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Training and minimum wages: first evidence from the introduction of the minimum wage in Germany

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Abstract
We analyze the short-run impact of the introduction of the new statutory minimum wage in Germany on further training at the workplace level. Applying difference-in-difference methods to data from the IAB Establishment Panel, we do not find a reduction in the training incidence but a slight reduction in the intensity of training at treated establishments. Effect heterogeneities reveal that the negative impact is mostly driven by employer-financed training. On the worker level, we observe a reduction of training for medium- and high-skilled employees but no significant effects on the training of low-skilled employees.

Keywords: Minimum wage, Training, Difference-in-differences

JEL Classification: C23, I26, J24, J38

1 Introduction

In the literature on minimum wages, there has been a long-lasting and still ongoing discussion on the effects of minimum wages on employment. In their surveys, Brown (1999) as well as Neumark and Wascher (2007) conclude that most studies until the late 1980s corroborated the conventional view that minimum wages reduce employment. In the 1990s, a new strand of research in applied microeconometrics failed to detect meaningful negative employment effects. This caused the literature to converge towards a debate on the size of such—mostly small—employment effects, as well as potential alternative channels of adjustment within affected firms (see among others, Addison 2017 or Bossler and Gerner 2016). Bárány (2016) argues that an increase in training might be one of these adjustment channels and may serve as an explanation for only small disemployment effects. According to the economic theory, minimum wages could negatively impact the incentives for employees to realize human capital accumulation due to lower expected returns from training. Secondly, they could reduce employers' willingness to finance further training as part of cost-saving strategies. In case of non-competitive labor markets, a counteractive increase in training investments could be used to restore productivity-dependent rents. Following these arguments, it seems sensible to complement empirical studies of employment effects (e.g., Bossler and Gerner 2016) with evidence on minimum wage-induced impacts on training at the workplace level.

Of the 28 member states of the European Union, 22 have a statutory minimum wage, while sectoral-specific minimum wages and collective bargaining regimes are used in the remaining six countries (Schulten 2016). In Germany, the statutory minimum wage came into force on 1 January 2015 after it was approved in parliament on 11 July 2014. It is the first compulsory minimum wage that it is valid to all employees with only minor exemptions.[1] The minimum wage was introduced in response to a period of two decades of a substantial decrease in collective bargaining coverage and an increase in wage inequality. The new minimum wage is largely binding, and employer expectations surveyed prior to the minimum wage introduction make adjustments in firm-financed training likely (Bossler 2017). Based on a biennial suggestion of the newly introduced minimum wage commission, the minimum wage can be adjusted by a legislative decree of the German Federal Government. The Minimum Wage Law §9(2) determines that the commission shall suggest a minimum wage that contributes to an appropriate minimum protection of workers and to fair and performing conditions of competition and does not jeopardize employment. While there is no clear connection between the height of the minimum wage and the cost of living, the law states that the development of the minimum wage should align the development of collectively bargained wages in Germany.

This article studies the minimum wage effects on training in course of an introduction of a statutory minimum wage in continental Europe. Applying difference-in-differences estimation techniques to data of the IAB Establishment Panel 2011–2015, the analysis contributes to the literature on training and minimum wages in three aspects. First, we present training effects of the new statutory minimum wage in Germany, which was introduced on 1 January 2015. Second, we can distinguish between three types of training (external training courses, internal training courses, and training on the job) and three skill groups (unskilled workers, workers with vocational qualifications, workers with university degrees). Third, we distinguish between training that is solely firm-financed and training which includes employee expenditures. The empirical analysis of minimum wage effects on training with establishment data is a useful supplement to investigations of individual data. However, employer-employee data that include the time period of the German minimum wage introduction are not yet available.

The article proceeds as follows. Section 2 describes the theoretical aspects presented in previous literature based on which we formulate empirical hypotheses. Section 3 describes the data and microeconometric methods of our analysis. Section 4 presents the empirical results including a sample description, the baseline difference-in-differences results, robustness checks, and effect heterogeneities with respect to sectors, types of training, the employees' financial participation at training costs, and training by qualification levels. Section 5 concludes.

2 Previous literature and hypotheses

In this section, we review the minimum wage literature with respect to effects on training. The implementation of the statutory minimum wage affects the individuals' and firms' training decisions, as it potentially changes the opportunity costs and the gains of training. According to the standard human capital theory, a large part of human capital is accumulated on the job. Employees often finance these investments through wage cuts since they can internalize future gains from training due to an increased productivity. However, a binding minimum wage may inhibit the ability of employers to cut wages to finance training costs (or the firm's share of training costs). Therefore, the implementation of minimum wages is predicted to decrease training of low-paid employees.

In the literature, Leighton and Mincer (1981) corroborate this conjecture by showing that US states with a relatively high proportion of low-skilled employees, and thus a relatively larger applicability of the federal minimum wage, exhibit lower training activities. Hashimoto (1982) argues that minimum wages enhance labor market competition through increasing competition for jobs. This, in turn, leads to a reduction in training. Lazear (1979) estimates the effects of an increase in the minimum wage on training intensity and finds a reduction between 3 to 15% from what it would have been in the absence of changes in the minimum wage. The negative effect of minimum wages on training is also supported in studies by Schiller (1994) who analyzes young labor market entrants that receive less training if they are paid the minimum wage and by Neumark and Wascher (2001) who exploit variation of minimum wages across US states.

Departing from the standard theoretical view that labor markets are competitive, Acemoglu and Pischke (2003) show that the effect of minimum wages on training depends on the labor market structure (Additional file 1). In the presence of labor market frictions, firms receive productivity-dependent rents by paying wages below productivity, i.e., rent = productivity − wage. A binding minimum wage redistributes some fraction of these rents from employers to employees. Even though training is costly, this creates an incentive for firms to increase productivity through training in order to restore the level of rents to some higher level.[2] In the empirical part of their article, Acemoglu and Pischke (2003) use the US National Longitudinal Survey of Youth for the years 1987 to 1992 and measure competitiveness by industry wage differentials and find some weak evidence that training is positively related to minimum wages among workers in less competitive sectors.

In line with these theoretical exploitations that minimum wages could in some circumstances even foster training, Arulampalam et al. (2004) find an increase in workers' training probability following the introduction of the national minimum wage in Britain. Their estimates show an increase in the training probability ranging between 8 and 11 percentage points. In another empirical analysis of the minimum wage in Britain, Riley and Bondibene (2017) show that firms respond to increased minimum wages by the use of productivity-enhancing HR instruments such as organizational changes and training.

Lechthaler and Snower (2008) contribute by accounting for another theoretical channel in which minimum wages may limit the internalization of gains from training. As minimum wages are theoretically associated with an employment reduction, firms cannot fully appropriate the gains in form of a higher productivity leading to a reduced incentive for training provision. This is especially the case for low-qualified workers whose employment is most endangered by minimum wages. Correspondingly, the calibration exercise by Lechthaler and Snower (2008) demonstrates that an increase in the minimum wage by 10% reduces training of low-skilled employees by 11.3% but increases training of medium-skilled employees by 4.1% and high-skilled employees by 1%.

Another strand of literature considers a labor demand-induced impact of minimum wages on skill formation. Cahuc and Michel (1996) suppose that due to the implementation of a minimum wage, the wages of low-skilled employees tend to rise relative to those of high-skilled labor. Thus, the relative demand for low-skilled employees decreases while the relative demand for high-skilled employees increases. Consequently, this creates a labor demand-induced incentive for human capital accumulation of the low-skilled in order to meet the increased demand for high-skilled employment. However, such human capital accumulation could be realized outside the workplace on the own initiative of the employee. Hence, training at the workplace level could disregard the suggested theoretical channel of an additional human capital accumulation. Moreover, the authors mention that training subsidies may serve the same objective while avoiding the negative externalities such as unemployment, and such subsidies may cover some of both the employees' and employers' incentives for additional training. This mechanism may be relevant in our case, as the German Federal Government announced an additional budget for training subsidies for unemployed and employees who are in danger of becoming unemployed shortly after the minimum wage was introduced.

A related argument is that minimum wages may lead to increasing demand-induced skill requirements. This increased skill demand is supported in a recent description by Gürtzgen et al. (2016) who illustrate that skill requirements for vacant minimum wage positions increased in 2015 after the German minimum wage was introduced. Hence, employers expect some additional skill accumulation in exchange for paying the minimum wage.

However, there are also other arguments predicting a decrease of training provision. First, when firms have a certain budget for personnel expenses, the increased wage costs have to be compensated by a reduction of some other fringe benefits (Belman and Wolfson 2014, p. 280). While such a cost reduction could well include several sorts of benefits, a reduction of training expenses could be one explicit channel to compensate for the increased personnel costs. Second, a more cautious hiring policy among minimum wage firms may cause training to decrease. The training literature suggests that newly hired employees typically require training to enable familiarization with job-specific tasks (Beckmann and Bellmann 2002), and the recent minimum wage literature in Germany suggests a (modest) employment effect mostly due to a reduction in hires (Bossler and Gerner 2016). Third, firms may want to encourage employee-initiated quits to reduce employment. This increased quit rate could be achieved by a reduction of training provision, which has shown to be associated with an increasing quit rate (Hübler and König 1999).

In recent empirical work, a reduction in employee turnover induced by minimum wages has been well documented (e.g., Dube et al. 2016), e.g., explained by the efficiency-wage theory. Some indications of a turnover-reducing effect in Germany are presented in Bossler and Gerner (2016). A turnover reduction induces an incentive to employers to invest in training of workers, as future returns are more likely to be internalized. Moreover, if the minimum wage reduces employee turnover, this also reduces the need to hire new personnel, which is also associated with higher training investments, as argued above. Additionally, and as suggested by Lang and Kahn (1998), new hires may be of higher average quality (i.e., productivity), which in turn may reduce the need for initial training of newly hired workers. However, some empirical investigations—independent of minimum wages—point at the opposite mechanism that qualified workers get more training than others (Dostie 2015, Hübler and König 1999). If this is the case, a minimum wage-induced change to the workforce composition could lead to an initially uncertain effect on training. We address this issue when we control for the workforce composition and new hires both by levels of qualification in our analysis (Section 4.2).

Finally, some literature discusses direct and indirect effects of training on wages. For example, Goux and Maurin (2000) use French data and show that the primary effect of training is a reduction of turnover but not of wages. Zwick (2006) finds that training increases the productivity but there was no significant rise of wages. This result is also confirmed by Görlitz (2011), who investigates the short-term impact of on-the-job training on wages using German linked employer-employee data. Hence, the empirical literature suggests that training yields a productivity-dependent rent that is not offset by increased wages.

All these theoretical considerations allow us to derive expectations in the form of empirical hypotheses. (1) The human capital theory predicts minimum wages to reduce training. (2) In some circumstances, this pessimistic prediction can be relaxed when (a) frictions allow for productivity-dependent rents in less competitive labor markets (Acemoglu and Pischke 2003) or (b) low-skilled employees have a labor demand-induced incentive to invest in training (Cahuc and Michel 1996). (3) The results by Lechthaler and Snower (2008) predict a decrease of training for low-skilled workers but a modest increase of training for medium- and high-skilled employees.

3 Data and methods

3.1 Data description

The data set of our empirical analysis is the IAB Establishment Panel,[3] a large annual establishment-level survey on personnel developments and personnel policies such as the provision of training. The survey comprises about 15,000 observations each year and the gross population consists of all registered establishments located in Germany that recorded at least one worker covered by the social security system. The sample is representative for industries, German states ("Bundesländer"), and differing establishment size categories.[4] The personal interviews are conducted by TNS Infratest Social Research in face-to-face on-site meetings with a personnel manager of the respective workplace. This procedure ensures a high data quality and a yearly continuation response rate of about 80%.

The survey follows establishments over time, and a unique establishment identifier allows us to construct a panel of workplaces over time covering the period from 2011 to 2015. We start our panel analysis in 2011 after the financial crisis, which marks a starting point of a period of fairly stable economic development. As the new German minimum wage was introduced on 1 January 2015, the panel includes four waves ahead of its introduction followed by one treatment year. The post-treatment information was collected in the 3rd quarter of 2015.

The IAB Establishment Panel surveys a wide range of measures concerning employers' training provision. Most importantly and very generally, employers are asked to give a yes/no response about the incidence of training at the respective plant. Following this binary distinction, the survey asks for the number of employees who participated in training activities within the last 6 months.[5] This number of trained employees allows us to construct a measure of training intensity, defined as trained employees as a fraction of total employment. A second set of questions asks for the types of training that are used at the respective establishment. This allows us to distinguish between training that is provided through external and internal courses as well as on-the-job training. Similarly, the survey allows a distinction between training that is solely employer-financed and training that includes a financial contribution by employees towards the training costs. Finally, a third question directly asks for the number of trained individuals by three levels of qualification (low, medium, and high). From this information, we can construct variables that measure the share of trained employees at each skill level.

3.2 Empirical methodology

3.2.1 Treatment assignment

For the pre-treatment panel wave of 2014, we have designed a questionnaire module that allows for a treatment assignment that can be used for a minimum wage evaluation using difference-in-differences estimation. The data include a measure on whether the respective workplace was affected by the minimum wage by asking whether at least one worker received a remuneration below the initial hourly minimum wage of € 8.50. A second question captures the number of employees receiving an hourly wage below € 8.50 at that point in time.

From these variables of 2014, we construct a first *binary treatment group* identifier that indicates establishments with at least one employee with a remuneration below € 8.50, which helps to delineate between treated and control plants. A second variable measures the *treatment intensity* from the fraction of affected employees. This creates a bite measure with a stronger weight on workplaces with larger fractions of affected employees. Both treatment variables are calculated from 2014 data but can be traced back and forth across panel years for the respective establishments. This sample construction results in an analysis sample of plants that existed in 2014.

3.2.2 Empirical approach

We structure our empirical analysis as follows. Before estimating difference-in-differences specifications, we present descriptive statistics that characterize our data sample as well as differences between the treatment group and the control group from ahead of the minimum wage introduction in 2014. In the main part of our analysis, we present treatment effects not only from difference-in-differences estimations but also from specifications that include time-varying control variables.

In the baseline difference-in-differences estimation, the measure for training (training incidence, training intensity) is regressed on the interaction of the treatment variable and a post minimum wage indicator:

$$\text{training}_{it} = (\text{treatment group}_i \cdot D2015_t)\delta + \theta_i + \tau_t + \epsilon_{it}, \tag{1}$$

where *treatment group* is either the *binary treatment* variable or the *intensity treatment*. The effect on the treatment group and treatment time interaction δ captures the treatment effect of the minimum wage introduction on the treated establishments. In the baseline specification, θ_i is a vector of time-constant firm-specific fixed effects that control for all constant differences in training between establishments (see, e.g., Hsiao 2003). Hence, it also controls for constant differences between the treatment and the control group, which is required in difference-in-differences specifications (Lechner 2010). Finally, τ_t

captures time fixed effects that are common to all establishments, which we operationalize by dummy variables for each year in the panel data, and ϵ_{it} is an idiosyncratic error term.[6]

When we estimate effects conditional on covariates, we simply add a vector of time-varying control variables x_{it} to the baseline specification:

$$\text{training}_{it} = (\text{treatment group}_i \cdot D2015_t)\delta + x'_{it}\beta + \theta_i + \tau_t + \epsilon_{\text{it}}. \quad (2)$$

Although the presented estimates are retrieved from fixed effect specifications, they can be replicated using differences-in-differences specifications with a time-constant control variable for the treatment group using OLS or random effects estimation. However, the Breusch-Pagan test points at the importance of time-invariant firm effects in our analysis and the Hausman test shows that such firm effects are correlated with our time-varying variables of interest.[7]

Finally, and despite the use of linear probability models, we can also replicate our results using non-linear panel estimators that control for time-constant heterogeneity. Such approaches include the Mundlak estimator (Mundlak 1978), in which the firms effects θ_i are modeled by time averages ($\theta_i = \bar{x}'_i\pi + w_i$), and the van Praag estimator (van Praag 2015), which additionally replaces x_{it} by $(x_{it} - \bar{x}_i)$.[8]

We round off our empirical analysis by estimating several effect heterogeneities using the baseline specification. In a first step, we estimate the difference-in-differences effect for competitive and non-competitive sectors. In a second step, we re-estimate the baseline specification while looking at alternative definitions of the endogenous training variable, yielding effects on (a) tree types of training, (b) training with and without financial participation of employees at the training costs, and (c) training by skill levels of the employees.

4 Empirical analysis and results

4.1 Descriptive statistics

The analysis sample and the variables of interest are described in Table 1. In total, we consider 58,209 establishment observations over the period from 2011 to 2015. Most of the training outcomes that we analyze contain only few observations with missing information. Two exceptions are the training by skill levels, which was not included in the 2012 survey, and the distinction by financial participation in training costs, which was only included in the surveys of 2011, 2013 and 2015. With respect to the treatment assignment, the descriptives show that 15% of the establishment observations are treated, implying that 85% of the observations are in the control group of unaffected plants.

We measure the training incidence as a binary indicator and the training intensity as the share of trained employees that were subject to training in the first half of the respective year. As we see from the sample averages, 70% of the workplaces in Germany provided some sort of training to at least one of the employees. Across all establishments, 32% of all employees participated in training. While the intensity of training among the medium qualified employees was especially high, this is mostly due to the fact that this group constitutes the largest group of employees in Germany. Training can be realized with or without financial participation of employees. While we exclude establishments without a clear assertion in this respect, the sample averages clearly show that the training is financed by employers in most cases. A final distinction is made by types of training. We

Table 1 Descriptive statistics of the total sample

Variable	Obs.	Mean	Std. dev.	Min	Max
Treatment assignment:					
Treatment group	58,209	0.146	0.353	0	1
Intensity treatment	58,209	0.053	0.168	0	1
Training incidence (dummies):					
Training	58,174	0.696	0.460	0	1
Training intensity (shares):					
Training	56,509	0.318	0.341	0	1
Training of low qualified	44,160	0.026	0.101	0	1
Training of medium qualified	45,994	0.240	0.292	0	1
Training of high qualified	44,829	0.051	0.139	0	1
Worker-financed training	31,080	0.068	0.206	0	1
Firm-financed training	31,080	0.245	0.326	0	1
External courses	56,470	0.278	0.332	0	1
Internal courses	56,470	0.228	0.330	0	1
On-the-job training	56,470	0.220	0.326	0	1
Explanatory variables:					
Share of medium qualified	58,209	0.590	0.276	0	1
Share of high qualified	58,209	0.097	0.190	0	1
Hiring rate	58,209	0.057	0.116	0	1
Skilled hiring rate	58,209	0.038	0.087	0	1
Collective bargaining	58,124	0.428	0.495	0	1
Works council	58,121	0.284	0.451	0	1
Lagged employment (in logs)	58,209	3.094	1.707	0	10.98
Part-time fraction	57,546	0.268	0.269	0	1
Technical level of capital (1 = outdated,..., 5 = novel)	58,021	2.787	0.763	1	5
High competitive pressure	58,209	0.352	0.316	0	1

Data source: IAB Establishment Panel 2011–2015, analysis sample

distinguish between external courses, internal courses, and on-the-job training.[9] The latter comprises on-the-job training in terms of job rotation, quality circles, and self-learning programs.

In conditional estimations of the difference-in-differences specification, we consider a large set of control variables. To account for potential changes in the workforce composition, we account for the workforce composition by levels of qualification. Moreover, we control for the hiring of new workers by skill levels, as newly hired workers may receive some initial training. Looking at other (more structural) firm-level characteristics, the description displays that 43% of the plants are covered by collective bargaining, and 28% report worker representation through a works council. Concerning the workforce composition, 27% of the employees are part-time workers, which is relevant as these are less likely to participate in training. We also account for the self-reported technical levels of capital and competitive pressure.

Table 2 displays descriptive differences of the variables by treatment status. While we did not have concrete expectations concerning descriptive differences in training,

Table 2 Testing differences between treatment and control group in 2014

	Treatment group (*t–i*)		Control group (*t–i*)		Difference
	Mean	sd	Mean	sd	
Training incidence (dummies):					
Training	0.591	0.492	0.703	0.457	−0.112***
Measures for training intensity (shares):					
Training	0.254	0.330	0.336	0.348	−0.082***
External courses	0.212	0.314	0.298	0.342	−0.086***
Internal courses	0.177	0.309	0.240	0.340	−0.063***
On-the-job training	0.178	0.306	0.233	0.335	−0.055***
Worker-financed training	0.052	0.179	0.069	0.205	−0.018***
Firm-financed training	0.179	0.298	0.243	0.321	−0.064***
Training of low qualified	0.040	0.135	0.026	0.099	0.014***
Training of medium qualified	0.190	0.274	0.251	0.298	−0.061***
Training of high qualified	0.021	0.087	0.056	0.149	−0.035***
Covariates:					
Share of medium qualified	0.542	0.306	0.585	0.280	−0.043***
Share of hight qualified	0.041	0.113	0.101	0.197	−0.060***
Hiring rate	0.084	0.146	0.057	0.120	0.027***
Skilled hiring rate	0.041	0.091	0.039	0.093	0.002
Collective bargaining	0.227	0.419	0.446	0.497	−0.219***
Works council	0.139	0.346	0.295	0.456	−0.156***
Lagged employment (in logs)	3.005	1.520	3.045	1.757	−0.040
Part-time fraction	0.377	0.298	0.264	0.268	0.113***
Technical level of capital	2.600	0.807	2.809	0.768	−0.208***
High competitive pressure	0.140	0.347	0.112	0.315	0.029***

Sample averages by treatment status before treatment in 2014. Data source: IAB Establishment Panel 2014. For the measures of training by financial participation of employees, we use data of 2013 as this information was not included in the 2014's panel wave
*** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$

almost all variables that measure training in some qualitative way indicate a lower average training provision in treated compared to untreated establishments. An exception is the training of low qualified employees, which is initially higher at treated establishments. A t test shows that most bivariate mean differences are statistically significant at a 1% significance level. These descriptive differences suggest that employees (except the low qualified employees) at affected plants were less likely to have participated in training measures ahead of the minimum wage introduction.

Also with respect to other observable variables, Table 2 reveals some meaningful differences between treatment and control groups in 2014. The treatment group is more likely to be covered by a collective bargaining agreement and less likely to possess a works council. Moreover, we observe some other typical differences: treated plants show a larger share of part-time employment, have a relatively outdated technical capital infrastructure, and face higher competitive pressure. As all these observable variables may also be correlated with the provision of training, we use them as controls for a robustness check that presents difference-in-differences estimates conditional on covariates. Finally,

Table 2 does not show significant differences in the hiring rate of skilled workers and the firm size measured by the number of employees.

4.2 Difference-in-differences estimation

Difference-in-differences estimates in Table 3 allow us to interpret the effect of the minimum wage introduction on training. The estimated coefficients are treatment effects as depicted by the δ-coefficient in equation (1), which is the effect of the treatment group–treatment time interaction. While Panel A displays effects on the training incidence, i.e., whether the firm provides training at all, Panel B presents effects on the training intensity, i.e., the fraction of trained employees in the respective establishment.

The effects of the minimum wage introduction on training incidence are small and insignificantly different from zero for both the binary treatment definition and the intensity treatment. The results imply that there is no effect on the very general decision whether or not to provide training to at least one employee. The validity of this conclusion is supported by the two placebo regressions in columns (2) and (4), which omits data from 2015 and artificially assigns treatment to observations of 2014.

Looking at the effect of the minimum wage on training intensity, Panel B reveals a negative impact of −0.018 from the binary treatment variable, implying that the minimum wage reduced training by 1.8 percentage points at treated workplaces. This effect is supported by the negative and significant impact in column (3). The effect size implies

Table 3 Difference-in-differences estimates on training

	(1)	(2)	(3)	(4)
Panel A: Dependent variable is the training incidence				
$DiD_{\text{binary treatment}}$	−0.003			
	(0.011)			
$DiD_{\text{binary placebo}}$		0.004		
		(0.009)		
$DiD_{\text{intensive treatment}}$			0.0001	
			(0.022)	
$DiD_{\text{intensive placebo}}$				−0.003
				(0.021)
Panel B: Dependent variable is the training intensity (share of trained employees)				
$DiD_{\text{binary treatment}}$	−0.018**			
	(0.008)			
$DiD_{\text{binary placebo}}$		−0.005		
		(0.007)		
$DiD_{\text{intensive treatment}}$			−0.041***	
			(0.015)	
$DiD_{\text{intensive placebo}}$				−0.020
				(0.014)
Observations	56,509	44,794	56,509	44,794

Difference-in-differences estimates with firm- and time-specific fixed effects. Cluster robust standard errors in parentheses (clusters are on the level of establishments). Dependent variables are describes in the titles of Panels A and B, where "training incidence" is a binary variable that takes the value 1 when firms provide training and 0 otherwise, and "training intensity" is a variable capturing the fraction trained employees (in the respective year) as of all employees. Data source: IAB Establishment Panel 2011–2015, analysis sample

*** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$

that the training intensity decreases by 0.4 percentage points for a 10 percentage points increase in the fraction of affected employees. The placebo tests in columns (2) and (4) are insignificantly different from zero supporting such negative effects. However, the negative sign of such placebo estimations points at the possibility of a trend towards lower training provision that may be independent of the introduction of the minimum wage. As parallel trends between the treatment group and the control group in the absence of the treatment is a crucial assumption for difference-in-difference estimates, we devote the next subsection to the inspection of trend differences.

As a first robustness check to the baseline effects, we estimate the minimum wage effect on the training intensity from conditional difference-in-differences specifications. This is particularly relevant as the workforce composition may change due to the minimum wage if firms hire more qualified workers (Lang and Kahn 1998), which may or may not require a different intensity of training. Table 4 controls for such covariates, where columns (1) and (5) control for the workforce composition by qualification levels only. While the results corroborate the conjecture that highly qualified employees receive more training, the effect of the minimum wage remains unchanged. In columns (2) and (6), we control for

Table 4 Conditional difference-in-differences estimates on the training intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable: training intensity (share of trained employees)							
$DiD_{\text{binary treatment}}$	−0.018***	−0.018**	−0.018**	−0.017**				
	(0.008)	(0.008)	(0.008)	(0.008)				
$DiD_{\text{intensive treatment}}$					−0.043***	−0.044***	−0.043***	−0.042***
					(0.015)	(0.015)	(0.015)	(0.015)
Share high skilled	0.087***		0.086***	0.085***	0.087***		0.086***	0.086***
	(0.021)		(0.021)	(0.021)	(0.021)		(0.021)	(0.021)
Share medium skilled	0.050***		0.048***	0.048***	0.050***		0.048***	0.047***
	(0.010)		(0.010)	(0.010)	(0.010)		(0.010)	(0.010)
Hiring rate		−0.028	−0.013	−0.016		−0.029	−0.013	−0.016
		(0.018)	(0.019)	(0.019)		(0.018)	(0.019)	(0.019)
Skilled hiring rate		0.050**	0.023	0.021		0.050**	0.024	0.021
		(0.025)	(0.025)	(0.025)		(0.025)	(0.025)	(0.025)
Collective bargaining				0.012**				0.012**
				(0.006)				(0.006)
Works council				0.009				0.009
				(0.011)				(0.011)
Lagged employment (logarithmic)				−0.012				−0.012
				(0.008)				(0.008)
Part-time share				−0.021**				−0.021**
				(0.011)				(0.011)
Technical level of capital				0.009***				0.009***
				(0.002)				(0.002)
High competition				−0.010*				−0.010*
				(0.005)				(0.005)
Observations	55,612	55,612	55,612	55,612	55,612	55,612	55,612	55,612

Additional controls as indicated by the respective coefficients. All other notes as in Table 3. Data source: IAB Establishment Panel 2011-2015, analysis sample

***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$

the general hiring rate as well as the hiring rate of qualified employees. The results show some indication that the hiring rate exerts a negative impact on training while the hiring of qualified employees has a positive impact, again the effect of the minimum wage on training remains unchanged. Very similar results are obtained form columns (3) and (7), where the estimations control for the workforce composition and the hiring rates. These results suggest that the effect of the minimum wage on training is not simply driven by the mechanism that the minimum wage changes the workforce through hiring different kinds of workers.

We additionally control for some more structural firm-level information in columns (4) and (8). While this conditioning has the advantage of controlling for potentially confounding covariates, it carries the risk of controlling for potentially endogenous factors. Additional control variables are collective bargaining coverage, works council representation, the fraction of part-timers, the technical level of capital, and an indicator for high competitive pressure. Suchlike the baseline estimates, both specifications show a negative and significant treatment effect of the minimum wage introduction on the share of trained employees, which in size remains virtually unchanged.

As treatment effects may vary by firm size, we test effect heterogeneities by firm size categories. We construct interaction variables between the treatment variable and firm size dummies, where we consider three firm size classes measured by the number of employees: $FS1 \leq 5$, $6 \leq FS2 < 250$ and $FS3 \geq 250$. Using control variables as in columns (4) and (8), we find

$$\beta_{DiD\_\text{binary treatment}} \cdot \text{FS1} = -0.018(0.015)$$
$$\beta_{DiD\_\text{binary treatment}} \cdot \text{FS2} = -0.015(0.009)$$
$$\beta_{DiD\_\text{binary treatment}} \cdot \text{FS3} = -0.042 **(0.020)$$
$$\beta_{DiD\_\text{intensive treatment}} \cdot \text{FS1} = -0.030(0.026)$$
$$\beta_{DiD\_\text{intensive treatment}} \cdot \text{FS2} = -0.050 *** (0.019)$$
$$\beta_{DiD\_\text{intensive treatment}} \cdot \text{FS3} = 0.002(0.055),$$

where cluster robust standard errors are in parentheses. Under binary treatment the interaction effect is only significant for large establishments. The picture changes when we consider the intensive treatment definition. In such regressions, middle-sized establishments have the strongest negative treatment effect. The influence on smaller and larger firms turns insignificant.

4.3 Inspecting the parallel trends assumption

Even though the placebo effects of the baseline specification in Table 3 fall short of statistical significance, they may point at a divergence of trends between the treatment and the control group. As parallel trends are a crucial assumption for difference-in-differences estimation, we devote this subsection to the inspection of group-specific trends, and we apply estimation strategies that have been proposed in the literature to deal with the divergence of trends in difference-in-differences estimation.

Figure 1 illustrates the average use of training for treated and control establishments in our analysis sample. Despite some year-specific variation in the data that is independent of the two groups, the unconditional illustration in Panel **a** questions the parallel trends assumptions by revealing a relative decline in the training intensity among the group of treated establishments. While this development can be seen as an interesting finding in itself, it may be independent of the introduction of the minimum wage.

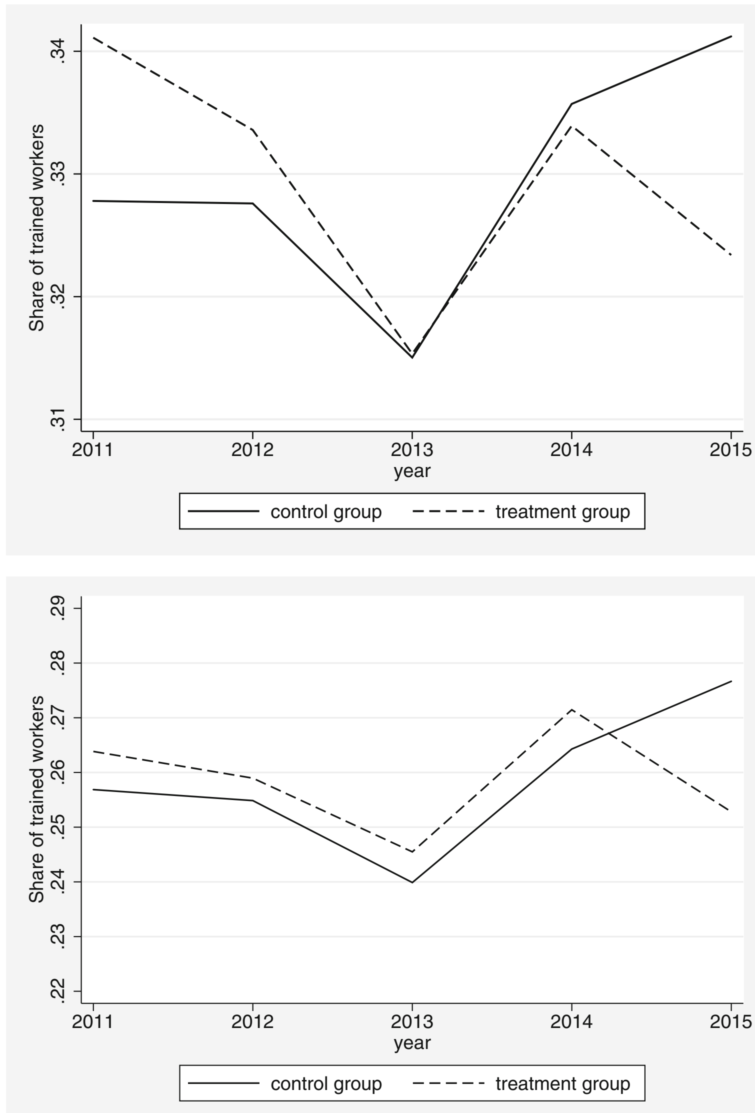

Fig. 1 Inspection of parallel trends between treatment and control group. **a** The trends of training by treatment status before propensity score matching. **b** The trends of training by treatment status for the treatment group and the matched control group, which is defined by radius matching. Source: IAB Establishment Panel, 2011–2015, analysis sample

To shed light on the presence of a treatment effect irrespective of a divergence in the group-specific training trends, we add a term that captures treatment group and time-specific heterogeneity to our empirical model of interest:

$$\text{training}_{it} = (\text{treatment group}_i \cdot D2015_t)\delta + \theta_i + \tau_t + \Psi_{\text{treatment group,t}} + \epsilon_{it} \qquad (3)$$

The treatment group and time-specific heterogeneity is specified by $\Psi_{\text{treat group,t}}$. As $\Psi_{\text{treatment group,t}}$ may correlate with the treatment effect interaction of interest, we attempt to control for this additional heterogeneity using two different approaches.

As a first attempt, we follow previous literature by Addison et al. (2015), Allegretto et al. (2011), and Neumark et al. (2014) who suggest to control for such treatment group and time-specific heterogeneities directly by the use of parametric trends. Following this argument, we specify such treatment group-specific trends as $\Psi_{\text{treatment group,t}} = T_t * \text{treatment group}_i * \psi$, where T_t represents a count variable for each panel wave and treatment group$_i$ indicates treated plants. The trend term is included to the baseline specification as formulated in equation (3), where ψ estimates the treated establishments' trend divergence from the control group, exploiting time variation from before the minimum wage introduction. As this imposes the assumption that the trend divergence is linear, we additionally check alternative—more flexible—specifications of such trends by the use of quadratic and cubic polynomials, i.e.,

$$\Psi_{\text{treatment group,t}} = T_t * \text{treatment group}_i * \psi_1 + T_t^2 * \text{treatment group}_i * \psi_2$$

$$\Psi_{\text{treatment group,t}} = T_t * \text{treatment group}_i * \psi_1 + T_t^2 * \text{treatment group}_i * \psi_2 + T_t^3 * \text{treatment group}_i * \psi_3$$

The treatment effect of the regressions that control for treatment group-specific trends are displayed in Table 5. The binary and the intensive treatment effects shrink to about one percentage point. Additionally, in such specifications the standard errors increase, as the specification leaves little time variation for identification of the true treatment effect. Hence, all point estimates turn insignificant. However, we can also emphasize that the point estimates remain negative and robust in all such trend specifications, pointing at a negative treatment effect of the minimum wage introduction that quantifies a decrease in training by one percentage point.

In a second attempt, we control for the group-specific time heterogeneity $\Psi_{\text{treatment group,t}}$ by the use of a matching procedure that compares treated establishments with control establishments conditional on pre-treatment developments in training, i.e., conditional on pre-treatment developments in the outcome variable. In the matching procedure, we want to compare establishments with similar levels of training in 2014 as well as similar growth rates in training 2011–2012, 2012–2013 and 2013-2014.

For this purpose, we use a propensity score matching (PSM) that allows us to equalize pre-treatment trends as illustrated in Panel **b** of Fig. 1.[10] PSM requires estimation of the propensity score $p(x)$ that represents the probability to be treated for each establishment

Table 5 Treatment effects controlling for treatment group-specific trends

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable is the training intensity (share of trained employees)					
$DiD_{\text{binary treatment}}$	−0.006	−0.008	−0.002			
	(0.009)	(0.012)	(0.014)			
$DiD_{\text{intensive treatment}}$				−0.012	−0.010	−0.011
				(0.020)	(0.020)	(0.028)
Linear trends	x	x	x	x	x	x
Quadratic trends		x	x		x	x
Cubic trends			x			x
Observations	56,509	56,509	56,509	56,509	56,509	56,509

Treatment group-specific time trends are specified as indicated in the respective columns. All other notes as in Table 3. Data source: IAB Establishment Panel 2011–2015, analysis sample in the data, which is an estimation of the treatment group identifier on the pre-treatment training level and pre-treatment growth rates of training as explanatory variables. Based on this prerequisite estimation, the treatment effect on the treated establishments can be formalized as follows:

$$\mathrm{ATT}^{\mathrm{PSM}} = \frac{1}{N_{\mathrm{treated}}} \sum_{i \in N_{treated}} [\, \Delta^{2015-14}\mathrm{training}_i - \Delta^{2015-14}\widehat{\mathrm{training}}_{M(i)|p(x)}] \tag{4}$$

where the outcome of interest is the growth in the training intensity (share of trained employees) from 2014 to 2015. For each treated establishment i, this outcome variable is compared with a group of establishments from the control group, i.e., close matches defined as $M(i)$, where the match-defining proximity is evaluated from the propensity score $p(x)$. The difference between the average of the treated establishments' training growth and matched control group's average training growth defines the treatment effect on the treated establishments.

The estimates in Table 6 present the average treatment effects on the treated establishments based on the propensity score matching procedure in equation (4), where the group of similar matches is defined by radius matching with a caliper of 0.03 percentage points. From a visual inspection of the pre-treatment trends of the treatment and the control group in Panel **b** of Fig. 1, this procedure serves to equalize trends, but it is insensitive to the point estimate of interest.[11] The effect in column (1) reveals a reduction in training by about −2.9 percentage points. A crucial assumption for propensity score matching is sufficient common support, i.e., a sufficiently large number of control establishments that are similar compared with their treated counterparts. As a robustness check, we follow the suggestion by Imbens (2015) and use a trimming procedure that reduces the analysis sample based on the support in the propensity score. In fact, we exclude 5% of the treated establishments whose propensity score has the least number of similar controls. The respective estimate is presented in column (2). As the point estimate remains unchanged, the trimming of the treatment group does not affect our results.

As the application of propensity score matching requires a binary treatment assignment, we cannot directly use the intensity treatment in our PSM. However, we follow a suggestion by Imbens and Wooldridge (2009) in using two separate binary treatments, dif-

Table 6 Treatment effects of a radius matching on pre-treatment trends

	(1)	(2)	(3)	(4)
	Dependent variable is the training intensity (share of trained employees)			
$ATT_{\text{binary treatment}}$	−0.029***	−0.029***		
	(0.010)	(0.009)		
$ATT_{\text{low treatment intensity}}$			−0.026**	
			(0.010)	
$ATT_{\text{low treatment intensity}}$				−0.034*
				(0.018)
Establishments	6724	6703	6453	5958

ATT is the average treatment effect on the treated. Dependent variables is the "training intensity" capturing the fraction trained employees (in the respective year) as of all employees. Data source: IAB Establishment Panel 2011–2015
*** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$

fering in their treatment intensity (more or less than 50% of the employees were affected by the minimum wage introduction). We use both treatments in separate estimations and exclude observations marked by the alternative definition respectively. The results in columns (3) and (4) of Table 6 reveal an effect on training which is slightly larger for the group of more severely affected establishments.

In total, the treatment effects from PSM reveal a robust negative effect on training, while providing an effective strategy to equalize pre-treatment trends. Combined with the baseline panel estimates in Tables 3 and 4 and the parametrization of group-specific trends in Table 5, the effect of the minimum wage introduction on training is in the range between −1 and −3 percentage points.

4.4 Effect heterogeneities

In what follows, we present heterogeneities of the effect of the minimum wage introduction on training. First, we estimate separate effects for competitive and non-competitive sectors. Thereafter, we use different outcome variables to estimate effects on three different types of training, on training with and without financial participation by the employees and on training by levels of qualification.

4.4.1 Sectoral differences

We first present separate treatment effects for groups of sectors by competitiveness.[12] In contrast to the standard human capital theory, the contribution by Acemoglu and Pischke (2003) shows that effects of minimum wages on training can be positive when labor markets are less competitive, i.e., when rents are prevalent in the market. As these rents are productivity-dependent, employers have an incentive to restore such rents by increasing the productivity through training. Minimum wages would otherwise redistribute the rents to the employees' side. In situations without minimum wages, it is less likely that the establishments attempt to enlarge rents via productivity-enhancing training without an employees' participation. This would be considered as unfair and would probably have negative consequences for the firm by reactions of the employees.

Table 7 shows that the adverse effect of the minimum wage introduction on training is much stronger in competitive than in less competitive sectors. Looking at the incidence of training, we observe significant effects for competitive sectors while the effect for non-competitive sectors is insignificant. When we view the training intensity, we find insignificant effects in both sectors but the negative coefficient is absolutely larger in competitive sectors. Our results do not support the prediction that minimum wages positively affect training in less competitive sectors (here services). However, we cannot exclude that under weak competition, the negative minimum wage effect on training is reduced.

4.4.2 Types of training

The data provide us with information on the types of training at the respective establishments allowing a distinction between external courses, internal courses, and on-the-job training. We use this information as separate outcome variables in separate regressions. Table 8 demonstrates that the introduction of the minimum wage reduces the training for all three types. No systematic differences can be observed.

Table 7 Treatment effects on training in competitive and non-competitive sectors

Dependent variable is the training intensity (share of trained employees)				
	(1)	(2)	(3)	(4)
	Competitive sectors		Non-competitive sectors	
$DiD_{\text{binary treatment}}$	−0.038***		−0.002	
	(0.013)		(0.013)	
$DiD_{\text{intensive treatment}}$		−0.039		−0.032
		(0.026)		(0.024)
Observations	17,492	17,492	15,121	15,121

The definition of competitive and non-competitive sectors is described in footnote 6. Further notes as in Table 3. Data source: IAB Establishment Panel 2011–2015

4.4.3 Financial participation of the employees

We now differentiate by training that is purely employer-financed and training with financial participation of the employees. Workers who themselves contribute to training by financial participation view training especially as a long-run mean of climbing the career ladder. They are investing in their future jobs and income streams. However, from the workers' perspective, the minimum wage induces an incentive to reduce training activities as marginal returns could be reduced when the minimum wage compresses the wage distribution or when workers expect a permanent wage compression such that skill premiums become less pronounced. On the other hand, employees may face a demand-induced incentive for skill accumulation (Cahuc and Michel 1996). Hence, the effect on employee-financed training is theoretically ambiguous. By contrast, employers have a clear cost-reducing incentive and are therefore predicted to decrease firm-financed training activities.

Table 9 demonstrates negative effects on the training incidence and the training intensity when it is purely firm-financed. However, we do not observe any significant effect on training that is completely or at least partially financed by employees. This result is in line with the theoretical expectation that employers try to reduce costs of training. Employees may still have an incentive to invest in human capital accumulation through training, and therefore, they participate in training costs if they are not financially constrained.[13]

Table 8 Separate treatment effects on types of training

Dependent variable is the training intensity (share of trained employees)						
	(1)	(2)	(3)	(4)	(5)	(6)
	External courses	Internal courses	On-the-job	External courses	Internal courses	On-the-job
$DiD_{\text{binary treatment}}$	−0.013*	−0.016**	−0.014*			
	(0.008)	(0.007)	(0.008)			
$DiD_{\text{intensive treatment}}$				−0.033**	−0.026*	−0.038***
				(0.014)	(0.014)	(0.014)
Observations	56,470	56,470	56,470	56,470	56,470	56,470

Dependent variables are the shares of trained employees with access to the types of training indicated by column titles. Training "on-the-job" includes training on-the-job in the narrow sense, quality circles, job rotation, and self-learning programs. Further notes as in Table 3. Data source: IAB Establishment Panel 2011-2015, analysis sample
***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$

Table 9 Treatment effects on training by financial participation at training costs

Dependent variable is the training intensity (share of trained employees)				
	(1)	(2)	(3)	(4)
	Financial participation	Firm financed	Financial participation	Firm financed
$DiD_{\text{binary treatment}}$	−0.003	−0.015*		
	(0.007)	(0.009)		
$DiD_{\text{intensive treatment}}$			−0.017	−0.033*
			(0.011)	(0.017)
Observations	31,080	31,080	31,080	31,080

Financial participation implies that the training is either fully or partially financed by the employees. Further notes as in Table 3. Data source: IAB Establishment Panel 2011, 2013 and 2015. Training by financial participation of the employees was not included in 2012 and 2014

4.4.4 Effects by workers' qualification

Finally, the IAB Establishment Panel contains information on whether the participants in training were unskilled workers, workers with a vocational certificate, or employees with a university degree have participated in training. This distinction allows us to add to the literature, where effects on training are mostly analyzed irrespective of the workers' initial education. The relevance draws on the possibility that firms may change the provision of training not only for low-qualified minimum wage workers but they may also adjust the provision of training measures for skilled personnel in order to compensate for the costs induced by the minimum wage.

However, we cannot distinguish between treated and untreated workers by skill levels. Hence, we have to be cautious when relating skills groups to the minimum wage. Nevertheless, we can estimate the reduced form effect of the minimum wage introduction on the training by skill groups. If the respective treatment effect on unskilled workers is negative, this could imply that

- Training is reduced for minimum wage affected and unaffected unskilled workers
- Training of affected workers increases and that of unaffected workers decreases, where the latter effect overcompensates the former
- Training of non-affected workers increases and that of affected workers decreases, where the latter overcompensates the former.

The effects in Table 10 do not show statistically significant effects on the training incidences for low and medium qualified workers (columns 1 to 2). This could be due to the fact that qualification decisions are involved in long-term considerations. However, it is interesting to note that the training incidence and intensity of high-skilled workers decreases—see columns (3) and (6). The latter effect is also observed for medium qualified employees—see column (5)—while the effect is insignificant for the low-skilled workers.

What might explain these effect differences? One possibility is that the previous training of workers with a university degree or a vocational certificate was much more intensive, so that a temporary reduction for these groups is less important. From a theoretical perspective, it seems plausible because the minimum wage induces an incentive for human capital accumulation among the least skilled, as the relative labor demand shifts towards relatively more skilled workers (see Section 2). This pressure to invest in training at the employees' side contrasts the cost-saving argument on the employers' side, such that

Table 10 Treatment effects on training by skill levels of employees

Dependent variable is the training intensity (share of trained employees)						
	(1)	(2)	(3)	(4)	(5)	(6)
	Low qualified	Medium qualified	High qualified	Low qualified	Medium qualified	High qualified
$DiD_{\text{binary treatment}}$	−0.003	−0.010	−0.004**			
	(0.003)	(0.007)	(0.002)			
$DiD_{\text{intensive treatment}}$				−0.011	−0.026**	−0.005**
				(0.008)	(0.012)	(0.002)
Observations	44,160	45,994	45,388	44,160	45,994	45,388

Notes as in Table 3. Data source: IAB Establishment Panel 2011, 2013, 2014 and 2015. The differentiation by skill level was not included in the 2012's panel wave

an insignificant effect seems plausible. Once more, we should emphasize that training decisions are based on long-term considerations and insofar not all effects can be captured in the first post-reform year, especially because the substitution between skilled and unskilled is highly expansive. Nevertheless, we believe that long-term oriented establishments start early with adjustment measures, as this is advantageous over reluctant behavior.

Another contrasting argument may be that training activities are increased for low-skilled hires because of a rise in skill demand for vacancies paid at the level of the minimum wage (Gürtzgen et al. 2016). It seems plausible that the respective applicants have to finance the acquisition of these skills if they cannot supply a sufficiently high productivity. However, as the data do not include information on the financial burden of training costs by skill levels, we cannot rule out employer-financed training for such hires.

5 Conclusions

We analyze effects of the introduction of the new statutory minimum wage on firm-level training in Germany. Human capital theory predicts that binding minimum wages prohibit wage cuts that are used by employers to finance training. If training costs are not offset by an increase in productivity, firms have to cut training costs. However, this pessimistic prediction can be relaxed when labor market frictions allow for productivity-dependent rents (Acemoglu and Pischke 2003). Moreover, an increase of relative demand for skilled workers may induce an incentive for human capital accumulation.

We apply a difference-in-differences estimation to data from the IAB Establishment Panel, a panel data set with comprehensive information on training and the bite of the minimum wage. The results do not provide evidence for a decrease in training incidence. However and more importantly, we find fairly robust evidence for a decrease in the training intensity (i.e., share of trained employees) in establishments affected by the minimum wage. The estimated effects of the binary treatment on the training intensity is roughly −1.8 percentage points across all specifications, including estimations with and without covariates. Under the consideration of an intensive treatment variable, the percentage point effect ranges between −0.039 and −0.041. Moreover, we find robust effects on different types of training, indicating that the reduction in training is not compensated by a change in the quality of such training.

When we estimate separate effects for rather competitive and non-competitive sectors, we find that the negative effects on the training intensity are driven by competitive sectors, whereas the effect in non-competitive sectors remains inconclusive. This effect heterogeneity corroborates predictions by Acemoglu and Pischke (2003) who have shown that market frictions and productivity-dependent rents can, in fact, allow for a positive effect of minimum wages on training. We also demonstrate that the minimum wage mostly affected employer-financed training but not training that is at least partially financed by employees. We conclude that employers have a minimum wage induced incentive to cut training costs when they cannot devise improved training regimes that would provide net productivity increases. The employees' incentives for human capital accumulation seem to be unchanged.

We finally present separate effects by skill groups. The results show that the effect is mostly driven by a reduction of training for medium and highly qualified employees. While this is at odds with predictions by Lechthaler and Snower (2008), it could well be that employers cannot further risk a diminishing productivity of low-skilled employees, and in contrast, they are able to cut training costs of employees that are typically not concerned by minimum wages.

As a caveat concerning our analysis, we should stress that it assesses short-run effects that were measured in the 3rd quarter of 2015, the year in which the minimum wage was introduced. As the effects of the minimum wage may – or may not – emerge in a longer-term adjustment period, we recommend further studies on this issue in the long-run. Such a long-run analysis would complement our results by capturing lagged training adjustments that should be equally important to policy makers. We also suggest supplementary analyses that look at the individual level independent of the workplace dimension. It is possible that the minimum wage affects human capital accumulation outside the workplace, which is neglected in our firm-level analysis. We further encourage an analysis that relates minimum wage effects on training to employment effects. It seems plausible that a reduction of costs (through a decline in employer-financed training) has helped to maintain the size of the workforce.

Endnotes

[1] Such exemptions include young employees until 18 years of age, apprentices, internships with a maximal duration of 3 months, long-term unemployment during the first 6 months of a subsequent employment, and volunteers. Until the end of 2017 already existing sectoral minimum wages were allowed to undercut the new statutory minimum wage.

[2] A detailed graphical description of this concept is presented in the figure of Additional file 1.

[3] Comprehensive descriptions of the IAB Establishment Panel are provided by Fischer et al. (2009) or Ellguth et al. (2014).

[4] In a comparison of the survey sample with the population of all establishments in Germany, Bossler et al. (2017) do not detect any meaningful selectivity in the survey response.

[5] In panel years before 2014, respondents were allowed to report the number of training measures instead of trained employees. This alternative reporting option was used by approximately 15% of the establishments and causes an

inconsistency within these years but also across time. We impute the number of trained employees for such cases using the procedure by Stegmaier (2012) and Hinz (2016). However, an exclusion of such observations of the waves 2011, 2012 and 2013 from our analysis sample does not cause any significant changes to our results.

[6] We report inference based on standard errors clustered at the establishment level.

[7] The test statistic of the Breusch-Pagan Lagrange multiplier test is 26,958.89 (p value 0.0000). Hence, the null hypothesis of zero firm-level variation is clearly rejected. The chi-squared test statistic of the Hausman test is 54.13 (p-value 0.0000). This rejection implies that firm-specific effects are correlated with time-varying observables pointing at the importance of fixed effect estimation.

[8] Results of these non-linear estimations are available on request.

[9] Training at a specific workplace can comprise all three types of training.

[10] We use a radius matching procedure as this equalizes trends, see Panel **b** of Fig. 1. While other matching procedures such as the kernel matching or nearest neighbor matching allow us to replicate the treatment effects, they fail to equalize trends from a visual inspection.

[11] The results are robust to alternative sizes of the caliper. Setting the caliper to values ranging between 0.01 and 0.06 yields point estimates ranging between −0.02 and −0.03.

[12] We use a distinction by sectors based on the results by Bachmann and Frings (2017) who estimate the extent of sector-specific monopsony power from elasticities of job-to-job transitions. Based on their results, all manufacturing sectors are competitive, whereas among the services, wholesale, retailing, hotels and restaurants, and other services, non-industrial organizations, and the public services are characterized by a high extend of monopsony power.

[13] The case of financial constraints is discussed by Acemoglu and Pischke (2003).

Additional file

Additional file 1: Minimum wages and training in monopsonistic labor market as derived by Acemoglu and Pischke (2003). The file describes the basic theoretical idea of the theory in Acemoglu and Pischke (2003) from a graph. (PDF 44 kb)

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