Raising the bar: minimum wages and employers' hiring standards

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Abstract

Many scholars have studied the employment effects of minimum wages, but little is known about effects on the composition of hires. I investigate whether Germany's minimum wage introduction raised hiring standards, using worker fixed effects as a proxy for worker productivity. For the least productive workers hired, the minimum wage led to a 4-percentile-point shift in the productivity distribution. This increase is missed using standard observable measures of worker productivity. The effects are larger with greater pre-reform screening intensity—indicating an employer response. This more selective hiring compensates about two-thirds of higher wage costs for the least productive hires.

Keywords: worker selection, hiring standard, screening, minimum wage, labor-labor substitution, unobserved ability, job stability.
JEL Codes: J23, J24, J31, J38, M51.

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The labor market effects of minimum wages have been studied for decades. Scores of papers on their employment effects notwithstanding, very little is known about how minimum wages affect the composition of hires. Yet a link is plausible: a biting minimum wage will exogenously make low-wage workers more expensive. For employers of sub-minimum wage workers, this may raise hiring standards as firms require greater productivity of applicants to ensure that low-wage jobs remain profitable. Despite the scarcity of evidence, it is important to understand if worker selection is a channel of employer adjustment to minimum wages. One reason is that adjustment along the quality margin may help explain muted employment effects of minimum wages: instead of hiring fewer workers to avoid additional wage costs, firms may accept and compensate cost increases by hiring more productive workers. It also matters from the perspective of worker welfare: firms' more selective hiring in response to a minimum wage may have adverse distributional consequences if individuals with very low productivity increasingly get stuck in unemployment.

In this paper I provide some of the first causal evidence on the effect of a minimum wage on employers' hiring standards by studying the 2015 introduction of a statutory minimum wage (SMW) in Germany. The new EUR 8.50 wage floor applied to all of Germany and all prime-age workers. To be able to compare establishments treated by the SMW introduction to untreated ones I exploit variation in employers' pre-reform wage structure. Using a difference-in-differences (DiD) framework, I estimate the effect of the reform on minimum hire quality, i.e., the ability of the least productive hire, which serves as my proxy for employers' hiring standards. My results suggest that the hiring of German establishments became more selective in response to the SMW introduction: the reform increased minimum hire quality by 0.086. This corresponds to a shift of treated firms' hiring standards by 4 percentiles in the worker productivity distribution and implies an hourly wage premium of 81 Euro cents or 9.6% of the new SMW. The effect is more pronounced among firms with higher self-reported screening intensity before the reform, strengthening the interpretation that it reflects a change in employers' selectiveness.

Complementing the main result are three supplementary findings. The first is that the change in hiring standards is not visible when I use age, education, experience or previous unemployment as a proxy for the productivity of new hires. The second relates to welfare effects on the firm side: using newly hired workers' past wages I provide suggestive evidence that, at the margin, firms recouped around two thirds of the increased wage costs by hiring

more productive workers. The third is that raised hiring standards seem to have had no negative distributional consequences for incumbent workers as those with very low ability became more likely to stay in their jobs.

My main analysis is at the firm level and uses administrative data for 2010-2016 on the entire workforces of 1,491 incumbent establishments. This sample is representative of medium-sized and large firms in the German private sector, i.e., it excludes establishments with fewer than 50 workers.[1] My measure for firms' exposure to the minimum wage introduction classifies those employers as treated that employed sub-minimum wage workers in 2010-2013, before the SMW introduction could have been anticipated. Though my main results also hold with continuous SMW exposure, my preferred measure is binary as this makes it more likely that measurement error in SMW exposure biases my results towards zero.

As a proxy for the productivity of new hires I utilize worker effects from a two-way fixed-effects log-wage regression as introduced by Abowd, Kramarz and Margolis (1999)(henceforth AKM). I use the AKM worker effects estimated for West Germany by Card, Heining and Kline (2013) for the period 2002-2009, ensuring that ability proxies are pre-determined for any new hires considered in my analysis. AKM worker effects capture those components of time-invariant individual productivity that are reflected in workers' wage earnings. This includes education or vocational qualifications but extends to unobserved cognitive and non-cognitive ability (Butschek and Sauermann 2019b). Using AKM worker effects I find a clear change in hire quality in response to the minimum wage introduction which I do not detect when using observed characteristics. I argue that this is because AKM worker effects provide higher resolution than observables: they are a better predictor of future residual wages than, e.g., age and education, particularly among low-wage workers.

Section IV begins by validating my measure of SMW exposure: the 2015 SMW introduction increased bottom starting wages and shifted employment in treated establishments from wage bins just below the minimum wage to wage bins just above it. I then show that the SMW introduction increased minimum hire quality, implying a shift of hiring standards from the 7th to the 11th percentile of AKM worker effects. The effect on minimum hire quality is robust to using alternative treatment classifications and more pronounced in firms most exposed to the minimum wage. That the quality of hires improved is not only true for the

[1]This restriction is a data limitation. As a consequence, my results do not apply to small firms. However, as I verify in Section V.E, my results are not driven by the size restriction.

minimum (firms' least productive hire) but also at the bottom of the hire ability distribution more generally, as well as for average hire quality among workers likely to be affected by the minimum wage.

There are some threats to identification, which I address in Section V.A. Most importantly, if the reform reduced the share of new hires whose ability I can measure this may spuriously produce the pattern I attribute to a shift of hiring standards. A similar argument applies to groups of workers whose ability may be systematically underestimated by AKM worker effects, such as women and young workers. I show that the reform did not significantly affect the share of hires for whom I have an AKM worker effect, nor the share of women or the age of hires.

To strengthen my interpretation that the SMW effect on minimum hire quality reflects a raising of employers' hiring standards I confront two alternatives in Section V.B. First, the increase in minimum hire quality may be a mechanical consequence of a drop in establishments' hiring rate: minimum hire quality may have increased only because each establishment's hire quality distribution became less dispersed. However, the SMW introduction did not reduce the hiring rate in my sample. Second, rather than reflecting an adjustment of qualitative labor demand, the increase in minimum hire quality may be driven by a change in labor supply. One such possibility is that due to the minimum wage, some of the least productive workers no longer applied to treated firms as they dropped out of the labor market altogether. Another labor supply mechanism would be that the direction of search changed, e.g., that better workers sorted into treated establishments. I provide evidence against both explanations. In addition, I find direct indications of a change in labor demand: the SMW effect on minimum hire quality is more pronounced for establishments where, according to a pre-reform survey, pre-hire screening played a bigger role.

In Section V.D, I attempt to shed some light on potential welfare implications for firms by relating the change in hiring standards to wages. To put a Euro figure on potential productivity gains, I replicate my main analysis of the SMW's effect using not AKM worker effects but raw past wages. The results suggest that the increase in worker quality compensated around two thirds of the increase in wage costs. As an analysis of firm welfare this has limitations: I can neither address screening costs nor capture negative welfare effects on firms forced out

of the market by the minimum wage.[2] I therefore view these results as indicative evidence that changing the selectiveness of hiring represents a channel of adjustment to the minimum wage that dampened negative effects on firms' profitability.

Another potential implication of my results is that the SMW made it more difficult for the least productive job-seekers to find employment. To track their employment prospects I analyze separate representative worker-level data in Section V.E. I estimate the post-reform employment probability of workers employed pre-reform for different parts of the ability distribution. Comparing this to analogous estimates for a placebo period suggests that the employment probability of workers with very low productivity slightly increased around the SMW introduction. Further analyses confirm the picture that low-productivity individuals' employment prospects did not deteriorate—neither for those previously unemployed nor in the form of increased exit from the labor force. Does this contradict increased hiring standards? Not if employment became more stable. Indeed I find that workers with the lowest productivity became relatively more likely to stay with their old employer compared to the placebo period. Similarly, the number of employment transitions went down for low-AKM individuals. It is plausible that as the minimum wage increased the value of employment, low-ability workers held on to their jobs more tightly; and that collectively this allowed them to compensate for fewer of them getting hired at treated firms. While these results suggest that the SMW introduction did not harm low-productivity workers already in the labor market, I cannot rule out that it reduced the employment prospects of new labor market entrants, for whom I do not observe an AKM worker effect.

This paper provides some of the first causal evidence on the effect of a minimum wage on hiring standards. I am aware of three closely related papers. First, Hirsch, Kaufman and Zelenska (2015) find that a US federal minimum wage hike increased restaurant managers' intentions of hiring older and more experienced workers. Looking at the ability of realized hires makes my analysis more informative of effective changes in hiring standards than using intentions stated in a survey. Second, Clemens, Kahn and Meer (2018) use data from job postings to show that employers' hiring criteria became stricter in response to US federal minimum wage increases. I reach a similar conclusion while employing a different empirical strategy. In addition, I shed light on the effectiveness of the adjustment strategy for firms

[2]Dustmann et al. (2020) find that the SMW introduction increased firm exit; while my sample is too small to study the effect on exit directly I show in the appendix that excluding exiting firms from the sample does not change my main results.

and explore potential adverse consequences for workers. Third, Horton (2018) shows that a negative minimum wage effect on hours worked is in part explained by firms' hiring of more productive workers. Their contrasting methods and settings make our papers highly complementary: we get consistent evidence of more selective hiring even though Horton (2018) experimentally studies the effect in an online market for task-based employment and I investigate it using observational data from a sample of firms representative of a brick-and-mortar labor market. While less productive workers in Horton (2018) remain adversely affected by 'upskilling' in the online labor market, they preserve their employment prospects in my setting by holding on to their jobs more, though I cannot test this for labor market entrants.

The paper also contributes to a literature on labor-labor substitution as a channel of adjustment to minimum wages. It offers an explanation why only some papers find evidence in favor: observed characteristics, which nearly all the papers use to distinguish between skill levels, are poor proxies of individual productivity. Only a minority of papers report findings consistent with substitution toward higher-skilled workers as proxied by age or education (Neumark and Wascher 1995, Fairris and Bujanda 2008, Bernini and Riley 2016, Clemens, Kahn and Meer 2018). More papers, though using similar skill proxies, find no evidence of substitution toward higher-skilled workers along observed dimensions (Portugal and Cardoso 2006, Fairris and Bujanda 2008, Giuliano 2013, Hirsch, Kaufman and Zelenska 2015, Cengiz et al. 2019). In line with the latter, I do not detect labor-labor substitution when using low education, previous unemployment, age or experience as ability measures.

By showing that screening plays a role, my analysis lends support to theoretical macro work highlighting the role of firm search for the formation of matches in the labor market (Villena-Roldán 2012, Wolthoff 2017). Moreover, the hypothesis I test in this paper is closely related to the predictions for minimum wage effects on hiring standards from matching models (Pries and Rogerson 2005, Brochu and Green 2013, Sengul 2017).

Finally, this paper broadens our understanding of how the introduction of a statutory minimum wage affected the German labor market. The paper's insights on hiring standards and job stability complement analyses of employment effects (Caliendo et al. 2018, Garloff 2019, Bossler and Gerner 2020, Dustmann et al. 2020). My results also speak to Bossler et al. (2020)'s study, which finds no SMW effects on firms' human capital investment through

apprenticeships or further training. This paper suggests that instead firms have sought to improve the productivity of their workforces through more selective hiring.

I Theoretical framework

In this section I sketch a toy model formalizing the intuition that an exogenous increase in wages may make hiring more selective.[3] Section A in the appendix discusses this further and characterizes the set-up more formally.

Suppose a risk-neutral firm uses only labor to produce a good, which it sells at price $p = 1$. Let the firm's output depend solely on 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.

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

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

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

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

When a minimum wage makes workers more expensive, firms' hiring standards rise. Importantly, this does not necessarily imply that firms will invest in better screening technology.

[3]The same intuition suggests that minimum wages may make firing more selective, by raising affected firms' incentive to get rid of the least productive workers. However, two factors interfere with this margin of adjustment: first, low-productivity workers' incentives go the other way—the boost to their earnings from a minimum wage means that they should be trying harder than before to keep their job. Second, institutional arrangements such as employment protection may constrain employers' ability to lay off unproductive workers. This makes the hiring margin a better testing ground for employer selectiveness.

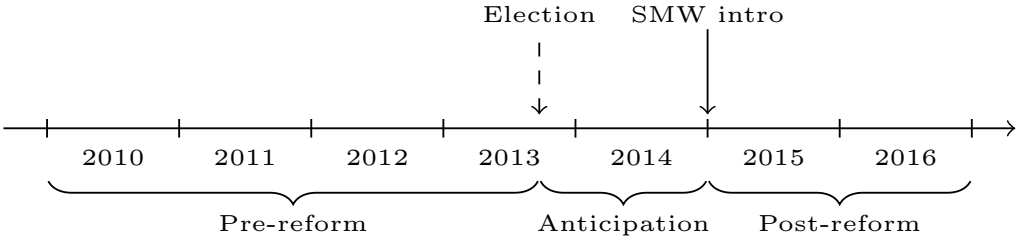
Section A in the appendix provides details and a review of existing search-and-matching models' predictions for minimum wage effects on hiring thresholds.

II Institutional setting and data

II.A Policy shock: statutory minimum wage introduction

Germany's statutory minimum wage (SMW) was set at EUR 8.50 per hour (gross) and came into effect on 1 January 2015. It applied to all of Germany's federal states (*Länder*) equally, irrespective of prior regional differences in wage levels.[4] Certain groups of workers were temporarily exempted: apprentices for the duration of their vocational training, teenagers up to their 18th birthday and interns for a maximum duration of three months; the long-term unemployed for the first six months of their new job;[5] and employees subject to a pre-existing sectoral minimum wage set below the new SMW, for a one-year transition period ending on 31 December 2015.

Figure 1: Timing of minimum wage introduction



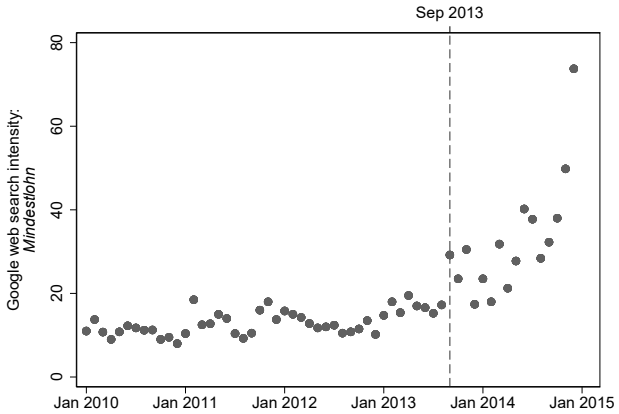
Anticipation The timing of the SMW introduction is sketched in Figure 1. Though the statutory minimum wage was written into law in August 2014, its introduction was widely anticipated earlier, most likely from the end of 2013 on. By December 2013, the general election of 22 September 2013 had resulted in a grand coalition between the Conservatives (CDU/CSU) and the Social Democrats (SPD). The SPD—the junior coalition partner—had

[4]How many workers would benefit from the SMW was the subject of considerable debate in 2014. Its bite in my sample of employers will be discussed in Section IV.

[5]The law adopted a strict definition of long-term unemployment: it applied only to individuals entering employment who had been unemployed for at least a year without interruption.

managed to push into the coalition agreement its election manifesto pledge of a statutory minimum wage.[6] In this the SPD had triumphed over the Conservatives, who in their election manifesto had rejected a statutory minimum wage, instead promoting additional sector- and region-specific minimum wages agreed by collective bargaining. As the Conservatives had been favoured to win the general election, it was difficult to anticipate the introduction of a statutory minimum wage prior to the general election, i.e., earlier than 22 September 2013.[7] This view is supported by Figure 2, which shows an index of Google web searches for "*Mindestlohn*" (minimum wage) in Germany over time: the graph remains broadly flat from 2010 through the general election of September 2013, when it starts rising.

Figure 2: Anticipation of minimum wage introduction



Concurrent policy changes The timing of Germany's SMW introduction on 1 January 2015 coincided with only one other labor market reform: the *Pflegestärkungsgesetz* granted workers the right to ten days of unpaid vacation if they were unexpectedly forced to care for a relative. During this time they would receive a government transfer (*Pflegeunterstützungsgeld*). This is a minor policy change unlikely to have interacted with the SMW introduction. Policy changes that became effective at other times in 2014 and 2015 made pensions more generous (July 2014), increased the social transfers to asylum seekers (March

[6]The result of the SPD's internal vote on the coalition agreement was announced on 14 December 2013.

[7]Newspapers reported at the end of May 2013 that Chancellor Merkel had reaffirmed her rejection of an SMW (https://www.handelsblatt.com/politik/deutschland/tagung-in-muenster-merkel-bekraeftigt-nein-zum-gesetzlichen-mindestlohn/8253832.html, http://www.spiegel.de/politik/deutschland/union-zum-gesetzlichen-mindestlohn-merkel-lehnt-forderung-der-cda-ab-a-901892.html). Only at the end of October were there reports that if a Grand Coalition came to pass it would likely introduce an SMW (see, e.g., https://www.zeit.de/wirtschaft/2013-10/mindestlohn-auswirkungen-arbeitsmarkt).

2015) and ended the requirement that asylum seekers only be considered for jobs if no EU nationals are available. In addition, an option of doubling the duration of paid parental leave by combining it with part-time work was introduced (July 2015). The only one of these reforms that may threaten to affect the quality of hires is the reduction of employment barriers for asylum seekers. However, it is unlikely that this loosening will confound the results of the minimum wage as it made it easier for all employers to hire (potentially more qualified) asylum seekers, not just for those establishments affected by the minimum wage.[8]

II.B Data sources

My main data source is the Integrated Employment Biographies (IEB), provided by the research data centre of the German Federal Employment Agency's research institute (IAB). The IEB are longitudinal individual-level spell data assembled from social security records. They contain information on workers' age, gender, education, qualification, occupation, industry, the timing of entry into and exit from employment, and total earnings from each employment spell (Antoni et al. 2019). There are person and establishment identifiers allowing me to follow individuals over time and to observe who is employed at the same establishment.

My data contain only a subset of the IEB that is built around a representative sample of German private-sector establishments, namely those 1,520 firms surveyed in one of the first three waves of the Linked Personnel Panel (LPP). The LPP is a matched employer-employee survey data set by IAB that is focused on human resource management practices (Kampkötter et al. 2016, Mackeben et al. 2018). However, I only use a single question from the LPP employer survey (see Section V.B); mostly, my empirical analyses are based on the administrative data for the workers of the 1,520 LPP establishments. For the period from 2010 through 2016, these data include everyone ever formally employed there, even if only for a day. The sample of establishments is representative for the German private sector by size, industry and region strata.[9] Unfortunately, establishments that employed fewer than

[8] In practice, a bias from the liberalization of the German labor market for asylum seekers is ruled out by my measure of individual productivity: AKM worker effects are only available for individuals who have been in regular employment in Germany before. See Section III.C for details.

[9] The LPP sample excludes firms entering the market after it was drawn (prior to the first survey wave of 2012). This prevents me from addressing dynamics at firms whose entry was induced by the minimum wage. I do, however, initially observe firms that exit the market after 2012 and I verify that they do not drive my main results.

50 workers are excluded.[10] This is not a design choice but a limitation of the only data set available to me that contained all workers of a set of firms (see appendix Section B for details). As the empirical analysis is at the establishment level I aggregate individual-level spell information in establishment-year cells.

From the individual-level social security data I use the employment records of full-time, part-time and marginal workers and exclude the remaining groups such as interns, apprentices and people in a part-time transition to retirement. The resulting sample includes 4,913,135 observations for 918,056 workers at 1,520 establishments.[11] Most relevant for my analysis of firm's selectiveness in hiring is the sample of worker inflows during 2010-16. This hire sample includes 448,013 observations for 440,813 workers at 1,512 establishments.

The final analysis sample is at the establishment-year level and includes the 1,491 establishments for which I can measure pre-reform minimum wage exposure and hire quality in at least one year.[12]

III Method

III.A Identification strategy

I use the following difference-in-differences (DiD) specification:

$$y_{jt} = \alpha + \beta TR_j * POST_t + \gamma_t + \delta_j + \epsilon_{jt}, \tag{3}$$

where I assign establishment j to the treated group ($TR_j = 1$) if it pays one or more workers strictly less than the future minimum wage in the pre-reform years (before policy anticipation is possible). The control group consists of all establishments that do not employ any minimum-wage workers in the pre-reform period. The pre-reform period includes the years 2010-2013; the post-reform period, $POST_t = 1$, is 2015-2016. I drop observations for the year 2014 from this DiD specification: in this year, the minimum wage is not yet effective

[10] In Section V.E I address the possibility that this size restriction drives my results and show this is not the case.

[11] Note that this is for the full flow sample, i.e., a cumulative total of everyone ever working at my sample of firms in 2010-16. This is more workers per firm than, e.g., the averages reported in Table 1, which provides the pre-reform average of start-of-year headcounts.

[12] The hire quality data I use are also provided by IAB—see Section III.C. Moreover, Section V.E uses an additional data set representative at the worker level—see also appendix Section I.

but there may already be an anticipation effect on hiring behaviour. There are fixed effects for years, γ_t, and establishments, δ_j. Beyond using establishment fixed effects to control for time-invariant heterogeneity I do not include co-variates. y_{jt} measures establishments' hiring standards (see Section III.C fur further detail). The DiD coefficient $\hat{\beta}$ provides an estimate of the effect of the minimum wage introduction on establishments' worker selection. Standard errors are clustered at the establishment level.[13]

The key identifying assumption is $\mathbb{E}\left[\epsilon_{jt}|TR_j * POST_t\right] = 0$: the selectiveness of hiring in treated and untreated establishments follows parallel counter-factual trends, i.e., would have been parallel in the entire analysis period if the minimum wage had not been introduced.[14] I can directly test whether this holds only for the pre-reform years by considering an event-study version of the DiD specification:

$$y_{jt} = \alpha + \beta_t TR_j * \gamma_t + \gamma_t + \delta_j + \epsilon_{jt}. \tag{4}$$

Now β_t is a year-specific DiD coefficient and 2013 is the reference year.[15] Parallel pre-reform trends in outcomes would mean that $\hat{\beta}_t$ is not significantly different from zero for 2010-2012. Anticipation effects of the minimum wage would show up as $\hat{\beta}_{t=2014}$ different from zero. $\hat{\beta}_t$ for 2015-2016 provides estimates of the minimum wage's short-run effect. While it is impossible to test for parallel counter-factual trends in the post-reform period a placebo analysis in Section V.A provides suggestive evidence in their support.

III.B Measuring establishment minimum wage exposure

My empirical strategy compares establishments treated by the minimum wage introduction to untreated establishments. I use establishments' pre-reform wage structure to determine their exposure to the SMW. My measure for SMW exposure is an indicator for whether any of the establishment's workers earned less than the future SMW (deflated using the CPI) in the years 2010-2013, when the minimum wage was not anticipated. As such, this dummy varies at the establishment level (but not over time). I follow Harasztosi and Lindner (2019) in using

[13] Some papers applying a similar identification strategy weight regressions with log employment (see, e.g., Harasztosi and Lindner 2019). I show in the appendix that doing so leaves my results qualitatively unchanged (see Figure A15).

[14] See Section D in the appendix for a discussion of a further, more subtle, assumption.

[15] Eq. (4) is shorthand for $y_{jt} = \alpha + \sum_{u=2010}^{2016} \beta_u TR_j * \mathbb{1}[u = t] + \sum_{u=2010}^{2016} \gamma_u \mathbb{1}[u = t] + \sum_{k=1}^{N} \delta_k \mathbb{1}[k = j] + \epsilon_{jt}$.

SMW exposure across the whole pre-reform period.[16] There is a theoretical motivation for my focus on a binary measure for SMW exposure rather than, e.g., the fraction of sub-SMW workers or a wage gap measure: I am looking for a shift in employers' hiring thresholds. If an establishment's lowest-paid workers exogenously become more expensive and this raises its hiring standard, recruitment should avoid workers with a productivity below the threshold - irrespective of whether the minimum wage bites for just a handful or many of its workers.[17]

The minimum wage sets a EUR 8.50 floor for hourly wages. The IEB data contain average daily wages but not hours worked. I therefore approximate hourly wages by focusing on establishments' full-time workers and dividing their working-day wages by 8, the length of a standard working day in Germany. This means that my SMW exposure measure does not consider establishments' part-time workers and "mini jobbers"[18]. This may be problematic if SMW exposure is measured with more error at establishments more heavily affected by the minimum wage, as may be the case if these establishments also employ a higher share of part-timers or mini jobbers. Indeed there is evidence that the share of minimum wage earners is particularly high among these groups, highlighting this concern (Machin, Manning and Rahman 2003). I next discuss how I deal with the potential bias arising from this.

Limiting non-classical measurement error In this section I argue that focusing on a binary, extensive-margin measure for SMW exposure helps me to limit potential biases from non-classical measurement error. First consider treated establishments. The share of part-timers and mini jobbers is higher at treated than at untreated establishments, as Table 1 shows. Existing evidence suggests that a greater share of part-timers and mini jobbers than of full-timers earn below the minimum wage (Machin, Manning and Rahman 2003). At the intensive margin, this would make me underestimate the SMW bite more at treated establishments. With the extensive-margin exposure measure, however, treated

[16] Column (4) of Table A3 in the appendix shows measuring SMW exposure only in July 2013 produces qualitatively similar results.

[17] There are theoretical arguments in both directions: on the one hand, employers of more sub-SMW workers have a larger incentive to raise and enforce hiring standards. On the other hand, frictions in the hiring process may make it more difficult to apply stricter hiring standards to all new hires for establishments with many sub-MW workers. Whether more heavily affected firms raise hiring standards more is explored using within-treatment variation in the SMW bite in Section IV.

[18] Mini jobs are also known as EUR 450 jobs or as marginal employment. These terms, used interchangeably, refer to employment in which a worker (regularly) earns no more than EUR 450 per month. The benefit to the firm is that social security contributions are reduced (if the worker participates in public insurance) or waived (otherwise).

establishments are correctly classified even if I ignore part-timers and mini jobbers. I will mis-classify some establishments as untreated - namely the ones where full-timers earn above the minimum wage but part-timers or mini jobbers earn below it. Note, however, that falsely assigning some treated employers to the control group in this way should bias my estimates towards zero. This is how using an extensive-margin measure for SMW exposure should help me to avoid overestimating the SMW effect.[19]

III.C Measuring employers' hiring standards

I measure establishment j's hiring standard using the ability of its least productive new hire i in a given year t, $\min_{j,t}\{ability_i\}$. The idea behind focusing on the bottom of the new hire productivity distribution is to be able to detect the shift of employers' hiring thresholds predicted by my toy model. In addition, the minimum wage has the largest bite for the lowest-earning workers, so that I expect its effect on worker selection to be concentrated in the left tail of the new hire ability distribution.[20]

Underlying $\min_{j,t}\{ability_i\}$ is a measure of individual worker productivity.[21] For this I use estimated worker effects from a two-way fixed-effects log-wage regression as introduced by Abowd, Kramarz and Margolis (1999).[22] These AKM worker effects capture the portion of workers' time-invariant productivity that is reflected in their wage earnings, including not only observable human capital but also typically unobserved aspects of productivity. For the purpose of measuring employers' hiring standards it is an advantage that AKM worker effects (a) vary continuously and (b) can distinguish between the ability of workers with the same level of schooling. As I discuss in more detail in Section V.C, education and other observables may be too coarse a measure or irrelevant for productivity in a minimum wage job.

[19]There is supplementary information on working hours for a sub-set of my data; however, its reporting differs across establishments, likely generating additional noise. As Table A3 in the appendix shows, using these data to measure exposure to the SMW introduction gives qualitatively similar, though attenuated, results.

[20]Despite the theoretical appeal of using minimum hire quality as a proxy for hiring standards one may be worried that the minimum makes the analysis vulnerable to outliers. Figure 6 in Section IV investigates the SMW's effect across the ability distribution of new hires.

[21]I use the words ability and productivity interchangeably.

[22]Estimated individual fixed effects have gained some popularity as a broad measure of worker quality, see the review in Butschek and Sauermann (2019b).

There are also significant limitations to using AKM worker effects as a productivity proxy. First, at best, AKM worker effects can capture those productivity differences that are reflected in wages. As, e.g., Jackson (2013) points out, the link between wages and productivity is very weak for certain occupations such as educators. In a context like Germany, where collective bargaining still plays an important role, the link between wages and productivity is likely to be dampened in parts of the private sector, too. Second, AKM worker effects as originally formulated will often capture not only time-invariant individual productivity but also a worker-firm match quality component. When such match effects are accounted for they typically explain around a quarter of the original AKM worker effects (Jackson 2013, Woodcock 2015). The assumption I make in this paper is that any bias to AKM worker effects from confounded match effects is orthogonal to the treatment status of individuals' future and different employers.[23] This appears reasonable given my choice of estimates from a non-overlapping period.

I use the AKM worker effects estimated by Card, Heining and Kline (2013)(henceforth CHK) for the interval 2002-2009. CHK implement the following AKM specification:

$$\ln(w_{ijt}) = \alpha_i + \psi_j + \gamma_t + x'_{it}\beta + r_{ijt}, \tag{5}$$

where $\ln(w_{ijt})$ is the natural logarithm of individual i's wage at establishment j in year t. There are additive fixed effects for individuals (α_i) and establishments (ψ_j) as well as a vector of time-varying individual-level controls (x_{it}), including age squared and age cubed as well as education categories interacted with the year dummies, age squared and age cubed. Using the largest connected set of establishments in West Germany and the universe of workers employed there during the interval from 2002 to 2009, CHK estimate Equation (5) separately for men and women.

Matching CHK's worker effect estimates with my sample of 2010-16 hires gives me an individual ability measure for 38.86% of hired workers.[24] However, I observe an ability measure for at least one hire at 99.01% of establishments. Measuring ability of only a subset

[23]Ideally I would estimate AKM worker effects net of match effects; unfortunately I lack access to the data this requires.

[24]One reason for the low individual-level coverage is that CHK's estimates are only for West Germany. In a robustness check I use a preliminary update of CHK's AKM worker effects that includes East German workers. This expands individual coverage to 51.58% of hired workers and produces similar results (see Figure A16 in the appendix).

of hires would be problematic if this selectively affected treated employers after the reform. I explore this issue in Section V.A.

III.D Summary statistics

Table 1: Firm characteristics by treatment status

A: Continuous characteristics				
	Treated		Control	
	mean	sd	mean	sd
Head count (1 Jan)	362.630	1478.003	154.849	184.199
Establishment age (yrs)	21.983	11.675	21.108	11.615
Female worker share	0.349	0.251	0.259	0.217
Mean worker age	42.309	3.917	43.994	3.335
Mean wage	94.533	31.870	117.200	25.633
Part-time worker share	0.120	0.166	0.088	0.133
Mini-job worker share	0.064	0.111	0.030	0.070
Mean daily starting wage (EUR)	79.308	26.311	99.782	23.433
Minimum daily starting wage (EUR)	44.368	20.358	72.755	19.205
Pre-reform hiring rate	0.251	0.398	0.143	0.301
Hire share with AKM FE	0.387	0.200	0.432	0.236
Ventile of establishment FE in distribution (1985-91)	11.938	5.450	14.859	4.443
Hires: minimum AKM FE	3.269	0.364	3.520	0.290
Observations	1,147		344	
B: Binary characteristics				
	Treated		Control	
	Share Yes	Frequency	Share Yes	Frequency
Manufacturing	0.295	338	0.358	123
Metal, electrical, automotive	0.251	288	0.328	113
Trade, transport, news	0.155	178	0.157	54
Business/financial services	0.177	203	0.087	30
Information, communication	0.122	140	0.070	24
Former East Germany	0.307	352	0.291	100
Observations	1,147		344	

Note: This table summarizes treatment and control establishments' average pre-reform characteristics for the main estimation sample. Panel A provides mean and standard deviation of continuous variables. Panel B gives means and frequencies for industry group and region dummies.

Table 1 provides descriptive statistics from 2013 for the estimation sample separately for treated and control establishments. Panel A reports means and standard deviations for continuous characteristics. The average treated establishment is larger and employs a greater share of women, part-time workers and mini jobbers. At treated establishments the mean wage is 19.3% lower, the mean starting wage is 20.5% lower and the hiring rate is larger. Panel B gives the share and number of establishments that display certain binary characteristics. The share in manufacturing as well as in metal, electrical and automotives is smaller among treated establishments. Their share in business or financial services and information and communication is bigger. Their share in trade, transport and news is similar, as is their share located in former East Germany. Note that those establishment characteristics that are not

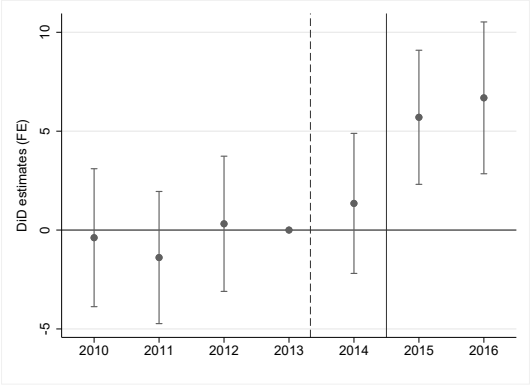
endogenous to the reform—i.e., mostly industry and location—are relatively time-invariant and will be captured by the establishment fixed effects in the regression specifications.

IV Main results

IV.A Validity of the treatment classification

I start with a plausibility test of my minimum wage exposure measure. That is, I test whether the SMW introduction had any bite for those establishments I identify as treated. If so, it should increase the wages establishments paid their lowest-earning new hires. Figure 3 presents the yearly DiD coefficients $\hat{\beta}_t$ from estimating the event-study regressions in Equation (4) with establishments' minimum starting wages as an outcome.[25] 2013 is the reference year. The DiD estimates for 2010-2012 are indistinguishable from zero, confirming the validity of the parallel-trends assumption for the pre-reform period. The 2014 coefficient shows that there is no significant anticipation of the minimum wage introduction in the bottom starting wages employers pay. Reassuringly, starting wages react positively to the introduction of the minimum wage at treated establishments.

Figure 3: Minimum wage effect on minimum starting wages: event study



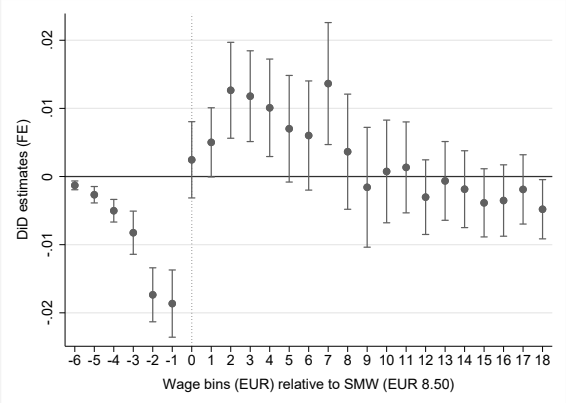
Note: This figure shows yearly DiD estimates for the effect of the minimum wage introduction on minimum starting wages (daily, in EUR). Estimates are from an establishment fixed-effects specification without covariates other than year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

[25] As starting wages I use the (daily) wage a worker earns during her first spell with a new employer. An employment spell lasts until the end of the calendar year at most.

To further explore the validity of my control and treated groups I use individual-level data to perform a test in the spirit of Cengiz et al. (2019).[26] That is, I estimate the effect of the minimum wage on the share of an establishment's workers in the 1-Euro wage bins around EUR 8.50, the level of the new SMW (see Section C in the appendix for details of the estimation procedure). If my classification of firms as treated or control is valid, the SMW introduction should shift workers in treated establishments from below-SMW wage bins to wage bins just above the SMW relative to control-group establishments. That is, wage bin-specific estimates of the minimum wage effect on the share of an establishment's workers in that wage bin should be negative for wage bins below the minimum wage, positive for a few wage bins above it and around zero for the rest of the wage distribution.

Figure 4 plots the estimated coefficients $\hat{\beta}_v$ to test whether this is the case. The bunching of workers above the minimum wage is slightly further to the right than one would expect; this is probably due to errors from approximating hourly wages. Otherwise, and with the exception of wage bin 7, the pattern arising from the coefficients provides strong support for the validity of my treatment classification. This is confirmed by wage histograms for workers of treated and control establishments before and after the SMW introduction (see Figure A1 in Section C in the appendix).

Figure 4: Minimum wage effect on treated and control firms' wage distributions



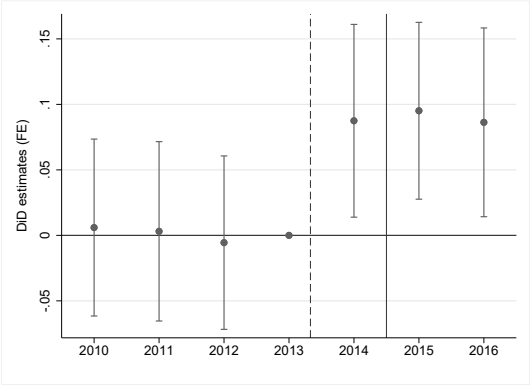
Note: This figure shows wage bin-specific DiD estimates for the effect of the minimum wage introduction on establishments' wage-bin employment shares: positive coefficients reflect that the minimum wage increased treated establishments' share of workers in the respective wage bin, i.e., shifted workers to this part of the establishment wage distribution. Estimates are from a wage bin-by-establishment fixed effects specification without covariates other than wage bin-by-year dummies. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

[26] I thank an anonymous referee for this valuable suggestion.

IV.B Effect on hire quality

Having established that the minimum wage had a bite for those establishments that I assign to the treated group I move on to the central question whether the minimum wage introduction made employers hire more selectively. To estimate the dynamic effects on minimum hire quality I again start with the fixed-effects event-study framework from Equation (4). Figure 5 presents the estimation results, revealing a persistent positive effect of the 2015 minimum wage introduction of around 0.09. It also shows a significant anticipation effect in 2014. This suggests that employers already factored higher future wages into their recruitment decisions the year before the minimum wage was binding. For the pre-reform period, the DiD estimates are closely centered around zero, providing evidence in support of parallel pre-reform trends.

Figure 5: Minimum wage effect on minimum hire quality: event study



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 CHK's individual AKM worker effects estimated 2002-2009. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

To condense the results from Figures 3 and 5 I estimate the before-vs-after DiD specification from Equation (3). For this I exclude data from the year 2014 to make sure my estimates do not capture anticipation effects. By this estimate the statutory minimum wage increased new hires' minimum daily pay by about EUR 6.60 and minimum hire quality by 0.086 (see Table A7 in the appendix). The effect on hire quality corresponds to a shift of treated firms' hiring standards from the 7th to the 11th percentile of workers' pre-reform productivity distribution. Section V.D explores this interpretation in more detail and relates the change in minimum hire quality to actual wages.

Next, I consider two alternative ways of classifying treated firms. First, I assign firms to the treated group based on the share of pre-reform sub-SMW *hires* rather than workers. Second, I consider 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. As Columns (2) and (3) of Table A2 in the appendix show, the alternative treatment classifications confirm the result that the SMW introduction increased minimum hire quality. The point estimates are somewhat smaller; a possible explanation is that mis-assignment of some firms attenuates the effect.

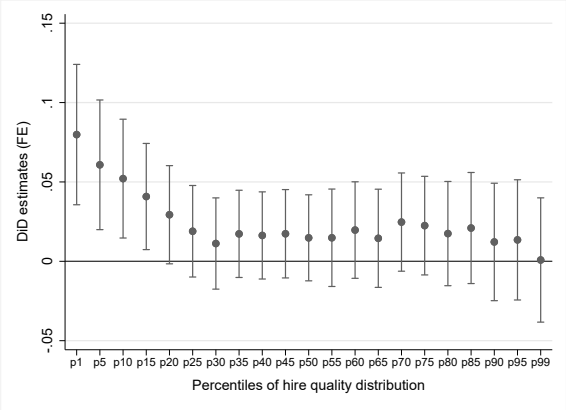
I also explore effect heterogeneity by the bite of the minimum wage. On the one hand, all firms in my treated group should have an incentive to raise their hiring standards. On the other hand, the strength of this incentive varies, so that the share of firms that do may be larger among firms that tend to employ many low-wage workers.[27] To test this I consider only the treated group and estimate the effect of a continuous measure of SMW exposure on minimum hire quality. As Column 1 of Table A8 in the appendix shows, there is a positive link between within-treatment group SMW bite and minimum hire quality, though it is not statistically significant. However, it is significant when I consider the alternative minimum wage exposure measures based on sub-SMW hires (Column (2)) and on below-p5 workers (Column (3)). The pattern is confirmed when I split up the treated group into the four quartiles of the minimum wage bite distribution, from least to most affected, and estimate Equation (4) separately by quartile, each time using the full control group. As Figure A7 in the appendix shows, the heterogeneity by bite is driven by the group of firms most affected by the minimum wage (quartile 4) while there is little difference between the lower-bite quartiles 1-3.

The analyses so far have focused on the effect of the SMW introduction on minimum hire quality, my measure of the hiring standard. However, a minimum wage-induced increase in the selectiveness of firms' hiring should probably be visible in the bottom of the ability distribution more generally. This would allay worries, for example, that minimum hire quality is too sensitive to outliers. Figure 6 explores the effect of the minimum wage on hire quality across different percentiles of the hire quality distribution. Reassuringly, it shows that the effect is concentrated in the bottom of the ability distributions of firms' new hires but is not

[27] Whether hiring more selectively is feasible may also depend on what screening technology is in place or feasible to introduce and what (fixed and continuous) costs are associated with it.

limited to the minimum. As a further alternative to minimum hire quality I check whether the SMW introduction also increases average hire quality, particularly among low-education hires. As Table A9 in the appendix shows, the effect is also positive and more pronounced in this worker sub-group, which is likely to be affected by the minimum wage. The next two sections investigate whether this increase in hire quality at the bottom of the distribution can indeed be attributed to employers hiring more selectively.

Figure 6: Minimum wage effect on hire quality across the hire ability distribution



Note: This figure shows DiD estimates for the effect of the minimum wage introduction on hire quality for different percentiles of each establishent's hire quality distribution. 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. Vertical bars denote 95% confidence intervals. Standard errors are clustered at the establishment level.

V Discussion

V.A Threats to identification

Systematic ability mis-measurement The individual ability measure used in this paper, estimated AKM worker fixed effects, doubtlessly contains substantial measurement error. Thanks to the DiD-strategy, however, even non-classical measurement error will only lead to an overestimation of the effect of the minimum wage on worker selection under very specific conditions. This would be the case if pre-determined ability had been systematically overestimated for new hires entering treated establishments after the reform, i.e., if the bias of the individual ability estimate were correlated with where and when the individual is eventually

hired. This is highly unlikely: the AKM worker effect used in the 2010-2016 analysis period is estimated using a non-overlapping earlier period (2002-2009) and is necessarily based on employment at other establishments than the one eventually hiring the worker.

Instead a potential threat to identification stems from the aggregation of individual ability measures to the firm level. As detailed in Section III.C I do not have an AKM worker effect estimate for all new hires. If a smaller share of hires were observed at treated establishments after the reform, I might mechanically overestimate minimum hire quality. One example of how this could come about is that more foreigners without work experience in Germany and hence without AKM worker effects were hired after the SMW introduction.[28] Such selective attrition may produce the same pattern as a change in the selectiveness of hiring. I test for the presence of selective attrition by using the share of new hires for whom an estimated AKM worker effect is available as an outcome. Column (1) of Table 2 suggests that the reform had no significant effect on the proportion of new hires for whom I have an ability measure, allaying concerns about selective attrition.

Table 2: Minimum wage effect on availability of hire quality measure

	(1) AKM coverage	(2) Female hires	(3) Hire age
DiD estimate (Treated*Post)=1	0.0024	-0.0105	0.2731
	(0.0117)	(0.0093)	(0.2840)
Mean (untreated establishments)	0.3981	0.2581	33.0127
Observations	8,473	8,473	8,473
Establishments	1,491	1,491	1,491
Adjusted R^2	0.0958	0.0003	0.0110

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. In all columns the dependent variable reflects certain characteristics of establishments' new hires: in (1) the share of hires for whom an AKM worker effect is available; in (2) the female hire share; and in (3) new hires' average age. DiD estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

A related threat arises from the possibility that AKM worker effects systematically underestimate individual ability for certain groups and that the minimum wage leads to fewer members of these groups being hired. This combination of a shift in the composition of hires and systematic mis-measurement for the newly under-represented groups may drive observed minimum hire quality up at treated establishments without the increase in hiring standards I am attempting to identify. I can test this possibility for two groups: women and younger

[28]This could be driven by the increase in my analysis period of the number of asylum seekers arriving in Germany.

workers. First, to the extent that there is still some discrimination against women in the labor market AKM worker effects may underestimate ability for women relative to men. It is therefore reassuring that the minimum wage did not significantly reduce the share of women hired (see Column (2) of Table 2). Second, there is evidence that AKM worker effects underestimate ability for younger workers (Butschek and Sauermann 2019b). Column (3) of Table 2 suggests, however, that this is unlikely to drive my results as the minimum wage did not significantly increase hires' average age.

Non-parallel counter-factual trends While the pre-reform trends in outcomes are parallel in the treatment and control groups, it is possible that minimum hire quality of treated and untreated establishments would have diverged (or converged) in the absence of the minimum wage introduction. The identifying assumption of parallel counter-factual trends itself is untestable. It is possible, however, to use a placebo test as indirect evidence for its plausibility. The variation in minimum wage exposure I use in this paper comes from differences in establishment wage structures. I stick to this idea in constructing a placebo treatment: using only employers untreated by the minimum wage, I split them into two roughly equal-sized groups based on their average wages. Those establishments with average wages below the median are assigned the placebo treatment and those with average wages at the median or greater make up the placebo control group. Figure A11 in the appendix reports the results from estimating event-study regressions of minimum hire quality on the placebo treatment. There is no clear pattern to the yearly coefficient estimates $\hat{\beta}_t$ and none of them is significantly different from zero. I interpret this as indicative evidence for the validity of the assumption of parallel counter-factual trends during the post-reform period.

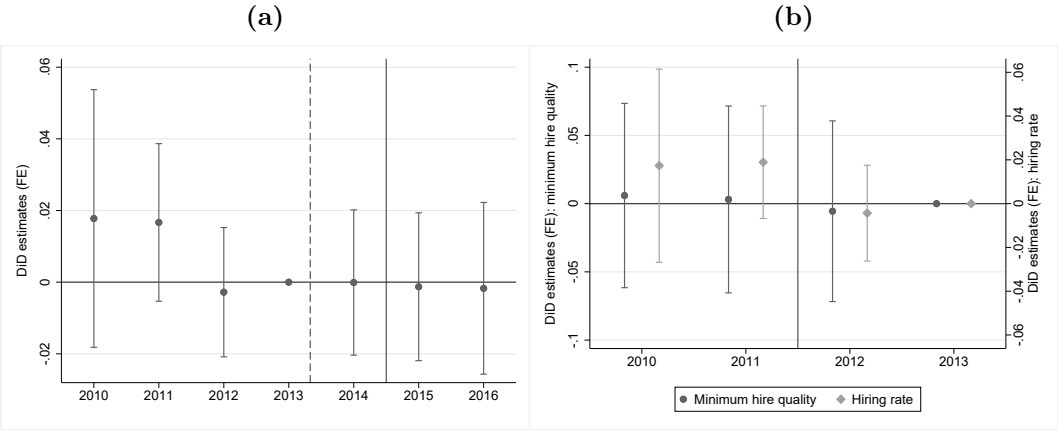
V.B Alternative interpretations

Mechanical effect of fewer hires An alternative to the firm selectiveness explanation for the SMW introduction's effect on minimum hire quality is that this was a mechanical consequence of a reduced number of hires. Suppose that employers hire randomly, not screening workers at all. Then, hires into a given establishment are a random draw from the ability distribution of available workers. If the hiring rate goes down, this will reduce the ability dispersion of new hires, raising hire quality at the bottom of the establishment hire quality distribution. Thus, if the minimum wage substantially reduced hiring, this may have me-

chanically increased minimum hire quality even with random hiring, delivering the empirical pattern I have hitherto interpreted as evidence of raised hiring standards. I present two tests of this interpretation.

First, I study the effect of the SMW introduction on establishments' hiring rate. Panel (a) of Figure 7 presents the yearly DiD coefficients from estimating Equation 4. There is no evidence of a reduction in the hiring rate around the SMW introduction - point estimates are centered closely around zero.[29] If anything, there is a slight break in the pre-reform period: between 2011 and 2012 the hiring rate at treated establishment drops, though none of the coefficients is significant. This variation in the pre-reform hiring rate suggests a graphical placebo test: zooming in on the 2012 drop in the hiring rate and checking whether minimum hire quality increases at the same time. Panel (b) of Figure 7 combines event-study coefficients for minimum hire quality (left y-axis) and for the hiring rate (right y-axis). It only considers the pre-reform period, using the drop in hiring in the treated group as a placebo experiment in the middle of the period. There is no indication that minimum hire quality moves the opposite way than the hiring rate, providing evidence against the mechanical explanation.

Figure 7: Minimum wage effect on hiring rate and pre-reform placebo test



Note: This figure shows yearly DiD estimates for: (a) the effect of the SMW introduction on the hiring rate and (b) the placebo effect of the 2012 drop in the hiring rate on minimum hire quality. Estimates are from an establishment fixed-effects specification without covariates other than year dummies. The hiring rate is the number of hires divided by employment. 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.

[29]The only other paper looking at the effect of the German SMW on firms' hiring rate also does not find a significant effect even though its sample of establishments is 9 times larger than mine (Bossler and Gerner 2020).

Second, accepting that the SMW introduction did not reduce the hiring rate in aggregate it is still possible that the hiring of low-wage workers went down.[30] For the second test consider therefore an extension of the random-hiring thought experiment: suppose that for low-wage jobs firms make a random draw from a worker ability distribution that is disjoint from the ability distribution of workers for higher-wage jobs. In this world, a hiring reduction among workers for low-wage jobs should still reduce the ability dispersion of new hires, increasing minimum hire quality; at the same time, however, it should reduce maximum hire quality *in this sub-group of workers*.[31] I therefore check whether the SMW introduction has a negative effect on maximum hire quality among a group of workers likely to be hired for low-wage jobs: individuals with neither a high school leaving certificate nor a vocational qualification. In addition, I estimate the minimum wage effect on a group of firms whose hires are more likely to be low-wage workers: the quarter of treated firms with the largest pre-reform share of low-wage workers. Table 3 presents the results. Maximum hire quality is not significantly different from zero for the low-wage sub-group of workers or for the sub-group of firms more likely to hire low-wage workers. This suggests that if the mechanical effect of fewer hires is at play among low-wage workers it is too small to be detectable with the available statistical power. It is therefore an unlikely explanation for the effect of the minimum wage on minimum hire quality.

Table 3: Minimum wage effect on maximum hire quality for sub-groups

	Maximum hire quality	
	(1)	(2)
Worker sub-group	Low-education hires	All hires
Sample of firms	All	Control and top bite quartile
DiD estimate (Treated*Post)=1	0.0028	-0.0146
	(0.0428)	(0.0258)
Mean (untreated establishments)	3.6430	4.1557
Observations	2,084	3,013
Establishments	791	630
Adjusted R^2	0.0318	0.0054

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is (1)maximum hire quality among new hires with less than Abitur and no vocational education; and (2) maximum hire quality overall. The estimation sample includes all firms in (1). In (2), the estimation sample consists of all control firms and the quarter of treated firms with the highest SMW bite, i.e., the firms with the largest pre-reform share of sub-SMW workers. 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.

[30] Section V.C finds no effect of the SMW introduction on the share of hires with low education. This does not support the idea that only the hiring of low-wage workers was reduced.

[31] An earlier version of this test looked at the overall maximum of hire quality, ignoring the possibility that hiring reductions only affect low-wage workers. I thank an anonymous referee for pointing this out.

Demand or supply So far in this paper I have interpreted the changes in minimum hire quality induced by the introduction of the minimum wage as evidence of shifts in firm hiring standards, i.e., a change in the demand for the quality of labor. In principle, however, they could also have been driven by changes in the self-selection of workers—i.e., the supply of the quality of labor. Several papers consider the possibility that labor supply responds to minimum wages, e.g., through labor market entry or search effort (Drazen 1986, Acemoglu 2001, Flinn 2006). Contrary to the priors of this theoretical literature, Adams, Meer and Sloan (2018) find no evidence that workers' labor market entry or search intensity respond to minimum wages.

Here I consider another possible supply-side response: that the SMW introduction affects the direction of search. In such a story the minimum wage would cause a greater number of (productive) workers to apply to affected employers, for instance, because these become more attractive as their wages (at least at the bottom) improve relative to those of untreated establishments.

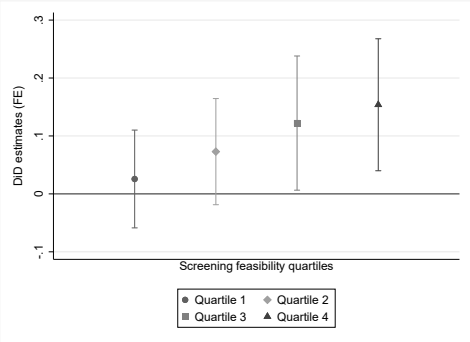
I first look to the literature for relevant evidence. In a descriptive study of the evolution of establishments' hiring criteria and job-filling success before and after the introduction of the German minimum wage, Gürtzgen et al. (2016) find that it becomes harder to fill low-wage vacancies. This does not support increased self-selection of low-wage workers toward treated firms. Dustmann et al. (2020) provide extensive causal evidence of a re-allocation of sub-SMW worker to higher-quality firms, for instance, better paying and higher AKM fixed-effect establishments. However, the re-allocation mechanisms the paper discusses also work through changes in labor demand rather than supply, for instance, as surviving firms absorb workers released by firms exiting the market.[32] It is still possible that low-wage workers became more likely to apply to higher-wage firms in response to the minimum wage. However, as my control establishments pay higher wages than the treated ones (see Table 1), increased self-selection of the more productive low-wage workers toward higher-wage firms is unlikely to drive my result—in fact, it should work the other way, in line with Gürtzgen et al. (2016).[33]

[32]In the first channel discussed, more good jobs are created and require workers (Acemoglu 2001); in the second and third workers are no longer demanded by firms forced out of the market by the SMW—firms that formerly exploited their monopsony power or product market frictions.

[33]For an additional test of worker self-selection based on the share of new hires who are voluntary movers coming from high-wage employers, see Section G in the Appendix.

Second I attempt to identify more direct evidence for the screening channel. Employers' hiring policies may depend on their history, ownership structure, management quality, production technology or task structure, generating between-establishment variation in the intensity of pre-hire screening. Assuming that employers with a tradition of screening are better placed to avoid low-productivity hires, their response to the SMW introduction should be more pronounced—if the reform-induced increase in minimum hire quality is due to employers raising their hiring standards.

Figure 8: Minimum wage effect by pre-reform screening intensity



Note: This figure shows DiD estimates for the effect of the minimum wage introduction on minimum hire quality by quartile of the pre-reform screening intensity. 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.

I use information from the 2012 wave of the Linked Personnel Panel (LPP), an employer survey on human resource management, to split the sample into four groups of establishments according to the quartile of the screening intensity distribution they inhabit.[34] I then estimate Equation (3) separately for each quartile. Figure 8 suggests that the estimated effect of the minimum wage on minimum hire quality is indeed bigger at establishments where screening plays a bigger role, increasing monotonically across quartiles. Table A10 in the appendix reports the point estimates underlying Figure 8 as well as significance tests for the difference between quartile-specific DiD estimates. As the number of establishments in each screening intensity quartile is small, only the difference between quartile 1 and 4 is significant at the

[34]See Figure A12 in the appendix for a histogram of the intensity of pre-hire screening for non-managerial hires.

10% level.[35] Despite power constraints, the heterogeneity by pre-reform screening intensity is strong evidence that the change in minimum hire quality is a demand-side phenomenon, driven by a shift of employers' hiring standards.[36]

V.C Observed characteristics vs. estimated productivity

A feature that sets this paper apart is that it relies on AKM worker effect estimates to measure individual productivity of new hires. The majority of papers looking at labor-labor substitution use observed characteristics such as age or coarse education cells—often, because these are the only proxies available. This section explores the comparison between the two types of ability proxy.

I start by testing whether the finding on increased hiring standards as measured by AKM-based productivity estimates can be replicated when using standard observables as ability proxies. A very common proxy is age (e.g., Neumark and Wascher 1995, Portugal and Cardoso 2006, Fairris and Bujanda 2008, Giuliano 2013, Hirsch, Kaufman and Zelenska 2015, Clemens, Kahn and Meer 2018, Cengiz et al. 2019). We know already from Table 2 that the estimated effect of the SMW introduction on the age of hires is not significantly different from zero. Another one is education: several papers approximate low skill by not having completed high school (e.g., Hirsch, Kaufman and Zelenska 2015, Cengiz et al. 2019). To obtain an equivalent to dropping out of high school that makes sense in the German context I follow CHK's definition of low skill: an education level lower than *Abitur* and no vocational qualification.[37] I use the share of low-skill hires by this definition as an outcome. Next is experience (e.g., Fairris and Bujanda 2008, Hirsch, Kaufman and Zelenska 2015), which I operationalize through new hires' average experience in days. Finally I look at past unemployment as a proxy for low skill using the share of hires entering from unemployment or average cumulative prior unemployment. Table 4 presents the results of estimating the

[35] Figure A13 in the appendix provides separate event-study graphs for each pre-reform screening intensity quartile. Estimates become even more noisy at this resolution, making it difficult to confirm or refute the monotonicity across quartiles from the event-study graphs. However, the group of establishments in the highest screening intensity quartile still stands out as showing the largest effect.

[36] Table A11 in the appendix estimates the effect of the SMW introduction on screening intensity as reported in subsequent waves of the LPP employer survey. There is only one pre-reform year, precluding comparison of pre trends. The DiD estimate is positive but insignificant. This could be due to a lack of precision: the sample is much smaller for post-reform waves; and the survey question does not ask about screening intensity for low-wage or low-skill hires but for all non-managerial hires. However, as Section I in the appendix points out the theoretical prediction on increased investment in screening is also ambiguous.

[37] In Germany vocational education plays a central role in producing a skilled workforce.

DiD specification in Equation (3) for these outcomes. There is no evidence that the minimum wage makes employers hire more selectively along these observed dimensions; using observable characteristics I cannot detect the increase in hiring standards I find when proxying the productivity of new hires with AKM worker effects.

Table 4: Minimum wage effect on observable hire quality

	(1) Age	(2) Share low-skilled	(3) Experience	(4) Share unemployed	(5) Total UE
DiD estimate (Treated*Post)=1	0.2731	0.0047	5.4552	0.0022	3.3528
	(0.2840)	(0.0059)	(77.8863)	(0.0095)	(13.6324)
Mean (untreated establishments)	33.0127	0.0483	3101.8603	0.2053	303.0417
Observations	8,473	8,473	8,473	8,473	8,473
Establishments	1,491	1,491	1,491	1,491	1,491
Adjusted R^2	0.0110	0.0402	0.0067	0.0184	0.0027

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. Dependent variable is, always for new hires: (1) average age; (2) the share that have neither a vocational qualification nor a secondary school diploma that qualifies them for university entry (*Abitur*); (3) average experience in days; (4) the share who entered from unemployment; and (5) average cumulative unemployment experience in days. DiD estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

One potential explanation for this discrepancy is that AKM worker effects increase the resolution at which ability is measured, revealing more subtle changes in the composition of newly hired workers than standard observables. This would help to reconcile diverging results in the literature: it may be more difficult statistically (or require a very large bite of the minimum wage) to identify labor-labor substitution using observed characteristics. This may explain why few papers find compelling observables-based evidence of increased hiring standards (Bernini and Riley 2016, Clemens, Kahn and Meer 2018) and most do not (Portugal and Cardoso 2006, Fairris and Bujanda 2008, Giuliano 2013, Hirsch, Kaufman and Zelenska 2015, Cengiz et al. 2019).

There is some support in the literature for the interpretation that AKM worker effect estimates may be better predictors of productivity than observables. Several papers provide indirect evidence by showing the merits of past wages as productivity proxies: Clemens and Wither (2019) argue that low past/current wages are better suited than young age for proxying low skill (with a view to identifying those workers for whom the minimum wage is most likely to bite). Horton (2018) uses past wages on an online labor market explicitly to measure the quality of hires. In line with this paper, his field experiment shows that a minimum wage increases the quality of new hires as measured by their past wages but that this

'upskilling' cannot be detected by looking at previous experience or, in most specifications, at demographic characteristics. He suggests that most productivity differences may be within rather than across demographic groups. Further support for this argument is provided by Butschek and Sauermann (2019b), who study the predictive power of estimated AKM worker effects. They show that AKM estimates predict cognitive and non-cognitive ability not just in aggregate but also conditional on the level of education.

I conclude this section with three pieces of suggestive evidence from my data that AKM worker effects are better predictors of ability than observables. First, as shown in Section V.D, I can replicate my main result using raw past wages, increasing my confidence that the AKM worker effects pick up real differences in ability. Second, pre-determined AKM worker effects explain more variation in residualized wages than do the other proxies, both overall and in the bottom half and bottom quarter of the wage distribution (see Section H, particularly Table A5, in the appendix). And third, the association between AKM worker effects and observables is smaller for low-wage workers than overall - consistent with the idea that observed characteristics are less informative about ability among low-wage workers (see Section H with Table A6 in the appendix).

V.D Hiring standards, wages and costs

This section relates hiring standards to wages both to facilitate the interpretation of the estimated increase in minimum hire quality and to tie the analysis back to potential welfare implications for firms.

I first use descriptive statistics to obtain a ball-park estimate of the pre-reform wage premium implied by the minimum wage-induced increase in hiring standards. The idea is to use the worker productivity distribution to translate the effect of the SMW introduction on hiring standards into a move from one AKM percentile to another, with an associated wage difference. For this I pool the indivual-level pre-reform data of all full-time employees of my treated and control firms. I then compute the percentiles of their estimated AKM worker effects and obtain average wages for each productivity percentile (see Table A12 in the appendix). This gives me a frame of reference for firms' hiring standards. Treated firms' pre-reform minimum hire quality is 3.27 on average. As Table A12 shows this corresponds to the 7th percentile of the AKM worker effect distribution of pooled pre-reform workers. The SMW introduction's estimated effect on minimum hire quality is 0.086, or a move from

the 7th to the 11th percentile of the productivity distribution. Comparing average wages for these percentiles implies an hourly wage premium of about 81 Euro cents (9.6% of the SMW) or a daily wage premium of EUR 6.51. This is very close to EUR 6.56, my estimate of the effect of the SMW on minimum daily starting wages reported in Table A7. Taken at face value, this suggests that at the margin, i.e. for the least productive workers hired, the productivity increase from more selective hiring almost entirely compensated the estimated increase in wages the firms faced.

I expand on this descriptive exercise by using new hires' raw past wages to replicate the main analysis of the minimum wage effect on hiring standards. That is, I re-estimate Equation (3) with new hires' minimum past average wages as an outcome. This gives me a direct estimate—in terms of daily wages—of how much the minimum wage raised the productivity of the least productive new hire. Table 5 reports the results from two operationalizations of 'past wages': Column (1) uses the individual average over the 5 pre-hire years[38] and Column (2) uses the individual average wage for the period 2005-2009. Column (3) reproduces the estimate from Section IV for the effect on minimum starting wages. Comparing columns (1) to (3) suggests that right at the hiring standard, around two thirds of the increase in wage costs were compensated for by hiring more productive workers. To check how sensitive this conclusion is to using the minimum, Columns (4)-(5) consider the past wages of hires slightly above the hiring standard (P10 of the new-hire ability distribution). Comparing this to P10 starting wages confirms the result.

Table 5: Minimum wage effects on new hires' past wages

	New hires' minimum			New hires' P10		
	(1)	(2)	(3)	(4)	(5)	(6)
	5-year avg wage	05-09 avg wage	Starting wage	5-year avg wage	05-09 avg wage	Starting wage
DiD estimate (Treated*Post)=1	4.1400	4.8049	6.5555	2.6299	3.8774	4.6756
	(1.4204)	(1.7124)	(1.2219)	(1.3217)	(1.6245)	(1.0455)
Mean (untreated establishments)	53.2842	52.4379	72.2839	58.3179	56.4124	77.9694
Observations	8,227	8,118	8,288	8,227	8,118	8,288
Establishments	1,491	1,491	1,491	1,491	1,491	1,491
Adjusted R^2	0.0134	0.0099	0.0234	0.0122	0.0061	0.0336

Note: *** p<0.01, ** p<0.05, * p<0.1, with standard errors clustered at the establishment level. In the left half of the table, the dependent variable is minimum past wage averaged across either (1) the 5 years prior to being hired or (2) the period 2005-09; and minimum starting wages (3). In the right half, the dependent variable is P10 of new hires' past wages averaged across either (4) the 5 years prior to being hired or (5) the period 2005-09; and P10 of starting wages (6). Estimates are from establishment fixed-effects specifications without covariates other than year dummies. The year 2014 is excluded to rule out anticipation effects.

[38]Note that this average may include a post-reform wage, e.g., for 2015, for workers hired in 2016.

To summarize, several indicative analyses relating hiring standards to actual (pre-reform) wages suggest that at the margin (i.e., for the lowest-productivity hires), much of the increase in wage cost faced by firms was compensated for by higher productivity. Importantly, this does not allow me to rule out negative minimum wage effects on firm profitability: I simply lack the data to fully estimate cost increases (such as screening costs) or to directly estimate effects on profits.[39]

V.E What happened to the lowest-productivity workers?

This paper has presented clear evidence of an improvement in the quality of hires at firms affected by the SMW introduction in Germany. It has also provided evidence supporting the interpretation that this reflects an increase in firms' hiring standards in response to the minimum wage. From a policy perspective, the elephant in the room is whether this result, irrespective of any aggregate employment effects, implies negative re-distribution effects among low-wage workers. That is, did the minimum wage make it more likely that some of the least productive workers were excluded from employment?

To address this question, this section uses SIAB, a 2% random sample of the German workforce, to study workers' employment prospects conditional on their estimated productivity.[40] As before, estimated AKM worker effects serve as an individual productivity measure.[41]

The key policy concern is that low-productivity workers, who would counterfactually have been hired by the treated establishments, may have become less likely to be in employment.[42] I ask two questions to test this hypothesis: first, did low-productivity workers' probability of being unemployed increase? Second, did low-productivity workers become more likely to exit the labor force?

To answer these questions I build on a recent approach that compares post-reform outcomes across different parts of the pre-reform wage distribution, net of the same difference in an earlier period (e.g., Horton 2018, Dustmann et al. 2020). Instead of the wage distribution

[39] In fact, Bossler et al. (2020) do provide evidence that the German minimum wage may have reduced firms' profitability.

[40] I thank one of the anonymous referees for encouraging me to tackle the worker side of my question.

[41] See Section I in the appendix for details on the data.

[42] Another possibility is that these workers have instead been hired by smaller establishments which are unobserved in my sample of employers. This does not seem to be the case: Figures A4 and A5 in the appendix show that the share of employed (unemployed) workers who entered a small firm (fewer than 50 workers) did not change.

I look at differences in the ability distribution. I first select those workers I observe in a given labor market state, e.g., employment in 2013 (prior to the SMW introduction) and group them according to their (time-invariant) AKM worker effect decile. I then estimate their productivity decile-specific probability of, e.g., also being employed in 2015 (after the SMW introduction). Because this probability seems likely to be lower for lower-AKM workers at any time I consider a comparison period two years earlier—i.e., I estimate the probability of being employed in 2013 by AKM decile for those workers employed in 2011. Plotting the employment probabilities profile across AKM deciles for the period of interest (2013-15) and the comparison period (2011-2013) allows me to obtain graphical evidence of a change in the employment prospects around the introduction of the minimum wage of very low AKM workers relative to the rest of the productivity distribution, net of baseline between-group differences.

That is, I estimate

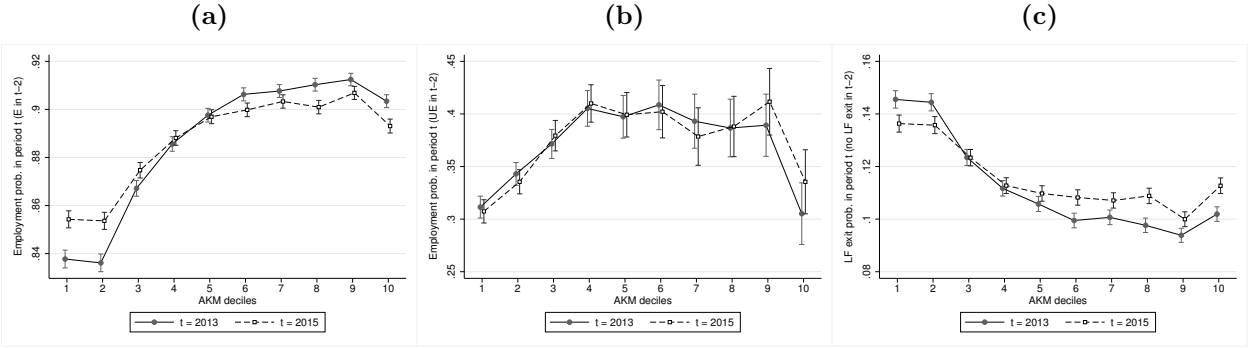
$$y_{it} = \alpha + \sum_{d=1}^{10} \beta_d \mathbb{1}[decile_i = d] + \epsilon_{it}, \tag{6}$$

for t conditional on some labor market status in $t-2$ with $t \in \{2013, 2015\}$. The figures presented below compare linear combinations of $\hat{\alpha} + \hat{\beta}_d$ from the two estimation periods.

I start by looking at the employment probability in t of individuals employed in $t-2$ by productivity decile. Contrary to the concern motivating this analysis, Panel (a) of Figure 9 suggests that the employment probability actually increased around the SMW introduction for previously employed low-productivity workers relative to the comparison period. Panel (b) repeats the analysis for the post-reform employment probability of low-productivity workers who were unemployed before the reform. The graph suggests that the job-finding probability of low-AKM workers remained similar as in the comparison period. Panel (c) looks at the probability of exiting the labor force conditional on being in the labor force two years prior. It shows that the probability of dropping out of the labor market decreased for low-productivity workers around the introduction of the minimum wage relative to the comparison period. Taken together, these figures do not support a deterioration of low-productivity workers' labor market prospects. If anything, they suggest a slight improvement.

At first glance, it seems difficult to square (weakly) better employment prospects of very low-productivity workers with more selective hiring by affected firms. One potential expla-

Figure 9: Employment prospects of workers previously employed, unemployed or in the labor force



Note: This figure shows AKM decile specific estimates of (a) the employment probability in t conditional on t-2 employment; (b) the employment probability in t conditional on t-2 unemployment; (c) the probability of dropping out of the labor force in t conditional on not dropping out and being observed in t-2. Vertical bars denote 95% confidence intervals (with heteroskedasticity robust standard errors). Source: SIAB

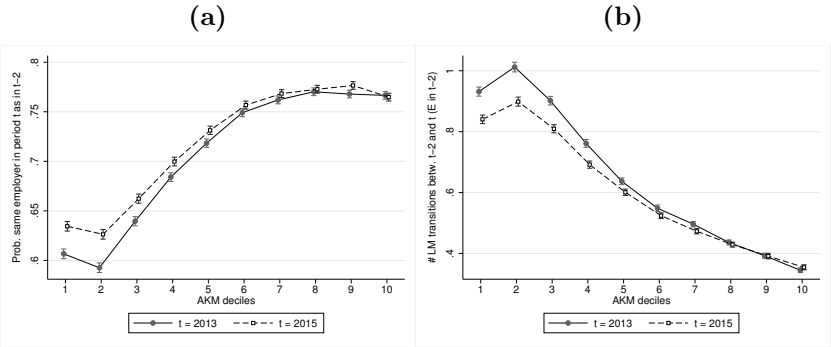
nation, however, is that low-productivity workers' employment relationships became more stable in response to the SMW introduction as the value of employment for them went up. This idea resonates with several papers documenting a minimum wage-induced increase of employment stability in other contexts (Portugal and Cardoso 2006, Brochu and Green 2013, Dube, Lester and Reich 2016, Gittings and Schmutte 2016). Figure 10 tests two predictions of this explanation.

Panel (a) checks whether low-productivity workers held on to their jobs more, so that fewer of them needed to get hired elsewhere. Indeed the probability of staying with their employer of those employed before the SMW introduction increased. Panel (b) looks at another measure of job stability, namely whether low-productivity workers experienced fewer changes of labor market status. Here, too, the answer is affirmative: the number of labor market transitions (in any direction) decreased around the SMW introduction for workers employed before the reform.[43]

One may worry that these changes in the employment prospects of low-productivity workers are due to factors other than the SMW introduction. To test this objection I return to the wage-based identification strategy mentioned above (Horton 2018, Dustmann et al. 2020). I loosely replicate Dustmann et al. (2020) for the sub-set of workers with low ability (bottom two AKM deciles) and use the key outcomes from above: employment probability, same-

[43]There is some indication from the establishment-level data that the average productivity of workers leaving the employer went up in response to the minimum wage (see Figure A14 in the appendix). This is also consistent with low-AKM workers valuing their jobs more highly.

Figure 10: Employment stability



Note: This figure shows AKM decile specific estimates of (a) the probability of being with the same employer in t as in t-2, conditional on t-2 employment; (b) 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

employer probability and number of labor market transitions. Figure A6 in the appendix supports a causal interpretation. It shows that the relative improvement in employment prospects and job stability among low-AKM workers is concentrated among workers earning below the SMW before the reform.

The results of the worker-level analyses in this section do not confirm the worry that increased hiring standards harmed the employment prospects of the lowest-productivity workers. They suggest that the low-skilled who would have counterfactually been hired by affected firms neither remained unemployed nor left the labor force. Instead these low-AKM workers appear to have stayed with their previous employers in greater numbers and experienced less churn, obviating the need for renewed hire. One qualification to this conclusion is that I cannot rule out adverse effects for all low-productivity job seekers as my analyses exclude individuals without AKM worker effects, particularly recent labor market entrants.

VI Conclusion

In this paper I study the effect of the introduction of a statutory minimum wage in Germany on the hiring standards of 1,491 private-sector employers. My empirical strategy is a difference-in-differences comparison of treated and untreated establishments, classified by whether their wage structure prior to the reform exposed them to the introduction of the SMW. To identify employers' hiring standards I do not rely on manager-reported intentions or hiring criteria but on the quality of establishments' realized hires, using the ability of the

least productive new hire in a year as a proxy for the hiring standard. The worker-level data I use allows me to observe every single worker inflow during a period of four years pre-reform and two years post-reform. Matching these with the AKM worker effects estimated by Card, Heining and Kline (2013) gives me an individual-level measure of time-invariant productivity. Using AKM worker effects as ability proxies implies that I measure employers' hiring standards with respect to both observed and unobserved worker productivity.

My DiD estimate of the SMW introduction's effect on establishments' minimum hire quality is an increase of 0.086, or a shift in workers' pre-reform productivity distribution by 4 percentiles. That this result identifies a causal effect is primarily threatened by two scenarios. The first is selective attrition or measurement, where the availability or accuracy of my worker ability proxy is systematically different for treated establishments after the reform. I show that this is not the case. The second potential threat is a deviation from parallel counterfactual trends after the reform. I provide suggestive evidence that this is unlikely: a placebo test shows that trends are roughly parallel between two groups of untreated employers that, like the treated and control groups, differ in their pre-reform wage structure.

I also explore the possibility that while the reform's effect on minimum hire quality is real, it does not reflect an increase of employers' hiring standards. There are two alternative explanations I address: the first is that a reform-induced reduction in the hiring rate mechanically increased establishments' minimum hire quality by shrinking the sample they draw from the worker ability distribution. Such a mechanism requires a drop in the hiring rate and implies a reduction in maximum hire quality among low-wage workers. I find neither. The second alternative interpretation is that various changes in workers' self-selection improved the applicant pool of treated establishments. Thus a change in the quality of labor supply, rather than raised hiring standards, could be driving the increase in minimum hire quality. I offer several pieces of evidence supporting the interpretation that demand for a higher quality of labor is the primary driver of my result. The most important is that the effect of the SMW introduction on minimum hire quality is concentrated in those establishments that report more pre-hire screening in the pre-reform period, i.e., where employers should be better able to distinguish worker quality before hiring.

One limitation of this paper is its focus on incumbent medium-sized and large firms. Because of the data I use I cannot address the effect of the minimum wage introduction on

the selectiveness of hiring at establishments with fewer than 50 workers and at firms that enter due to the minimum wage.

The paper's central result that a minimum wage raises employers' hiring standards is consistent with the findings of the two other causal studies addressing the question, written contemporaneously and independently (Clemens, Kahn and Meer 2018, Horton 2018). With respect to Clemens, Kahn and Meer (2018), key differences are that my paper emphasizes hiring standards with respect to unobserved productivity rather than observed characteristics and that I explore the effectiveness of the adjustment strategy for firms and the consequences for workers who might be adversely affected by more selective hiring. The main distinctions between Horton (2018) and this paper are the method and the type of labor market studied. It increases the confidence in our shared finding that in both the randomized experiment and the policy reform a minimum wage raised hiring standards; and that this applies in an online as well as a brick-and-mortar labor market.

An important additional finding of this paper is that the change in hiring standards I find is not detectable using new hires' education, age, experience or previous unemployment. I argue that this is because these observables are too coarse to capture individual ability differences in the bottom of the wage distribution. This insight may help to explain why only some papers find labor-labor substitution effects of minimum wages.

Another result has implications for firm welfare. Using new hires' past wages to put a Euro figure on the marginal productivity improvement I estimate that firms were able to recoup around two thirds of increased wage costs for their least productive hire. This suggests that adjusting their hiring standards may have helped firms to soften negative effects of the minimum wage on their profits.

A worrying implication of these findings for policy-makers would be that even without negative aggregate employment effects there could be some losers from the introduction of a minimum wage—if there are workers who were deemed too unproductive to be hired at the new wage floor. Indeed, Horton (2018) finds evidence in the context of an online labor market that low-productivity workers' probability of getting hired was reduced by the imposition of a minimum wage. I also cannot rule this out for all low-productivity workers as I do not have AKM worker effects for recent labor market entrants. However, for labor market incumbents, my worker-level analyses of the German SMW introduction suggest an increase in the propensity of low-productivity workers to stick with their employers. This increase in

job stability appears to have made fewer hires of low-productivity workers necessary, offsetting any adverse effects firms' raised hiring standards may otherwise have had on these workers' employment prospects.

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