	(0.0015)	0.0051***	(0.0015)
works council: no (ref.)						
uses 1EJ: (impossible)	0.0040	(0.0043)	0.0037	(0.0041)	0.0041	(0.0043)
uses 1EJ: yes	-0.0147***	(0.0024)	-0.0146***	(0.0025)	-0.0146***	(0.0024)
uses 1EJ: no (ref.)						
uses JCS	-0.0039	(0.0030)	-0.0038	(0.0030)	-0.0039	(0.0030)
used JCS in previous year	0.0012	(0.0022)	0.0012	(0.0022)	0.0011	(0.0022)
ln weekly hrs.	-0.0125	(0.0303)	-0.0135	(0.0306)	-0.0128	(0.0305)
weekly hrs. missing	-0.0739	(0.1770)	-0.0792	(0.1791)	-0.0756	(0.1780)
district unemp. rate (ln)	-0.0223***	(0.0078)	-0.0220***	(0.0077)	-0.0223***	(0.0077)
district tightness	-0.0119***	(0.0042)	-0.0123***	(0.0043)	-0.0119***	(0.0042)
year = 2001 (ref.)						
year = 2002	0.0120***	(0.0016)	0.0120***	(0.0016)	0.0120***	(0.0016)
year = 2003	0.0386***	(0.0051)	0.0386***	(0.0051)	0.0385***	(0.0051)
year = 2004	0.0382***	(0.0030)	0.0380***	(0.0030)	0.0382***	(0.0030)
year = 2005	0.0380***	(0.0106)	0.0375***	(0.0105)	0.0381***	(0.0106)
year = 2006	0.0523***	(0.0062)	0.0520***	(0.0060)	0.0526***	(0.0062)
year = 2007	0.0428***	(0.0103)	0.0427***	(0.0102)	0.0429***	(0.0103)
year = 2008	0.0508***	(0.0059)	0.0506***	(0.0057)	0.0509***	(0.0060)
year = 2009	0.0584***	(0.0059)	0.0582***	(0.0057)	0.0585***	(0.0059)
year = 2010	0.0750***	(0.0063)	0.0747***	(0.0061)	0.0751***	(0.0064)
uses EGZ in current year	0.0035*	(0.0020)			0.0036*	(0.0021)
used EGZ in previous year			0.0007	(0.0015)	0.0011	(0.0016)
intercept	4.5964***	(0.1548)	4.6046***	(0.1573)	4.5984***	(0.1561)
# observations	13,540,849		13,540,849		13,540,849	
# groups (spells)	4,463,891		4,463,891		4,463,891	
# clusters (establishments)	33,960		33,960		33,960	
# obs. per group (min)	1		1		1	
# obs. per group (avg)	3.033		3.033		3.033	
# obs. per group (max)	10		10		10	
R^2 within	0.045		0.045		0.045	
R^2 between	0.255		0.256		0.254	
R^2 overall	0.282		0.283		0.281	

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.

Table 20: Spell fixed-effects regression results of main sample, full models 4–6

	(4)		(5)		(6)	
	b	SE	b	SE	b	SE
tenure: 0--6 months	-0.0762***	(0.0019)	-0.0753***	(0.0020)	-0.0753***	(0.0028)
tenure: 7--12 months	-0.0632***	(0.0018)	-0.0637***	(0.0019)	-0.0636***	(0.0024)
tenure: 12--36 months	-0.0264***	(0.0009)	-0.0265***	(0.0010)	-0.0265***	(0.0013)
tenure: >36 months (ref.)						
occ.: managers (ref.)						
occ.: professionals	0.0468***	(0.0047)	0.0468***	(0.0047)	0.0469***	(0.0049)
occ.: technicians/assc. profess.	0.0305***	(0.0061)	0.0305***	(0.0061)	0.0305***	(0.0065)
occ.: clerical support	0.0044	(0.0038)	0.0044	(0.0038)	0.0044	(0.0037)
occ.: service and sales	-0.0204***	(0.0064)	-0.0204***	(0.0064)	-0.0205***	(0.0069)
occ.: agriculture/forestry/fishery	-0.0373***	(0.0097)	-0.0372***	(0.0097)	-0.0373***	(0.0091)
occ.: craft etc. trades	-0.0373***	(0.0067)	-0.0373***	(0.0067)	-0.0373***	(0.0071)
occ.: plant/operators/assemblers	-0.0428***	(0.0066)	-0.0428***	(0.0066)	-0.0427***	(0.0068)
occ.: elementary occupations	-0.0556***	(0.0084)	-0.0556***	(0.0084)	-0.0557***	(0.0086)
occ.: disabled	0.0870	(0.0834)	0.0873	(0.0834)	0.0874	(0.0838)
occ.: early retirees	-0.0944	(0.0608)	-0.0945	(0.0608)	-0.0942	(0.0621)

occ.: (missing or unknown)	-0.1881***	(0.0220)	-0.1881***	(0.0220)	-0.1876***	(0.0220)
% low-skilled workers (ref.)						
% mid-skilled workers	0.0112	(0.0475)	0.0112	(0.0475)	0.0134	(0.0495)
% high-skilled workers	0.1041*	(0.0606)	0.1040*	(0.0606)	0.1080*	(0.0624)
% workers with unknown skill level	0.0165	(0.0376)	0.0165	(0.0377)	0.0188	(0.0397)
% workers with tenure 0-6 months (ref.)						
% workers with tenure 7-12 months	-0.0420***	(0.0104)	-0.0421***	(0.0104)	-0.0411***	(0.0100)
% workers with tenure 12-24 months	-0.0554***	(0.0078)	-0.0554***	(0.0078)	-0.0556***	(0.0098)
% workers with tenure 25-60 months	-0.0756***	(0.0072)	-0.0757***	(0.0073)	-0.0761***	(0.0077)
% workers with tenure >60 months	-0.0837***	(0.0082)	-0.0838***	(0.0082)	-0.0841***	(0.0079)
% female workers	0.0163	(0.0188)	0.0163	(0.0188)	0.0169	(0.0175)
% minor-employed workers	-0.0766***	(0.0156)	-0.0766***	(0.0156)	-0.0773***	(0.0172)
% part-time workers	-0.0080	(0.0163)	-0.0080	(0.0163)	-0.0090	(0.0158)
% foreign workers	0.0336	(0.0401)	0.0336	(0.0401)	0.0398	(0.0435)
% workers aged 18-24 (ref.)						
% workers aged 25-49	-0.0474**	(0.0208)	-0.0474**	(0.0209)	-0.0496**	(0.0218)
% workers aged 50+	-0.1103***	(0.0233)	-0.1104***	(0.0233)	-0.1120***	(0.0244)
new establishment	0.0159	(0.0148)	0.0157	(0.0148)	0.0151	(0.0151)
growth rate	-0.0080**	(0.0034)	-0.0080**	(0.0034)	-0.0080**	(0.0036)
churning rate	0.0044**	(0.0018)	0.0044**	(0.0018)	0.0048***	(0.0016)
total # of workers (ln)	0.0262***	(0.0045)	0.0262***	(0.0045)	0.0261***	(0.0042)
overtime hours: (missing)	0.0083	(0.0068)	0.0083	(0.0068)	0.0076	(0.0067)
overtime hours: yes	-0.0003	(0.0011)	-0.0003	(0.0011)	-0.0002	(0.0011)
overtime hours: no (ref.)						
profits: unknown	-0.0019	(0.0038)	-0.0019	(0.0038)	-0.0018	(0.0045)
profits: very good (ref.)						
profits: good	-0.0025	(0.0027)	-0.0025	(0.0027)	-0.0025	(0.0027)
profits: satisfactory	-0.0086***	(0.0025)	-0.0086***	(0.0025)	-0.0084***	(0.0025)
profits: average	-0.0140***	(0.0028)	-0.0140***	(0.0028)	-0.0137***	(0.0027)
profits: poor	-0.0175***	(0.0035)	-0.0175***	(0.0035)	-0.0174***	(0.0036)
profits: (not applicable)	-0.0072**	(0.0034)	-0.0072**	(0.0034)	-0.0070*	(0.0037)
works council: (missing)	0.0078***	(0.0025)	0.0078***	(0.0025)	0.0080***	(0.0027)
works council: yes	0.0051***	(0.0015)	0.0051***	(0.0015)	0.0050***	(0.0015)
works council: no (ref.)						
uses 1EJ: (impossible)	0.0040	(0.0043)	0.0040	(0.0043)	0.0041	(0.0043)
uses 1EJ: yes	-0.0147***	(0.0024)	-0.0146***	(0.0024)	-0.0147***	(0.0024)
uses 1EJ: no (ref.)						
uses JCS	-0.0037	(0.0030)	-0.0037	(0.0030)	-0.0039	(0.0031)
used JCS in previous year	0.0014	(0.0022)	0.0014	(0.0022)	0.0012	(0.0024)
ln weekly hrs.	-0.0129	(0.0302)	-0.0129	(0.0302)	-0.0144	(0.0306)
weekly hrs. missing	-0.0760	(0.1765)	-0.0762	(0.1765)	-0.0838	(0.1784)
district unemp. rate (ln)	-0.0222***	(0.0078)	-0.0222***	(0.0078)	-0.0178**	(0.0085)
district tightness	-0.0118***	(0.0042)	-0.0118***	(0.0042)	-0.0112**	(0.0045)
year = 2001 (ref.)						
year = 2002	0.0120***	(0.0016)	0.0120***	(0.0016)	0.0112***	(0.0019)
year = 2003	0.0389***	(0.0050)	0.0389***	(0.0050)	0.0360***	(0.0063)
year = 2004	0.0383***	(0.0030)	0.0383***	(0.0030)	0.0358***	(0.0038)
year = 2005	0.0384***	(0.0106)	0.0384***	(0.0106)	0.0369***	(0.0108)
year = 2006	0.0524***	(0.0062)	0.0524***	(0.0062)	0.0487***	(0.0068)
year = 2007	0.0431***	(0.0103)	0.0431***	(0.0103)	0.0407***	(0.0108)
year = 2008	0.0510***	(0.0059)	0.0510***	(0.0059)	0.0483***	(0.0065)
year = 2009	0.0585***	(0.0059)	0.0585***	(0.0059)	0.0557***	(0.0066)
year = 2010	0.0752***	(0.0063)	0.0752***	(0.0063)	0.0724***	(0.0068)
uses EGZ in current year	0.0039*	(0.0020)	0.0039*	(0.0022)	0.0039*	(0.0023)
EGZ intensity in establishment	-0.0004***	(0.0001)	-0.0004***	(0.0001)	-0.0004***	(0.0001)
tenure: 0--6 months * EGZ			-0.0046	(0.0035)	-0.0048	(0.0040)
tenure: 7-12 months * EGZ			0.0027	(0.0030)	0.0025	(0.0031)
tenure: 12-36 months * EGZ			0.0006	(0.0020)	0.0005	(0.0021)
tenure: >36 months * EGZ (ref.)						
intercept	4.5999***	(0.1542)	4.6004***	(0.1542)	4.6163***	(0.1576)

district hiring subsidy intensity			0.4729* (0.2453)
# observations	13,540,849	13,540,849	13,524,299
# groups (spells)	4,463,891	4,463,891	4,463,891
# clusters (establishments/districts)	33,960	33,960	412
# obs. per group (min)	1	1	1
# obs. per group (avg)	3.033	3.033	3.030
# obs. per group (max)	10	10	10
R^2 within	0.045	0.045	0.045
R^2 between	0.255	0.256	0.251
R^2 overall	0.282	0.283	0.278

Dependent variable is ln of real daily wage of regular workers. “EGZ”: hiring subsidy. “EGZ intensity in establishment” is coded as the number of subsidized workers minus one, set to zero for non-subsidized establishments. Significance levels: *** 1 %, ** 5 %, * 10%. Source: own calculations based on augmented LIAB data, 2001–2013.