Online Appendix

Raising the bar: minimum wages and employers' hiring standards

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A Theoretical framework

A.1 A toy model

Consider a risk-neutral firm that uses only labor to produce a good, which it sells at price $p = 1$. The firm's output depends solely on the ability a of its single worker, where $a \sim N(m_a, \sigma_a^2)$. Before production starts, the firm receives an application from one worker. If it hires the worker, it has to pay her the market wage w, which is exogenously given and independent of ability. The firm then produces and sells all of its output. If the firm does not hire the worker, it produces zero output and incurs no costs. Thus the firm's profit is given by $\Pi(a, w) = a - w$ if it hires the applicant and by $\Pi(a, w) = 0$ otherwise.

When ability is perfectly observed, the firm will hire the worker if $a \geq w$. Similarly, when a is unobserved but its distribution is known, the firm will make a hire if $m_a \geq w$.

Now consider the case when a is imperfectly observed and the firm receives a noisy ability signal $z = a + \epsilon$, where $\epsilon \sim N(0, \sigma_\epsilon^2)$ and ϵ is uncorrelated with ability. Suppose that the firm chooses the best linear prediction of ability given the signal. Then the firm will hire the worker if, conditional on the ability signal, expected profits are non-negative:

$$\mathrm{E}[\Pi|z] = m_a + \frac{\sigma_a^2}{\sigma_a^2 + \sigma_\epsilon^2}(z - m_a) - w \geq 0 \tag{1}$$

or, equivalently, if the ability signal is weakly greater than the hiring threshold t:

$$z \geq \frac{1}{\sigma_a^2}[(\sigma_a^2 + \sigma_\epsilon^2)w - \sigma_\epsilon^2 m_a] = t. \tag{2}$$

This threshold goes up when the wage is increased (such as by a minimum wage introduction that bites):

$$\frac{dt}{dw} = \frac{\sigma_a^2 + \sigma_\epsilon^2}{\sigma_a^2} > 0. \tag{3}$$

This formalizes the intuition that when workers become more expensive, firms' hiring becomes more selective (see Section A.3 below for the proofs). This does not necessarily imply that firms will invest in better screening technology: when I incorporate a choice of screening technology into the model, e.g., that the firm may improve the precision of the ability signal by reducing $\sigma_\epsilon^2(s)$, I am unable to sign the derivative of the optimal screening investment with respect to the wage. An even simpler model set-up with binary (high and low) worker ability and a noisy signal of the type confirms that the predicted effect of an exogenous wage increase on the investment in screening technology is ambiguous.

Taking firms' screening technology as given, the model yields the prediction tested in this paper that the introduction of a biting minimum wage will increase firms' hiring thresholds. One qualification to this is that in my empirical analyses I proxy the hiring threshold (the lowest ability signal the firm would tolerate) with my ability measure for the least productive new hire, which is not exactly the same. Note, however, that the expected ability of a new hire with $z = t$ is simply the wage and thus will also go up when a minimum wage is introduced.

A.2 Theoretical predictions from the literature

Pries and Rogerson (2005) introduce screening to the hiring process. They consider a matching model with employer learning that treats the quality of the worker-firm match as both an inspection and an experience good. In their framework, employers hire workers when the match productivity signal they receive is above a certain threshold. Studying the impact of various types of regulation in this set-up they predict that a minimum wage increase will raise firms' hiring thresholds. Brochu and Green (2013) also model screening in a matching model with endogenous separations. They, too, obtain the analytical prediction that, under some reasonable conditions, minimum wages will increase the hiring standard. Both Pries and Rogerson (2005) and Brochu and Green (2013) model screening as costless to the firm. Sengul (2017) relaxes the assumption that screening is costless and explicitly models firms' choice of screening. She also gets the analytical result that increases in minimum wages increase the threshold value of the match productivity signal. The mapping of these models' predictions to the hypothesis tested in this paper is imperfect in the sense that they all view workers as ex-ante homogeneous. That is, ex-post match productivity differs across worker-firm matches but ex-ante worker ability does not. It is reassuring that the set-up I consider above—which is highly simplified but uses heterogeneous workers—yields a similar prediction for the ability signal threshold.

A.3 Proofs of theoretical claims

Ad Equation (1):

$$\begin{aligned}
\mathrm{E}[\Pi|z] &= \mathrm{E}[a-w|z] \\
&= \mathrm{E}[a|z]-w \\
&= \mathrm{E}[a]+\frac{\mathrm{Cov}[a,z]}{\mathrm{Var}[z]}(z-\mathrm{E}[z])-w \\
&= \mathrm{E}[a]+\frac{\mathrm{Cov}[a,a+\epsilon]}{\mathrm{Var}[a+\epsilon]}(z-\mathrm{E}[a+\epsilon])-w \\
&= m_a+\frac{\sigma_a^2}{\sigma_a^2+\sigma_\epsilon^2}(z-m_a)-w.
\end{aligned}$$

Ad Equation (2):

$$\begin{aligned}
&\mathrm{E}[\Pi|z] \geq 0 \\
\Leftrightarrow\ & m_a+\frac{\sigma_a^2}{\sigma_a^2+\sigma_\epsilon^2}(z-m_a)-w \geq 0 \\
\Leftrightarrow\ & \frac{\sigma_\epsilon^2}{\sigma_a^2+\sigma_\epsilon^2}m_a+\frac{\sigma_a^2}{\sigma_a^2+\sigma_\epsilon^2}z-w \geq 0 \\
\Leftrightarrow\ & \frac{\sigma_a^2}{\sigma_a^2+\sigma_\epsilon^2}z \geq w-\frac{\sigma_\epsilon^2}{\sigma_a^2+\sigma_\epsilon^2}m_a \\
\Leftrightarrow\ & z \geq \frac{\sigma_a^2+\sigma_\epsilon^2}{\sigma_a^2}w-\frac{\sigma_\epsilon^2}{\sigma_a^2}m_a \\
\Leftrightarrow\ & z \geq \frac{1}{\sigma_a^2}[(\sigma_a^2+\sigma_\epsilon^2)w-\sigma_\epsilon^2 m_a].
\end{aligned}$$

Ad Equation (3):

$$\begin{aligned}
\frac{dt}{dw} &= \frac{d}{dw}\frac{1}{\sigma_a^2}[(\sigma_a^2+\sigma_\epsilon^2)w-\sigma_\epsilon^2 m_a] \\
&= \frac{\sigma_a^2+\sigma_\epsilon^2}{\sigma_a^2} > 0.
\end{aligned}$$

B Further data description

The data I use combine information from three sources centered on the Linked Personnel Panel (LPP), a matched employer-employee survey (IAB 2012, 2014, 2016). Roughly speaking I observe a fraction of the universe of the German private-sector workforce built around a representative sample of firms[1] that participate in the LPP survey.

1. The LPP employer survey (3 waves from 2012, 2014 and 2016) provides the starting point: those 1520 establishments that were interviewed in one of the three survey

[1]Strictly speaking, the data contain establishment rather than firm identifiers. As a consequence, several observed establishments may be part of the same company but I will not know.

waves form my sample of firms. These establishments are a stratified random sample of private-sector employers in Germany by size, region and broad industry category that at the time of sampling employed 50 or more workers liable for social security contributions (i.e., excluding marginally employed individuals or "mini jobbers"). The only information I use from the LPP employer survey is the variable for screening intensity.

2. The backbone of my analysis data is formed by individual-level labor market histories from the IAB's Integrated Employment Biographies (IEB). However, I only observe workers who at some point in the period from 2010 to 2016 were employed at one of the 1520 LPP establishments (IAB 2018). I rely on these social security records (rather than any survey data) as they provide accurate information on the timing of hiring and on wages and because they allow me to observe the full workforces of these firms. That is, unlike many other data sets (including from the IAB) the IEB extract I use is a true flow sample of everyone entering and exiting the 1520 establishments in the 7 years I observe.[2] However, the advantages of these data come at a cost: the data do not allow me to look at types of firms that are not included in the LPP - most importantly firms with fewer than 50 workers. As there was no standard data product from the IAB with the features my analyses require, I was fortunate to have access to these data through work on the LPP, despite the limitations the data have.[3]

3. The AKM worker effect estimates I use are the originals from Card, Heining and Kline (2013)(CHK). That is, I do not estimate AKM worker effects myself. CHK's estimates, based on the universe of the West German workforce, were made available to interested researchers (IAB 2018b). They can be matched with my workers at LPP establishments using a person identifier.[4]

C Supplementary material: validity of treatment classification

Estimation details I use the individual-level panel data set of full-time workers employed at my treated and control firms between 2010 and 2016.[5] I deflate wages using the CPI and approximate hourly wages by dividing working-day wages by 8.

[2] This ruled out using the IAB's LIAB, for example.

[3] FDZ now offers the information I combined from separate data sources (LPP survey data, IEB employee records) as an integrated standardized data asset called LPP-ADIAB (IAB 2019b); note, however, that these data may differ slightly from the ones I used. AKM person effects (IAB 2020) may be added to LPP-ADIAB upon request (Bellmann et al. 2020). The newly launched SIEED offers the same administrative information as LPP-ADIAB, but for a larger and even more representative sample of firms (IAB 2020b). It includes AKM person effects from IAB (2020b), but it contains no survey information.

[4] To my knowledge, CHK's estimated AKM worker and firm effects are no longer available for use by new projects. They have been superseded by Bellmann et al. (2020)'s updated estimates.

[5] Where there are several spells per individual I keep the highest-wage observation for each person-establishment-year cell.

Next I create 1-Euro wage bins relative to the EUR 8.50 minimum wage that range from -6 to 18. I collapse the data at the establishment*year*wage bin level to obtain the share of a firm's workers employed in each wage bin. (I create an observation with this share set to zero for "empty" establishment*year*wage bin cells in years where the firm did have workers, just not in that wage bin.)

Using the resulting yearly panel of establishment-wage bin observations I estimate the following fixed-effects DiD specification:

$$y_{wjt} = \alpha + \sum_{v=-6}^{18} \beta_v \mathbb{1}[v = w] * TR_j * POST_t + \gamma_{wt} + \delta_{wj} + \epsilon_{wjt}, \tag{4}$$

where y_{wjt} is the share of establishment j's workers in wagebin w in year t and TR_j and $POST_t$ are defined as in the rest of the paper. This closely follows Cengiz et al. (2019) to create an extension of my main DiD specification in Equation (3)(in the main text): now, in Equation (4), the before-vs-after DiD coefficient β_v is wage bin-specific and the fixed effects are wage bin-by-year, γ_{wt}, and wage bin-by-establishment, δ_{wj}. Standard errors are clustered at the establishment level as before. Figure 4 in the main text plots $\hat{\beta}_v$.

Descriptive evidence I next use the worker-level data to plot wage histograms for 2013 and 2015 separately for workers employed at treated and control establishments. While the wage data is noisy, Figure A1 confirms that around the SMW introduction the pooled wage distribution of workers at control establishments does not change much and that the pooled wage distribution of workers at treated establishment has mass shifted from just below the minimum wage to just above.

D Stable unit treatment value assumption

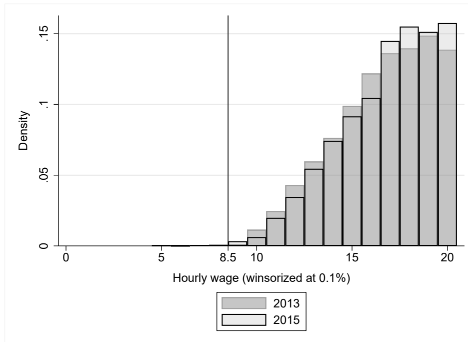
In addition to the parallel-trends assumption discussed in Section IIIA in the main text, identifying a causal effect in a DiD setting requires the stable unit treatment value assumption (SUTVA) to hold. SUTVA maintains that units do not interfere with each other. More specifically, SUTVA says that one unit receiving the treatment must not influence the outcome of another establishment (particularly a control group one). Here I provide some indicative correlation evidence for the validity of SUTVA in Section VA in the main text.

In my setting, SUTVA would be violated if, by hiring more selectively, establishments treated by the minimum wage "poached" productive workers that would else have been hired by establishments untreated by the minimum wage. Such a hire stealing effect may reduce minimum hire quality at control group establishments, causing me to overestimate the effect of the minimum wage on employers' worker selection.

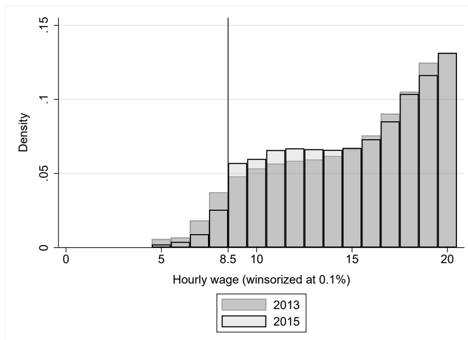
To address this threat I identify circumstances where hire stealing effects are more and less likely and test whether heterogeneity in the effect of the minimum wage is consistent with the SUTVA violation. That a treated establishment's recruitment directly affects untreated

Figure A1: Wage histograms for workers at control and treated establishments

(a)



(b)



Note: This figure shows histograms of wages in 2013 and 2015 separately for the pooled workers of control and treated establishments. The vertical line denotes the SMW.

establishments by depriving them of a good potential hire is more likely in tight labor markets where there are many vacancies for each unemployed worker, causing employers to compete for hires. If the hire stealing effect biased my findings, the effect of the minimum wage on worker selection should be less pronounced in slack labor markets—where one establishment's hiring has little effect on other establishments' choices from the pool of unemployed—than in tight labor markets.

Table A1 looks at effect heterogeneity by labor market tightness, considering variation in the vacancy-to-unemployment ratio across German federal states. Column (1) estimates Equation (3) (from the main text) for establishments in states where the average pre-reform vacancy-to-unemployment ratio is smaller than the median, i.e., for slacker labor markets. Column (2) does the same for establishments in tighter labor markets. The estimated minimum wage effect is bigger in slack labor markets. This is contrary to the prediction of the SUTVA violation considered. One may interpret this as a piece of suggestive evidence that the hire-stealing effect is unlikely to perceptibly bias my findings. It is important to remember, however, that this comparison is far from ceteris-paribus: German federal states with different vacancy-to-unemployment ratios are likely to vary in many unobserved dimensions that may also influence the effect of the minimum wage on firms' worker selection. Moreover, German federal states do not coincide with local or regional labor markets.

While I cannot present a conclusive empirical test to rule out the hire-stealing effect the latter appears theoretically unlikely. It is not plausible in practice that employers formerly paying below the minimum wage would suddenly, by paying the minimum wage, lure workers who would otherwise have ended up in higher-wage establishments in any systematic way.

Table A1: Effect on hire quality by state labor market tightness

	(1) Slack labor markets	(2) Tight labor markets
DiD estimate (Treated*Post)=1	0.1137	0.0604
	(0.0333)	(0.0303)
Mean (untreated establishments)	3.5085	3.4583
Observations	3,624	3,968
Establishments	764	731
Adjusted R^2	0.0109	0.0210

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. The dependent variable is minimum hire quality, with CHK's individual AKM worker effects estimated 2002-2009 used as a hire quality measure. (1) reports the estimated effect of the minimum wage introduction on establishments in federal states (*Länder*) with below-median average pre-reform labor market tightness (V/U ratio); (2) does the same for establishments in federal states with a vacancy-unemployment ratio above the median. DiD estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

E Alternative treatment classifications

Column (2) of Table A2 assigns firms to the treated group based on the share of pre-reform sub-SMW *hires* rather than workers. Column (3) considers firms treated if, before the reform, they employed workers earning less than the 5th percentile of the year-specific pooled full-time worker wage distribution. Point estimates are also positive and significant but smaller in the alternative treatment classifications. This is consistent with attenuation bias in the alternative classifications: in (2) some firms that did in fact employ sub-minimum wage workers may have been assigned to the control group—for instance, those that did not hire sub-SMW workers pre-reform but would have hired such workers post-reform. Conversely, in (3) some firms assigned to the treatment group likely did not employ sub-SMW workers, namely those that pre-reform employed full-time workers earning more than the SMW but less than the 5th percentile of the full-time wage distribution.

There is supplementary information on individual hours for a sub-set of my data. These data come from the German accident insurance. The hours data have hardly been used for scientific purposes because of a number of reasons: (1) they are available only for the years 2010-2014; (2) they contain a substantial share of missing values; (3) they contain either contractual, actual or estimated working hours, but it is unknown which of the three. When investigating questions at the individual level, the third point can be addressed by apply-

Table A2: Minimum wage effect on minimum hire quality, alternative treatment classifications

	Minimum hire quality		
	(1)	(2)	(3)
	Sub-SMW worker share	Sub-SMW hire share	Low-wage worker share
DiD estimate (Treated*Post)=1	0.0861	0.0432	0.0526
	(0.0227)	(0.0216)	(0.0287)
Mean (untreated establishments)	3.4819	3.5014	3.5330
Observations	7,592	7,592	7,592
Establishments	1,491	1,491	1,491
Control establishments	344	419	208
Adjusted R^2	0.0150	0.0133	0.0131

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is minimum hire quality, measured by CHK's individual AKM worker effects estimated 2002-2009. The columns vary in the measure for establishment minimum wage exposure they use: (1) uses firms' pre-reform share of *workers* earning below the future minimum wage; (2) uses the pre-reform share of below-SMW *hires*; and (3) uses the pre-reform share of low-wage workers, i.e., earning below P5 of the overall wage distribution for that year. DiD estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

ing establishment fixed effects and assuming that reporting differences within employers are negligible, making within-establishment comparisons valid. In my setting, however, it is precisely the differences (in hourly wages) across establishments that I am interested in. Relying on hours data that report different things across establishments will thus almost certainly introduce additional noise, which is why I view it as a robustness check. Column (3) of Table A3 shows that my main results are indeed attenuated, but remain qualitatively similar, when I use the supplementary hours data to compute hourly wages for the construction of an SMW exposure measure.

Table A3: Minimum wage effect on hire quality: MW bite measures

	(1)	(2)	(3)	(4)
D_treat_post=1	0.0861		0.0368	0.0475
	(0.0227)		(0.0193)	(0.0190)
MW_post		0.1259		
		(0.0541)		
Mean (untreated establishments)	3.4819	3.4819	3.4819	3.4819
Observations	7,592	7,592	7,522	7,490
Establishments	1,491	1,491	1,477	1,444
Adjusted R^2	0.0150	0.0134	0.0133	0.0140

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is minimum hire quality, where hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. In (1) and (2) minimum wage exposure (MW) is computed based on full-time workers' wages. In (3) MW is calculated using additional data on hours worked. In (4) MW is measured only on 1 July 2013 (rather than averaged over the pre-reform period). DiD estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

F Selective establishment attrition

There is evidence the SMW introduction increased firm exit (Dustmann et al. 2020). Unfortunately, estimating the minimum wage effect on firm exit in my sample is not feasible as there are too few closure events.[6] Table A4 reports the number of establishment exits by year separately for the treated and control groups.

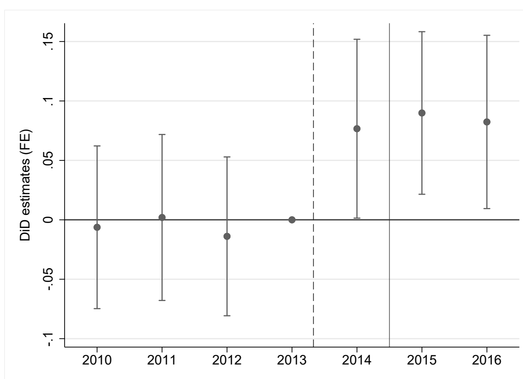
Table A4: Firm exit by treatment status

Year	Control	Treated
2010	5	1
2011	5	7
2012	10	10
2013	4	17
2014	4	23
2015	5	17
2016	4	16
Total	37	91
Establishments	354	1155

Note: This table gives the number of exits from the market separately for treatment and control establishments and by year. An establishment is assumed to have exited when there are no more employment spells bearing its establishment identifier.

Is it possible that the SMW-induced increase in minimum hire quality is due to treated firms with the lowest hiring standards going out of business? The primary reason this should not be the case is that Equations (3) and (4) in the main text are fixed-effects specifications - that is, they use the within-establishment variation over time to identify the SMW's effect. To verify that this is true I exclude establishments that exit the market during the observation period and estimate the SMW effect on minimum hire quality on the resulting sample. As Figure A2 shows, this leaves the results virtually unchanged.

Figure A2: Minimum wage effect on minimum hire quality (surviving establishments)



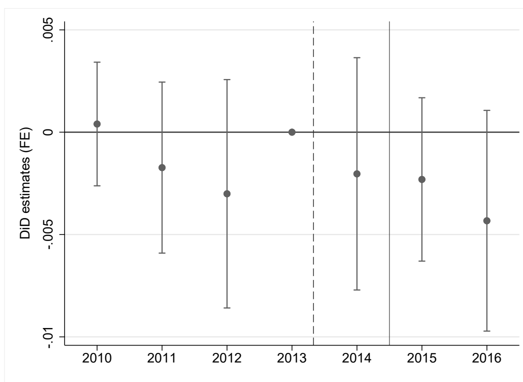
Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on minimum hire quality. Estimates are from an establishment fixed-effects specification without covariates; the estimation sample excludes firms that close down at some point during the analysis period. Hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

[6] In my data establishment exit is not directly observed. However, I can identify establishment closures by assuming that a firm has exited the market when I no longer observe employment spells under its establishment ID.

G More on supply vs demand

Here I present a test for a change in the direction of worker search. Ideally, one would have data on applicants rather than hires.[7] Lacking such information I consider a subset of hires for whom it is likely that the workers self-selected into their new establishments—voluntary job changers—and check whether the share of these coming from high-wage establishments goes up.[8] I then use the share of voluntary transitions from a high-wage establishment among new hires as a proxy for establishment attractiveness in the regression framework from Equation (3) in the main text. As Figure A3 shows, the introduction of the minimum wage does not make treated establishments significantly more attractive by this metric. This is a further indication that worker self-selection does not drive the increase in minimum hire quality.

Figure A3: Minimum wage effect on establishment attractiveness



Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on the share of hires coming directly from high-wage establishments. Estimates are from an establishment fixed-effects specification without covariates other than year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

H Are AKM worker effects better productivity proxies than observables?

To measure the amount of variation in wages explained by the different ability proxies I first pool the individual-level employment spells of full-time workers at my treated and control establishments for the pre-reform period (2010-2013).[9] I then residualize log hourly wages using a dummy for East Germany, continuous establishment size, 14 occupational groups (Matthes, Meinken and Neuhauser 2015) and 21 industry sections (NACE Rev.2 (2008)

[7] It is tempting to use the LPP to look at the number of applications. Unfortunately, this information is available only for skilled workers, who earn above the minimum wage.

[8] To empirically identify voluntary job changes I use employment-to-employment (E-E) transitions with an employment gap of less than thirty days. I define those as coming from a high-wage employer whose previous employer's estimated AKM establishment effect was in ventile 11 or higher.

[9] As elsewhere in the paper I approximate hourly wages by dividing full-time daily wages by 8.

Level 1). Next I separately regress log residualized wages on each of the pre-determined ability proxies and use the adjusted R-squared to compare how much of the variation in residualized wages they explain overall, in the bottom half and the bottom quarter of the wage distribution. Table A5 summarizes this comparison. It shows that the share of the variance in residual wages explained by AKM worker effects is bigger than that explained by observables. This is true not just overall (Columnn (1)) but also when only low-wage workers are considered (Columns (2) and (3)). In particular, the explanatory power of AKM worker effects is around tenfold that of age and education and this difference is most pronounced among low-wage workers.

Table A5: Relative explanatory power of ability proxies for residual wages

	(1) All workers	(2) Bottom half	(3) Bottom quarter
AKM worker effect	0.2770	0.0542	0.0282
Age (years)	0.0261	0.0077	0.0010
Low education	0.0210	0.0044	0.0007
Experience (days)	0.0275	0.0444	0.0235
Unemployment experience (days)	0.0467	0.0380	0.0169
Mean log residual wage	0.0211	-0.1885	-0.3337
Observations	1,230,426	583,175	258,244

Note: This table reports the adjusted R-squared from separate regressions of residualized wages on one of the following ability proxies: AKM worker effects, age, low education, experience in days, cumulative unemployment experience in day. The columns each refer to five regressions on different sub-sets of the pooled workforces of treated and control firms for 2010-2013: (1) is for all workers, (2) is for workers in the bottom half of the wage distribution and (3) is for workers in the bottom quarter of the wage distribution.

Finally, I test whether in the pre-reform period the association between AKM worker effects and observables is lower among sub-minimum wage new hires. This would support the interpretation that I do not find an effect of the minimum wage on hiring standards measured by observables because these characteristics, unlike AKM worker effects, do not pick up relevant productivity differences between low-wage workers. Intuitively, this appears plausible if, for example, educational qualifications are largely irrelevant for productivity in a cleaning job. I estimate the partial correlations between estimated AKM worker effects and observed characteristics using those workers who were hired by my treated and control firms in the pre-reform period (2010-13). Table A6 shows that the association between AKM worker effects and observables is generally smaller for new hires earning below the future minimum wage than for those earning above, with virtually no difference for experience.[10] The share of the variance in AKM worker effects explained by low-skill and entry from unemployment is also lower among sub-SMW workers.

[10]The negative sign of the age coefficient for both groups highlights that age may be problematic as an ability proxy - conditional on the other observables it is not even positively correlated with estimated productivity in my sample.

Table A6: Relationship between AKM worker effects and observed worker ability

	(1) Wage < SMW	(2) Wage ≥ SMW
	(1)	(2)
Age in years	-0.00611	-0.00090
	(0.00031)	(0.00023)
Less than Abitur or equiv, no voc quali	-0.05963	-0.21748
	(0.00479)	(0.00368)
Overall cumulative work experience in days	0.00003	0.00003
	(0.00000)	(0.00000)
Establishment entry from UE dummy	-0.00836	-0.09063
	(0.00461)	(0.00291)
AKM person effect SD	.29121	.37025
Observations	16031	79104
Adjusted R^2	0.05551	0.08188

Note: *** p<0.01, ** p<0.05, * p<0.1, with heteroskedasticity-robust standard errors. Coefficients are from a cross-sectional OLS regression of AKM worker effects on observed worker characteristics. The table compares two groups of new hires in the pre-reform period (2010-13): Column (1) reports results for the sub-sample of workers with starting wages below the minimum wage and (2) is for workers with starting wages at or above the minimum wage.

I Individual-level data and analyses

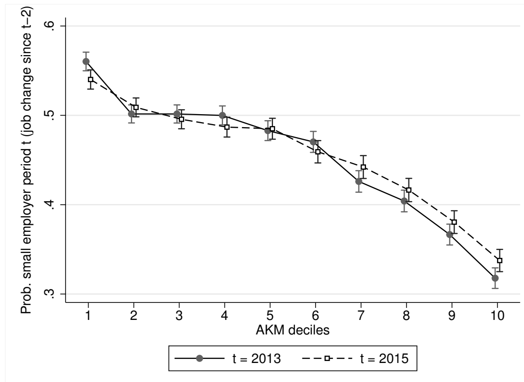
Section VE in the main text studies the effect of the SMW introduction on low-productivity workers' employment prospects. The data used in the main analyses of this paper are unsuitable for this task: they are representative at the level of the firm, not the worker. Section VE therefore uses the IAB's SIAB, which is representative of both unemployed and employed workers in Germany (IAB 2019). SIAB contains a 2% random sample of everyone ever registered as employed or unemployed in Germany between 1991 and 2017 or in West Germany between 1975 and 1991 (Antoni et al. 2019).

As elsewhere in the paper I use estimated AKM worker effects as an individual productivity measure. IAB was unable to provide the original AKM worker effect estimates for 2002-09 from CHK that the main part of this paper uses. Instead I was given the current standard data product (IAB 2020). The latter AKM worker effect estimates are an extension of CHK that includes East Germany, uses slightly modified estimation intervals (2003-2010 for the worker effects I use) and estimates men and women jointly (Bellmann et al. 2020). (The establishment-level results from the main part of the paper are robust to using a preliminary version of these updated AKM worker effect estimates, see Figure A16.)

I use SIAB data for 2010-2016. There are 938,286 individuals. In each year I keep a maximum of one employment spell and one unemployment spell per person. The share of unemployment spells is around 9.5 percent; among employment spells, around 67.60% are full-time spells, 18.15% are part-time spells (excluding mini jobs) and 14.25% are mini-job spells. Around 5.7% of spells are exit spells (i.e., are followed by at least two months of absence from observed employment or unemployment). For 65.3% of spells an AKM worker effect is available.

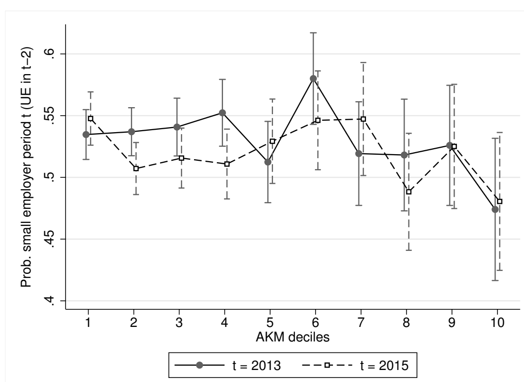
Figures A4-A6 below, also referred to in the main text, use SIAB data.

Figure A4: Share of job changers employed in t-2 who enter a small firm in t, by AKM decile



Note: This figure shows AKM decile specific estimates of the share of job changers (employed in t-2) whose employer in t is smaller than 50 workers. (50 is the lower establishment size restriction of the establishment-level data in this paper). Vertical bars denote 95% confidence intervals (with heteroskedasticity robust standard errors). Source: SIAB

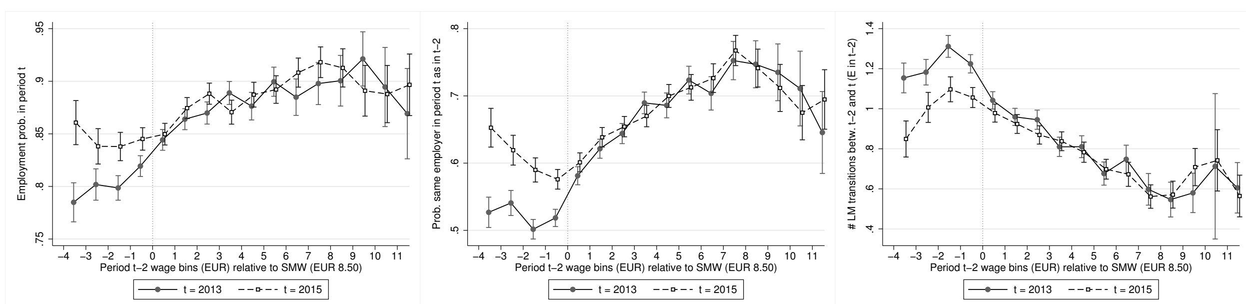
Figure A5: Share of job finders unemployed in t-2 who enter a small firm in t, by AKM decile



Note: This figure shows AKM decile specific estimates of the share of job finders (unemployed in t-2) whose employer in t is smaller than 50 workers. (50 is the lower establishment size restriction of the establishment-level data in this paper). Vertical bars denote 95% confidence intervals (with heteroskedasticity robust standard errors). Source: SIAB

Figure A6: Minimum wage effects on employment and job stability for low-AKM workers

(a) **(b)** **(c)**



Note: This figure shows wage-bin specific estimates for workers in the bottom two AKM worker effect deciles of (a) the employment probability in t conditional on t-2 employment; (b) the probability of being with the same employer in t as in t-2, conditional on t-2 employment; (c) the number of labor market transitions between t-2 and t conditional on t-2 employment. Vertical bars denote 95% confidence intervals (with heteroskedasticity robust standard errors). Source: SIAB

J Additional tables and figures

Table A7: Minimum wage effect on starting wages and hire quality

	(1) Starting wages	(2) Hire quality
DiD estimate (Treated*Post)=1	6.5555	0.0861
	(1.2219)	(0.0227)
Mean (untreated establishments)	72.2839	3.4819
Observations	8,288	7,592
Establishments	1,491	1,491
Adjusted R^2	0.0234	0.0150

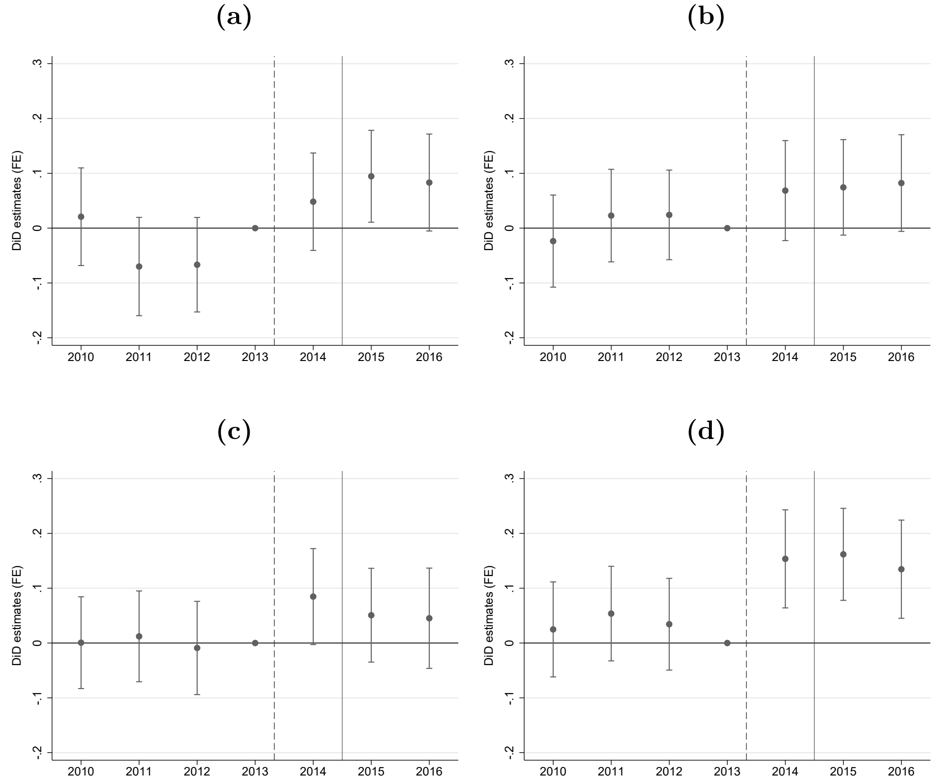
Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is minimum starting wage (1) and minimum hire quality (2), where hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. Estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

Table A8: Effect of SMW bite on minimum hire quality, treated firms only

	Minimum hire quality		
	(1) Sub-SMW worker share	(2) Sub-SMW hire share	(3) Low-wage (<P5) worker share
Continuous DiD estimate (Bite*Post)	0.0822	0.1455	0.1047
	(0.0555)	(0.0597)	(0.0375)
Mean (untreated establishments)	3.4819	3.4819	3.4819
Observations	5,996	5,649	6,671
Establishments	1,147	1,072	1,283
Adjusted R^2	0.0169	0.0148	0.0156

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is minimum hire quality, measured by CHK's individual AKM worker effects estimated 2002-2009. All specifications use only treated firms. They vary in their measure of establishment minimum wage exposure: (1) uses firms' pre-reform share of *workers* earning below the future minimum wage; (2) uses the pre-reform share of below-SMW *hires*; and (3) uses the pre-reform share of low-wage workers, i.e., earning below P5 of the overall wage distribution for that year. DiD estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

Figure A7: Minimum wage effect by pre-reform SMW bite: event study



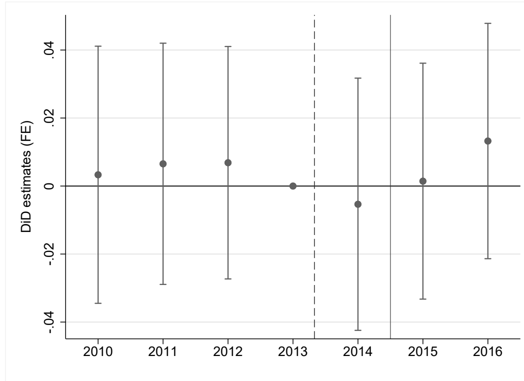
Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on minimum hire quality by minimum wage bite quartile. Estimation is separate for each quartile and always uses the whole control group: (1) compares the quarter of treated firms with the lowest bite to the control group and (4) compares the quarter with the highest bite to the control group. Estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects. Hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. SMW bite is the average pre-reform share of sub-minimum wage workers. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Table A9: Minimum wage effect on average hire quality

	Average hire quality	
	(1)	(2)
	All hires	Low-education hires
DiD estimate (Treated*Post)=1	0.0240	0.0654
	(0.0132)	(0.0361)
Mean (untreated establishments)	3.8128	3.5757
Observations	7,592	2,084
Establishments	1,491	791
Adjusted R^2	0.0020	0.0046

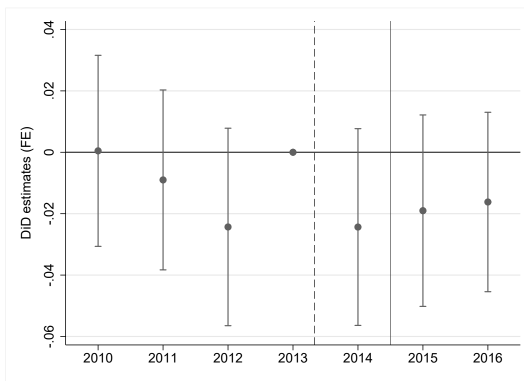
Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is average hire quality for all new hires (1); and average hire quality for new hires with less than Abitur and no vocational education (2). Hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. Estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

Figure A8: Minimum wage effect on new hires' AKM coverage



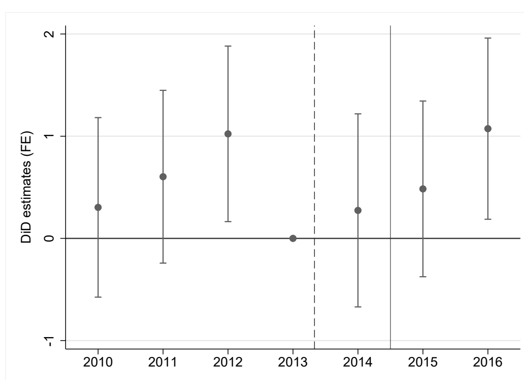
Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on the share of new hires for whom an AKM worker effect estimate from CHK is available. Estimates are from an establishment fixed-effects specification without covariates other than year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Figure A9: Minimum wage effect on women's hire share



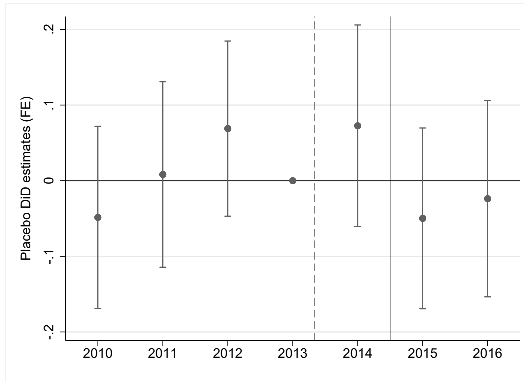
Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on the female share of new hires. Estimates are from an establishment fixed-effects specification without covariates other than year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Figure A10: Minimum wage effect on new hires' average age



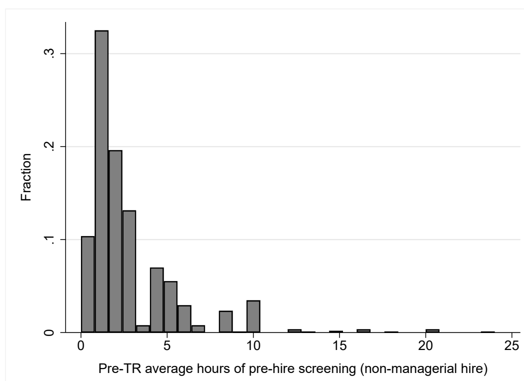
Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on new hires' mean age. Estimates are from an establishment fixed-effects specification without covariates other than year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Figure A11: Placebo effect on minimum hire quality: event study



Note: This figure shows yearly DiD estimates for the effect of a placebo treatment on minimum hire quality. Estimates are from an establishment fixed-effects specification without covariates other than year dummies. Establishments affected by the actual treatment (minimum wage) are excluded. The placebo treatment is assigned to those untreated establishments with below-median average wages. Hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Figure A12: 2012 screening intensity distribution



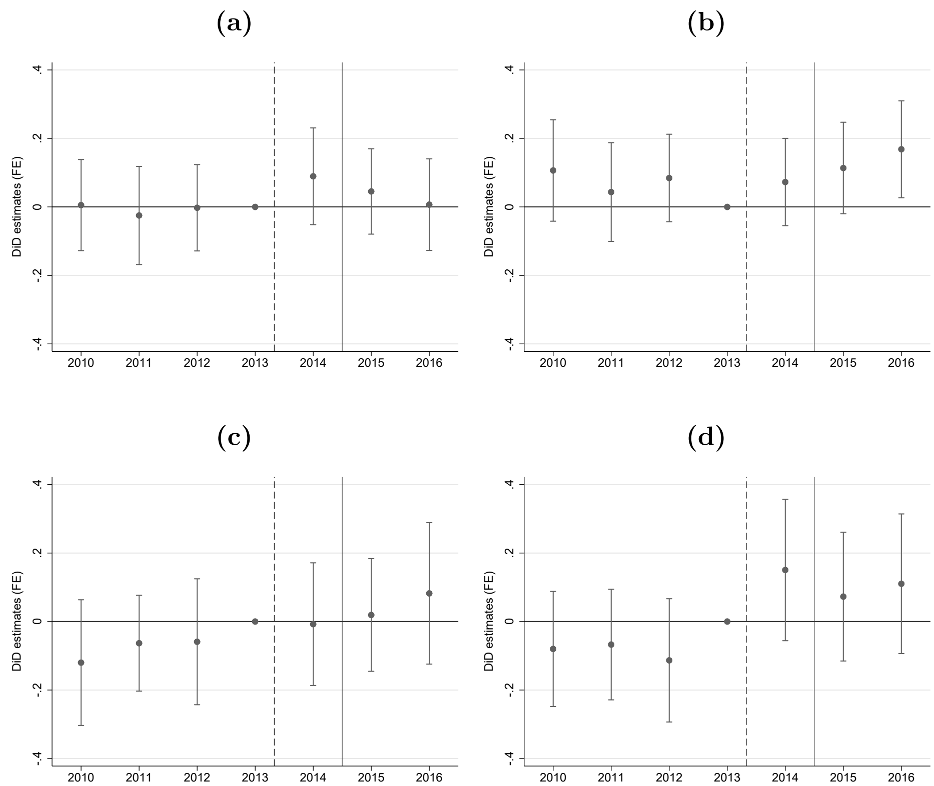
Note: This figure shows a histogram for establishment screening intensity (in hours) for an average successful applicant for a non-managerial job. Screening intensity is self-reported in a 2012 employer survey (LPP 2012).

Table A10: Minimum wage effect on hire quality by pre-reform screening intensity

	(1) Q1	(2) Q2	(3) Q3	(4) Q4
DiD estimate (Treated*Post)=1	0.0257	0.0729	0.1222	0.1538
	(0.0429)	(0.0466)	(0.0586)	(0.0579)
Mean (untreated establishments)	3.5027	3.4686	3.4720	3.5505
Observations	2,099	1,510	796	1,437
Establishments	426	297	152	282
Adjusted R^2	0.0112	0.0167	0.0246	0.0159

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is minimum hire quality. Samples in (1)-(4) are the 4 quartiles of the pre-reform screening intensity distribution. Test of differences between quartile-specific DiD coefficients: (1)-(2): p = 0.456; (1)-(3): p = 0.183; (1)-(4): p = 0.075. Screening intensity is self-reported in a 2012 employer survey (LPP 2012). Hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. DiD estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

Figure A13: Minimum wage effect by pre-reform screening intensity: event study



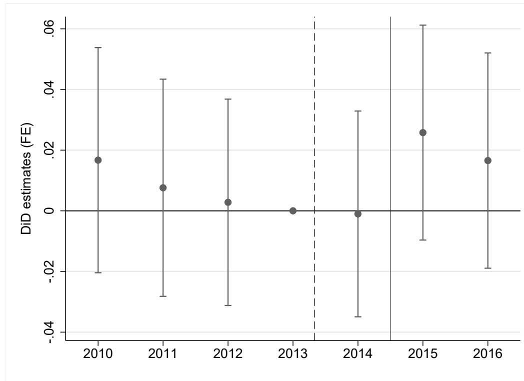
Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on minimum hire quality separately for each quartile of establishments' pre-reform screening intensity. (1) is the quarter of firms with the lowest 2012 screening intensity and (4) is the quarter with the highest. Estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects. Hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. Screening intensity is self-reported in a 2012 employer survey (LPP 2012). Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Table A11: Minimum wage effect on employer-reported screening intensity

	(1)
DiD estimate (Treated*Post)=1	0.2286
	(0.3535)
Mean (untreated establishments)	2.9294
Observations	1,885
Establishments	1,420
Adjusted R^2	0.0136

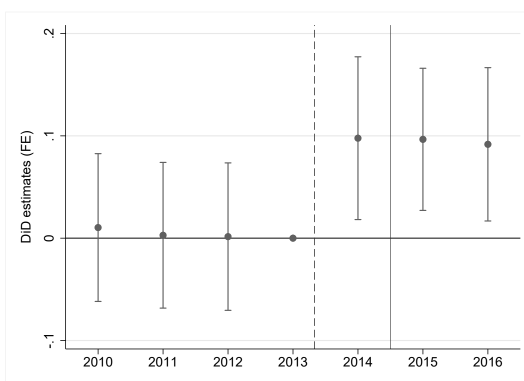
Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is employer-reported screening intensity for positions without management responsibilies: "On average, how many hours do you spend on screening a successful candidate in interviews, tests, etc? This refers to the total time an applicant is present in your selection process on average." Estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects. Source: LPP employer survey 2012, 2014, 2016

Figure A14: Minimum wage effect on average quality of leavers: event study



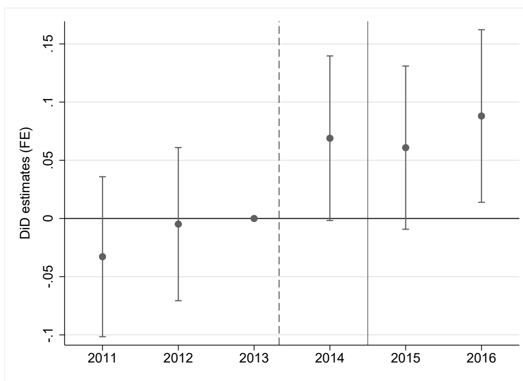
Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on mean leaver quality, i.e., of workers separating from the firm (for whatever reason). Estimates are from an establishment fixed-effects specification without covariates other than year dummies. Leaver quality is measured by CHK's individual AKM worker effects estimated 2002-2009. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Figure A15: Minimum wage effect on minimum hire quality (log employment weighting)



Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on minimum hire quality. Estimates are from an establishment fixed-effects specification without covariates other than year dummies and with log(employment) weights. Hire quality is measured by CHK's individual AKM worker effects estimated 2002-2009. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Figure A16: Minimum wage effect on minimum hire quality (preliminary CHK update)



Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on minimum hire quality. Estimates are from an establishment fixed-effects specification without covariates other than year dummies. Hire quality is measured by a preliminary update of CHK's individual AKM worker effects. These are estimated 2003-2010 (hence the shortened period of analysis) and include workers from both West and East Germany. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

Table A12: Mean AKM and wage by AKM percentiles

AKM Percentile	Average AKM	Average wage
1	2.49	117.82
2	2.89	97.67
3	3.04	84.81
4	3.13	84.44
5	3.19	81.72
6	3.24	81.00
7	3.27	81.27
8	3.30	82.38
9	3.32	84.21
10	3.34	84.50
11	3.36	87.78
12	3.37	88.50
13	3.39	87.00
14	3.40	89.65
15	3.41	89.01
16	3.43	88.28
17	3.44	93.20
18	3.45	94.25
19	3.46	96.08
20	3.47	95.97
21	3.48	99.72
22	3.48	95.50
23	3.49	97.43
24	3.50	98.02
25	3.51	100.78
26	3.52	99.08
27	3.53	100.52
28	3.53	107.47
29	3.54	103.56
30	3.55	104.98
31	3.56	102.97
32	3.56	105.46
33	3.57	108.15
34	3.58	109.50
35	3.59	107.84
36	3.59	110.83
37	3.60	111.64
38	3.61	113.77
39	3.61	111.51
40	3.62	117.67
41	3.63	119.67
42	3.63	116.16
43	3.64	121.13
44	3.65	115.39
45	3.65	122.25
46	3.66	118.06
47	3.67	124.42
48	3.68	123.76
49	3.68	123.04
50	3.69	124.53
Observations	95135	95135

*Note:*This table reports mean AKM worker effect and mean wage for the first 50 percentile of the pre-reform productivity distribution as measured by estimated AKM worker effects.

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