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Reallocation Effects of the Minimum Wage

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Appendix A  Additional Results

A. 1  Distribution of Hourly Wages Before and After the Introduction of the Minimum Wage

Figure A.1 provides evidence on the effect of the minimum wage on the hourly wage distribution, taken from the report of the German Minimum Wage Commission in 2018 (Mindestlohnkommission, 2018). Panel (a) shows the density of hourly wages in 2014 (one year before the minimum wage hike) and in 2015 and 2016 (one and two years after the minimum wage hike). Panel (b) shows the number of employees by wage bin (the frequency wage distribution) for those three years. Both figures are based on the German Structure of Earnings Survey (SES, Verdienststrukturerhebung, VSE/VE data), a data set similar to that used by Harasztosi and Lindner (2019) to assess bunching at the minimum wage in Hungary. The figures show a clear effect of the minimum wage on the hourly wage distribution: after the introduction of the minimum wage, there are missing jobs below the minimum wage of 8.50 EUR, a spike at the minimum wage and excess jobs just above the minimum wage. In line with the existing literature (e.g., Autor, Manning, Smith, 2016; Brochu, Green, Lemieux, and Townsend, 2015), the density of hourly wages is similar in the three years for hourly wages above about 11 EUR. Panel (b) further highlights that the excess number of jobs between the 8.50 EUR and up to the 11 EUR wage bins is roughly similar to the missing number of jobs below the minimum wage of 8.50 EUR, underscoring that the overall job loss due to the minimum wage must be limited.

Figure A.2 shows the density figures separately for East and West Germany. As expected, the minimum wage policy has a substantially larger impact on the East German wage distribution, as wages are generally lower in East than in West Germany.

Figure A.3 shows the pre-reform (2014) hourly wage distribution estimated using our data (the so-called Employee Histories provided by the Institute for Employment Research in Nuremberg (BEH)). Whereas information on daily wages and workers’ full-time status (i.e., full-time, part-time, marginal) is available throughout our study period, information on working hours is available only from 2011 to 2014. The 2014 hourly wage distribution in our data is very similar to the hourly wage distribution obtained from the German Structure of Earnings Survey (see panel (a) of Figure A.1). Our data source thus provides reliable information on the bite of the minimum wage prior to its
introduction. However, it cannot be used to calculate changes in hourly wages by baseline wage bin after the introduction in the minimum wage. In 2015 and 2016, we thus proxy hourly wages using information on daily wages and workers’ full-time status (full-time, part-time and marginal employment), as described in Section 2.2 in the main paper.

References


Figure A.1: The Effect of the Minimum Wage on the Hourly Wage Distribution

(a) Density of Hourly Wages

(b) Number of Employment Relationships By Gross Hourly Wages

Notes: The figures show the density and frequency distribution of wages in Germany before and after the introduction of the minimum wage. Panel (a) plots the density of hourly wages in 2014, one year before the minimum wage came into effect, and in 2015 and 2016, one and two years after the minimum wage came into effect. Panel (b) shows the number of employment relationships (in thousands) by hourly wage bins, again in 2014, 2015 and 2016. Both figures are taken from the 2018 Report issued by the German Minimum Wage Commission and are calculated based on the Structure of Earnings Survey (Verdienstrhebung, VSE).

Figure A.2: The Effect of the Minimum Wage on the Hourly Wage Distribution: West vs East Germany

Notes: The figures show the density of the hourly wages one year before and two years after the introduction of the minimum wage in West Germany (top panel) and East Germany (bottom panel). Both figures are taken from the 2018 Report issued by the German Minimum Wage Commission and are calculated based on the Structure of Earnings Survey (Verdienststrukturvergleich, VSE).
Source: Report by the German Minimum Wage Commission (“Zweiter Bericht zu den Auswirkungen des gesetzlichen Mindestlohns”), 2018. Figure 2.7.
Figure A.3: Hourly Wage Distribution in 2014 (Own Calculations Based on the BEH)

Notes: The figure shows the density of hourly wages in 2014, one year before the minimum wage hike in the BEH where reported working hours have been adjusted according to the procedure described in Appendix B1.
Source: Employee Histories (Beschaftigtenhistorik) provided by the Institute for Employment Research in Nuremberg (BEH), 2014.
A. 2 Which Workers Are Affected by the Minimum Wage?

Table A.1 examines the characteristics of minimum wage workers in a regression framework. We run a regression of a dummy variable that is equal to one if the individual earns less than 8.50 EUR in 2014 on the same set of demographic, industry and establishment characteristics as displayed in Table 1 in the main paper. The multivariate regression framework shows a similar pattern as Table 1: workers residing in East Germany, non-German citizens, women, low-skilled, and marginally employed workers face an increased risk of being a minimum wage worker. Workers at smaller establishments and those who work in Transportation; Accommodation and the Food Services sector are also more exposed to the minimum wage.
Table A.1: Who are the Minimum Wage Workers?

| (1) \(4.5 < \text{Wage}_{2014} \leq 8.5\) & (2) \(4.5 < \text{Wage}_{2014} \leq 12.5\) |
|---|---|
| In East Germany & 0.115 | 0.219 |
| & (0.011) | (0.024) |
| Not German citizen & 0.043 | 0.110 |
| & (0.003) | (0.003) |
| Women & 0.039 | 0.086 |
| & (0.004) | (0.007) |
| Medium-skilled & -0.064 | -0.095 |
| & (0.002) | (0.004) |
| High-skilled & -0.097 | -0.227 |
| & (0.003) | (0.004) |
| Age 24-44 & -0.041 | -0.087 |
| & (0.001) | (0.002) |
| Age 45-59 & -0.042 | -0.094 |
| & (0.001) | (0.002) |
| Unemployed in previous year & 0.083 | 0.145 |
| & (0.001) | (0.001) |
| Part-time & 0.077 | 0.084 |
| & (0.002) | (0.005) |
| Marginally employed & 0.357 | 0.414 |
| & (0.004) | (0.005) |
| Manufacturing; Electricity; Waste Management & -0.011 | 0.051 |
| & (0.005) | (0.011) |
| Construction; Wholesale and Retail & -0.023 | 0.044 |
| & (0.006) | (0.011) |
| Transportation; Accommodation and Food Services & 0.106 | 0.226 |
| & (0.006) | (0.012) |
| Information, Communication; Finance, Insurance; Real Estate & -0.029 | 0.006 |
| & (0.007) | (0.013) |
| Professional Services; Administrative and Support Services & 0.040 | 0.240 |
| & (0.007) | (0.012) |
| Public Administration; Education; Human Health & -0.069 | 0.002 |
| & (0.006) | (0.012) |
| Arts, Entertainment; Other Services & 0.079 | 0.157 |
| & (0.007) | (0.014) |
| 5-19 employees & -0.059 | -0.093 |
| & (0.002) | (0.001) |
| 20-49 employees & -0.085 | -0.136 |
| & (0.003) | (0.002) |
| 50+ employees & -0.118 | -0.227 |
| & (0.004) | (0.004) |
| Constant & 0.208 | 0.498 |
| & (0.007) | (0.012) |

Number of observations: 7,068,718
R-squared: 0.21

Notes: This table examines the characteristics of minimum wage workers in a regression framework. In panel (a) we regress an indicator variable that is equal to one if the individual earns an hourly wage of less than 8.50 EUR in 2014 on whether or not the individual resides in East Germany; whether or not she is a German citizen; sex; education (excluded category: low-skilled); unemployment status in the previous year; current full-time status (excluded category: full-time employment); industry (excluded category: Agriculture and Mining); and establishment size (excluded category: establishments with 1-4 employees).
A. 3  Proxied Hourly Wage Changes at Different Quantiles by Initial Wage Bin

Figure A.4 shows the mean, and the 10th, 25th, 50th, 75th, and 90th percentiles of changes in proxied log hourly wages between 2014 and 2016 for each 1-Euro baseline wage bin. The black thick line shows the average wage change (in percent) that is needed for full compliance with the minimum wage, calculated as the wage growth required to achieve an hourly wage of 8.50 EUR minus the observed average growth in actual hourly wages between 2012 and 2014, thus accounting for mean reversion. The mean log hourly wage change is close to what we would expect under full compliance to the policy. Nevertheless, wage changes are below than those required for full compliance for 10th and 25th percentile changes, likely due to measurement error stemming from proxying hourly wages in the post policy years.

Figure A.4: Compliance with the Minimum Wage: Changes in Proxied Hourly Wages at Different Quantiles by Initial Wage Bin

Notes: The figure shows the 10th, 25th, 50th, 75th, and 90th percentiles as well as the mean of changes in proxied log hourly wages by baseline wage bins. The black thick line shows the wage change required for compliance with the minimum wage, calculated as the wage growth required to achieve an hourly wage of 8.50 EUR minus the observed average growth in actual hourly wages between 2012 and 2014, thus accounting for mean reversion.

Source: Employee Histories (Beschaftigtenhistorik) provided by the Institute for Employment Research in Nuremberg (BEH), 2011-2016.
A. 4 Wage Spillover Effects for Apprentices.

Figure A.5 studies whether there are any wage spillover effects on apprentices, a group that has been exempted from the minimum wage regulation but may be indirectly affected by the minimum wage. Wages of apprentices are usually set by Chambers of Commerce for a given occupation at levels considerably below those of inexperienced unskilled workers and increase over the course of the typically three-year apprenticeship program, even though it is in principle possible for firms to pay wages higher than the standard training wage. The figure plots two-year changes in proxied hourly wages by baseline wage bin relative to the 2011 vs 2012 pre-policy period, for individuals who are apprentices in both periods. There is no indication of a wage spillover effect on apprentices. This is perhaps not surprising: Given the much lower wage levels for apprentices than inexperienced unskilled workers, firms may not feel compelled to raise wages for apprentices due to fairness concerns. The absence of spillover effects for apprentices does not mean that spillover effects did not arise for other exempted groups, such as the long-term unemployed. Umkehrer and vom Berge (2020), for example, find that establishments did not apply this exemption and that the long-term unemployed experienced similar wage growth as the rest of the population.

References

Figure A.5: Proxied Hourly Wage Growth for Apprentices

Notes: The figure shows two-year changes in proxied hourly wages (in logs) by baseline wage bin for the periods 2012 vs 2014 to 2014 vs 2016, relative to the 2011 vs 2013 pre-policy period, for apprentices who are exempted from the minimum wage. The sample is restricted to apprentices aged between 16 and 25 in their first year of training at baseline who are still in apprenticeship training with the same employer two years later (apprenticeships typically last three years). The black vertical line indicates the minimum wage of 8.50 EUR per hour. We control for individual characteristics at baseline (age, education, sex, district and industry fixed effects, full-time status) and cluster standard errors at the district level.
A. 5  Reallocation Effects: Establishments’ Average Daily Wages vs Establishments’ Wage Premia (Demographic Controls Only).

Figure A.6 compares, based on the individual approach, the reallocation effects of the minimum wage for two measures of establishment quality: establishments’ average daily wages (panel (a); see also panel (a) in Figure 4 in the main paper) and establishments’ daily wage premia (panel (b)), calculated as the establishment’s average of the residuals from an individual wage regression that controls for workers’ demographic characteristics (age, sex, foreign status and education), but *not* for workers’ full-time status (full-time, part-time, marginal employment). Therefore, the establishment’s daily wage premia in panel (b) takes out differences in pay that can be attributed to differences in the demographic composition of the establishment’s workforce. The increase in establishments’ average daily wages between 2014 and 2016 (relative to 2013 and 2011) in panel (a) is around 1.8% (s.e. 0.4) for the workers earning between 4.5 EUR and 8.5 EUR, while the corresponding increase in establishments’ wage premia in panel (b) is only slightly lower (1.4%, s.e. 0.3)–highlighting that only a small fraction of the increase in establishments’ average daily wages can be attributed to low-wage workers moving to establishments with a “better” workforce. Rather, the increase in establishments’ average daily wages is mainly driven by low-wage workers moving to establishments that offer more full-time and fewer marginal jobs and pay higher wage premia to the same observable worker type.
Figure A.6: Reallocation Effects of the Minimum Wage: Establishments’ Average Daily (Log) Wages vs Establishments’ Wage Premia (Demographic Controls only)

Notes: Panel (a) shows the two-year changes in the establishments’ average log daily wages by baseline wage bin for the periods 2012 vs 2014 to 2014 vs 2016 relative to the 2011 vs 2013 pre-policy period (see also panel (a) of Figure 4 in the main paper). Panel (b) displays the two-year changes in establishments’ average daily wage premia (demographic controls only), calculated as the establishments’ average of the residuals from an individual log wage regression that controls for workers’ demographic characteristics (age, education, sex, nationality, district and industry fixed effects), but not for workers’ full-time status (full-time, part-time, marginal employment). The black vertical line indicates the minimum wage of 8.50 EUR per hour. We control for individual characteristics at baseline (age, education, sex, district and industry fixed effects, full-time status) and cluster standard errors at the district level.
A. 6  Wage, Employment and Reallocation Effects of the Minimum Wage in Areas with Stable vs Declining Unemployment Rates.

In Figures A.7, A.8 and A.9, we investigate, based on the individual approach, whether the employment, wage, and reallocation effects of the minimum wage hike differ in areas with stable and declining unemployment rates over the period 2011 to 2016. While the aggregate unemployment rate fell in Germany over this period, the decline was not equally distributed across districts. In about half of the districts, the unemployment rate remained roughly stable, changing by less than ±0.5 percentage points between 2011 and 2016. The other half of districts experienced a drop in the unemployment rate over the period of at least 0.5 (and up to 5) percentage points. Unfortunately, only few districts experienced an increase in the unemployment rate so that we cannot assess the effects of the minimum wage in a recessionary environment.

The figures highlight that the estimated employment, wage and reallocation effects are similar in stable and booming districts. Importantly, there is no indication of a displacement effect of the minimum wage in districts with either stable or declining unemployment rates. If anything, the wage effects of the minimum wage hike appear to be slightly larger in districts with declining unemployment rates, while the reallocation effects (estimated either using AKM establishment fixed effects or establishments’ average log daily wages) appear to be more prominent in districts with stable unemployment rates. Furthermore, pre-policy 2012 and 2014 changes are much smaller in magnitude than post-policy changes and very similar across the two types of districts, highlighting that wages, employment and establishments’ characteristics evolved similarly in stable and booming districts before the minimum wage hike. These results thus corroborate our conclusion that our estimated effects reflect the causal effects of the minimum wage, rather than changes in local economic conditions that differentially affect workers at the lower and upper parts of the wage distributions.
Figure A.7: Wage and Employment Effects of the Minimum Wage: Districts with Stable vs Declining Unemployment Rates

Notes: The figures show the employment and wage effects of the minimum wage in districts where the unemployment rate remained roughly stable between 2011 and 2016 (i.e., changed by less than 0.5 percentage points) and in districts where the unemployment rate declined by at least 0.5 percentage points during the same period. In panels (a) and (b) we plot two-year changes in proxied log hourly wages by baseline wage bin for the periods 2012 vs 2014 to 2014 vs 2016 relative to the 2011 vs 2013 pre-policy period. In panels (c) and (d) we plot two-year changes in the probabilities that a worker remains employed, once again relative to the 2011 vs 2013 pre-policy period. (Recall that all workers are employed at baseline). The black vertical lines indicate the minimum wage of 8.50 EUR per hour. We control for individual characteristics at baseline (age, education, sex, nationality, full-time status, district fixed effects and industry fixed effects) and cluster standard errors at the district level.
Figure A.8: Reallocation Effects of the Minimum Wage: Districts with Stable vs Declining Unemployment Rates (Part I)

(a) Stable Unemployment: Establishments’ Average Log Daily Wages

(b) Declining Unemployment: Establishments’ Average Log Daily Wages

(c) Stable Unemployment: Establishments’ Wage Premium

(d) Declining Unemployment: Establishments’ Wage Premium

(e) Stable Unemployment: Establishments’ AKM Fixed Effects

(f) Declining Unemployment: Establishments’ AKM Fixed Effects

Notes: The figures show the reallocation effects of the minimum wage in districts where the unemployment rate remained roughly stable between 2011 and 2016 (i.e., changed by less than 0.5 percentage points) and in districts where the unemployment rate declined by at least 0.5 percentage points during the same period. Panels (a) and (b) show two-year changes in the establishments’ average log daily wages by baseline wage bin for the periods 2012 vs 2014 to 2014 vs 2016 relative to the 2011 vs 2013 pre-policy period. Panels (c) and (d) display two-year changes (relative to the 2011 vs 2013 period) in establishments’ wage premium, Panels (e) and (f) show two-year changes (relative to the 2011 vs 2013 period) in AKM establishment fixed effects. We control for individual characteristics at baseline and cluster standard errors at the district level.
Figure A.9: Reallocation Effects of the Minimum Wage: Districts with Stable vs Declining Unemployment Rates (Part II)

(a) Stable Unemployment: Establishments’ Churning Rates

(b) Declining Unemployment: Establishments’ Churning Rates

(c) Stable Unemployment: Establishment Size (Log Number of Employees)

(d) Declining Unemployment: Establishment Size (Log Number of Employees)

(e) Stable Unemployment: Establishments’ Predicted Revenues per Worker

(f) Declining Unemployment: Establishments’ Predicted Revenues per Worker

Notes: The figures show the reallocation effects of the minimum wage in districts where the unemployment rate remained roughly stable between 2011 and 2016 (i.e., changed by less than 0.5 percentage points) and in districts where the unemployment rate declined by at least 0.5 percentage points during the same period. Panels (a) and (b) show two-year changes in establishments’ churning rates by wage bin for the 2012 vs 2014 to 2014 vs 2016 periods, relative to the 2011 vs 2013 period. Panels (c) and (d) display two-year changes in establishment size; panels (e) and (f) show two-year changes in establishments’ predicted productivities (log revenues per worker; see Appendix B.4 for details). We control for individual characteristics at baseline and cluster standard errors at the district level.
A. 7 Wage, Employment and Reallocation Effects of the Minimum Wage in Areas Barely and Heavily Exposed to the Minimum Wage.

In Figures A.10, A.11, and A.12, we study, based on the individual approach, the wage, employment and reallocation effects of the minimum wage by district-level exposure to the minimum wage. The left panels show the effects of the minimum wage hike in districts with below median exposure to the policy, while the right panels show the effects in districts with above median exposure, measured by the districts’ gap measure as defined in equation (3) in the paper. The figures highlight that the estimated employment, wage and reallocation effects are similar in districts more or less exposed to the minimum wage. Importantly, there is no indication of a displacement effect in either type of district. If anything, wage effects appear to be larger in districts with higher exposure to the minimum wage. The estimated reallocation effects are similar in the two types of districts, with the exception of the lowest wage bin of between 4.5 EUR and 6.5 EUR for which reallocation responses are more muted in low-exposure districts. This latter finding may reflect a higher margin of measurement error in the hourly wage variable for very low-wage workers in barely exposed districts.
Figure A.10: Wage and Employment Effects of the Minimum Wage: Districts with Low vs High Exposure to the Minimum Wage

Notes: The figures show the employment and wage effects of the minimum wage in districts with low (below median GAP measure) and high (above median GAP measure) exposure to the minimum wage. In panels (a) and (b) we plot two-year changes in proxied log hourly wages by baseline wage bin for the periods 2012 vs 2014 to 2014 vs 2016 relative to the 2011 vs 2013 pre-policy period. In panels (c) and (d) we plot two-year changes in the probabilities that a worker remains employed by baseline wage bin, once again relative to the 2011 vs 2013 pre-policy period. (Recall that all workers are employed at baseline). The black vertical lines indicate the minimum wage of 8.50 EUR per hour. We control for individual characteristics at baseline and cluster standard errors at the district level.
Figure A.11: Reallocation Effects of the Minimum Wage: Districts with Low vs High Exposure to the Minimum Wage (Part I)

Notes: The figures show the reallocation effects of the minimum wage in districts with low (below median GAP measure) and high (above median GAP measure) exposure to the minimum wage. Panels (a) and (b) show two-year changes in the establishments’ average log daily wages by baseline wage bin for the periods 2012 vs 2014 to 2014 vs 2016 relative to the 2011 vs 2013 pre-policy period. Panels (c) and (d) display two-year changes (relative to the 2011 vs 2013 period) in establishments’ wage premium. Panels (e) and (f) show two-year changes (relative to the 2011 vs 2013 period) in AKM establishment fixed effects. We control for individual characteristics at baseline (age, education, sex, nationality, full-time status, district fixed effects and industry fixed effects) and cluster standard errors at the district level.
Figure A.12: Reallocation Effects of the Minimum Wage: Districts with Low vs High Exposure to the Minimum Wage (Part II)

Notes: The figures show the reallocation effects of the minimum wage in districts with low (below median GAP measure) and high (above median GAP measure) exposure to the minimum wage. Panels (a) and (b) show two-year changes in establishments’ churning rates by wage bin for the 2012 vs 2014 vs 2016 periods, relative to the 2011 vs 2013 period. Panels (c) and (d) display two-year changes (relative to the 2011 vs 2013 period) in establishment size; panels (e) and (f) show two-year changes in establishments’ predicted productivities (log revenues per worker; see Appendix B.4 for details). We control for individual characteristics at baseline and cluster standard errors at the district level.
A. 8 One-Year Changes in Proxied Hourly Wages, Employment and Reallocation.

In Figures A.13 and A.14 we assess, based on the individual approach, the impact of the wage, employment and reallocation effects of the minimum wage by studying one-year changes in outcomes, rather than two-year changes as in our baseline specification. In particular, we estimate the following regression equation:

$$ y_{it} - y_{i,t-1} = \sum_k (1[b_{k-1} < w_{it-1} \leq b_k] \gamma_{k,2012} + 1[b_{k-1} < w_{it-1} \leq b_k] \delta_{kt}) + \beta X_{i,t-1} + e_{it} \quad (A.1) $$

where, similarly to the two-year analysis, we calculate the one-year changes by taking differences in outcome variables measured as of June 30th in years $t$ and $t - 1$. For instance, we calculate the change in establishment’s quality as $q_{j(t,t)}^{l=t-1} - q_{j(t,t-1)}^{l=t-1}$. The parameters $\gamma_{k,2012}$ measure one-year changes in outcomes by baseline wage bin in the pre-policy 2011-2012 period (the baseline period), while the parameters $\delta_{kt}$ capture one-year changes in outcomes by wage bin relative to the 2011-2012 baseline period. The advantage of studying one-year as opposed to two-year changes is that we have two, instead of only one, pre-policy periods (relative to the 2011 vs 2012 baseline period) to assess pre-policy trends. A disadvantage is that we can study the effects of the minimum wage hike only one year after the minimum wage came into effect. Parameter estimates for the 2012 vs 2013 period ($\delta_{k,2013}$) arguably serve as a placebo, whereas parameter estimates for the 2013 vs 2014 period potentially capture anticipation effects of the policy. The German government announced the introduction of the minimum wage on April 2, 2014 (see Bossler, 2020) and hence by June 30, 2014 firms and workers were aware of the policy change that became effective on January 1st, 2015. Overall, results based on one-year changes confirm our findings from the two-year benchmark analysis. For all outcomes, post-policy coefficients (the blue line) clearly exceed coefficients for both pre-policy periods (the green and red lines). Coefficient estimates for both pre-policy periods tend to be close to zero, as we would expect in the absence of pre-trends.

References

Figure A.13: Employment and Wage Effects of the Minimum Wage: One-year Changes

Notes: The figures show the wage and employment effects of the minimum wage focusing on one-year (as opposed two-year) changes in hourly wages and employment. In panel (a) we plot one-year changes in proxied log hourly wages by baseline wage bin for the periods 2012 vs 2013 to 2014 vs 2015 relative to the 2011 vs 2012 pre-policy period. In panel (b) we plot one-year changes in the probabilities that a worker remains employed by baseline wage bin, once again relative to the 2011 vs 2012 pre-policy period. (Recall that all workers are employed at baseline). The black vertical lines indicate the minimum wage of 8.50 EUR per hour. We control for individual characteristics at baseline (age, education, sex, nationality, full-time status, district fixed effects and industry fixed effects) and cluster the standard errors at the district level.
Figure A.14: Reallocation Effects of the Minimum Wage: One-Year Changes

Notes: The figures show the reallocation effects of the minimum wage focusing on one-year (as opposed to two-year) changes in establishment outcomes. Panel (a) shows one-year changes in the establishments’ average log daily wages by baseline wage bin for the periods 2012 vs 2013 to 2014 vs 2015 relative to the 2011 vs 2012 pre-policy period. Panel (b) displays one-year changes (relative to the 2011 vs 2012 period) in establishments’ average daily wage premium, calculated as the establishments’ average of the residuals from an individual log wage regression that controls for workers’ demographic characteristics (age, sex, nationality and education) and workers’ full-time status (full-time, part-time, marginal employment). Panel (d) shows one-year changes in establishment size; panel (e) in churning rates (the combined number of workers who leave and join the establishment, divided by the number of employees at baseline) and panel (f) in establishments’ predicted productivities (log revenues per worker; see Appendix B.4 for details). We control for individual characteristics at baseline (age, education, sex, nationality, full-time status, district fixed effects and industry fixed effects) and cluster standard errors at the district level.
A. 9  Relationship Between Individual Daily Wage Changes and Changes in Establishments’ Average Daily Wages.

Table A.2 studies the relationship between changes in individual daily wages and changes in establishments’ average daily wages. We use this estimated relationship to assess the daily wage growth due to reallocation (in percent of total increase in individual daily wages) in Table 3 in the main paper.

In panel A of Table A.2, we estimate the following regression for workers who switch establishments between 2011 and 2013:

$$y_{it} - y_{i,t-2} = \alpha + \beta \left( q_{l}^{i,l(t)} - q_{l}^{i,l(t-2)} \right) + \epsilon_{it} \quad \text{(A.2)}$$

where $y_{it} - y_{i,t-2}$ is the change in average log daily wages for establishment switcher $i$ and $q_{l}^{i,l(t)} - q_{l}^{i,l(t-2)}$ is the difference between the new and old establishment’s average log daily wage, measured in period $t$ for the new establishment and in period $t-2$ for the old establishment.

In our baseline analysis, our measures for establishment quality refer to the base period $t-2$ for both new and old establishments, to ensure that we pick up movements to establishments that were of higher quality already before the introduction of the minimum wage as opposed to within-establishment changes in quality possibly induced by the minimum wage. In panel B of Table A.2, we therefore regress changes in daily log wages of establishment switchers on changes in establishments’ average log daily wages, now measured in period $t-2$ for both the old and the new establishment:

$$y_{it} - y_{i,t-2} = \alpha + \beta \left( q_{l}^{i,l(t-2)} - q_{l}^{i,l(t-2)} \right) + \epsilon_{it} \quad \text{(A.3)}$$

Column (1) shows the estimates for all establishment switchers in our sample whose hourly wage at baseline was between 4.5 EUR and 20.5 EUR while column (2) shows estimates for low-wage establishment switchers who earned less than 8.50 EUR at baseline and are the main targets of the minimum wage. Since the minimum wage primarily induces upgrading to “better” establishments, column (3) additionally restricts the sample to switchers who moved to an establishment with higher average daily wages. Estimates in both columns (1) and (2) of panel A show that workers
who move to an establishment paying 10% higher average daily wages experience a 7.5% increase in their individual’s daily wage on average. The relationship is even stronger when we focus on individuals who moved to establishments with higher average daily wages in column (3). Estimates are similar in magnitude when we measure establishments’ average daily wages at the base period for both the new and old establishment in panel B.

In Table A.3, we repeat the analysis but now regress changes in (proxied) log hourly wages on changes in establishments’ hourly wage premia, calculated as the establishment’s average of the residuals from an individual wage regression that controls for workers’ demographic characteristics (age, sex, nationality and education) as well as for workers’ full-time status (full-time, part-time, marginal employment). Columns (1) and (2) show that switching to an establishment paying a 10% higher hourly wage premium leads to a 6.1% increase in (proxied) hourly wages. The effects are similar if we measure the hourly wage premium of the new establishment in the current (panel A) or baseline period (panel B), or if we restrict our sample to low-wage workers earning less than 8.5 EUR at baseline. As in Table A.2, coefficient estimates are larger when we focus on individuals who moved to establishments that pay a higher hourly wage premium (column (3)).
Table A.2: Changes in Individual (Log) Daily Wages and Changes in Establishments’ Average (Log) Daily Wages (Pre-Policy Establishment Switchers)

<table>
<thead>
<tr>
<th>Panel A: Change in establishment’s quality</th>
<th>Change in the switcher’s daily wage</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>New establishment’s daily wage minus old</td>
<td>0.74</td>
</tr>
<tr>
<td>establishment’s daily wage</td>
<td>(0.003)</td>
</tr>
</tbody>
</table>

Panel B: Change in establishment’s quality (measured using only the pre-period)

| New establishment’s daily wage minus old | 0.72 | 0.73 | 0.87 |
| establishment’s daily wage               | (0.003) | (0.005) | (0.008) |

Baseline wage is between 4.5 and 20.5 4.5 and 8.5 4.5 and 8.5
Movers All All only upgrade

Notes: The table shows the relationship between changes in individual log daily wages and changes in establishments’ average log daily wages for workers who switch establishments between 2011 and 2013. In panel A, the change in establishments’ average log daily wages for establishment switchers is measured as the difference between the new and old establishments average log daily wage, measured in period $t$ for the new establishment and in period $t-2$ for the old establishment (formally $q_{l(j,t)}^t - q_{l(j,t-2)}^t$). In panel B, establishments’ average log daily wages refer to the base period $t-2$ for both the new and the old establishment (formally $q_{l(j,t-2)}^{t-2} - q_{l(j,t-2)}^{t-2}$). Column (1) shows the estimates for all establishment switchers in our sample whose hourly wage at baseline was between 4.5 and 20.5 euros, while column (2) shows estimates for low-wage establishment switchers who earned less than 8.50 EUR at baseline. Column (3) additionally restricts the sample to switchers who moved to an establishment with higher average daily wage.
Table A.3: Change in Establishment’s Wage Premium and the Change in (proxied) Hourly Wages Among Pre-Policy Switchers

<table>
<thead>
<tr>
<th></th>
<th>Change in the switcher’s proxied hourly wage</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td><strong>Panel A: Change in establishment’s quality</strong></td>
<td></td>
</tr>
<tr>
<td>New establishment’s wage premium</td>
<td>0.65</td>
</tr>
<tr>
<td>minus old establishment’s wage</td>
<td>(0.004)</td>
</tr>
<tr>
<td>premium</td>
<td></td>
</tr>
<tr>
<td><strong>Panel B: Change in establishment’s quality (measured using only the pre-period)</strong></td>
<td></td>
</tr>
<tr>
<td>New establishment’s wage premium</td>
<td>0.61</td>
</tr>
<tr>
<td>minus old establishment’s wage</td>
<td>(0.004)</td>
</tr>
<tr>
<td>premium</td>
<td></td>
</tr>
<tr>
<td>Baseline wage is between</td>
<td>4.5 and 20.5</td>
</tr>
<tr>
<td>Movers</td>
<td>All</td>
</tr>
</tbody>
</table>

Notes: The table show the relationship between changes in (proxied) hourly wages and changes in establishment’s wage premium, which is calculated as the average wage residual in the establishment obtained from an individual wage regression that controls for workers’ demographic characteristics (age, sex, nationality and education) and workers’ full-time status (full-time, part-time, marginal employment). We estimate this relationship for workers who switch an establishment between 2011 and 2013. In particular, the table reports $\beta$ coefficients from equation A.2. Panel A reports results where the change in establishment quality is measured as the difference between the wage premium at the new establishment and the wage premium at the old establishment, formally, $q_{l(t)} = j(i,t) - q_{l(t-2)}$. Panel B uses an alternative quality measure which measures the change in quality using the baseline wage premium at the new establishment, formally, $q_{l(t)} = j(i,t) - q_{l(t-2)}$. Column (1) shows the estimates for establishment switchers whose hourly wage was between 4.5 and 20.5 euros in the baseline, while column (2) shows estimates for low-wage switchers. Column (3) reports estimates when we restrict the sample to low-wage switchers who moved to better quality establishments.
A. 10 Relationship Between Individual Proxied Hourly Wage Changes and Changes in Establishments’ AKM Establishment Fixed Effects

In Table A.4, we repeat the exercise described in Section A.9, but now regress changes in individual (proxied) log hourly wages on changes in AKM establishment fixed effects, focusing once again on workers who switch establishments between 2011 and 2013. Since AKM establishment fixed effects are estimated on full-time workers, we restrict the sample to full-time workers. In panel A, AKM establishment fixed effects are estimated over the \( t - 8 \) to \( t - 2 \) period for the old establishment, and the \( t - 6 \) to \( t \) period for the new establishment, whereas AKM establishment fixed effects are estimated over the \( t - 8 \) to \( t - 2 \) period for both the old and the new establishment in panel B. Columns (1), (3) and (5) show the OLS relationship between changes in AKM establishment fixed effects and changes in individual proxied hourly log wages for all establishment switchers (column (1)), low-wage establishment switchers (column (3)) and low-wage workers who upgrade to an establishment with a higher AKM establishment fixed effect (column (5)). It is well understood that these AKM establishment effects are noisily estimated (see e.g. Kline et al., 2020 or Bonhomme et al., 2020) and consequently, the OLS estimates in equation A.4 suffer from attenuation bias. To deal with measurement error in the AKM establishment fixed effects, we implement a split sample IV as in Goldschmidt and Schmieder (2017, see Figure A-8 in their Online Appendix). We divide workers into two equally sized random samples and then estimate separate AKM establishment fixed effects in the two samples, which we label AKM1 and AKM2. We then use changes in establishment fixed effects based on the AKM2 sample as instruments for changes in establishment fixed effects in the AKM1 sample. The 2SLS estimates are shown in Columns (2), (4) and (6).

2SLS estimates in panel A indicate that a 10% change in AKM establishment fixed effects is associated with a change in proxied log hourly wages of roughly the same magnitude, whether or not we focus on all establishment switchers in our sample (column (2)) or low-wage establishment switchers (column (3)). This one-to-one relationship between changes in AKM establishment fixed effects and changes in individual wages is consistent with the previous findings in Germany (Card et al., 2013) and in the United States (Sorkin, 2018). Here, we show that the one-to-one relationship also holds for low-wage switchers.

When we estimate AKM establishment fixed effects over the \( t - 8 \) vs \( t - 2 \) period for both
the old and the new establishment in panel B, we obtain somewhat smaller estimates of 0.85 for all establishment switchers (column (2)) and 0.80 for low-wage establishment switchers (column (4)). We however obtain an estimate of larger than one when we restrict the sample to low-wage switchers who move to an establishment with a higher AKM establishment fixed effect (column (6)).

References


Table A.4: Changes in Individual Proxied (Log) Hourly Wages and Changes in Establishments’ AKM Fixed Effects (Pre-Policy Establishment Switchers)

<table>
<thead>
<tr>
<th>Panel A: Change in establishment’s quality</th>
<th>Change in the switcher’s proxied hourly wages</th>
<th>(1) OLS</th>
<th>(2) 2SLS</th>
<th>(3) OLS</th>
<th>(4) 2SLS</th>
<th>(5) OLS</th>
<th>(6) 2SLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>New establishment’s AKM minus old establishment’s AKM</td>
<td>0.90</td>
<td>1.02</td>
<td>0.89</td>
<td>1.00</td>
<td>1.05</td>
<td>1.31</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.010)</td>
<td>(0.012)</td>
<td>(0.011)</td>
<td>(0.014)</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Change in establishment’s quality (measured using only the pre-period)</th>
<th>Change in the switcher’s proxied hourly wages</th>
<th>(1) OLS</th>
<th>(2) 2SLS</th>
<th>(3) OLS</th>
<th>(4) 2SLS</th>
<th>(5) OLS</th>
<th>(6) 2SLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>New establishment’s AKM minus old establishment’s AKM</td>
<td>0.65</td>
<td>0.85</td>
<td>0.61</td>
<td>0.80</td>
<td>0.82</td>
<td>1.13</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.006)</td>
<td>(0.009)</td>
<td>(0.012)</td>
<td>(0.010)</td>
<td>(0.016)</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Baseline wage is between</th>
<th>4.5 &amp; 20.5</th>
<th>4.5 &amp; 20.5</th>
<th>4.5 &amp; 8.5</th>
<th>4.5 &amp; 8.5</th>
<th>4.5 &amp; 8.5</th>
<th>4.5 &amp; 8.5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Movers</td>
<td>All</td>
<td>All</td>
<td>All</td>
<td>All</td>
<td>upgraders</td>
<td>upgraders</td>
</tr>
</tbody>
</table>

Notes: The table shows the relationship between individual changes in proxied log hourly wages and changes in establishments’ AKM fixed effects for workers who switch establishments between 2011 and 2013. In panel A, AKM establishment fixed effects are estimated over the t-8 to t-2 period for the old establishment, and the t-6 to t period for the new establishment. In panel B, AKM establishment fixed effects are estimated over the t-8 to t-2 period for both the old and the new establishment. Columns (1), (3) and (5) show the OLS relationship between changes in AKM establishment fixed effects and changes in individual proxied hourly (log) wages for all establishment switchers (column (1)), low-wage establishment switchers (column (3)) and low-wage workers who upgrade to an establishment with a higher AKM establishment fixed effect (column (5)). Columns (2), (4) and (6) show corresponding split sample IV estimates where workers are divided into two equally sized random samples and use changes in AKM establishment fixed effects based on one sample as instrument for changes in AKM establishment fixed effects in the other sample.
A. 11 Alternative Sample Restrictions: Individual Approach

In our baseline analysis we drop workers older than 60, and hence are close to retirement, from the analysis. In Figure A.15, we report, based on the individual approach, wage, employment and reallocation effects of the minimum wage when workers aged between 60 and 65 are included in the analysis. Estimates are very similar to our baseline estimates.

We further focus, for multiple job holders, on the main job. The main job is defined as the job with the highest daily wage or, in case of multiple jobs with identical wage, the job with the highest full-time status. In Figure A.16, we report wage, employment and reallocation effects of the minimum wage when we instead focus, for multiple job holders, on the least paying, rather than the highest paying job. Once again, estimates are very similar to our baseline estimates.

When the minimum wage was introduced, the government implemented temporary exemptions for workers in some specific industries, all of which were phased out by 2016. The exempted industries were: 1 "Hairdressing" 2 "Temporary Employment Agencies (East only)" 3 "Meat Industry" 4 "Agriculture including Gardening" 5 "Textile (East only)". As expected, exempted industries are low-wage industries. Around 39% of all workers in exempted industries earned less than 8.50 EUR per hour in 2013, compared to 13.5% in the population as a whole. Hence, while only 3% of all employment spells in our sample are in exempted industries, the share of minimum wage workers working in these industries is larger (5.3%). In our benchmark individual analysis, we include workers in exempted industries in our sample (both in the baseline period t-2 and in the current period t). In Figure A.17, we report, based on the individual approach, wage, employment and reallocation effects of the minimum wage when we instead drop all employment spells in exempted industries (both in the baseline and current period). Once again, estimates are very similar to our baseline estimates.
Figure A.15: Employment, Wage, and Reallocation Effect of the Minimum Wage: Including Older Workers

Notes: The figures examine the employment, wage and reallocation effects of the minimum wage when older workers aged between 60 and 65 at baseline are also included in the sample. Estimates refer to coefficients $\delta_{kt}$ in regression equation (2) in the main paper. Panel (a) shows two-year changes in proxied log hourly wages by baseline wage bin for the 2012 vs 2014 to the 2014 vs 2016 periods, relative to the 2011 vs 2013 pre-policy period. Panel (b) shows two-year changes in the probability that a worker remains employed, once again relative to the 2011 vs 2013 pre-policy period. (Recall that all workers are employed at baseline). Panels (c) and (d) show two-year changes in establishments' average log daily wages and establishments' AKM fixed effects by baseline wage bin, while panels (e) and (f) display two-year changes in establishments' churning rates and in establishments' predicted productivities (log revenues per worker, see Appendix B.4 for details). The black vertical lines indicate the minimum wage of 8.50 EUR per hour. We control for individual characteristics at baseline and cluster the standard errors at the district level.
Figure A.16: Employment, Wage, and Reallocation Effect of the Minimum Wage: Keeping the Least Paying Job

Notes: The figures examine the employment, wage and reallocation effects of the minimum wage when we keep the least paying job (as opposed to the highest paying job as in our baseline sample) for multiple job holders. Estimates refer to coefficients $\delta_{kt}$ in regression equation (2) in the main paper. Panel (a) shows two-year changes in proxied log hourly wages by baseline wage bin for the 2012 vs 2014 to the 2014 vs 2016 periods, relative to the 2011 vs 2013 pre-policy period. Panel (b) shows two-year changes in the probabilities that a worker remains employed by baseline wage bin, once again relative to the 2011 vs 2013 pre-policy period. (Recall that all workers are employed at baseline). Panels (c) and (d) show two-year changes in establishments’ average log daily wages and establishments’ AKM fixed effects, while panels (e) and (f) display two-year changes in establishments’ churning rates and in establishments’ predicted productivities (log revenues per worker, see Appendix B.4 for details). The black vertical lines indicate the minimum wage of 8.50 EUR per hour. We control for individual characteristics at baseline and cluster the standard errors at the district level.
Figure A.17: Employment, Wage, and Reallocation Effect of the Minimum Wage: Dropping Exempted Industries

Notes: The figures examine the employment, wage and reallocation effects of the minimum wage when we drop industries that were temporarily exempt from the minimum wage from the sample. Estimates refer to coefficients $\delta_{kt}$ in regression equation (2) in the main paper. Panel (a) shows two-year changes in proxied log hourly wages by baseline wage bin for the 2012 vs 2014 to the 2014 vs 2016 periods, relative to the 2011 vs 2013 pre-policy period. Panel (b) shows two-year changes in the probabilities that a worker remains employed by baseline wage bin, once again relative to the 2011 vs 2013 pre-policy period. (Recall that all workers are employed at baseline). Panels (c) and (d) show two-year changes in establishments’ average log daily wages and establishments’ AKM fixed effects, while panels (e) and (f) display two-year changes in establishments’ churning rates (the combined number of workers who leave and join the establishment, divided by the number of employees at baseline) and in establishments’ predicted log productivities (log revenues per worker, see Appendix B.4 for details). We control for individual characteristics at baseline and cluster the standard errors at the district level.
A. 12 Districts versus Commuting Zones: Regional Approach

We conduct the regional analysis at the district level, distinguishing between 401 districts in Germany. This regional unit may be too small to correspond to a “local labor market”. We have therefore repeated the regional analysis at the commuting zone level which may better capture the idea of a local labor market, distinguishing between 141 commuting zones. We report wage and employment effects of the minimum wage in Figure A.18 and reallocation effects in Figure A.19. Reassuringly, estimated effects are similar regardless of whether we conduct the regional analysis at the district or commuting zone level.
Figure A.18: Wage and Employment Effects of the Minimum Wage: Regional Approach (141 Commuting Zones)

Notes: Panels (a) and (c) trace out how average proxied log hourly wages in the the commuting zone (panel a) and log employment (the log number of employed workers, panel c) evolve in commuting zones differentially exposed to the minimum wage, relative to the pre-policy year 2014 (the dashed blue line). Plotted effects refer to coefficients $\gamma_\tau$ in regression equation (4). The figures also plot a linear time trend estimated for the 2011-2014 pre-policy years and then updated for later years (the solid black line). Panels (b) and (d) display the deviations between the coefficient estimates and the linear time trend.
Figure A.19: Reallocation Effects of the Minimum Wage: Regional Approach (141 Commuting Zones)

Notes: This figure depicts the de-trended relationship between commuting zone-level exposure to the minimum wage, measured by the gap measure as in equation (3) in the main text, and the (log) number of establishments (panel (a)); the (log) number of small businesses with 1 or 2 employees (panel (b)); average establishment size (in logs, panel (c)); average AKM establishment fixed effects (estimated over the 7-year t-8 to t-2 window for year t), panel (d)); the (log) of the number of exiting small businesses (panel (e)), and the average predicted log establishment productivity (log predicted revenues per worker, see Appendix B.4 for details) in the commuting zone. We plot the deviations between coefficients $\gamma_\tau$ in regression equation (4) in the main text and the linear time trend estimated for the 2011-2014 pre-policy period and updated for later years.
A. 13 Minimum Wages and the Sorting of Workers to Establishments: Regional Approach

In Figure A.20, we investigate, using the regional approach, whether the minimum wage affected the sorting of workers to establishments. To measure assortativity between workers and establishments, we calculate the co-variance between the worker and establishment fixed effects obtained from AKM regressions, estimated over 7-year rolling windows (i.e., 2010-2016 for the year 2016, 2009-2015 for the year 2015, etc.) We then apply the regional approach (conducted at the district level) and estimate equation (4) in the main paper using the co-variance as the dependent variable. The figure plots the deviations between the estimates of $\gamma_T$ and the linear time trend estimated for the pre-policy years and updated for the post-policy years. The figure suggests that assortativity declines in more relative to less exposed districts after the introduction of the minimum wage: two years after the introduction of the minimum wage, an increase in the local gap measure of 0.01 (which corresponds to an increase of about one standard deviation) results in a decline in the co-variance between worker and establishment fixed effects by about 0.008.
Figure A.20: Worker-Establishment Assortativity and Exposure to the Minimum Wage

Notes: The figure depicts the de-trended relationship between district-level exposure to the minimum wage, measured by the gap measure as in equation (3) in the main text, and the covariance between worker and establishment AKM fixed effects in the district. AKM establishment and worker fixed effects are estimated over 7-year t-6 to t window for year t. We plot the deviations between coefficients $\gamma_2$ in regression equation (4) in the main text and the linear time trend estimated for the 2011-2014 pre-policy period and updated for later years.
A. 14 Assessing the Parallel Trend Assumption in the Regional Approach

Panels (a) and (c) in Figure 7 in the main paper display a pre-trend in local employment in more vs less exposed districts prior to the introduction of the minimum wage that appears to be roughly linear. Next, we adopt the “honest approach” to parallel trends proposed by Rambachana and Roth (2019) to probe the robustness of our finding of no minimum-wage induced disemployment effects to alternative assumptions about different trends in more versus less exposed districts. In this approach, one must only impose restrictions on the possible differences in trends between treated and control groups. We bound the change in slope of the differential trend between treated and control groups between two periods using the following formula (see equation 7 in their paper):

\[
\Delta^{SD} := \{ \delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq M, \forall t \} \tag{A.4}
\]

where \( \delta_t \) refers to the difference in trends between the treated and untreated groups at time \( t \). In this formula, \( M \) governs the maximum possible error of the linear extrapolation. In the special case where \( M = 0, \Delta^{SD}(0) \), the difference in trends between treated and control groups, is exactly linear. Unfortunately, nothing in the data itself can place an upper bound on the parameter \( M \) (see the discussion in Rambachana and Roth, 2019). To pick a value for \( M \), we use the point estimates and the variance-covariance matrix in the pre-policy period. Assuming that error terms are jointly normally distributed, we calculate the median of average (absolute) deviations from the trend in the pre-policy period.

For employment, this method leads to a value of \( M = 0.1058 \). Such a value is large economically. Since the estimated slope of the local linear trend in employment in panel (c) of Figure 7 is 0.52, a value of \( M = 0.1058 \) allows for a ±20% deviation from that trend line per year—which accumulates to a ±80% deviation after four years. The employment estimates are summarized in panel B of Figure A.21. For completeness, we also report estimates for wages in panel (a) of the figure. Here, the above method leads to a value of \( M = 0.065 \). Once again, this is a conservative estimate, taking into account that panel (a) of Figure 7 provides little indication that more and less exposed districts experienced differential trends in local wages prior to the introduction of the minimum wage.

Figure A.21 highlights that as the maximum deviation from the linear trend, \( M \), increases, the confidence intervals around the estimates become wider, as expected. Nevertheless, panel
(a) clearly shows a significantly positive wage effect of the minimum wage even for the most conservative value for $M$, with the lower bound of the confidence interval equal to 0.35, indicating that an increase in the gap measure by 0.01 (which corresponds to an increase of about one standard deviation) results in an increase in the local wage by 0.35%. Turning to the employment effects in panel B, even for the most conservative value of $M$ we can rule out that local employment decreases by more than 0.7% in response to an increase in the gap measure by 0.01. Scaling by the baseline wage response (local wages increase by 0.85% in response to an increase in the gap measure by 0.01, Table 7), we obtain a lower bound employment elasticity with respect to the own wage of $-0.007/0.0085=-0.81$. While this is not a small elasticity, our robust confidence intervals rule out an employment elasticity with respect to own wages of -1—a value considered to be reasonable by for example Neumark and Wascher (2010).

We would like to emphasize that a value of $M = 0.1058$ is conservative. If we pick less conservative and empirically more plausible values for $M$, for example $M = 0.03$ corresponding to a 6% deviation from the linear trend per year, we can rule out employment declines by more than 0.38% in response to an increase in the gap measure by 0.01 according to the lower bound of the robust confidence interval—which, if scaled by the baseline wage response of 0.85, implies a labor demand elasticity of -0.4.

**References**


Figure A.21: Sensitivity Estimates on Employment and Wages Based on Rambachana and Roth (2019)

Notes: We implement the Rambachana and Roth (2019) and calculate the confidence set for various values of $M$, where $M$ is the bounds on change in the slope (see equation A.4).
A. 15 Reallocation to Establishments Less Exposed to the Minimum Wage (Individual Approach)

Figure A.22 studies whether the minimum wage leads to reallocation of low-wage workers away from establishments heavily exposed to establishments less exposed to the minimum wage. We measure an establishment’s exposure to the minimum wage first as the fraction of workers in the establishment earning less than the minimum wage (panel (a)). Our preferred measure for the establishment’s exposure to the minimum wage is the establishment’s average wage gap, calculated according to equation (C.1) in Appendix C (panel (b)).

Figure A.22 displays two-year changes in the fraction of minimum wage workers in the establishment (panel (a)), and two-year changes in the establishment’s gap measure (panel (b)) by baseline wage bin for the periods 2012-2014 to 2014-2016, relative to the 2011-2013 pre-policy period. In both panels, establishments’ exposure to the minimum wage is measured at the base period \( t - 2 \) for both the current and previous establishment. The figures show a clear reallocation of workers away from establishments heavily affected by the minimum wage. When we use the establishment’s fraction of workers earning less than the minimum wage as a measure for exposure (panel (a)), some of the reallocation seems to take place already in 2014 when the minimum wage had been announced but had not yet come into effect. This may reflect an anticipation effect: establishments may increase wages of workers below but close to the minimum wage already after the announcement of the minimum wage in compliance with the minimum wage law. Such increases may have a large effect on the establishments’ fraction of workers below the minimum wage, but little effect on the establishment’s wage gap. Reassuringly, coefficient estimates for the 2012-2014 period (red coefficients) are closer to zero when we use the wage gap to measure exposure (panel (b)).
Figure A.22: Reallocation Effects of the Minimum Wage: Establishments’ Exposure to the Minimum Wage

(a) Share of Workers Earning Less than the Minimum Wage in the Establishment

(b) Establishments’ Gap Measure

Notes: The figures investigate the reallocation of workers to establishments less exposed to the minimum wage following the minimum wage hike. Panel (a) displays two-year changes in the establishment’s share of workers earning a wage less than the minimum wage at baseline by baseline wage bin for the 2012 vs 2014 to 2014 vs 2016 periods, relative to the 2011 vs 2013 pre-policy period. Panel (b) displays two-year changes (relative to the 2011-2013 pre-policy period) in the establishment’s gap measure; see Appendix C, equation C.1 for the formula. Estimates refer to coefficients $\delta_{it}$ in regression equation (2) in the main paper. The black vertical line indicates the minimum wage of 8.50 EUR per hour. We control for individual characteristics at baseline (age, education, sex, nationality, full-time status, district fixed effects and industry fixed effects) and cluster standard errors at the district level.
A. 16 Heterogeneous Wage, Employment and Reallocation Effects of the Minimum Wage (Individual Approach)

Figure A.23 explores, based on the individual approach, heterogeneity in the wage and employment effects of the minimum wage. Reported estimates refer to our baseline difference-in-difference estimates that compare excess wage growth and excess employment changes (relative to the 2011 vs 2013 pre-policy period) of workers who earned less than the minimum wage of 8.50 EUR and more than 12.50 EUR at baseline, as in column (4) of Table 2. The estimates reveal that low-wage women experience smaller wage gains and smaller employment gains following the increase in the minimum wage than low-wage men (rows 2 and 3). While wage gains following the minimum wage appear to be larger among East than West Germans, both experience similar employment gains (rows 4 and 5). Similarly, wage gains are somewhat higher for workers who were employed in districts with high exposure to the minimum wage, employment gains are similar in districts more or less exposed to the minimum wage (rows 6 and 7). While wage gains vary somewhat across sectors, employment gains are similar across sectors (rows 8 to 15). Importantly, there is no evidence for a displacement effect of the minimum wage for any of the subgroups considered.

Figure A.24 explores, based on the individual approach, heterogeneity in the reallocation effects of the minimum wage, using establishments’ average daily wages (panel (a)) and establishments’ AKM fixed effects (panel (b)) as measures for quality. Reported estimates refer once again to our baseline difference-in-difference estimates that compare excess changes in establishment quality (relative to the 2011 vs 2013 pre-policy period) of workers who earned less than the minimum wage of 8.50 EUR and more than 12.50 EUR at baseline, as in column (1) of Table 4. The patterns of heterogeneity are very similar across the two measures of establishment quality. Reallocation effects seem to be more pronounced for women, and for workers in the non-tradable sectors such as retail and food and hotels. The reallocation effects are also particularly large in FCSL (food, cleaning, security and logistics services) occupations that tend to be the target of domestic outsourcing (see Goldschmidt and Schmieder, 2017). This latter finding suggests that minimum wages can potentially reverse trends in domestic outsourcing, as the benefits of outsourcing to lower wage establishments are more limited when minimum wages are higher.

References

Figure A.23: Heterogeneity of Employment and Wage Responses

Notes: The figures show heterogeneous effects of the minimum wage on hourly wages (panel a) and employment (panel b). Row (1) shows the benchmark estimate when all workers are included in the sample (as in Table 3). In rows (2) and (3), the sample is split into men and women, respectively. Rows (4)-(5) show the wage and employment effects of the minimum wage separately for West and East Germany, while rows (6) and (7) report effects separately for districts with below and above median exposure to the minimum wage. Rows (8) and (9) display wage and employment effects separately for workers who were employed in the tradable and in non-tradable sectors at baseline (see Dauth et al. (2017) for a definition of tradable vs non-tradable industries). Rows (10)-(14) display the wage and employment effects of the minimum wage separately for workers employed in five different industries at baseline. Rows (15) reports estimates for workers in FCSL occupations (food, cleaning, security and logistics services) that tend to be the target of domestic outsourcing (see Goldschmidt and Schmieder, 2017). Estimates refer to our baseline difference-in-differences estimates that compare excess wage growth and excess employment changes (relative to the 2011 vs 2013 pre-policy period) of workers who earned less than the minimum wage of 8.50 EUR and workers who earned more than 12.50 EUR at baseline (as in column (4) of Table 2). All regressions control for individual characteristics at baseline (age, education, sex, nationality, full-time status, district fixed effects and industry fixed effects) and standard errors are clustered at the district level.
Figure A.24: Heterogeneity of Reallocation Effects

Notes: The figures show heterogeneous effects of the minimum wage on establishments' daily wage (panel a) and AKM establishment effects (panel b). Row (1) shows the benchmark estimate when all workers are included in the sample (as in Table 3). In rows (2) and (3), the sample is split into men and women, respectively. Rows (4)-(5) show the wage and employment effects of the minimum wage separately for West and East Germany, while rows (6) and (7) report effects separately for districts with below and above median exposure to the minimum wage. Rows (8) and (9) display wage and employment effects separately for workers who were employed in the tradable and in non-tradable sectors at baseline (see Dauth et al. (2017) for a definition of tradable vs non-tradable industries). Rows (10)-(14) display the wage and employment effects of the minimum wage separately for workers employed in five different industries at baseline. Rows (15) reports estimates for workers in FCSL occupations (food, cleaning, security and logistics services) that tend to be the target of domestic outsourcing (see Goldschmidt and Schmieder, 2017). Estimates refer to our baseline difference-in-differences estimates that compare excess wage growth and excess employment changes (relative to the 2011 vs 2013 pre-policy period) of workers who earned less than the minimum wage of 8.50 EUR and workers who earned more than 12.50 EUR at baseline (as in column (4) of Table 2). All regressions control for individual characteristics at baseline (age, education, sex, nationality, full-time status, district fixed effects and industry fixed effects) and standard errors are clustered at the district level.
Appendix B  Data Appendix

B. 1 Calculating Working Hours in the Employee Histories (BEH)

We use information on hours worked from DGUV (German Social Accident Insurance). Employers typically directly report total hours worked to the DGUV. For the years 2011 to 2014, employer notifications took place through the social security notification system, allowing us to link information on individual working hours to information on earnings and employment histories in the BEH for this period.

Employers have to notify the social security institutions of every employment relationship covered by the social security system, including marginal employment relationships (so-called Mini-Jobs), at least once per year. For employment relationships that start or end during a given calendar year, employers have to report the start and end date of the employment relationship. Between 2011 and 2014, employers notified social security institutions about the total hours worked during the notification period (i.e., January 1 to December 31 for continuing relationships; January 1 (or the start date) till the end date of the employment relationship for jobs that ended during the calendar year; and the start date of the employment relationship until December 31 (or the end date) for jobs that started during the calendar year) separately for each job. Four different reporting variants were allowed: i) actual hours worked, ii) contractual hours worked, iii) hours according to a collective bargaining agreement or the annual fixed full-time reference value calculated by the DGUV (or fractions thereof for part-time and marginal jobs) and iv) an educated guess. The reported data do not allow us to directly distinguish which of these four cases employers chose. We develop a simple heuristic to make reported hours comparable across employers. Our final measure for working hours correspond to contractual working hours plus overtime, the measure used by the government to compute hourly wages to insure compliance with the minimum wage law.

We calculate hourly wages in the following steps. In the first step, we adjust reported (contractual or actual) working hours such that they refer to hours per week, as the length of the notification periods varies. In the second step, we separate notifications into two groups: those that most likely include days of annual and sick leave (i.e., contractual hours) and those that most likely do not (i.e., actual hours). To do so, we assume that full-time notifications of less than 35 hours per week consti-
tute actual working hours, and full-time notifications of more than 35 hours per week constitute contractual working hours. We further assume that an establishment uses the same notification variant for all its employees. We classify establishments where at least 90% of its full-time workers are reported to work less than 35 hours per week as establishments reporting actual working hours. Similarly, establishments where at least 90% of its full-time workers are reported to work more than 35 hours per week are classified as reporting contractual hours. Then, we apply an adjustment factor of 1.19 to all full-time workers in establishments classified as reporting actual working hours, to convert actual working hours into contractual working hours.

The adjustment factor of 1.19 is motivated as follows. The number of effective working days is considerably lower than the number of potential working days per year, due to paid days of annual and sick leave. In the period under study, the average number of effective working days was roughly 210, implying an adjustment factor of 1.19 (250/210). We use a reduced adjustment factor to all part-time and marginally employed workers in establishments classified as reporting actual working hours, to account for the fact that part-time and marginally employed workers are entitled to fewer days of annual and sick leave.

There are establishments that we are not able to classify as reporting either actual or contractual working hours according to the procedure above. This can happen in three cases: if the establishment does not employ any full-time worker; if the establishment employs more than 10%, but less than 90%, of full-time workers who are reported to work more than 35 hours per week; or if the establishment employs more than 10%, but less than 90%, of full-time workers who are reported to work less than 35 hours per week. We randomly classify jobs in these establishments as reporting actual or contractual working hours, taking into account the value of reported working hours for each job. Specifically, for each reported value of working hours we compute, based on the subsample of establishments that we are able to directly classify as reporting actual or contractual working hours, the share of jobs that report actual vs contractual working hours. In non-classified establishments, we then randomly assign jobs as reporting actual or contractual working hours according to these shares, given the reported working hours of the job.

In the final step, we account for the fact that overtime might not be adequately captured in reported working hours. This is certainly the case for establishments that report contractual

---

1About 33% of establishments with at least one full-time worker cannot be classified directly.
working hours (option ii), as well as for establishments that report hours according to the collective 
bargaining agreement or the annual fixed full-time reference value (option (iii)). Since we are likely 
to classify many of the establishments that use these two options of reporting working hours as 
reporting actual hours according to our heuristic, we adjust all notifications for overtime, including 
those classified as reporting actual working hours.\textsuperscript{2}

In Table B.1, we report average unadjusted and adjusted working hours for the years 2011 and 
2014. For full-time workers, average unadjusted hours amount to only 34.8 per week and increase 
to 39.8 per week after adjustment. The difference between unadjusted and adjusted working 
hours is smaller for part-time and marginally employed workers, as expected. In Table B.2, we 
contrast adjusted weekly working hours in the BEH with official statistics reported in the Structure 
of Earnings Survey (SES, Verdienststrukturerhebung, VSE/VE data) of the German Statistical 
Office (Statistisches Bundesamt, 2016) and with the self-reported contractual hours in the German 
Socio-Economic Panel (GSOEP). The BEH estimates are very close to the SES for all three full-time 
status categories and for both men and women. The average adjusted working hours in the BEH 
also match self-reported average working hours in the SOEP quite closely. The largest discrepancy 
arises for marginally employed workers, possibly due to differences in the definition of marginal 
employment in the data sets.

In Table B.3 we compare the fraction of workers earning below 8.50 EUR in 2014 according to 
three data sources: our own calculations based on the BEH, based on the SES, which is arguably the 
most reliable data source on the distribution of hourly wages in Germany, and based on the SOEP. 
For full-time workers, the estimates on fraction affected are very similar in the three data sources. 
For part-time and marginal workers, however, discrepancies between the three data sets are larger. 
We somewhat over-estimate exposure to the minimum wage relative to the SES, but under-estimate 
exposure to the minimum wage relative to the SOEP. The economy-wide fraction affected based 
on the BEH once again falls in between that in the SES and the SOEP when we use the share of 
workers in each employment category in the BEH as weights to average across full-time status 
categories in the three data sets. If instead we use the respective employment shares in the three 
data sources as weights, the differences in fraction affected in the three data sets become smaller,

\textsuperscript{2}The adjustment factors are computed based on the German SOEP data set, which asks respondents about both 
paid and unpaid monthly overtime hours. We increase weekly hours of full-time, part-time and marginally employed 
workers in 2014 by 1.24, 0.56 hours and 0.19 hours, respectively. The adjustment factors in 2011 to 2013 vary slightly.
due to a smaller share of Mini Jobs in the SOEP even after taking sampling weights in the SOEP into account.

We conclude that our imputation procedure does a reasonable job in aligning average working hours and exposure to the minimum wage to other data sets.

References

Table B.1: Unadjusted and Adjusted Average Weekly Hours in the BEH

<table>
<thead>
<tr>
<th></th>
<th>2011 unadjusted</th>
<th>2011 adjusted</th>
<th>2014 unadjusted</th>
<th>2014 adjusted</th>
</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>26.7</td>
<td>30.3</td>
<td>26.5</td>
<td>30.1</td>
</tr>
<tr>
<td>Full-time</td>
<td>34.8</td>
<td>39.8</td>
<td>34.8</td>
<td>39.7</td>
</tr>
<tr>
<td>Part-time</td>
<td>22</td>
<td>24.9</td>
<td>21.8</td>
<td>24.6</td>
</tr>
<tr>
<td>Marginally employed</td>
<td>8.4</td>
<td>9.2</td>
<td>8.3</td>
<td>9.1</td>
</tr>
</tbody>
</table>

Notes: The table reports unadjusted and adjusted (for days of annual, sick leave and overtime) average working hours per week in the BEH for the years 2011 and 2014, separately for full-time workers, part-time workers, and marginally employed workers.
Table B.2: Average Weekly Hours in the BEH, in the Structure of Earnings Survey (SES) and the German Socioeconomic Panel (GSOEP) in 2014

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>BEH adjusted</td>
<td>SES</td>
<td>SOEP</td>
</tr>
<tr>
<td><strong>Full-time</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All</td>
<td>38.8</td>
<td>39.1</td>
<td>39.2</td>
</tr>
<tr>
<td>Men</td>
<td>38.9</td>
<td>39.1</td>
<td>39.4</td>
</tr>
<tr>
<td>Women</td>
<td>38.5</td>
<td>39</td>
<td>38.4</td>
</tr>
<tr>
<td><strong>Part-time</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All</td>
<td>24.3</td>
<td>23.9</td>
<td>23.6</td>
</tr>
<tr>
<td>Men</td>
<td>25.2</td>
<td>23.8</td>
<td>24.5</td>
</tr>
<tr>
<td>Women</td>
<td>24</td>
<td>23.9</td>
<td>23.5</td>
</tr>
<tr>
<td><strong>Marginally employed</strong></td>
<td>8.7</td>
<td>8.2</td>
<td>11.6</td>
</tr>
<tr>
<td>All</td>
<td>8.6</td>
<td>8</td>
<td>14.1</td>
</tr>
<tr>
<td>Women</td>
<td>8.7</td>
<td>8.2</td>
<td>10.7</td>
</tr>
</tbody>
</table>

Notes: The table shows average working hours per week in 2014 according to three data sources: the BEH (column (1)), the Structure of Earnings Survey (SES) reported by the German Statistical Office (column (2)) and the GSOEP (column (3)). To make the sample in the BEH as similar as possible to the statistics reported by the German Statistical Office, apprentices and workers in partial retirement, and workers employed in private households and extra-territorial organisations (T, U according to NACE Rev.2 industry classification) are excluded. Working hours in the BEH have been adjusted to account for days of annual and sick leave, but exclude overtime adjustment. To reduce the effect of outliers, full-time employment excludes hours below 30, part-time employment excludes hours above 40 and marginal employment excludes hours below 2 and above 20. For the GSOEP, contractual hours are used and similar sample restrictions as in the BEH are applied.

Source: Employee Histories (Beschäftigtenhistorik) of the Institute for Employment Research in Nuremberg (BEH); SOEPv33.1 survey year 2014; Statistisches Bundesamt (2016).
Table B.3: Fraction Affected by the Minimum Wage According to Structure of Earnings Survey (SES), SOEP and the BEH in 2014

<table>
<thead>
<tr>
<th></th>
<th>(1) BEH, adjusted</th>
<th>(2) SES</th>
<th>(3) SOEP</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Full-time</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All</td>
<td>4.4</td>
<td>4.4</td>
<td>5.4</td>
</tr>
<tr>
<td>Men</td>
<td>3</td>
<td>3.3</td>
<td>4.2</td>
</tr>
<tr>
<td>Women</td>
<td>7.4</td>
<td>6.6</td>
<td>7.9</td>
</tr>
<tr>
<td><strong>Part-time</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All</td>
<td>13.1</td>
<td>9.9</td>
<td>18</td>
</tr>
<tr>
<td>Men</td>
<td>20.7</td>
<td>15.3</td>
<td>22.9</td>
</tr>
<tr>
<td>Women</td>
<td>11.5</td>
<td>8.8</td>
<td>17.2</td>
</tr>
<tr>
<td><strong>Marginally employed</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All</td>
<td>47.4</td>
<td>39.7</td>
<td>53.4</td>
</tr>
<tr>
<td>Men</td>
<td>48.8</td>
<td>39.7</td>
<td>60.8</td>
</tr>
<tr>
<td>Women</td>
<td>46.7</td>
<td>39.7</td>
<td>50.9</td>
</tr>
<tr>
<td><strong>Economy-wide fraction affected</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Emp. status as in BEH</td>
<td>13.6</td>
<td>11.6</td>
<td>16.2</td>
</tr>
<tr>
<td>Official numbers</td>
<td>13.6</td>
<td>11.2</td>
<td>12.2</td>
</tr>
</tbody>
</table>

Notes: The table shows the fractions of workers earning below 8.50 EUR in 2014 according to three data sources: the BEH (column (1)), the Structure of Earnings Survey (SES) reported by the German Statistical Office (column (2)), and the Socio-Economic Panel (column (3)). To make the sample in the BEH as similar as possible to the statistics reported by the German Statistical Office, apprentices and workers in partial retirement are dropped. Activities of households and extra-territorial organisations (T, U according to NACE Rev.2) are excluded. Working hours in the BEH have been adjusted to account for days of annual and sick leave, but exclude overtime adjustment. To reduce the effect of outliers, full-time employment excludes hours below 30, part-time employment excludes hours above 40 and marginal employment excludes hours below 2 and above 20. We apply similar adjustments to the SOEP and focus, as in the BEH, on the main job. In the SOEP, we use contractual hours to calculate hourly wages.

We calculate the economy-wide fractions in two different ways. First, we average the fraction of affected workers in each full-time status category (full-time, part-time, marginal employment) using the respective employment shares in the BEH as weights (61.5% for full-time, 21.5% for part-time and 17.0% for marginal employment). Second, we average across full-time status categories using the respective employment shares in each of the three data sets (as reported by the German Statistical Office for the SES) as weights.

Employee Histories (Beschaftigtenhistorik) of the Institute for Employment Research in Nuremberg (BEH); SOEPv33.1 survey year 2014; Statistisches Bundesamt (2016).
B. 2 Correction of the Full-time/Part-time Variable in the BEH

In 2011, a new occupational code was introduced in the BEH. This switch lead to a significant increase in the number of missing observations in the variable that allows us to distinguish between full-time and part-time (including marginal) employment in that year. We follow the procedure suggested by Ludsteck and Thomsen (2016) to impute missing observations. We perform an additional plausibility check for those individuals who remain employed in the same establishment and occupation, but switch full- or part-time status between 2011 and 2012. In case this switch is not accompanied by a plausible change in daily wages (a reduction of at least 15% for full- to part-time switchers and an increase of at least 10% for part- to full-time switchers), we assume that the 2012 information is correct and adjust the full-time/part-time notification for the 2011 notification accordingly.

References


B. 3 Estimating AKM Establishment Effects

We calculate AKM fixed effects for establishments within rolling time windows of seven years, starting 2005-2011 and ending 2008-2014, based on a sample of full-time workers employed on June 30 between 16 and 65 years of age.

To impute the top-coded wages, we first define age-education cells based on five age groups (with 10-year intervals) and three education groups (no post-secondary education, vocational degree, college or university degree). Within each of these cells, following Dustmann et al. (2009) and Card et al. (2013), we estimate Tobit wage equations separately by year while controlling for age; firm size (quadratic, and a dummy for firm size greater than 10); occupation dummies; the focal worker’s mean wage and mean censoring indicator (each computed over time but excluding observations from the current time period); and the firm’s mean wage, mean censoring indicator, mean years of schooling, and mean university degree indicator (each computed at the current time period by excluding the focal worker observations). For workers observed in only one time period, the mean wage and mean censoring indicator are set to sample means, and a
dummy variable is included. A wage observation censored at value c is then imputed by the value $X\hat{\beta} + \hat{\sigma}\Phi^{-1}[k + u(1 - k)]$, where $\Phi$ is the standard normal CDF, $u$ is drawn from a uniform distribution, $k = \Phi [(c - X\hat{\beta})/\hat{\sigma}]$, and $\hat{\beta}$ and $\hat{\sigma}$ are estimates for the coefficients and standard deviation of the error term from the Tobit regression.

To calculate the AKM establishment and person effects, we first regress log wages (imputed if the wage is censored) on a cubic in age, controlling for year and establishment effects. We then subtract estimated age effects from log wages and regress log wages, net of age effects, on year, establishment and worker fixed effects.

References


B. 4 Predicted Establishment Productivity

Since the individual-level administrative records that we use for our main analysis do not include information on the productivity of establishments (such as revenues per worker), we compute a measure of predicted productivity for each establishment in our baseline individual sample by adding information from the IAB Establishment Panel (IAB-EP). The IAB Establishment Panel survey is an annual survey of up to 10,000 establishments that can be linked to the administrative records through unique establishment identifiers. This data source has relatively reliable information on revenues per worker, our measure for establishment productivity. The IAB Establishment Panel survey is, however, too small for implementing our benchmark individual and regional analysis directly.

To calculate predicted productivity for each establishment in the individual sample, we proceed as follows. We use the 2014 wave of the IAB Establishment Panel (around 7,000 establishments), which includes survey answers on revenues that relate to 2013. We then regress log revenues per worker on state dummies, industry dummies, establishment size dummies and average earnings of full-time workers. To reflect the survey design of the IAB Establishment Panel we calculate
establishment size dummies based on regular employment excluding workers in Mini Jobs. The obtained R-squared is 0.47; see column (1) of Table B.4. We explore a richer specification in column (2) of Table B.4, where we add log establishment size (in full-time equivalents including marginal jobs), the share of high skilled and full-time workers, AKM establishment fixed effects and establishment averages of AKM worker fixed effects as regressors. Adding these variables improves the R-squared only slightly, to 0.52.

In a final step, we use the estimated coefficients in column (1) and establishment characteristics measured at baseline \((t−2)\) for both previous \((t−2)\) and current \((t)\) establishments to predict the (log) productivity for each establishment in the individual sample. Changes in these predicted log productivities then serve as outcome variables in our individual reallocation regressions. By using establishment characteristics at baseline also for current establishments, we follow the way we compute changes in establishment quality for our other quality measures and ensure that changes in predicted productivity are driven by compositional changes rather than within-establishment changes in characteristics used to predict productivity. In the regional sample, we use the regression coefficients from column (1), and establishment characteristics measured at \((t−2)\) to predict (log) productivity for each establishment. We then calculate the log of mean productivities across establishments in the district, weighting by establishment size (number of employees) in period \(t\).
Table B.4: Establishment Productivity (Log Revenues Per Worker) and Establishment Characteristics

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1</td>
<td>Model 2</td>
</tr>
<tr>
<td>Average Wage (log)</td>
<td>0.672</td>
<td>0.570</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.108)</td>
</tr>
<tr>
<td>Employment (log, regular + mini jobs)</td>
<td>0.117</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td></td>
</tr>
<tr>
<td>Share of full-time workers</td>
<td>0.508</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.077)</td>
<td></td>
</tr>
<tr>
<td>Share of high-skilled workers</td>
<td>0.090</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.062)</td>
<td></td>
</tr>
<tr>
<td>AKM establishment effects</td>
<td>0.581</td>
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</tr>
<tr>
<td></td>
<td>(0.107)</td>
<td></td>
</tr>
<tr>
<td>Average AKM person effects</td>
<td>0.510</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.095)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>9.255</td>
<td>7.757</td>
</tr>
<tr>
<td></td>
<td>(0.113)</td>
<td>(0.263)</td>
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<tr>
<td>Observations</td>
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<td>7189</td>
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<tr>
<td>R-squared</td>
<td>0.47</td>
<td>0.52</td>
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<tr>
<td>RMSE</td>
<td>0.67</td>
<td>0.62</td>
</tr>
<tr>
<td>State FEs</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry FEs</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Establishment size (10 categories)</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes: The table shows the estimated relationship between log revenues per worker and some establishment-level characteristics such as the state the establishment is located in, industry affiliation, establishment size (calculated based on regular employment excluding marginally employed workers, 10 categories) and average earnings of full-time workers. In column (2) we further add log establishment size (including marginal employment, in full-time equivalents), the share of high skilled and full-time workers, AKM establishment fixed effects and the establishment average of AKM worker fixed effects as regressors.

Source: IAB Establishment Panel, 2014 wave, linked to Employee Histories (Beschaftigtenhistorik) of the Institute for Employment Research in Nuremberg (BEH).
B. 5 Imputation of Missing Values of AKM Establishment Effects and the Poaching Index

As described in Section B.3, we estimate AKM effects for establishments in the largest connected set, resulting in missing values for establishments outside the largest connected set. Furthermore, AKM establishment effects cannot be computed for establishments that do not employ any full-time workers. In our baseline individual sample, 10.4% of individuals work in an establishment with missing AKM establishment effects either in the baseline or in the current period. Rather than dropping these workers from our sample—which might induce differential selection into the sample before and after the introduction of the minimum wage and hence could bias our estimates—we impute establishment effects for these workers. To do so we regress establishment fixed effects (computed over the 7-year window \( t - 6 \) to \( t \)) on establishment size (9 categories), the share of full-time, part-time and native workers in the establishment (5 categories each), and industry affiliation (5-digit level) in year \( t \). We then replace missing values with predicted values using the coefficients from the regression and establishment characteristics in year \( t \). Figure B.1 compares the reallocation estimates when we impute or drop observations with missing AKM establishment effects. Reassuringly, the estimated reallocation effects are very similar, suggesting that the selectivity of missing values does not bias our results.

The poaching index measures the share of new hires coming from other establishments over all new hires. This index cannot be calculated for establishments which did not hire over the past two years. Similarly to the imputation of missing AKM establishment effects, we impute a poaching index for non-hiring establishments by regressing the poaching index in 2013 (which refers to new hires between 2011 and 2013) on establishment size (9 categories) and the share of full-time, part-time and native workers in the establishment (5 categories each) in that year. We then replace missing values with predicted values using the coefficients from the regression and establishment characteristics in year \( t \). Figure B.2 compares the reallocation estimates when we impute and drop observations with missing poaching index. Reassuringly, estimates are very similar.
Figure B.1: Estimated Reallocation Effects Using Imputed or Not Imputed AKM Establishment Effects

(a) Imputed AKM Establishment Effects (Benchmark specification)

(b) Not Imputed AKM Establishment Effects

Notes: The figures compare the reallocation effects of the minimum wage (two-year changes in AKM establishment fixed effects by baseline wage bin for the periods 2012 vs 2014 to 2014 vs 2016 periods, relative to the 2011 vs 2013 pre-policy period) when missing values of establishment’s AKM establishment effects are imputed (panel a) or dropped (panel b).
Figure B.2: Estimated Reallocation Effects Using Imputed or Not Imputed Poaching Index

Notes: The figures compare the reallocation effects of the minimum wage (two-year changes in establishments’ poaching index by baseline wage bin) when observations with missing poaching index (i.e., establishments that did not hire over the past two years) are imputed (panel (a)) or dropped (panel (b)).
Appendix C  Labor Market Effects of the Minimum Wage: Establishment-level Analysis

C. 1 Measuring Establishments’ Exposure to the Minimum Wage: The Establishment’s Gap Measure

In this section we study the effects of the minimum wage on establishments, exploiting variation in the exposure to the minimum wage across establishments. Our estimation sample is based on the “full sample” (see Section 2.2 in the main text), collapsed to the establishment-year level. We then draw a 50% random sample of establishments to comply with the data protection rules of the IAB, and focus on establishments with no more than 500 employees.

Similar to the regional analysis, we measure an establishment’s exposure to the minimum as the establishment’s gap measure:

\[ GAP_{jt} = \frac{\sum_{i \in I_j} h_{it} \max \{0, MW - w_{it}\}}{\sum_{i \in I_j} h_{it} w_{it}} \]  

(C.1)

Here, \( h_{it} \) denotes the weekly hours worked of worker \( i \) (employed at establishment \( j \)), \( MW \) is the minimum wage, and \( w_{it} \) refers to the worker’s hourly wage. The measure (if multiplied by 100) reflects the average wage increase (in percent) necessary to bring all workers in the establishment up to the minimum wage. The establishment gap measure in 2013, averaged across establishments (weighting all establishments equally), is 0.12, with a standard deviation of 0.29. Roughly half of establishments in our sample did not employ any minimum wage worker in 2013 (and hence have a gap measure of 0), whereas nearly 18% of establishments only employed minimum wage workers in 2013. Establishments more exposed the minimum wage are smaller on average. Yet, even within establishment size categories (in the empirical analysis below, we distinguish between establishments with 1-2, 3-4, 5-20, 21-34, 35-150 and 150-500 employees), there is considerable variation in the establishment gap measure, and the standard deviation of the gap measure within establishment size categories.
C. 2 Estimation Approach

We relate the establishment’s gap measure at baseline, $GAP_{jt-2}$, to two-year wage and employment growth in the establishment ($\frac{y_{jt} - y_{jt-2}}{y_{jt-2}}$) and estimate the following regression:

$$\frac{y_{jt} - y_{jt-2}}{y_{jt-2}} = \zeta_t + \alpha_r + \tau_s + \gamma_{2013}GAP_{jt-2} + \delta_tGAP_{jt-2} + e_{jt}$$ (C.2)

where $\zeta_t$ are year effects, $\tau_s$ are 1-digit industry fixed effects and $\alpha_r$ are region specific fixed effects. When studying wage growth as an outcome, we restrict the sample to establishments that survive between $t-2$ and $t$. In the employment regression exiting establishments are included in the sample (with an employment growth equal to -1). In regression equation C.2, the coefficient $\gamma_{2013}$ captures the general relationship between the baseline gap measure and employment and wage growth in the establishment that occurs irrespective of the minimum wage. As in the individual analysis, we would expect some mean reversion and selective survival also in the establishment analysis: low-wage establishments (i.e., establishments with a larger gap measure) may be less likely to survive and may hence shrink relative to high wage establishments (i.e., $\gamma_{2013} < 0$ in the employment regression), but may, conditional on establishment survival, experience exceptionally high wage growth (i.e., $\gamma_{2013} > 0$ in the wage regression). The coefficients $\delta_{2015}$ and $\delta_{2016}$ are then informative about how wage growth and employment growth in the establishment evolve in more exposed compared to less exposed establishments after the introduction of the minimum wage relative to the 2011-2013 pre-policy period, and hence capture the effects of the minimum wage. The coefficient estimate for the pre-policy year 2014 ($\delta_{2014}$) provides a useful falsification check and should be zero provided that the minimum wage was not anticipated and that the relationship between wage and employment growth in the establishment and the establishment’s gap measure, stemming from mean reversion and selective survival, remains constant over time. We cluster standard errors at the district level.

It should be noted that the employment responses to the minimum wage estimated using the establishment level approach capture somewhat different effects than the employment responses estimated using the individual and regional approach. Unlike the individual approach, but similar to the regional approach, employment effects from the establishment level approach pick up potential declines in employment from reduced hiring of unemployed workers. At the same time,
if, as suggested by our findings from the individual and regional approach, workers reallocate from low-wage establishments heavily affected by the minimum wage to high-wage establishments less affected by the minimum wage, the establishment-level approach may yield, contrary to the individual and regional approach, negative employment effects.

C. 3 Baseline Results

We first estimate regression equation (C.2) jointly for all establishments. Estimates in column (1) of Table C.1 refer to an unweighted regression, while estimates in column (2) refer to a weighted regression, using baseline employment as weights. Both weighted and unweighted estimates in panel (a) corroborate our findings from the individual and regional approach that the minimum wage raised wages. It should first be noted that establishments more exposed to the minimum wage generally, even in the absence of a minimum wage policy, experience higher wage growth conditional on establishment survival than establishments less exposed to the minimum wage (i.e., the coefficient on $ GAP_{j,2011} $ is positive). The relationship between the baseline gap measure in the establishment and wage growth, however, intensifies in the post-policy periods of 2013 vs 2015 and 2014 vs 2016 (i.e., the coefficients on $ GAP_{j,2013} $ and $ GAP_{j,2014} $ are positive), suggesting that the minimum wage indeed boosts wages in heavily affected establishments relative to less affected establishments. Reassuringly, the coefficient estimate for the 2012-2014 “placebo” period is close to zero, increasing our confidence that the excess establishment wage growth (relative to the 2011 vs 2013 period) is caused by the minimum wage and not driven by differential pre-trends between establishments more or less affected by the minimum wage.

Turning to the employment estimates in panel (b), the findings point toward a modest negative employment effects of the minimum wage, while the findings in panel (c) further suggest that the minimum wage increased the probability of establishment exit. In terms of magnitude, according to the unweighted regression estimates in column (1), a 10% increase in the establishment’s gap measure at baseline increases wages by 0.88% and reduces employment growth in the establishment by 0.27% in the 2014 vs 2016 post-policy periods (relative to the 2011 vs 2013 pre-policy period). As a result, the implied employment elasticity with respect to the own wage (the labor demand elasticity in the standard model) is -0.31 (s.e. 0.04). The implied labor demand elasticity is with
-0.26 (s.e. 0.07) slightly smaller when we use the weighted regression estimates in column (2).

In combination with our findings from the individual and regional approach, the negative employment response following the minimum wage hike based on the establishment level approach likely reflects the reallocation of workers from more exposed (typically smaller) to less exposed (typically larger) establishments, rather than the displacement of low-wage workers or an overall decline in employment in the economy.

C. 4 Heterogenous Effects by Establishment Size

Next, we provide additional direct evidence in favor of compositional shifts toward larger (less exposed) establishments, by showing that small establishments reduce employment, whereas larger establishments expand employment, in response to the minimum wage. To explore such heterogeneous impacts of the minimum wage by establishment size, we allow the coefficient estimates $\gamma_{2013}$ and $\delta_t$ in regression equation C.2 to vary by establishment size category and add establishment size dummies interacted with year effects as additional controls. In columns (3) to (5) of Table C.1, we report minimum wage effects on wages, employment and establishment exit for three establishment size categories (1-4, 5-124, 125-500 employees). We display coefficients for the 2014 vs 2016 post-policy period and the 2012 vs 2014 “placebo” pre-policy period for more detailed establishment size categories in Figure C.1.

The findings first highlight that whereas the wage effects of the minimum wage are roughly similar across establishment size categories (panels (a) in Table C.1 and Figure C.1), the employment effects of the minimum wage are heterogeneous across establishment size categories. Results by more detailed establishment categories show that increased exposure to the minimum wage leads to employment reductions in small and medium-sized establishments with less than 35 employees, and leads to employment increases in larger establishments with more than 150 employees (panel (c) in Figure C.1). Results on establishment exit further show that small establishments with less than 5 employees, but not medium-sized or larger establishments, are more likely to exit the market when being more strongly affected by the minimum wage (panel (c) in Table C.1 and panel (e) in Figure C.1). Reassuringly, similar patterns are not observed for the 2012-2014 “placebo” period, suggesting that these estimates indeed reflect a causal relationship (panels (b), (d) and (e)).
Overall, the results presented here highlight that small, low-wage establishments seem to lose from the minimum wage policy, while larger, low-wage establishments seem to gain. The findings further support our findings from the individual and regional approach that the minimum wage induced a reallocation from smaller and more exposed establishments to larger and less exposed establishments.
Table C.1: Employment and Wage Effects of the Minimum Wage: Establishment-level Evidence

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3) by establishment size</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1-500</td>
<td>1-500</td>
<td>1-4</td>
<td>5-150</td>
<td>151-500</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel (a): Wage Growth</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \text{GAP}<em>{2014} ) (rel. to ( \text{GAP}</em>{2011} ))</td>
<td>0.088</td>
<td>0.120</td>
<td>0.077</td>
<td>0.139</td>
<td>0.130</td>
</tr>
<tr>
<td>( \text{GAP}<em>{2013} ) (rel. to ( \text{GAP}</em>{2011} ))</td>
<td>0.088</td>
<td>0.123</td>
<td>0.077</td>
<td>0.142</td>
<td>0.176</td>
</tr>
<tr>
<td>( \text{GAP}<em>{2012} ) (rel. to ( \text{GAP}</em>{2011}, ) placebo)</td>
<td>0.005</td>
<td>0.007</td>
<td>0.004</td>
<td>0.006</td>
<td>0.042</td>
</tr>
<tr>
<td>( \text{GAP}_{2011} )</td>
<td>0.104</td>
<td>0.092</td>
<td>0.111</td>
<td>-0.006</td>
<td>-0.031</td>
</tr>
<tr>
<td>Number of establishments</td>
<td>3,229,128</td>
<td>3,229,128</td>
<td>1,727,474</td>
<td>1,466,120</td>
<td>35,534</td>
</tr>
</tbody>
</table>

| **Panel (b): Employment Growth** |      |      |                           |      |      |
| \( \text{GAP}_{2014} \) (rel. to \( \text{GAP}_{2011} \)) | -0.027 | -0.031 | -0.019                    | -0.044 | 0.159 |
| \( \text{GAP}_{2013} \) (rel. to \( \text{GAP}_{2011} \)) | -0.013 | -0.019 | -0.008                    | -0.025 | 0.009 |
| \( \text{GAP}_{2012} \) (rel. to \( \text{GAP}_{2011}, \) placebo) | 0.002 | 0.002 | 0.003                     | 0.000 | -0.069 |
| \( \text{GAP}_{2011} \) | -0.128 | -0.159 | -0.122                    | -0.053 | 0.002 |
| Number of establishments | 3,828,313 | 3,828,313 | 2,213,019 | 1,578,704 | 36,590 |

| **Panel (c): Establishment Exit** |      |      |                           |      |      |
| \( \text{GAP}_{2014} \) (rel. to \( \text{GAP}_{2011} \)) | 0.018 | 0.030 | 0.013                     | 0.002 | -0.003 |
| \( \text{GAP}_{2013} \) (rel. to \( \text{GAP}_{2011} \)) | 0.010 | 0.020 | 0.006                     | 0.002 | -0.031 |
| \( \text{GAP}_{2012} \) (rel. to \( \text{GAP}_{2011}, \) placebo) | -0.001 | 0.006 | -0.003                    | -0.007 | 0.000 |
| \( \text{GAP}_{2011} \) | 0.123 | 0.116 | 0.094                     | -0.017 | -0.116 |
| Number of establishments | 3,828,313 | 3,828,313 | 2,213,019 | 1,578,704 | 36,590 |

Employment Weight | no | yes | no | no | no |

**Notes:** The table investigates the relationship between the establishment’s exposure to the minimum wage at baseline (the establishment’s gap measure as defined in equation C.1) and establishment wage growth (conditional on survival, panel (a)); establishment employment growth (including establishment exit, panel (b)); and the probability of establishment exit (panel (c)). The regression results are based on regression equation C.2 in the text. We first report results for all establishments up to 500 employees (columns (1) and (2)) and then split the sample by establishment size but restrict the effect of establishment baseline characteristics to be the same across establishment size categories (columns (3) to (5)). The \( \text{GAP}_{2011} \) coefficients trace out the general relationship between the establishment’s gap measure at baseline and establishment outcomes, irrespective of the minimum wage, in the 2011-2013 pre-policy period. The \( \text{GAP}_{2014} \) and \( \text{GAP}_{2013} \) coefficients show the relationship between the establishment’s exposure to the minimum wage at baseline and changes in establishment outcomes 1 and 2 years after the implementation of the minimum wage policy (between 2013 and 2015 and 2014 and 2016), relative to the 2011 vs 2013 pre-policy period. The \( \text{GAP}_{2012} \) coefficient provides a placebo check. Estimates in Column (2) are weighted by baseline (t-2) employment, while all other columns report unweighted estimates. Standard errors are clustered at the district level.
Figure C.1: Evidence for Reallocation - Effect of the Minimum Wage by establishment Size (Establishment Level Approach)

(a) Wages, 2016 vs 2014

(b) Wages, Placebo (2014 vs 2012)

(c) Employment, 2016 vs 2014

(d) Employment, Placebo (2014 vs 2012)

(e) Establishment Exit, 2016 vs 2014

(f) Establishment Exit, Placebo (2014 vs 2012)

Notes: The figures investigate the relationship between the establishment’s exposure to the minimum wage at baseline (the establishment’s gap measure as defined in equation C.1) and establishment wage growth (conditional on survival, panels (a) and (b)); employment growth (including establishment exit, panels (c) and (d)); and the probability that the establishment exits (panels (e) and (f)), separately for six establishment size categories. Estimates are based on equation C.2 estimated separately for the six size categories, where we restrict the time, industry, and region effects to be the same across all establishments. Reported estimates in panels (a), (c) and (e) refer to changes in establishment outcomes between 2014 and 2016 (i.e., two years after the introduction of the minimum wage) relative to the 2011 vs 2013 pre-policy period (i.e., \( \delta_{2016} \) in equation C.2), while reported estimates in panels (b), (d) and (f) refer to the 2012 vs 2014 pre-policy period (i.e., \( \delta_{2016} \) in equation C.2) and serve as a placebo check. Standard errors are clustered at the district level.
Appendix D  Minimum Wages and Imperfect Competition on the Labor Market

In this section we present a simple monopsonistic model of the labor market with heterogeneous firms in which minimum wages naturally induce a reallocation of workers from less to more productive firms. In the model, firms have some wage setting power in the labor market as workers do not only care about wages but also about non-pecuniary aspects of the job, as in for example Card et al. (2018). In consequence, more productive firms will be larger and will set higher wages even for the same worker type. For simplicity, we abstract away from imperfections in the product market.

D. 1 Set-up

Workers’ maximization problem. There is only one type of labor, low-skilled workers. Workers do not only derive utility from the wage a particular firm pays, but also from the work environment that a firm provides. Workers value these non-pecuniary job characteristics differently, and the indirect utility of worker $i$ working at firm $j$ is

$$u_{ij} = \beta \log w_j + \epsilon_{ij}$$

Here $\log(w_j)$ is the wage that firm $j$ pays to all its workers and $\epsilon_{ij}$ denotes her idiosyncratic preferences for working at the firm, capturing for example commuting time, how well she gets along with her co-workers or boss, or her preferences for the working schedule the firm provides.

The indirect utility of not-employed workers equals:

$$u_{ib} = b + \epsilon_{ib}$$

We assume that $\epsilon_{ij}$ and $\epsilon_{ib}$ are independent draws from a type I Extrem Value distribution. Workers choose to work for the firm that offers the highest utility. Hence, by standard arguments (McFadden, 1977), the probability that a worker chooses to work for firm $j$ (rather than remaining unemployed or at any other firm that has entered the market) equals:
\[ P \left( \arg \max_{k \in J^M} \{ u_{ij}, u_{ib} \} = j \right) = \frac{\exp (\beta \log w_j)}{\exp \beta + \sum_{k \in J^M} \exp (\beta \log w_k)} = \lambda^M \exp (\beta \log w_j) \]

where \( J^M \) denotes the set of firms that enter the market under monopsonistic competition. Note that \( \lambda^M = \frac{1}{\exp \beta \sum_{k \in J^M} \exp (\beta \log w_k)} \) is a constant that does not vary across firms. Due to idiosyncratic non-pecuniary job characteristics, some workers choose to work for a firm even if the firm pays lower wages than other firms. As a result, firms face an upward-sloping labor supply curve:

\[ l_j = N \times P \left( \arg \max_{k \in J^M} \{ u_{ij}, u_{ib} \} = j \right) = N \lambda^M \exp (\beta \log w_j) \]

where \( N \) is the population of low-wage workers. Taking logs, we obtain

\[ \log l_j = \log N + \log \lambda^M + \beta \log w_j \] (D.1)

This expression highlights that the labor supply elasticity that a monopsonistic firm faces is equal to \( \beta \). The probability that a worker is unemployed can be similarly derived as

\[ \text{unemp rate} = N \lambda^M \exp (b) \]

**Product market.** For simplicity, we assume that product markets are perfectly competitive and hence firms are price takers on the product market. Firms face a market-level inverse product demand function \( p = D(q) \), where \( p \) denotes the product price and \( q \) denotes the amount of output produced by all firms operating in the market.\(^3\)

**Firms’ maximization problem.** Firms are heterogenous, modelled here as differences in their variable costs \( \psi_j \). Firms produce output \( q_j \) according to a production function with decreasing returns to scale:

\[ q_j = \frac{1}{\psi_j} \log l_j \]

Without loss of generality, we rank firms according to \( \psi \), from the most efficient firms with lowest marginal costs to the least efficient firms with the highest marginal costs. In addition to variable costs, firms face fixed costs \( F \) that neither vary by the number of workers hired nor by the amount

\(^3\)All results that we highlight below continue to hold if firms have some price-setting power in the product market. Allowing for product market imperfections complicates the notation and the discussion without adding new insights.
of output produced. Firms choose how many workers to hire by maximizing profits:

$$\pi_j = \max_l \left\{ \frac{p}{\psi_j} \log l_j - w_j l_j - F \right\}$$  \hspace{1cm} \text{(D.2)}$$

Computing the first order condition, we obtain $$\frac{p}{\psi_j} \frac{1}{l_j} = w_j \left[ \frac{\beta + 1}{p} \right],$$ where $$\beta$$ denotes the firm’s labor supply elasticity $$\left( \frac{dl_j}{dw_j} \right)$$. Taking the logarithm, we obtain:

$$\log l_j = \log \left( \frac{p}{\psi_j} \frac{\beta}{\beta + 1} \right) - \log w_j = -\log \psi_j + \log \left( \frac{\beta}{\beta + 1} \right) - \log \left( \frac{w_j}{p} \right)$$  \hspace{1cm} \text{(D.3)}$$

Using that firms face an upward sloping labor supply curve given by equation (D.1), firm $$j$$’s optimal employment and wage choices under monopsonistic competition can be expressed as

$$\log l_j^M = \frac{\beta}{1 + \beta} \left[ \log \left( \frac{p}{\psi_j} \frac{\beta}{\beta + 1} \right) \right] + \frac{1}{1 + \beta} \left[ \log N + \log \lambda^M \right]$$  \hspace{1cm} \text{(D.4)}$$

and

$$\log w_j^M = \frac{1}{1 + \beta} \left[ \log \left( \frac{p}{\psi_j} \frac{\beta}{\beta + 1} \right) \right] - \frac{1}{1 + \beta} \left[ \log N + \log \lambda^M \right].$$  \hspace{1cm} \text{(D.5)}$$

These expressions lead to the following Lemma:

**Lemma 1.** Under monopsonistic competition in the labor market,

1. more efficient firms employ more workers ($$l_j^M$$ is decreasing in $$\psi_j$$);
2. more efficient firms pay higher wages ($$w_j^M$$ is decreasing in $$\psi_j$$);
3. more efficient firms make higher profits ($$\pi_j^M$$ is decreasing $$\psi_j$$).

**Proof.** To see that more efficient firms (i.e., firms with lower $$\psi_j$$) employ more workers, differentiate equation (D.4) with respect to $$\psi_j$$:

$$\frac{d \log l_j^M}{d \log \psi_j} = -\frac{\beta}{1 + \beta} < 0.$$  \hspace{1cm} \text{(D.6)}$$

As firms face an upward sloping labor supply curve (equation D.1), it immediately follows that more efficient and hence larger firms must pay higher wages. To see that more efficient firms make higher profits, differentiate firms’ profit (given by equation D.2) with respect to $$\psi_j$$ and make use of
the envelope theorem:

$$\frac{d\pi_j}{d\psi_j} = -\frac{p}{\psi_j^2} \log l^M_j = -\frac{p}{\psi_j} q_j < 0.$$  

**Closing the Model.** Firms will enter the market only if they make positive profits. Since firms’ profits are decreasing in $\psi_j$, there exists a threshold $\bar{\psi}^M$ such that firms with marginal cost above this threshold choose not to enter the labor market. In equilibrium, the quantity demanded by consumers must be equal to the output produced by firms, which pins down the product price $p^M$ under monopsonistic competition:

$$D(p^M) = \sum_{j \in J^M} \frac{1}{\psi_j} \log l^M_j$$

where $J^M$ is the set of firms that entered the market under monopsonistic competition (i.e., firms for whom $\psi_j < \bar{\psi}^M$).

**D. 2 The Impact of the Minimum Wage**

Next, consider the introduction of a minimum wage $\bar{w}^{MW}$ set at a level such that it is binding for at least some firms. Firms which used to pay wages below the minimum wage (in real terms, i.e., $w_j^M/p^M < \bar{w}^{MW}/p^{MW}$) now have two options: either they exit the market, or they raise wages to comply with the minimum wage. The least efficient firms will no longer be profitable if they pay the minimum wage and they thus leave the market. The threshold below which firms operate in the market ($\bar{\psi}^{MW}$) is therefore lower in the presence of a binding minimum wage (i.e., $\bar{\psi}^{MW} < \bar{\psi}^M$), and the minimum wage causes the least efficient firms to exit the market.

Firms with marginal costs $\psi_j$ below $\bar{\psi}^{MW}$ but high enough such that they paid wages below the minimum wage before its introduction (i.e., $w_j^{MW}/p^{MW} > w_j^M/p^M$) instead choose to pay the minimum wage to their workers. These firms now act as price takers in the labor market and de facto face an infinitely elastic labor supply curve, as they no longer need to raise wages in order

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4It should be noted that firms, which experience a small increase in their wage costs $w_j/p$ due to the introduction of the minimum wage, will experience only a second order decrease in their profit. This is because $w_j/p$ has been set to maximize profits; hence, the first order condition of the maximization problem implies that changes in $w_j$ have no (first-order) effects on profits. Nevertheless, for larger changes in $w_j/p$, profits will decrease.
to attract more workers. In consequence, these firms pay higher wages and expand employment in response to the minimum wage. To see this more formally, note that minimum wage firms maximize their profit as:

\[ \pi_j = \max_{l_j} \left\{ \frac{p}{\psi_j} \log l_j - \bar{w}^{MW} \cdot l_j - F \right\} \]

Their optimal choice of labor thus equals

\[ \log l_j^{MW} = -\log \psi_j - \log \frac{\bar{w}^{MW}}{p^{MW}}. \]  

Contrasting employment levels with and without a minimum wage yields (see equation D.3):

\[ \log l_j^{MW} - \log l_j^M = \log \frac{\beta + 1}{\beta} + \log \frac{w_j^M}{p^M} - \log \frac{\bar{w}^{MW}}{p^{MW}}. \]

We have \( \log \frac{\beta + 1}{\beta} + \log \frac{w_j^M}{p^M} - \log \frac{\bar{w}^{MW}}{p^{MW}} > 0 \) and hence \( l_j^{MW} > l_j^M \) for all firms for whom the minimum wage is binding, provided that the minimum wage is not too high.

Finally, consider more efficient firms that paid a wage above the minimum wage before its introduction (i.e., \( \bar{w}^{MW}/p^{MW} < w_j^M/p^M \)). It is ambiguous what happens to the employment choices and wage offers of these firms. On the one hand, some firms have exited the market, which reduces competition in the labor market and ceteris paribus allows surviving firms to pay lower wages and yet attract the same number of workers. On the other hand, some surviving firms raise wages in response to the minimum wage, which ceteris paribus forces more efficient firms to also increase their wage offers to attract the same number of workers. In consequence, the impact of the minimum wage on \( \lambda = \frac{1}{\exp \beta + \sum_{i \in I} \exp (\beta \log w_i)} \), and hence wage offers and employment of more efficient firms (see equations (D.4) and (D.5)), is unclear.  

We summarize the properties of the new equilibrium in Lemma 2:

**Lemma 2.** Suppose that the minimum wage is binding in real terms for at least some firms (i.e., \( \bar{w}^{MW}/p^{MW} > w_j^M/p^M \) for some firms). Assume as well that the minimum wage is set at a relatively low level such that
\[ \log \frac{\hat{\beta} + 1}{\hat{\beta}} + \log \frac{\bar{w}_M^j}{p_M^j} - \log \frac{\bar{w}_{MW}^j}{p_{MW}} > 0 \] will hold for all firms.

1. The minimum wage causes the least efficient firms to exit the market (i.e., \( \bar{\psi}_j^{MW} < \bar{\psi}_j^M \)).

2. Surviving firms which in the absence of the minimum wage would have paid (in real terms) a wage below the minimum wage (i.e., firms for which \( \bar{w}_{MW}^j/p_{MW} > w_M^j/p_M^j \)) will comply with the minimum wage policy and increase their wages and employment.

3. Surviving firms which in the absence of the minimum wage would have paid (in real terms) a wage above the minimum wage (i.e., firms for which \( \bar{w}_{MW}^j/p_{MW} < w_M^j/p_M^j \)) may reduce or increase their wages and employment.

It is now easy to see that a minimum wage induces a reallocation of workers to firms that were larger (higher \( l_j^M \)), paid higher wages (higher \( w_j^M \)), and were more profitable (higher \( \pi_j^m \)) before the introduction of the minimum wage. The least efficient firms—which are also the firms that pay the lowest wages, are the smallest and the least profitable—exit the market in response to the minimum wage. Workers in these firms will either become unemployed or move to other more efficient and hence higher paying, larger and more profitable firms. Since idiosyncratic preferences follow a continuous type 1 extreme value distribution, there must be some workers who reallocate to more efficient firms rather than becoming unemployed. Furthermore, the minimum wage pushes up wages in at least some surviving firms, making these firms more attractive for workers and reducing the wedge between the marginal product and the marginal cost in the firm, providing a further mechanism why workers may reallocate to more efficient firms.

It is worth highlighting that the impact of the minimum wage on overall employment is ambiguous. On the one hand, increased exit ceteris paribus lowers employment. On the other hand, firms for which the minimum wage is binding increase wages and thus become more attractive for previously unemployed workers. These firms generally expand employment. As discussed above, more efficient firms that pay wages above the minimum wage may increase or decrease employment.

It is also worth highlighting that moving to a more efficient firm may not make all workers better off, despite being paid a higher wage. By revealed preferences, some workers chose to work for the least efficient and lowest paying firms prior to the introduction of the minimum wage because of the high level of utility from non-pecuniary job characteristics they received in these
firms. Once these firms exit, workers in these firms may move up to higher paying firms, but at the expense of a lower utility from non-pecuniary characteristics.

Nevertheless, the average welfare of workers can be assessed by applying the standard formula of expected utility (see Small and Rosen, 1981):

\[
E \left[ \max_{b,k \in J_{MW}} u_{ib}, u_{ik} \right] - E \left[ \max_{b,k \in J_{M}} u_{ib}, u_{ik} \right] = \log \left( \exp b + \sum_{k \in J_{MW}} \exp (\beta \log w_{k}^{MW}) \right) - \\
- \log \left( \exp b + \sum_{k \in J_{M}} \exp (\beta \log w_{k}^{M}) \right)
\]

It is easy to see that the impact of the minimum wage on the average welfare of workers depends on the difference between \(\lambda_{MW} = \frac{1}{\exp b + \sum_{k \in J_{MW}} \exp (\beta \log w_{k}^{MW})}\) and \(\lambda_{M} = \frac{1}{\exp b + \sum_{k \in J_{M}} \exp (\beta \log w_{k}^{M})}\): if \(\lambda_{MW} > \lambda_{M}\), then workers will be better off on average after the introduction of a minimum wage. Notice also that the unemployment rate in the model, \(N\lambda \exp(b)\), is a linear function of \(\lambda\) and hence

\[\lambda_{MW} < \lambda_{M} \iff \text{unemp}_{MW} < \text{unemp}_{M}\]

In consequence, the average welfare of workers will increase in response to the introduction of the minimum wage if and only if unemployment falls and employment increases.

References

