Earnings Inequality and the Minimum Wage: Evidence from Brazil

Online Appendix—Not for Publication

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Abstract

This document contains the online appendices for Engbom and Moser (2022a). The online appendices are structured as follows. Online Appendix A gives further details on the datasets introduced in Section 2. Online Appendix B presents additional empirical results building on Section 4. Online Appendix C adds to the description of our equilibrium model from Section 5. Online Appendix D provides more information on the model estimation routine and results extending what is presented in Section 6. Finally, Online Appendix E shows further results related to the simulated impact of the minimum wage considered in Section 7.

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Contents of the Online Appendix

A Data Appendix

This appendix provides further details on the datasets introduced in Section 2, including subsections on further data description (Appendix A.1), additional summary statistics (Appendix A.2), a comparison of official labor force statistics and sample sizes in the RAIS administrative data (Appendix A.3), an-dan exploration of explained wage dispersion when (not) controlling for observable and unobservable heterogeneity (Appendix A.4).

A.1 Dataset description

This section provides a description of the three main datasets used in the analysis of the main text. Further details are relegated to the replication materials disseminated as Engbom and Moser (2022b).

Administrative linked employer-employee data (RAIS). Our main data source is the *Relação Anual de Informações Sociais* (RAIS), a linked employer-employee register by the administered by Brazil's Ministério da Economia (2020). We use the RAIS microdata with person and firm identifiers covering the period 1985–2018 available to interested researchers under a confidentiality agreement with the Brazilian ministry.

Firms' survey response is mandatory, and misreporting is deterred through audits and threat of fines. The earliest available data go back to 1985, with coverage becoming near universal from 1994 onward. The data contain detailed information on job characteristics, with approximately 66 million formal sector employment spells recorded in 2018. Although reports are annual, we observe for every job spell the date of accession and separation in addition to average monthly earnings. We keep for each worker the highest-paid among each year's longest employment spells. As Brazil's minimum wage is set in terms of monthly earnings, henceforth we interchangeably refer to this income concept as "earnings" or "wages."

The main text presents results of both plug-in and leave-one-out bias corrected variance components of log wages based, following the methodology and code by KSS. For our main analysis, we restrict attention to the largest leave-one-out connected set but do not impose any additional restrictions on either earnings, firm size, or the minimum number of switchers across firms. For a set of additional estimation results of the AKM wage equation (1), we keep workers with earnings not equal to or weakly above the minimum wage.

We devise our own cleaning procedure for these data, starting with the raw text files, benefiting from guidance by the data team at IPEA. Our cleaning procedure consists of three stages. The first stage reads in and standardizes the format of the raw data files that were transmitted to us at the region-year level, saving a set of compatible region-year files. The second stage reads in all region files within a year and applies a set of cleaning and recoding procedures to the data to make them consistent within each year, saving a set of yearly files. The third stage reads in all yearly files and applies a set of cleaning procedures to the data to make them consistent across years. Whenever possible, we use the official crosswalks provided by IBGE to convert industry (IBGE, CNAE 1.0, and CNAE 2.0 classifications), occupation (CBO 1994 and CBO 2002), and municipality codes (IBGE classification).

Cross-sectional household survey data (PNAD). A substantial fraction of Brazil's working-age population is not formally employed and hence not covered by the RAIS. To address this gap, we complement our analysis using data from the *Pesquisa Nacional por Amostra de Domicílios (PNAD)*, a nationally representative annual household survey from 1996 to 2012 administered by Instituto Brasileiro de Geografia e Estatística (2019), or IBGE in short. Respondents are asked to produce a formal work permit (*Carteira de Trabalho e Previdência Social assinada*). Following Meghir et al. (2015), we classify as informal all self-employed and those in remunerated employment without a work permit.

The PNAD data collection consists of a double-stratified sampling scheme by region and municipality, interviewing a representative of households in Brazil. The survey asks the household head to respond on behalf of all family members and report a rich set of demographic and employment-related questions. In particular, the survey asks a question about whether the respondent holds a legal work permit. We use the answer to this survey question to identify individuals as working in the formal or in the informal sector. Survey questions regarding income and demographics of the respondent household members are comparable to the U.S. CPS. We keep only observations that satisfy our selection criteria and have non-missing observations for labor income, whose variable definition we harmonize across years.

The raw microdata are publicly available for download starting from 1996 at ftp://ftp.ibge.gov.br/Trabalho_e_Rendimento/. For basic cleaning, starting with the raw data in text format, we use the standardized cleaning procedures adopted from the Data Zoom suite developed at PUC-Rio and available for replication online at http://www.econ.puc-rio.br/datazoom/english/index.html. From there, we apply a set of procedures to clean and recode key variables used in our analysis.

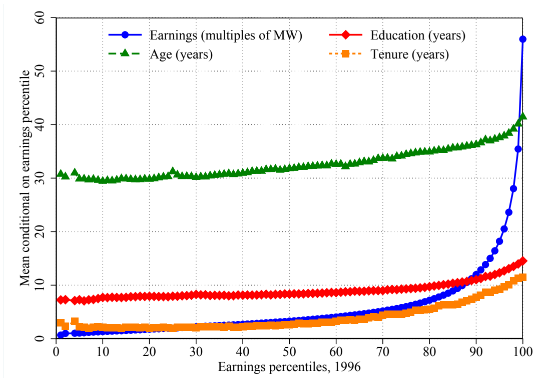
Longitudinal household survey data (PME). We also use a second household survey, the *Pesquisa Mensal de Emprego (PME)*, conducted between 2002 and 2012 in Brazil's six largest metropolitan regions: Belo Horizonte, Porto Alegre, Recife, Rio de Janeiro, Salvador, and São Paulo, and administered by Instituto Brasileiro de Geografia e Estatística (2020), or IBGE in short. The advantage of this dataset is that it features for every respondent two continuous four-month interview spells separated by an eight-month pause. Starting in 2002, this short panel component allows us to compute monthly transition rates of workers between different employment states, including formal and informal employment. For presentation purposes, we label formal sector workers as "employed," and pool informal sector workers and the unemployed under the label "nonemployed." We distinguish between the disaggregated categories in our empirical analysis of minimum wage effects later.

The raw microdata are publicly available for download starting from March 2002 at ftp://ftp.ibge.gov.br/Trabalho_e_Rendimento/. For basic cleaning, starting with the raw data in text format, we use the standardized cleaning procedures adopted from the Data Zoom suite developed at PUC-Rio and available for replication online at http://www.econ.puc-rio.br/datazoom/english/index.html. From there, we apply a set of procedures to clean and recode key variables used in our analysis, similar to the procedures that we applied to the PNAD data.

A.2 Summary statistics

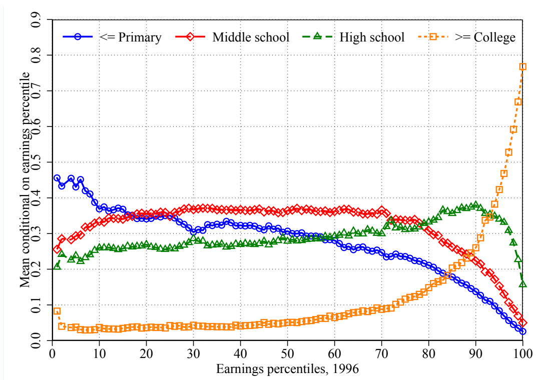
Summary statistics for administrative linked employer-employee data (RAIS). Figure A.1 shows mean values of basic descriptive variables—monthly earnings in multiples of the minimum wage, years of education, age in years, and job tenure in years—throughout the earnings distribution for 1996 in panel A and for 2018 in panel B. Zooming in on educational attainment, which increased significantly over this period, Figure A.2 shows the distribution of eduaction degrees grouped into individuals with primary school or lower levels of education, middle school, high school, and college or higher levels of education for 1996 in panel A and for 2018 in panel B.

Figure A.1. RAIS cross-sectional summary statistics, 1996 and 2018



Notes: Figure shows mean monthly earnings ("wages"), years of education, age, and tenure across wage percentiles for 1996 in panel A and for 2018 in panel B. All statistics are for adult male workers of age 18–54. *Source:* RAIS, 1996 and 2018.

Figure A.2. RAIS education degree shares, 1996 and 2018



Notes: Figure shows shares of education degrees across wage percentiles for 1996 in panel A and for 2018 in panel B. All statistics are for adult male workers of age 18–54. *Source:* RAIS, 1996 and 2018.

Summary statistics for cross-sectional household survey data (PNAD). Table A.1 presents summary statistics on the PNAD data.

Table A.1. Summary statistics for cross-sectional household survey data (PNAD)

		Formal real wage		Informal real wage		Employment	Formal
	# Workers	Mean	Std. dev.	Mean	Std. dev.	rate	share
1996	74,487	7.01	0.81	6.26	0.81	0.95	0.68
1997	78,731	7.02	0.79	6.26	0.82	0.94	0.68
1998	79,060	7.03	0.78	6.26	0.81	0.93	0.67
1999	81,230	6.97	0.77	6.21	0.79	0.93	0.66
2000							
2001	89,102	6.93	0.74	6.20	0.81	0.93	0.66
2002	90,855	6.90	0.73	6.19	0.81	0.93	0.66
2003	91,490	6.84	0.71	6.12	0.77	0.92	0.67
2004	94,526	6.85	0.69	6.15	0.77	0.94	0.68
2005	97,348	6.89	0.67	6.19	0.77	0.93	0.68
2006	97,757	6.94	0.66	6.25	0.76	0.94	0.69
2007	95,598	6.97	0.65	6.30	0.78	0.94	0.71
2008	93,677	7.00	0.65	6.35	0.76	0.95	0.72
2009	95,170	7.02	0.63	6.36	0.76	0.94	0.73
2010							
2011	84,910	7.07	0.62	6.51	0.75	0.95	0.76
2012	86,031	7.13	0.62	6.56	0.78	0.95	0.76

Notes: Table shows summary statistics on wages, employment rates, and formal employment shares between 1996 and 2012. All statistics are for adult male workers of age 18–54. Real wages are measured in 2012 BRL and in logs. Surveys are not available for census years 2000 and 2010. *Source:* PNAD, 1996–2012.

Summary statistics for longitudinal household survey data (PME). We present summary statistics on the PME data in Table A.2.

Table A.2. Summary statistics for longitudinal household survey (PME)

	# Workers	Transition rate employed-nonemployed	Transition rate employed-nonemployed
2002	94,280	0.08	0.05
2003	140,734	0.09	0.06
2004	146,847	0.08	0.05
2005	154,159	0.08	0.05
2006	153,646	0.08	0.04
2007	154,338	0.09	0.05
2008	150,104	0.10	0.05
2009	149,762	0.10	0.04
2010	150,443	0.10	0.04
2011	145,012	0.11	0.04
2012	121,211	0.10	0.04

Notes: Table shows number of workers and monthly transition rates between employment (i.e., formal employment) and nonemployment (i.e., informal employment + unemployment). All statistics are for adult male workers of age 18–54. *Source:* PME, 2002–2012.

A.3 Details of sample selection and sample size

Table A.3 shows official labor force statistics (panel A) and different sample sizes computed in the RAIS administrative data (panel B). Comparing the numbers of formal sector workers, we find that the two data sources are compatible. In 1996, official statistics state that there are around 26,603,108 formal sector workers in Brazil and 29,600,720 unique workers recorded in RAIS. In 2018, official statistics state that there are around 53,348,529 formal sector workers in Brazil and 55,740,072 unique workers recorded in RAIS. That RAIS captures a somewhat larger number of workers is not surprising given that official statistics are based on survey data with respect to a reference week or month, while RAIS covers any employment spells during the entire calendar year. Cumulatively applying our selection criteria based on gender (only males), age (18–54), and nonmissing key information (earnings, worker and employer identifiers, and employment dates), our RAIS sample comprises 17,201,101 workers in 1996 and 27,602,584 workers in 2018.[1]

Table A.3. Official labor force statistics and sample sizes in RAIS administrative data

	1996	2018
Panel A. Official labor force statistics		
Labor force	68,225,692	102,576,163
Unemployment rate	0.076	0.117
Informality rate	0.578	0.411
Total formal employment	26,603,108	53,348,529
Panel B. Sample sizes in RAIS administrative data		
Total number of jobs	34,260,198	66,214,692
Number of unique workers	29,600,720	55,740,072
Number of unique + male workers	18,940,516	31,439,096
Number of unique + male + prime-age workers	17,377,682	28,007,974
Number of workers satisfying additional selection criteria	17,201,101	27,602,584

Notes: Table shows official labor force statistics (panel A) and sample sizes calculated based on the RAIS administrative data (panel B) for 1996 and 2018. Official labor force statistics are from International Labour Organization (2022), or ILO in short, that are made accessible via the World Bank Open Data platform, indicator ID: SL.TLF.TOTL.IN. Unemployment rate for 1996 is from Instituto de Pesquisa Econômica Aplicada (2020), or IPEA in short, and is derived from the PNAD household survey data for 1996, while that for 2018 is from IBGE and is derived from the PNAD-Contínua household surveys for October–December 2018 from Instituto Brasileiro de Geografia e Estatística (2021). Informality rate for 1996 is from Instituto de Pesquisa Econômica Aplicada (2021), or IPEA in short, and is derived from the PNAD household survey data for 1996 based on "Definition I" of informal employment, while that for 2018 is from IBGE and is derived from the PNAD-Contínua household survey data for October–December 2018 from Instituto Brasileiro de Geografia e Estatística (2022), or IBGE in short. Additional selection criteria in the last line of the table include a requirement of nonmissing values for earnings, worker identifiers, employer identifiers, and employment dates. *Source:* ILO, IBGE, IPEA, and RAIS, 1996 and 2018.

The main difference between the number of workers reported in the official labor force statistics and our final sample in RAIS is due to our selection based on gender (only males) and age (18–54). The minimum wage is significantly more binding for men outside of this age range as well as for women. Thus, our analysis focuses on a population subgroup that is relatively less affected by the minimum wage. In this sense, our results may understate the effects of the minimum wage on Brazil's full population.

[1] In a previous version of the paper, we also excluded workers with earnings below the minimum wage and those at firms below a minimum employment threshold, which led to a slightly smaller sample size than that reported here.

A.4 The importance of unobserved worker heterogeneity in wages

Let i index workers, t index years, and let $edu(i) \in \{e_1, e_2, \ldots, e_N\}$ be the educational attainment group of individual i, which in our data is a permanent worker characteristic. The log wage of individual i in year t is denoted y_{it}. Then we can decompose the total variance of log wages as

$$Var(y_{it}) = \underbrace{Var(\mathbb{E}[y_{it}|edu(i)])}_{\text{Between-education-group variance}} + \underbrace{Var(y_{it} - \mathbb{E}_i[y_{it}|edu(i)])}_{\text{Within-education-group variance}}, \tag{A.1}$$

The first term on the right-hand side of equation (A.1) is the variance of education-mean log wages, which we call the between-education-group variance. The second term is the variance of worker-year level deviations from the education-mean log wages, which we call the within-education-group variance. For education to be a meaningful skill proxy, we require the between-education-group variance to make up a significant share of the total variance of log wages. Implementing the decomposition in equation (A.1) on the RAIS data from 1994–1998, we find that out of a total variance of wages of 75.0 log points, around 14.1 log points (19 percent) are due to the between-education-group variance component, while 60.9 log points (81 percent) are due to the within-education-group variance component. From this, we conclude that, while education is a significant predictor of wages in the data, the vast majority of wage dispersion is within education groups.

We can extend our analysis to other worker and job attributes as observable skill proxies. To this end, we estimate a sequence of Mincerian wage equations with cumulatively added controls for observable worker characteristics, unobservable time-invariant worker" characteristics, and unobservable time-invariant firm characteristics. Observable worker characteristics include education-specific age dummies, education-specific year dummies, hours dummies, and occupation dummies. Table A.4 shows the results from four different specifications estimated on the RAIS data. With rich controls for observable worker characteristics (column 1), the coefficient of determination (R^2) is 48.6 percent, while the root mean squared error (RMSE) is 0.623. This suggests that the largest share of wage heterogeneity and significant dispersion in absolute terms is not explained by observables. Adding firm dummies to the first specification (column 2) leads to an R^2 of 76.4 percent, suggesting that firms are an important in wage determination. However, as AKM have argued, some of this explained variance may itself be attributable to unobserved worker heterogeneity. When we add worker dummies instead of firm dummies to the first specification (column 3), we find an R^2 of 88.3 percent and an RMSE of 0.347, meaning that the explanatory power is almost twice as high when controlling worker observable and unobservable characteristics relative to when controlling only for observables. Together, the estimation results from this sequence of wage equations suggest that unobservable worker characteristics constitute an important share of overall wage dispersion, more important than education and a number of other observable characteristics. [2]

[2] All wage equations here are estimated on the largest available set of workers, whereas Section 2.2 of the main text reports estimates based on the leave-one-out connected set of workers and firms. Note also that all numbers reported here are with respect to the plug-in estimators of the R^2 and the RMSE. However, Section 2.2 presented leave-one-out corrected estimates of variance components, which leads to small changes relative to the plug-in estimates.

Table A.4. Explained wage dispersion when (not) controlling for observable and unobservable heterogeneity

	(1)	(2)	(3)
Coefficient of determination (R^2)	0.486	0.764	0.883
Root mean square error (RMSE)	0.623	0.427	0.347
Observations (mm)	83.2	82.9	78.2
Education-specific age dummies	✓	✓	✓
Education-specific year dummies	✓	✓	✓
Hours dummies	✓	✓	✓
Occupation dummies	✓	✓	✓
Firm dummies	✗	✓	✗
Worker dummies	✗	✗	✓

Notes: This table shows the coefficient of determination (R^2) and root mean square error (RMSE) for different specifications that project log wages on various controls: observable worker characteristics (column 1), observable worker characteristics and firm dummies (column 2), and observable worker characteristics and worker dummies (column 3). Observable worker characteristics include education-specific age dummies, education-specific year dummies, hours dummies, and occupation dummies. Note that the number of observations vary across specifications because we drop singletons based on combinations of the controls included in each specification. *Source:* RAIS, 1994–1998.

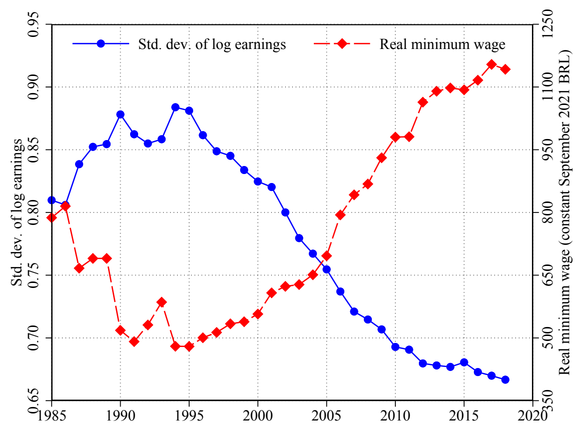
B Empirical Appendix

This appendix provides further details on the empirical exercises conducted in Section 4, including subsections on the evolution of earnings inequality and the minimum wage over time (Appendix B.1), the minimum wage spike (Appendix B.2), the incidence of minimum wage jobs (Appendix B.3), the evolution of the effective bindingness of the minimum wage (Appendix B.4), additional details on the effects of the minimum wage on wage inequality (Appendix B.5), additional regression results using alternative specifications (Appendix B.6), additional regression results using alternative controls (Appendix B.7), additional regression results using alternative time periods (Appendix B.8), additional regression results using alternative polynomial orders for region-specific time trends (Appendix B.9), additional regression results that cluster standard errors at the state level or at the mesoregion level (Appendix B.10), a comparison between our findings and subsequent work by Haanwinckel (2020) (Appendix B.11), a comparison of the relative bindingness of the minimum wage between Brazil and the U.S. (Appendix B.12), a comparison between the distributions of (changes in) log wages in current BRL and in multiples of the current minimum wage (Appendix B.13), and hours worked in relation to the bindingness of the minimum wage (Appendix B.14).

B.1 The evolution of earnings inequality and the minimum wage

Figure B.1 shows a strong negative comovement between the minimum wage and the standard deviation of log wages between 1985 and 2018, with a time series correlation of -0.947.

Figure B.1. Evolution of wage inequality and the real minimum wage, 1985–2018



Notes: Statistics are for males of age 18–54. Real minimum wage is the annual mean of the monthly time series. The correlation between the two time series is -0.947. *Source:* RAIS and IPEA, 1985–2018.

B.2 The (relatively small) spike at the minimum wage

Much of the previous empirical literature has interpreted the mass of workers employed at the minimum wage as a measure of the bindingness of the wage floor (Flinn, 2006, 2010). However, theoretical labor market models in the spirit of Burdett and Mortensen (1998) predict that frictional wage dispersion for identical workers can be sustained absent any mass points in the wage distribution, including at the minimum wage. This suggests that any spike of workers at the minimum wage may be thought of independently from, or maybe in addition to, the effect of the minimum wage on the rest of the wage distribution. Although there is substantial heterogeneity in the empirical bindingness of the minimum wage across population subgroups in our administrative data from Brazil, we robustly find a relatively small spike at the wage floor for male workers of age 18–54. The remainder of this subsection is dedicated to studying the (relatively small) spike at the minimum wage in Brazil, both in the cross section and over time.

Share of workers earning exactly, below, or around the minimum wage. Panel A of Figure B.2 plots the state-level distribution of the share of workers earning exactly the minimum wage against the relative bindingness of the minimum wage measured by the Kaitz-50 index, defined as the log minimum-to-median wage, in 1996 and 2018. The average share of workers earning exactly the minimum wage is stable around two percent throughout this period.[3] We find a weak positive correlation between the Kaitz-50 index and the share of workers earning the minimum wage, but the share remains mostly below six percent even in states where the minimum wage is most binding.

We can broaden our definition of a "mass point" to three alternative measures, the evolution of which from 1996 to 2018 is depicted in panel B of Figure B.2. The share of workers earning exactly the minimum wage, shown by the blue line with circles, remains approximately flat at two percent between 1996 and 2018. A little under 3.5 percent of workers in 1996 and around 5.5 percent of workers in 2018 report earning exactly or less than the minimum wage, shown by the red line with diamonds.[4] Our most generous definition includes workers within a 5 percent band around the minimum wage, shown in green. This most generous measure evolves from 3.5 to 5.5 percent over this period, far from the roughly 30 percent of workers between the old and the new minimum wage.

Figure B.3 shows that that there is substantial heterogeneity in the share of workers earning exactly the minimum wage across states (panel A) and mesoregions (panel B). At the same time, only a small fraction of regions have a share of workers earning exactly the minimum wage above seven percent.

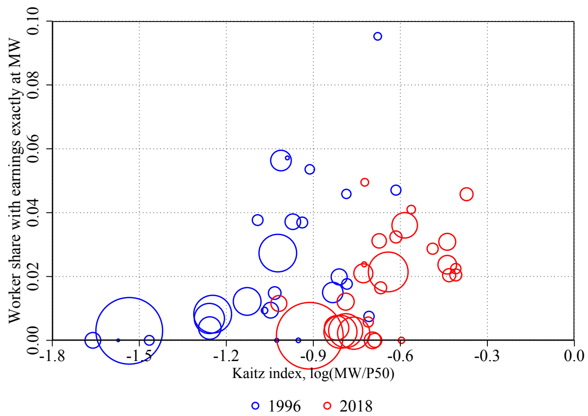
Histograms of wages around the minimum wage. Figure B.4 shows a histogram of wages in multiples of the minimum wage in 1996 and 2018. Figure B.5 shows a similar histogram for wages in logarithms.

[3] For comparison, 3.3 percent of hourly paid workers in the U.S. earned the prevailing federal minimum wage or less in 2015 (U.S. Bureau of Labor Statistics, 2017).

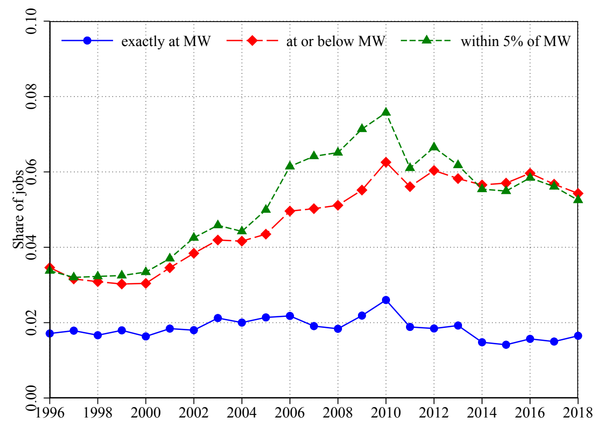
[4] Observations with earnings strictly below the minimum wage are likely due to a mix of legal exceptions, misreporting, and illegal employment.

Figure B.2. Share of workers at and around the minimum wage, 1996–2018

A. Share earning exactly the minimum wage by state-years



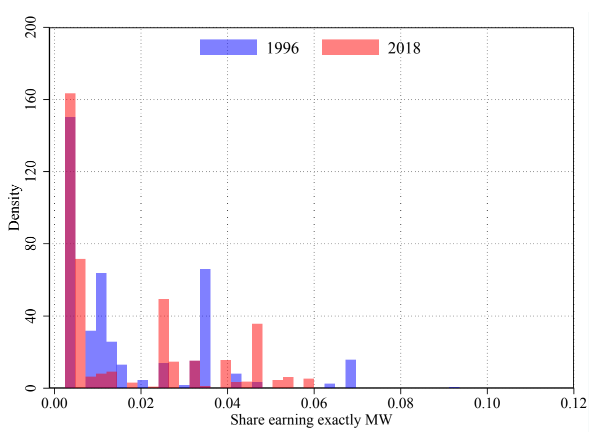
B. Share exactly at, below, or around minimum wage



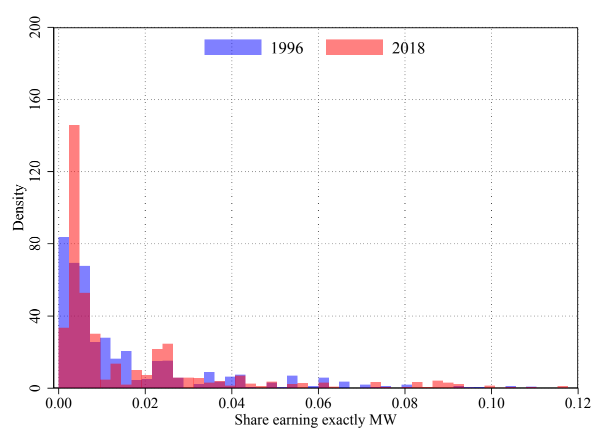
Notes: This figure shows the share of workers with earning exactly at, below, or around the minimum wage. Panel A shows share of male workers of age 18–54 earning exactly the minimum wage against the Kaitz-50 index, $kaitz_{st}(50) \equiv \log w_t^{min} - \log w_{st}^{P50}$, across states in 1996 and 2018. Area of circles is proportional to population size. In panel B, the blue line shows share of workers earning exactly the minimum wage, the red line shows share at or below the minimum wage, and the green line plots share within 5 percent of the minimum wage. *Source:* RAIS, 1996–2018.

Figure B.3. Histogram of share of workers earning exactly the minimum wage, 1996 and 2018

A. Across states



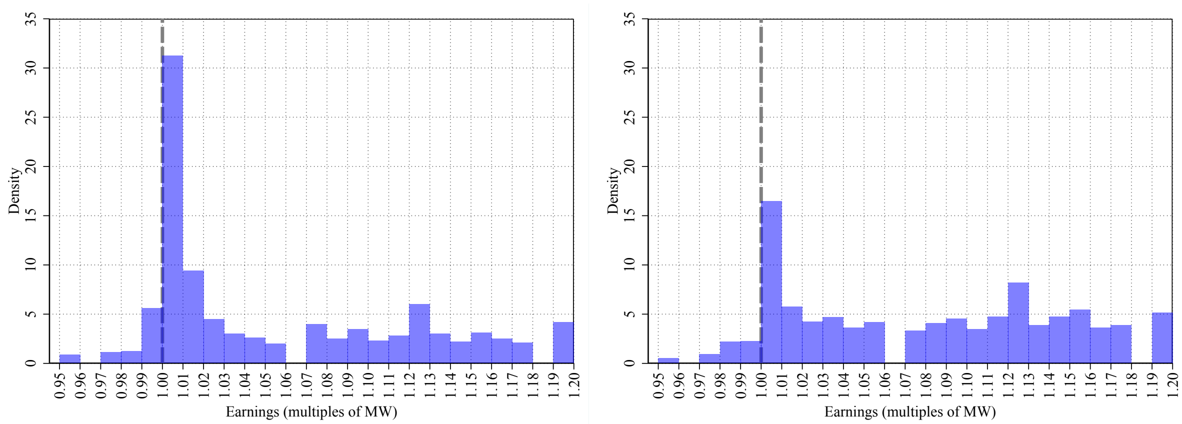
B. Across mesoregions



Notes: This figure shows histograms of the share of workers earning exactly the minimum wage across different population subgroups by state (Panel A) and by mesoregion (Panel B). Blue bars show the distribution in 1996, while red bars show the distribution in 2018. *Source:* RAIS, 1996 and 2018.

Figure B.4. Histogram of wages around the minimum wage, 1996 and 2018

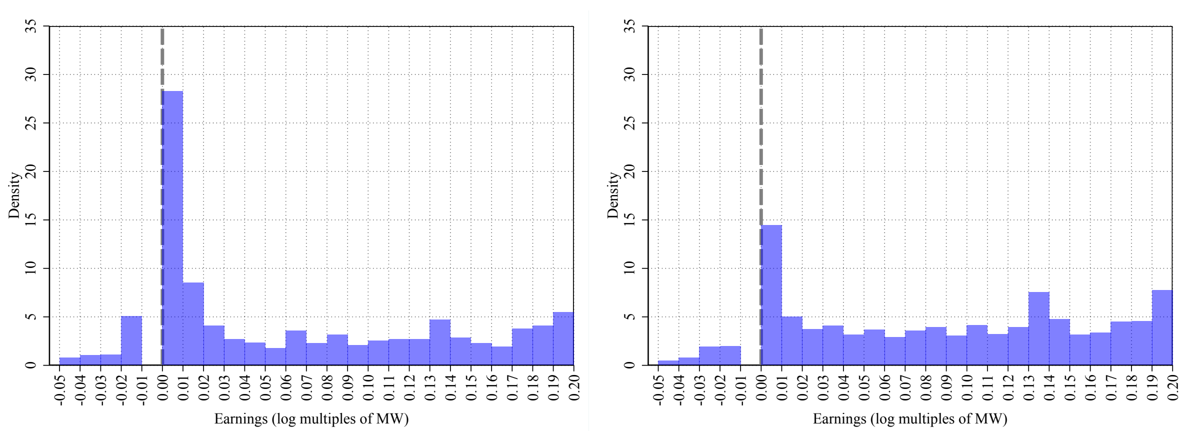
A. 1996 B. 2018



Notes: This figure shows histgrams of the wage distribution, measured in multiples of the minimum wage, zoomed in around the minimum wage (dashed vertical line) for 1996 (Panel A) and 2018 (Panel B). Bar width is set to 0.01, i.e., one centavo (subdivision of Brazilian Reais). *Source:* RAIS, 1996 and 2018.

Figure B.5. Histogram of log wages around the minimum wage, 1996 and 2018

A. 1996 B. 2018

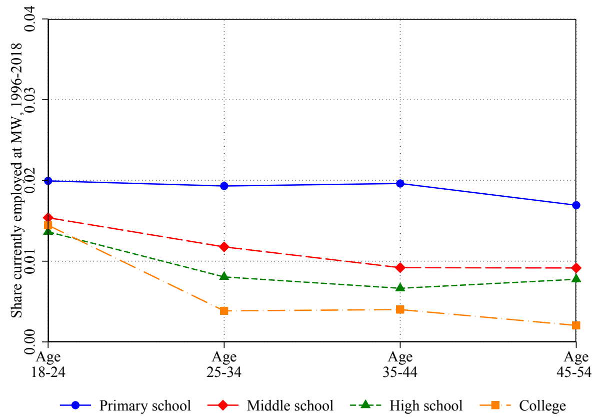


Notes: This figure shows histgrams of the log wage distribution, measured in log multiples of the minimum wage, zoomed in around the minimum wage (dashed vertical line) for 1996 (Panel A) and 2018 (Panel B). Bar width is set for 0.01, i.e., approximately one percent. *Source:* RAIS, 1996 and 2018.

B.3 Who earns the minimum wage in Brazil?

Figure B.6 shows the share of workers currently earning the minimum wage by education group. The share of minimum wage earners is higher at younger ages and lower educational attainments. Yet the maximum share of minimum wage earners, namely that of workers of age 18–24 with at most a primary school degree, is around four percent.

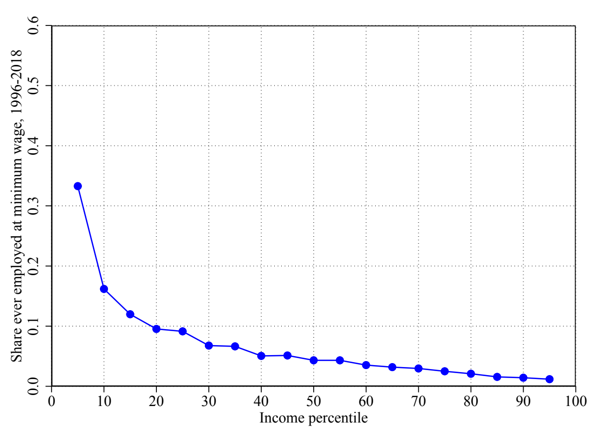
Figure B.6. Share of workers currently earning the minimum wage, by education group



Notes: Figure shows share of workers earning exactly the minimum wage by education groups (colored lines) and age groups (x-axis values) during the period from 1996 to 2018. *Source:* RAIS, 1996–2018.

A striking feature of the Brazilian labor market is that, in spite of a relatively small share of workers earning the minimum wage at any point in time, a surprisingly large share of workers ever—currently, in the past, or in the future—earn the minimum wage. Figure B.7 plots the share of workers who have ever earned the minimum wage during our sample period of 1996–2018.

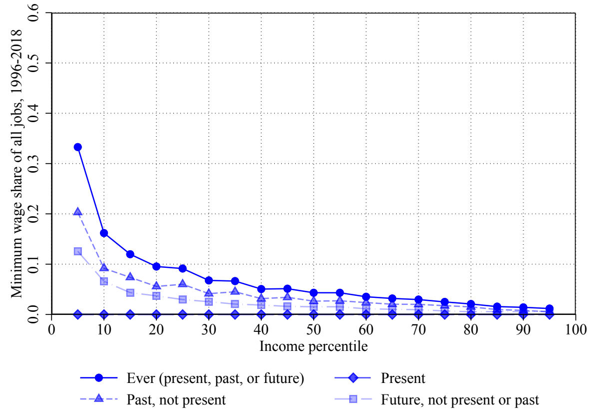
Figure B.7. Share of workers who ever earned the minimum wage from 1996–2018



Notes: Figure shows the share of workers who have ever (currently, in the past, or in the future) earned exactly the minimum wage by current income percentile. Current income percentiles are created by ranking workers within a given year according to their current wage for each year between 1996 and 2018. *Source:* RAIS, 1996–2018.

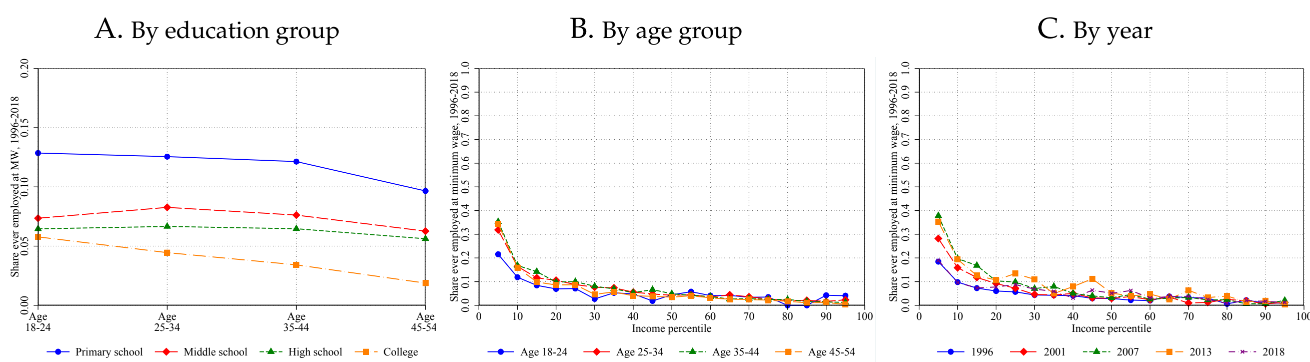
Figure B.8 decomposes the share of workers who ever earned the minimum wage into those who earn the minimum wage currently, in the past, or in the future between 1996 and 2018.

Figure B.8. Decomposition of share of workers who ever earned the MW, 1996–2018



Notes: Figure shows the share of workers who have ever (currently, in the past, or in the future) earned exactly the minimum wage. The different colored lines show the share of workers who ever, currently, in the past, and in the future earn the minimum wage across current income percentiles. Current income percentiles are created by ranking workers within a given year according to their current wage for each year between 1996 and 2018. *Source:* RAIS, 1996–2018.

Figure B.9. Share ever employed at minimum wage, by subgroups



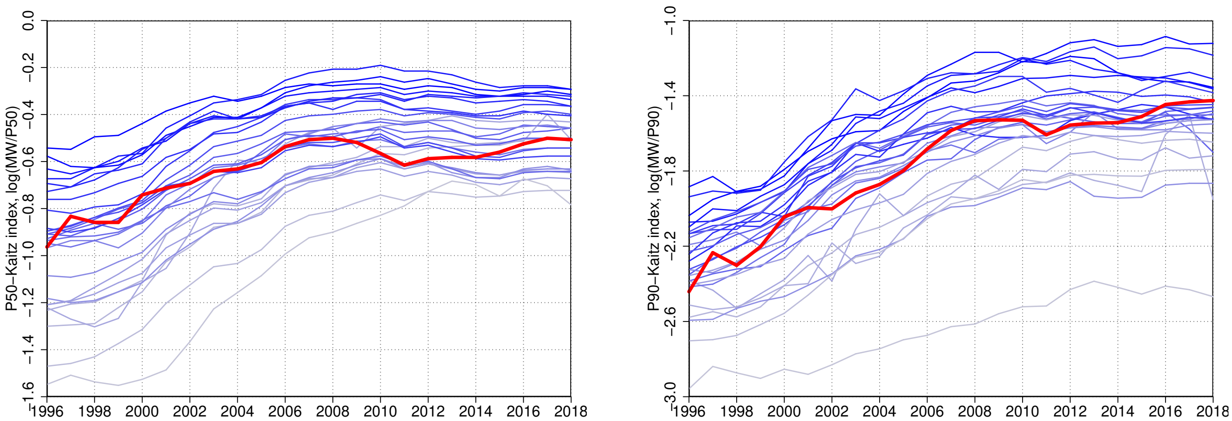
Notes: Figure shows the share of workers who have ever (currently, in the past, or in the future) earned exactly the minimum wage by current income percentile, separately by education group (Panel A), by age group (Panel B), and by year (Panel C). Current income percentiles are created by ranking workers within a given year according to their current wage for each year between 1996 and 2018. *Source:* RAIS, 1996–2018.

B.4 Evolution of Kaitz indices by state

Figure B.10. Data: Evolution of Kaitz indices by state, 1996–2018

A. Kaitz-50 index

B. Kaitz-90 index



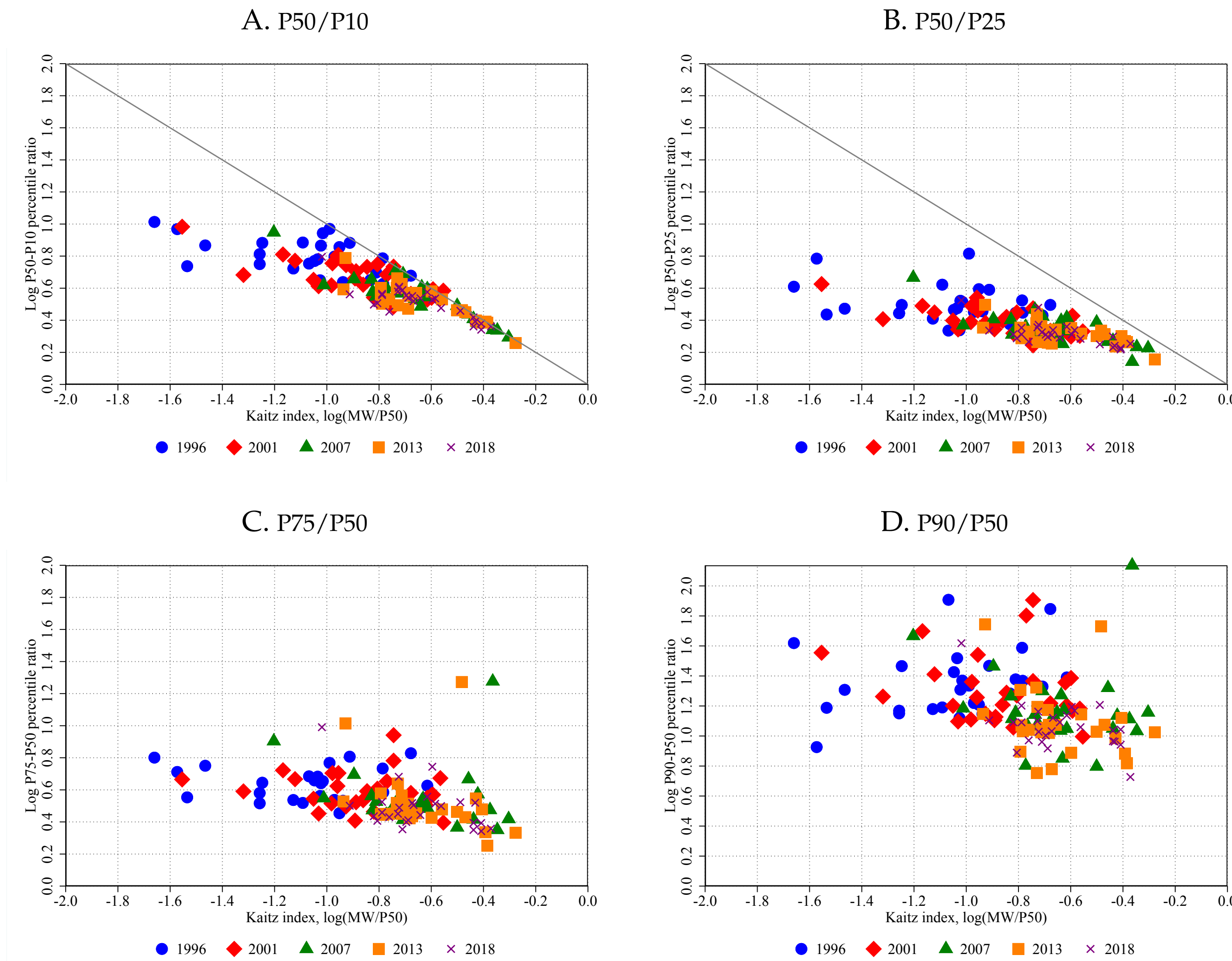
Notes: The Kaitz-p index for state s in year t is defined as $kaitz_{st}(p) = \log(\text{federal minimum wage}_t) - \log\left(w^{\mathrm{P}p}\right)$ for earnings percentiles $p \in \{50, 90\}$. Each blue line markets one of Brazil's 27 states. The red line represents the weighted mean across states. *Source:* RAIS, 1996–2018.

B.5 Additional results on the effects of the minimum wage on wage inequality

We now show that the inverse relationship between state-year level bindingness of the minimum wage and wage inequality generalizes to the full set of states over time. To see this, we define the *Kaitz-50 index* as $kaitz_{st}(50) \equiv \log w_t^{min} - \log w_{st}(50)$, that is, the log difference between the minimum wage prevailing at time t, w_t^{min}, and the median wage of subgroup s at time t, $w_{st}(50)$.[5] Figure B.11 plots the relation between different log wage percentile ratios and the Kaitz-50 index. Panel A plots empirical lower-tail inequality, measured by the P50/P10, against the Kaitz-50 index across Brazilian states over time. The negative 45 degree line marks states where the minimum wage is binding at the tenth percentile of the wage distribution. Panel B repeats the same exercise for the P50/P25 ratio. Both plots show a negative relationship lower-tail inequality and the Kaitz-50 index that grows more pronounced for more binding states in the cross section and over time. For comparison, the remaining two panels show a weaker relationship between top inequality, measured by the P75/P50 in panel C and by the P90/P50 in panel D, and the Kaitz-50 index.

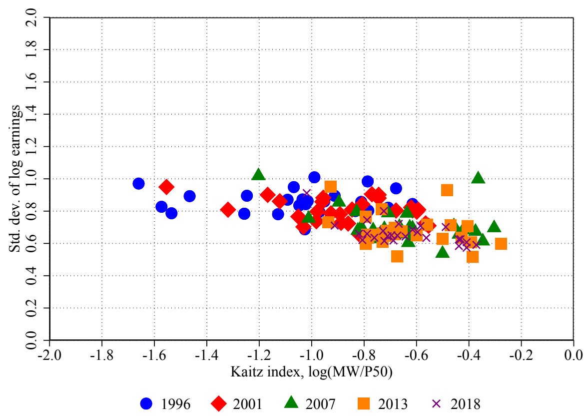
[5] Recall that Figure B.10 in Appendix B.4 shows that variation in the Kaitz-50 index and the Kaitz-90 index across Brazilian states is large initially and decreases as the minimum wage increases, while approximately preserving the ranking of states over time.

Figure B.11. Log wage percentile ratios across Brazilian states over time, 1996–2018



Notes: Figure plots various log wage percentile ratios against the Kaitz-50 index, defined as $kaitz_{st}(50) \equiv \log w_t^{min} - \log w_{st}^{P50}$, where w_t^{min} is the minimum wage prevailing at time t and $w_{st}(50)$ is the median wage in subgroup s at time t. Each marker represents a combination of a state s and year t for each of Brazil's 27 states between 1996 and 2018. *Source:* RAIS, 1996–2018.

Figure B.12. Data: Standard deviation of log wages across states over time, 1996–2018



Notes: Figure plots the standard deviation of log wages against the Kaitz-50 index, $kaitz_{st}(50) \equiv \log w_t^{min} - \log w_{st}^{\mathrm{P50}}$, with each marker representing one state-year combination for each of Brazil's 27 states between 1996 and 2018. *Source:* RAIS.

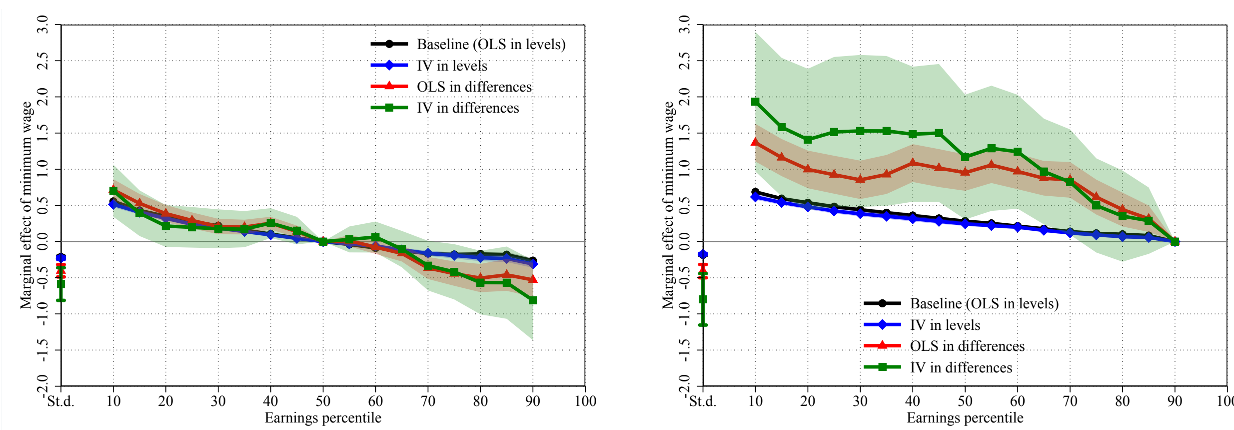
B.6 Additional regression results: Alternative specifications

Figure B.13 compares our baseline results (black line with circles) with three alternative specifications that consist of an IV specification in levels (blue line with diamonds), an OLS specification in differences (red line with triangles), and an IV specification in differences (green line with squares). For all specifications, relative to P50, percentiles above the median and reaching up as high as the 90th percentile remain statistically significant. Relative to P90, spillover effects are present up to at least the 70th wage percentile. Notably, the IV specification in levels closely matches our baseline results, which were estimated via OLS in levels.

Therefore, our main conclusions regarding the reach of spillovers remain unchanged under alternative specifications.

Figure B.13. Alternative specifications

A. Relative to P50 B. Relative to P90



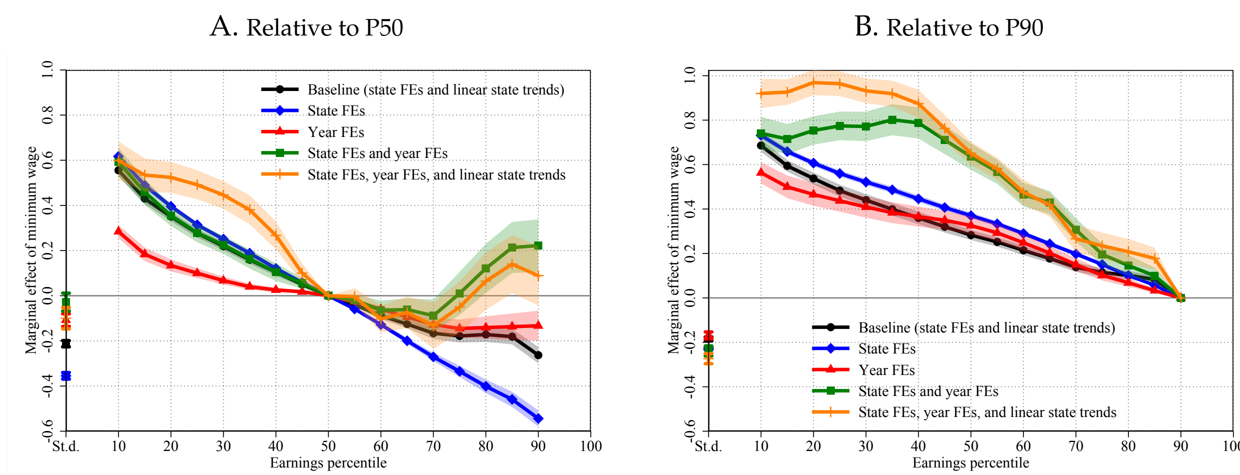
Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). Each panel shows the results from four different specifications (colored markers and lines, plus error bars or shaded areas). The black circles and line correspond to the baseline specification from the main text, which is estimated via OLS in levels. The blue diamonds and line correspond to the same specification estimated via IV in levels. The red triangles and line correspond to the same specification estimated via OLS in differences. The green squares and line correspond to the same specification estimated via IV in differences. The IV strategy instruments the Kaitz-p index and its square using an instrument set that consists of the log real statutory minimum wage, its square, and the log real statutory minimum wage interacted with the mean of the log real median wage for the region over the full sample period. Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The four error bars and four shaded areas represent 99 percent confidence intervals based on regular (i.e., not clustered) standard errors. *Source:* RAIS, 1996–2018.

B.7 Additional regression results: Alternative controls

Figure B.13 compares our baseline results (black line with circles) with four specifications using alternative sets of controls, namely only state fixed effects (blue line with diamonds), only year fixed effects (red line with triangles), state fixed effects and year fixed effects (green line with squares), and a final specification with state fixed effects, year fixed effects, and state-specific linear time trends (orange line with plus signs). For all specifications, relative to P50, percentiles above the median and reaching up as high as the 90th percentile remain statistically significant. Relative to P90, spillover effects are consistently present up to the 90th wage percentile.

Therefore, our main conclusions regarding the reach of spillovers remain unchanged under alternative sets of controls.

Figure B.14. Alternative controls



Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). *Source:* RAIS, 1996–2018.

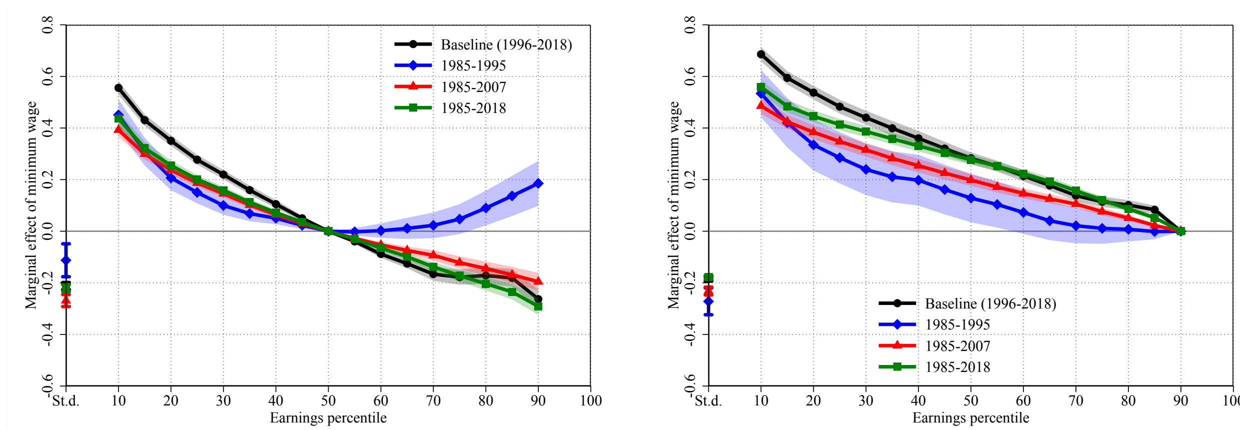
B.8 Additional regression results: Alternative time periods

Figure B.13 compares our baseline results (black line with circles) with three specifications using alternative time periods, namely the high-inflation period of 1985–1995 (blue line with diamonds), a period from 1985–12007 that is equally long as but earlier than our baseline period (red line with triangles), and the period of all available years from 1985–2018 (green line with squares). For all specifications, relative to P50, percentiles above the median and reaching up as high as the 90th percentile remain statistically significant. Relative to P90, all but the early high-inflation period from 1985–1995 show spillover effects that are consistently present up to the 90th wage percentile. Results for the early high-inflation period are measured more noisily and are significant only below the 60th percentile of wages.

Therefore, our main conclusions regarding the reach of spillovers remain unchanged under alternative time periods and, furthermore, Brazil early high-inflation period does not exhibit significantly stronger spillovers than the later period we study.

Figure B.15. Alternative time periods

A. Relative to P50 B. Relative to P90



Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). *Source:* RAIS, 1996–2018.

B.9 Additional regression results: Polynomial orders for state-specific trends

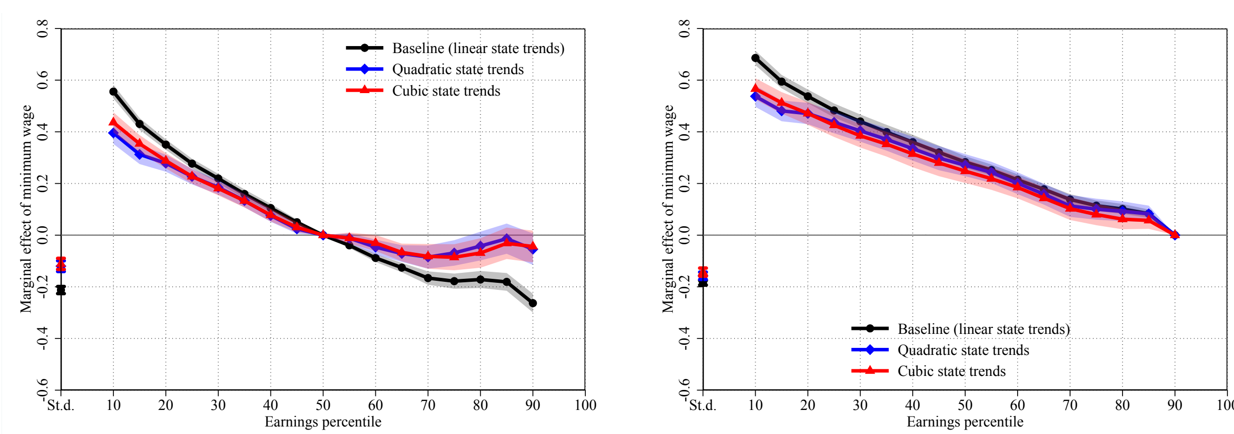
There has been a fruitful debate concerning the potential benefits and harms from including region-specific time trends in econometric studies of the minimum wage—see for example Neumark et al. (2014), Allegretto et al. (2017), and Neumark and Wascher (2017). To demonstrate the robustness of our findings to the concerns raised by this debate, Figure B.16 shows additional specifications based on the econometric methodology in Section 4.2. Specifically, we vary the polynomial degree order for state-specific time trends between one and three (with the degree-zero specifiction being that with only state fixed effects presented in Appendix B.14). Figure B.16 presents the results from these alternative specifications.

Starting with a description of Figure B.16, we note that specifications with either a quadratic or cubic trend show similar patterns to one another in terms of estimated marginal effects, particularly in the lower tail of the wage distribution. The estimated patterns are also similar to our baseline specification with state-specific linear time trends. All three specifications deliver significant effects of the minimum wage above the median, when measured relative to P50, and up to the 90th percentile of wages, when measured relative to P90.[6]

Therefore, our main conclusions regarding the reach of spillovers remain unchanged under alternative polynomial orders for state-specific time trends.

Figure B.16. Alternative trends

A. Relative to P50 B. Relative to P90



Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). Each panel shows the results from three different specifications (colored lines and error bars or shaded areas) with different polynomial degrees of the region-specific time trends: linear (black circles and line), quadratic (blue diamonds and line), and cubic (red triangles and line) polynomials in the calendar year—all estimated using OLS in levels. Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The three error bars and three shaded areas represent 99 percent confidence intervals based on regular (i.e., not clustered) standard errors. *Source:* RAIS, 1996–2018.

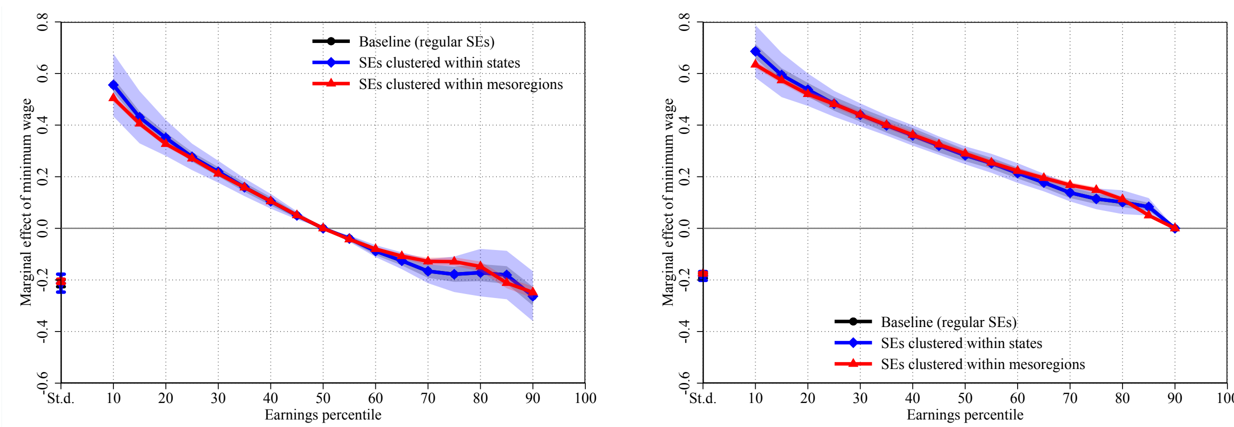
[6]While our finding of far-reaching spillover effects of the minimum wage are robust to the choice of state-specific time trends, we note that including state-specific higher-order time trends in this specification may possibly be asking for too much from our comparably short state-year panel of 23 years. For this reason, we prefer the specification with state fixed effects and only state-specific linear time trends as our baseline.

B.10 Additional regression results: Clustering standard errors

Figure B.17 shows additional specifications based on the econometric methodology in Section 4.2 of the main text. Specifically, although the number of clusters at the state level falls below common thresholds for clustering (Cameron and Miller, 2015), we here cluster standard errors at the state level. In contrast, Section 4.2 shows results based on unadjusted standard errors. The main take-away from Figure B.17 is that standard errors become somewhat larger under clustering of standard errors at the state level, though they become smaller again when estimating our specification and clustering standard errors at the mesoregion level. Therefore, our main conclusions regarding the reach of spillovers remain unchanged under alternative ways of clustering standard errors.

Figure B.17. Alternative standard errors

A. Relative to P50 B. Relative to P90



Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). Each panel shows the results from three different specifications (colored lines and error bars or shaded areas) with different ways of constructing standard errors: estimating equation equation (2) at the state level with regular (i.e., not clustered) standard errors (black circles and line), estimating equation equation (2) at the state level with standard errors clustered at the state level (blue diamonds and line), and estimating equation equation (2) at the mesoregion level with standard errors clustered at the mesoregion level (red triangles and line). Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The error bars and shaded areas represent 99 percent confidence intervals based on standard errors that are clustered either at the state level (black and blue markers and lines) or at the mesoregion level (red markers and line). *Source:* RAIS, 1996–2018.

B.11 Comparison with Haanwinckel (2020)

In complementary work, Haanwinckel (2020) follows our approach of estimating spillover effects of the minimum wage in Brazil using a methodology based on the seminal framework by Lee (1999) and the more recent contribution by Autor et al. (2016). Using a subset of 14 years of the RAIS data from 1996–2001, 2005–2009, and 2011–2013, Haanwinckel (2020) reports significant spillover effects of the minimum wage up to the 40th wage percentile with a 95 percent confidence interval, and up to the 30th wage percentile with a 99 percent confidence interval. The standard error bands estimated by Haanwinckel (2020) are relatively large above the median, rendering the point estimates indistinguishable from zero. In contrast, our baseline results using the 1996–2018 data indicates spillovers that are significant up to at least the 75th percentile of the wage distribution.

There are several notable differences between our empirical approach and that in Haanwinckel (2020). We discuss two key differences here. One difference is the exact econometric specification, which in the case of Haanwinckel (2020) includes a national quadratic time trend in addition to the state fixed effects and state-specific linear trends that we include in our baseline specification. We show below, by including a national quadratic trend, that this difference in specifications does not systematically change our insights, though some error bands increase significantly. A second difference is the set of years on which these specifications are estimated. We show below, by estimating our specification as well as those including a national quadratic trend, that this difference does explain some of the discrepancy in findings between our work and that by Haanwinckel (2020). Furthermore, estimating our baseline specification and those with national quadratic trends on the full sample of 34 years of the RAIS data from 1985–2018, we robustly find significant spillovers up to at least the 75th wage percentile with comparably narrow standard error bands.

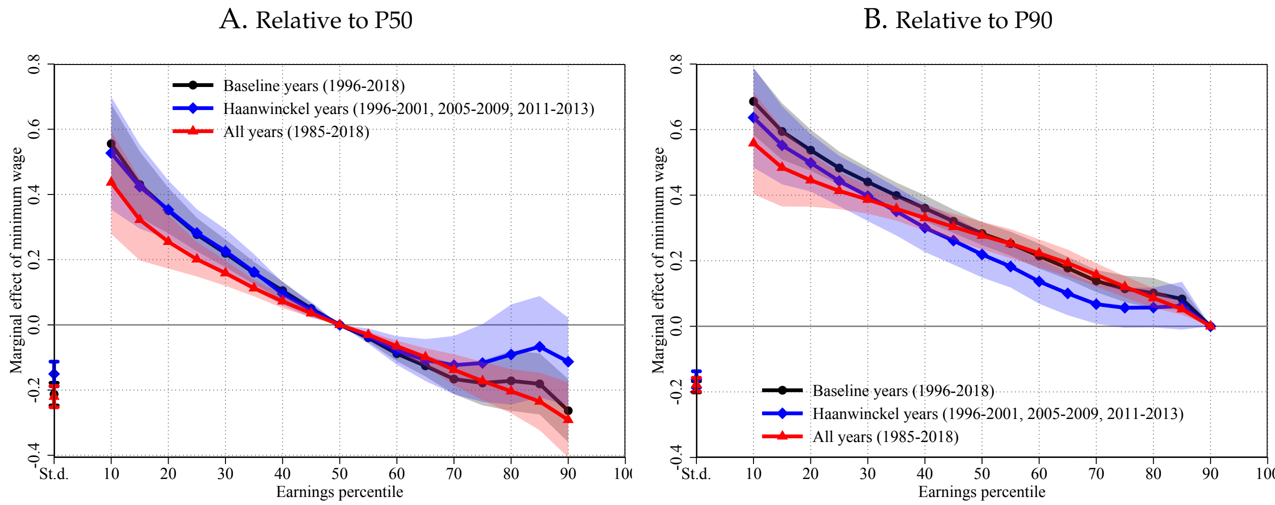
To establish these results, we proceed in two steps. First, we reestimate our baseline specification for different sets of years to investigate their sensitivity to the choice of time period. Second, we implement the specifications from Haanwinckel (2020) on our baseline period (1996–2018) and compare the results to those from the same specification using either the restricted set of years used in Haanwinckel (2020) (1996–2001, 2005–2009, and 2011–2013), or the full set of years in the RAIS data (1985–2018).

Alternative results using baseline specification estimated on different time periods. We reestimate our baseline specification with state fixed effects and state-specific linear trends on three sets of years of the RAIS data, namely our baseline period comprising 1996–2018 (black line with circles), the years used in Haanwinckel (2020) comprising 1996–2001, 2005–2009, and 2011–2013 (blue line with diamonds), and the full period comprising 1985–2018 (red line with triangles). To remain conservative and also to match the choice in Haanwinckel (2020), we cluster standard errors at the state level.

Figure B.18 shows the resulting estimates of these three specifications. Several points are worth noting. First, our baseline estimates remain significant up to the 90th percentile, both relative to P50 and also relative to P90. Second, the point estimates based on the subset of years used in Haanwinckel (2020) are close to ours in the lower tail, and slightly less pronounced in the upper tail, relative to P50. They are uniformly below ours, relative to P90. Third, a the same time, standard errors are significantly higher when using a subset of years, particularly in the upper tail. This results in point estimates above the 70th percentile being insignificant based on the years used in Haanwinckel (2020), while they remain significant in our baseline. Fourth, using additional years going back to 1985 leads to similar point estimates and standard error bands compared to our baseline estimates.

From this, we conclude that—starting from our baseline specification—the choice of years matters somewhat for the estimated reach of spillovers, though the estimated effects remain significant up to the 70th percentile of wages.

Figure B.18. Comparison with Haanwinckel (2020): State FEs and linear state time trends, OLS in levels

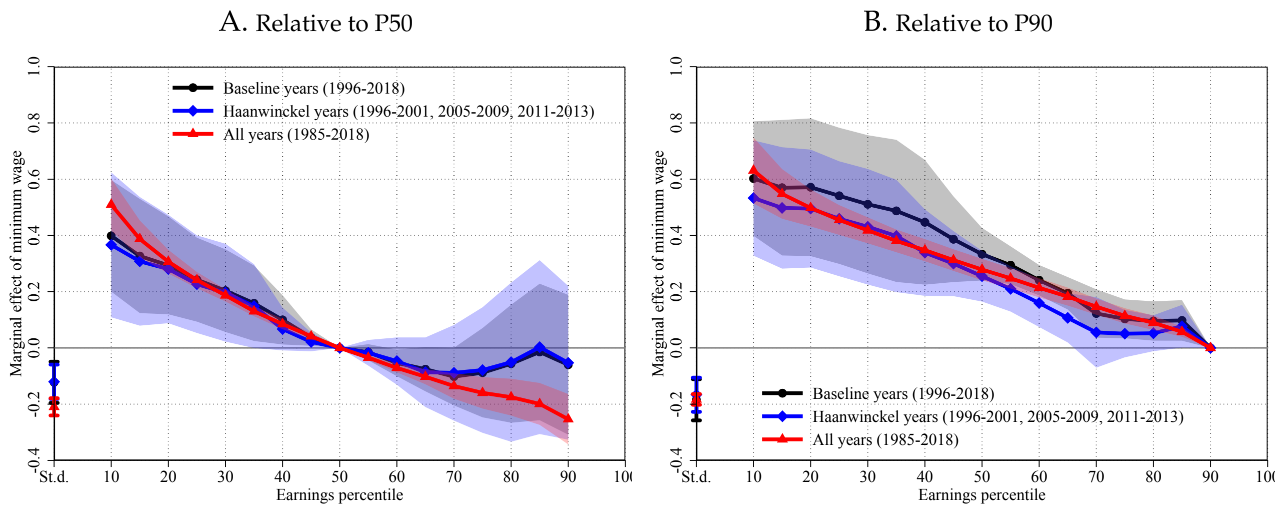


Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). Each panel shows three sets of estimation results (colored lines and error bars or shaded areas) based on different time periods: our baseline period from 1996–2018 (black circles and line), the subset years used in Haanwinckel (2020), comprising 1996–2001, 2005–2009, and 2011–2013 (blue diamonds and line), and all available years from 1985–2018 (red triangles and line). The included controls comprise a set of state fixed effects and state-specific linear time trends, which corresponds to the baseline specification in the main text. The specification is estimated via OLS in levels. Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The error bars and shaded areas represent 99 percent confidence intervals based on standard errors that are clustered at the state level. *Source:* RAIS, 1985–2018.

Alternative results using specifications from Haanwinckel (2020) estimated on different time periods. Next, we estimate specifications that include the same set of controls as in Haanwinckel (2020): state fixed effects, state-specific linear trends, and a national quadratic trend. We first estimate these specifications via OLS in levels. As above, we estimate the specification on three different time periods, namely our baseline period comprising 1996–2018 (black line with circles), the years used in Haanwinckel (2020) comprising 1996–2001, 2005–2009, and 2011–2013 (blue line with diamonds), and the full period comprising 1985–2018 (red line with triangles). As above, we cluster standard errors at the state level.

The results are presented in Figure B.19. There are a couple of take-aways. First, the inclusion of a national quadratic trends slightly attenuates the point estimates and significantly increases the standard error bands. Second, using our baseline years continues to yield significant effects of the minimum wage up to the median and again at the 60th wage percentile, relative to P50. All estimated effects remain significant up to the 90th wage percentile, relative to P90. Third, the subset of years used in Haanwinckel (2020) yield point estimates that are substantially similar to our baseline but not statistically significant above the 35th wage percentile, relative to P50. Point estimates are somewhat attenuated and remain significant up to the 65th wage percentile, relative to P90. Finally, using the full set of years yields point estimates that lie within the standard error bands of the other two sets of estimates but themselves have significantly tighter error bands.

Figure B.19. Comparison with Haanwinckel (2020): State FEs, linear state time trends, and quadratic national trend, OLS in levels

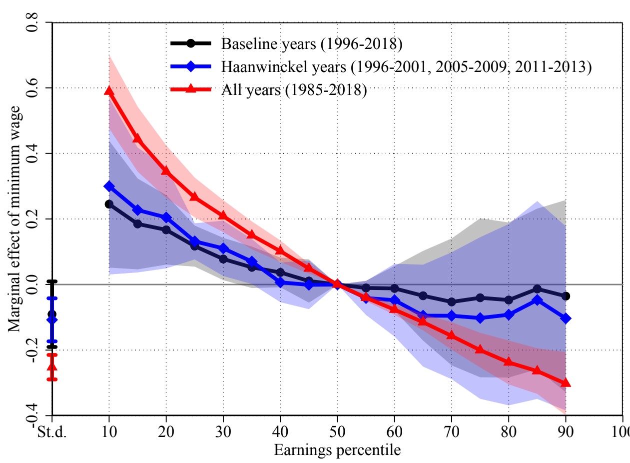


Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). Each panel shows three sets of estimation results (colored lines and error bars or shaded areas) based on different time periods: our baseline period from 1996–2018 (black circles and line), the subset years used in Haanwinckel (2020), comprising 1996–2001, 2005–2009, and 2011–2013 (blue diamonds and line), and all available years from 1985–2018 (red triangles and line). The included controls comprise a set of state fixed effects, state-specific linear time trends, and a national quadratic time trend, which corresponds to the specification in Haanwinckel (2020). The specification is estimated via OLS in levels. Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The error bars and shaded areas represent 99 percent confidence intervals based on standard errors that are clustered at the state level. *Source:* RAIS, 1985–2018.

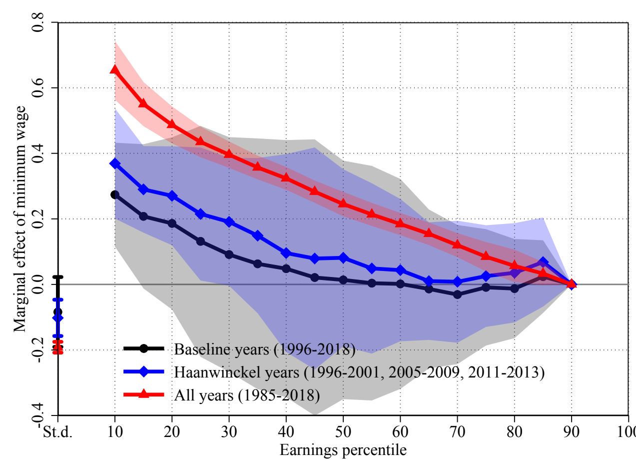
Next, we estimate specifications that include state fixed effects, state-specific linear trends, and a national quadratic trend via IV in levels. Figure B.20 shows the results. Both the estimates for our baseline set of years and that used in Haanwinckel (2020) yield estimates that are significant up to the 30th wage percentile but not above, due to very wide standard error bands, relative to P50. Relative to P90, the error bands are even wider. Both of these results suggest that the inclusion of a quadratic national trend is leaving little variation over and above that induced by the instrumented Kaitz-p indices over the period from 1996 to 2018. In contrast, the specification based on the full set of years continues to show significant effects of the minimum wage up to the 90th percentile.

Figure B.20. Comparison with Haanwinckel (2020): State FEs, linear state time trends, and quadratic national trend, IV in levels

A. Relative to P50



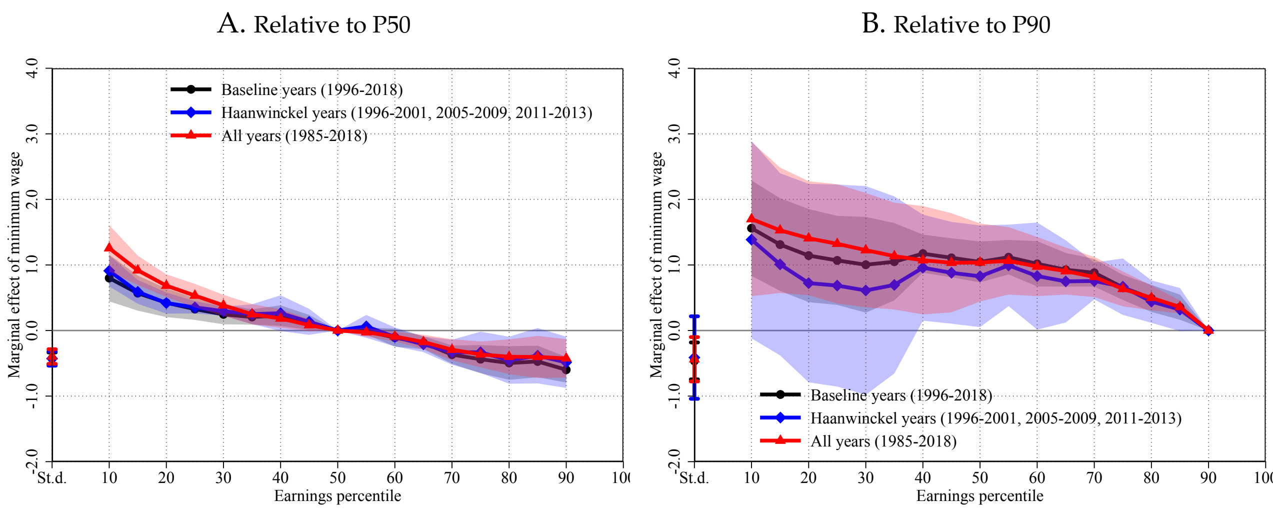
B. Relative to P90



Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). Each panel shows three sets of estimation results (colored lines and error bars or shaded areas) based on different time periods: our baseline period from 1996–2018 (black circles and line), the subset years used in Haanwinckel (2020), comprising 1996–2001, 2005–2009, and 2011–2013 (blue diamonds and line), and all available years from 1985–2018 (red triangles and line). The included controls comprise a set of state fixed effects, state-specific linear time trends, and a national quadratic time trend, which corresponds to the specification in Haanwinckel (2020). The specification is estimated via IV in levels. The IV strategy instruments the Kaitz-p index and its square using an instrument set that consists of the log real statutory minimum wage, its square, and the log real statutory minimum wage interacted with the mean of the log real median wage for the region over the full sample period. Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The error bars and shaded areas represent 99 percent confidence intervals based on standard errors that are clustered at the state level. *Source:* RAIS, 1985–2018.

Next, we estimate specifications that include state fixed effects, state-specific linear trends, and a national quadratic trend via OLS in differences. Figure B.21 shows the results. Qualitatively, the results are similar to the OLS specification in levels above. All specifications show some significant effects above the median and up to the 85th percentile, relative to P50. At the same time, the standard error bands are relatively wider for the specification using only the subset of years in Haanwinckel (2020). This becomes particularly evident when looking at the results relative to P90, which have error bands large enough to render some of the lower—but not the higher—wage percentiles insignificant. As above, the specification estimated on the full set of years remains significant throughout most of the wage distribution.

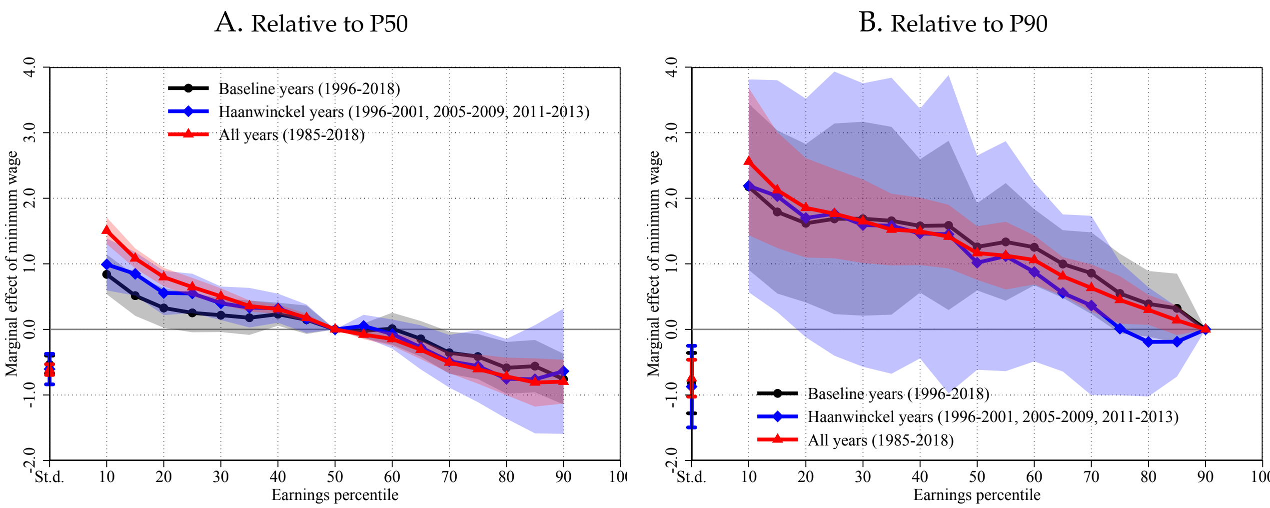
Figure B.21. Comparison with Haanwinckel (2020): State FEs, linear national trend, OLS in differences



Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). Each panel shows three sets of estimation results (colored lines and error bars or shaded areas) based on different time periods: our baseline period from 1996–2018 (black circles and line), the subset years used in Haanwinckel (2020), comprising 1996–2001, 2005–2009, and 2011–2013 (blue diamonds and line), and all available years from 1985–2018 (red triangles and line). The included controls comprise a set of state fixed effects and a national linear time trend, which corresponds to the specification in Haanwinckel (2020). The specification is estimated via OLS in differences. Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The error bars and shaded areas represent 99 percent confidence intervals based on standard errors that are clustered at the state level. *Source:* RAIS, 1985–2018.

Finally, we estimate specifications that include state fixed effects, state-specific linear trends, and a national quadratic trend via IV in differences. Figure B.22 shows the results. Qualitatively, the results are similar to the previous specification using OLS in differences. One notable difference is that, while our baseline estimates remain significant throughout most of the wage distribution, both relative to P50 and relative to P90, the estimates using the years in Haanwinckel (2020) are all significant up to the 85th wage percentile using our baseline set of years, but significant only up to the 15th wage percentile using the subset of years in Haanwinckel (2020).

Figure B.22. Comparison with Haanwinckel (2020): State FEs, linear national trend, IV in differences



Notes: Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2). Each panel shows three sets of estimation results (colored lines and error bars or shaded areas) based on different time periods: our baseline period from 1996–2018 (black circles and line), the subset years used in Haanwinckel (2020), comprising 1996–2001, 2005–2009, and 2011–2013 (blue diamonds and line), and all available years from 1985–2018 (red triangles and line). The included controls comprise a set of state fixed effects and a national linear time trend, which corresponds to the specification in Haanwinckel (2020). The specification is estimated via IV in differences. The IV strategy instruments the Kaitz-p index and its square using an instrument set that consists of the log real statutory minimum wage, its square, and the log real statutory minimum wage interacted with the mean of the log real median wage for the region over the full sample period. Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The error bars and shaded areas represent 99 percent confidence intervals based on standard errors that are clustered at the state level. *Source:* RAIS, 1985–2018.

From this, we conclude that our results are largely robust to the set of specfications used in Haanwinckel (2020), although for some specifications the standard error bands associated with the inclusion of quadratic national trends for the time period starting in 1996 leads to strikingly wide error bands. It is worth noting that our results are similarly pronounced and tightly estimated when using the full set of years from 1985 to 2018. In contrast, the results using the subset of years from Haanwinckel (2020) tend to deliver more noisy estimates, leading point estimates to turn insignificant lower in the wage distribution than would otherwise be the case.

Summary. From the above analysis, we conclude that the results presented in the main text are robust to alternative specifications, including those presented in Haanwinckel (2020).

Considering additional years of data between 1996 and 2018 (and, separately, between 1985 and 2018) allows us to exploit significantly more variation in the effective bindingness of the federal minimum wage—see Appendix B.12—which leads us to find minimum wage spillovers that robustly reach up to at least the 75th wage percentile. This finding is reassuring given that our structural model, as well as the alternative structural model developed by Haanwinckel (2020), predict spillover effects throughout most of the wage distribution.

To conclude, we note that there are other important differences between our work and the complementary analysis contained in Haanwinckel (2020), over and above the exact specification and choice of years. For example, Haanwinckel (2020) uses the population of men and women between the ages of 18 and 54, while the analysis of the current paper (Engbom and Moser, 2022a) and that in Engbom and Moser (2018) is restricted to only men. These differences likely explain the remaining divergence between our results and those presented in Haanwinckel (2020).

B.12 Comparison of relative bindingness of the minimum wage, Brazil versus U.S.

Table B.1 compares the relative bindingness of the minimum wage, as proxied by lower-tail wage inequality, between Brazil and the U.S.

Table B.1. Lower-tail wage inequality in Brazil and in the U.S.

	Brazil		
Year	Baseline (1996–2018)	All years (1985–2018)	U.S.
1979			−0.64
1980			−0.65
1981			−0.68
1982			−0.71
1983			−0.73
1984			−0.73
1985		−0.68	−0.74
1986		−0.73	−0.74
1987		−0.81	−0.73
1988		−0.76	−0.72
1989		−0.80	−0.72
1990		−0.90	−0.72
1991		−0.85	−0.71
1992		−0.81	−0.72
1993		−0.76	−0.73
1994		−0.82	−0.71
1995		−0.84	−0.71
1996	−0.79	−0.79	−0.71
1997	−0.77	−0.77	−0.69
1998	−0.74	−0.74	−0.69
1999	−0.72	−0.72	−0.69
2000	−0.68	−0.68	−0.68
2001	−0.67	−0.67	−0.68
2002	−0.65	−0.65	−0.69
2003	−0.58	−0.58	−0.69
2004	−0.58	−0.58	−0.70
2005	−0.59	−0.59	−0.71
2006	−0.55	−0.55	−0.70
2007	−0.54	−0.54	−0.70
2008	−0.53	−0.53	−0.71
2009	−0.52	−0.52	−0.74
2010	−0.52	−0.52	−0.73
2011	−0.53	−0.53	−0.72
2012	−0.51	−0.51	−0.74
2013	−0.52	−0.52	
2014	−0.54	−0.54	
2015	−0.55	−0.55	
2016	−0.52	−0.52	
2017	−0.51	−0.51	
2018	−0.51	−0.51	
Minimum	−0.79	−0.90	−0.74
Maximum	−0.51	−0.51	−0.64
Range	0.28	0.39	0.10
Standard deviation	0.09	0.13	0.02

Notes: This table shows the relative bindingness of the minimum wage, as proxied by lower-tail wage inequality, for Brazil between 1996 and 2018 (column 1), Brazil from 1985 to 2018 (column 2), and the U.S. from 1979 to 2012. Lower-tail inequality is measured using the mean log wage percentile ratio P10/P50 computed across states in a given year. "Minimum" denotes the minimum of the mean log wage percentile ratio taken across all years in a given country. "Maximum" denotes the maximum of the mean log wage percentile ratio taken across all years in a given country. "Range" denotes the range (i.e., the maximum minus the minimum) of the mean log wage percentile ratio taken across all years in a given country. "Standard deviation" denotes the standard deviation of the mean log wage percentile ratio taken across all years in a given country. The data for Brazil are the same as used in the analysis presented in the main text and cover 1985 to 2018. The data for the U.S. are from Autor et al. (2016), who report these statistics between 1979 and 2012. *Source:* RAIS, 1985–2018, and Autor et al. (2016).

B.13 Comparison of wages in nominal terms and in multiples of the minimum wage

In this subsection, we compare the distributions of (changes in) wages across two numeraires: current BRL and the current minimum wage. We demonstrate that while the minimum wage does share certain attributes of a numeraire (Neri and Moura, 2006), it does so imperfectly. Consequently, it is not purely mechanical that wages throughout the wage distribution are affected by the minimum wage, as documented in Section 4.2.

To see why the distinction between the two numeraires matters, consider the following example. Suppose worker A gets paid 2.0 times the minimum wage throughout their employment spell that lasts from January to June of a given calendar year. Suppose worker B also gets paid 2.0 times the minimum wage but their employment spell lasts from January to December. Now suppose that the statutory minimum wage stands at BRL 400 from January to June, then increases to BRL 500 from July to December. Then the two workers' wages are the same in multiples of the current minimum wage (i.e., both receive constant pay equal to 2.0 times the current minimum wage) but differ in terms of nominal BRL (i.e., worker A has a mean wage of BRL 400 that consists of a constant stream, while worker B has a mean wage of BRL 450 that changes levels over time). Therefore, the numeraire matters for our measure of wage dispersion within and across individuals.

We begin our analysis with the variable in RAIS that contains each job's mean wage in multiples of the current minimum wage in each year, which is the one underlying our entire analysis in the main text. To obtain a wage measure in nominal BRL, we multiply the mean wage in multiples of the current minimum wage by the months-of-employment-weighted mean of the minimum wage prevailing during that year, using each job's start and end months. Our measurement likely contains some noise because both wages and the minimum wage itself can change over time within a job spell.

Comparing log wages in current BRL and multiples of the current minimum wage, we note that only 2.4 percent of all workers have a constant wage in terms of multiples of the current minimum wage between two consecutive years. For comparison, that number is 0.2 percent in nominal terms. That the latter number is smaller than the former suggests that the minimum wage does serve some numeraire function. At the same time, both shares are small, highlighting the importance of idiosyncratic wage changes.

Figure B.23 shows histograms of (changes in) wages in each of the two numeraires. Panel A shows that the distribution of log wages has a relatively more pronounced spike in multiples of the current minimum wage, which is our baseline wage measure. The distribution of one-year changes in log wages in panel B shows less dispersion in multiples of the minimum wage, particularly in the right tail. However, both distributions are significantly dispersed, indicating that by both wage measures are far from fixed at a constant multiple of the numeraire.

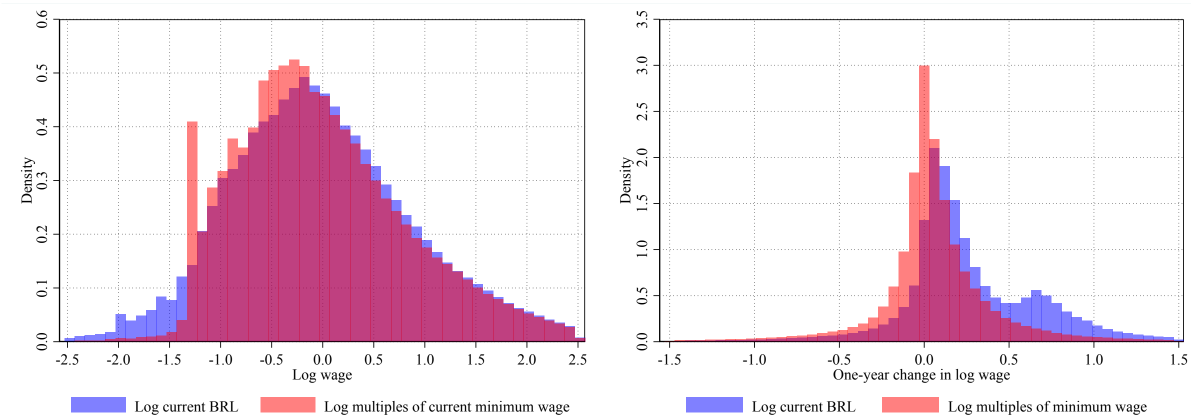
Another way to assess the minimum wage's numeraire function is a simple variance decomposition, where we write the variance of some outcome variable of interest, y_{it}, across individuals i and years t as

$$Var(y_{it}) = \underbrace{Var(\mathbb{E}[y_{it}|i])}_{\text{Variance of individual-level means}} + \underbrace{Var(y_{it} - \mathbb{E}[y_{it}|i])}_{\text{Variance of dispersion around individual-level means}} . \tag{B.1}$$

Here, the outcome variable y_{it} can be either log wages or the one-year changes in log wages. Table B.2 shows the results from the variance decomposition in equation (B.1) for different population subgroups (all workers, only stayers in the same occupation, only stayers at the same employer), for log wages versus one-year changes in log wages, and for the two numeraires: current BRL and the current minimum wage. Starting with wage levels in panel A, the total variance of log wages is slightly higher in current BRL than in multiples of the current minimum wage, 88.8 log points compared to 75.0 log points.

Figure B.23. Histograms of (changes in) log wages, nominal versus multiples of minimum wage

A. Levels B. One-year changes



Notes: Figure shows levels of (panel A) and one-year changes in (panel B) log wages in terms of two numeraires: current BRL and the current minimum wage. That is, each panel shows the distribution of earnings in log current BRL (blue bars), calculated as the logarithm of the mean multiples of the current minimum wage recorded during each calendar year multiplied by the mean minimum wage prevailing during the months of employment during that year, and the log mean multiples of the current minimum wage (red bars). For panel A, log wages in each numeraire are normalized to be mean zero. *Source:* RAIS, 1994–1998.

Around 25 percent of the total variance is attributable to the variance of dispersion around individual-level means when measured in current BRL, compared to 12 percent when measured in multiples of the current minimum wage. This suggests that the minimum wage partially serves as a numeraire for wages in the economy. Turning now to one-year changes in log wages, we see that, at the same time, there is substantial dispersion in wages from one year to the next, regardless of the numeraire, with variances of 25.1 log points in current BRL and 17.9 log points in multiples of the current minimum wage. A similar share of the total variance of one-year changes in log wages is due to the variance of dispersion around individual-level means, around 73 percent in current BRL compared to 70 percent in multiples of the current minimum wage. This tells us that earnings are not fixed, not even in terms of multiples of the current minimum wage. Finally, Panels B and C show the same variance decompositions for workers who remain in the same occupation (panel B) or at the same employer (panel C) between years, leading us to draw broadly similar conclusions.

We conclude that, while the minimum wage does share certain attributes of a numeraire (Neri and Moura, 2006), it does so imperfectly. Thus, our empirical results in Section 4.2 are unlikely to be driven solely by this phenomenon.

Table B.2. Comparison of wages in nominal terms and in multiples of the minimum wage

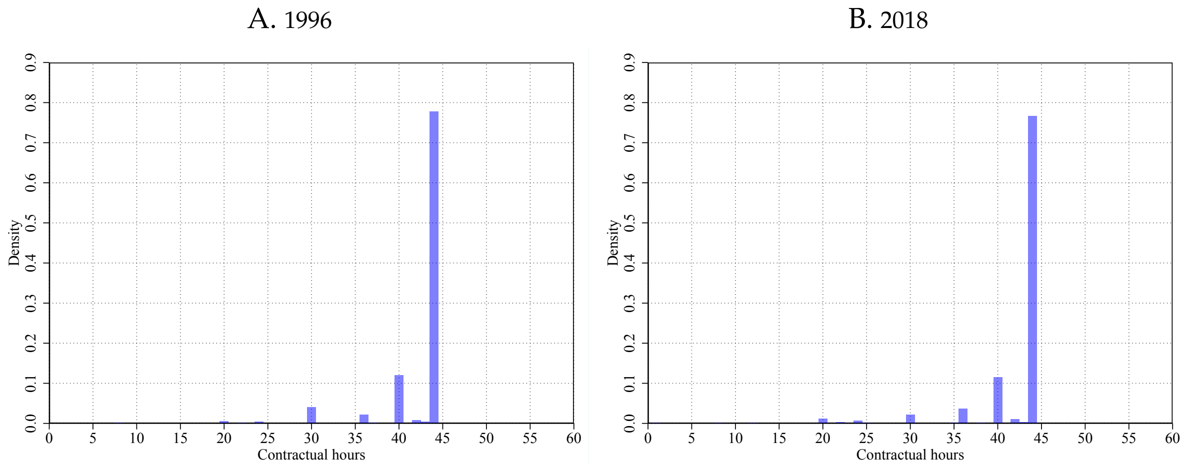
	Log wages		One-year changes in log wages	
	Current BRL	Current MW	Current BRL	Current MW
Panel A. All workers				
Total variance	0.888	0.750	0.251	0.179
Variance of individual-level means	0.667 (75%)	0.661 (88%)	0.069 (27%)	0.054 (30%)
Variance of dispersion around individual-level means	0.221 (25%)	0.088 (12%)	0.182 (73%)	0.125 (70%)
Panel B. Only stayers in same occupation				
Total variance	0.791	0.768	0.174	0.115
Variance of individual-level means	0.727 (92%)	0.728 (95%)	0.071 (41%)	0.055 (48%)
Variance of dispersion around individual-level means	0.064 (8%)	0.040 (5%)	0.103 (59%)	0.060 (52%)
Panel C. Only stayers at same employer				
Total variance	0.791	0.778	0.157	0.101
Variance of individual-level means	0.739 (93%)	0.741 (95%)	0.065 (41%)	0.052 (51%)
Variance of dispersion around individual-level means	0.052 (7%)	0.036 (5%)	0.091 (58%)	0.049 (49%)

Notes: This table shows the total variance of log wages and of one-year changes in log wages based on equation (B.1). Panel A shows the results for the whole population, while panel B shows those for workers who stay in the same occupation between consecutive years and panel C shows those for workers who stay at the same employer between years. *Source:* RAIS, 1994–1998.

B.14 Hours distribution and its relation to the bindingness of the minimum wage

Most adult males in Brazil's formal sector work in a full-time contract, defined as either 40 work hours (spread across 5 days) or 44 work hours (spread across 6 days) per week. Figure B.24 shows the raw distribution of hours for this population for 1996 in panel A, and for 2018 in panel B. In the initial period, around 74 percent of all workers work 44 hours per week and another 15 percent work 40 hours per week, constituting around 89 percent of workers in full-time employment. Only arounnd five percent of all employees are in employment arrangements with less than 35 contractual work hours per week.[7] Furthermore, the comparison between panels A and B show that there is no evidence over time—as the minimum wage increases—of a shift towards shorter work weeks in the aggregate. In contrast, there was a small reduction in the share of workers with 30 hour contracts that in the aggregate shifted toward 44 hour contracts.

Figure B.24. Histogram of contractual hours, 1996 and 2018

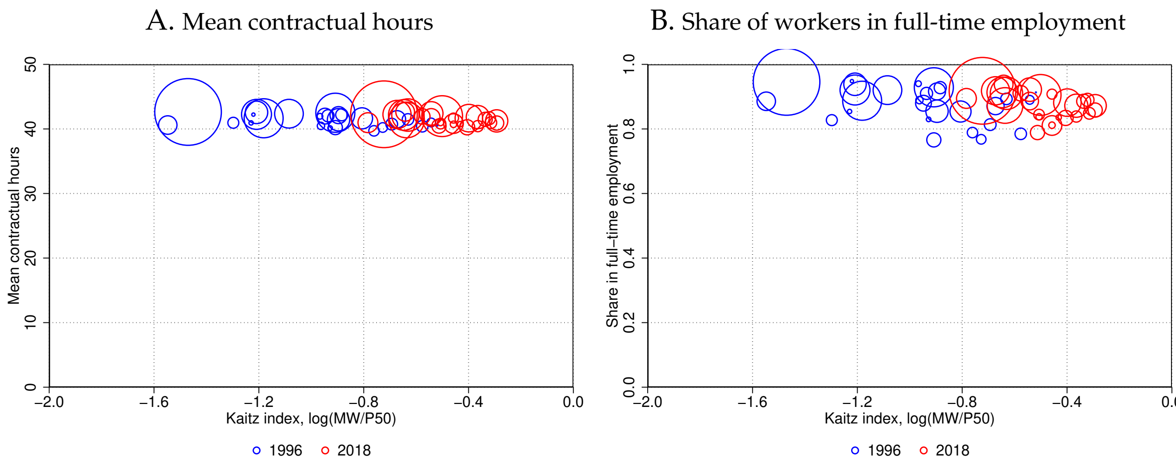


Notes: Figure shows density of contractual work hours for period 1996–2000 in panel A and for 2018 in panel B. A small number of observations reporting more than 60 hours are omitted from the graphs. *Source:* RAIS, 1996 and 2018.

Going beyond the aggregate statistics, there is also little systematic covariation between the relative bindingness of the minimum wage and work hours across Brazilian states. Figure B.25 shows that both the share of full-time employment in panel A and the mean number of hours in panel B stay constant as the minimum wage increases between 1996 and 2018. While in 1996 there is a weak systematic negative relationship between the full-time worker share and the bindngnes of the minimum wage, measured by the Kaitz-50 index, this appears almost entirely driven by transitory fluctuations and permanent state-specific heterogeneity, rather than a correlation with the rising minimum wage over time within states.

[7] According to the Bureau of Labor Statistics a higher share, around 12 percent of employees, works part-time (less than 35 hours) in the US in 2017.

Figure B.25. Relation between contractual hours and bindingness of the minimum wage



Notes: Figure shows for male workers of age 18–49 the share in full-time employment, defined as working 40 hours or more (panel A), and the mean number of contractual work hours (panel B), against the Kaitz-50 index, $kaitz_{st}(50) \equiv \log w_t^{min} - \log w_{st}^{P50}$, across states in 1996 and 2018. Area of circles is proportional to population size. *Source:* RAIS, 1996 and 2018.

C Model Appendix

This appendix provides further details on the equilibrium model presented in Section 5, including subsections on the model-implied employment distribution (Appendix C.1), the steady-state firm size mapping (Appendix C.2), the equilibrium definition (Appendix C.3), and the numerical solution algorithm (Appendix C.4).

C.1 The employment distribution

The employment distribution, $G(z|a,s)$, is given by the Kolmogorov Forward Equation (KFE),

$$0 = -\Big(\delta(a,s) + sp(a,s)(1 - F(z|a,s))\Big)G(z|a,s)e(a,s) + p(a,s)u(a,s)F(z|a,s)$$

The first term reflects the number $G(z|a,s)e(a,s)$ of workers employed at firms with productivity at most z. These workers flow into unemployment at rate $\delta(a,s)$. They receive outside offers at rate $sp(a,s)$, which in equilibrium they accept if they come from more productive firms, $1 - F(z|a,s)$. The last term reflects inflows into firms with productivity at most z. Since no employed worker accepts a job offer from a less productive firm, the only inflows are from unemployment. In particular, the unemployed receive job offers at rate $p(a,s)$. Rearranging, we get the expression in the paper,

$$G(z|a,s) = \frac{p(a,s)F(z|a,s)}{\delta(a,s) + sp(a,s)(1 - F(z|a,s))}\frac{u(a,s)}{e(a,s)}$$

C.2 The steady-state firm size mapping

The size $l(w,v|a,s)$ of a firm posting vacancies v with wage w in market (a,s) is given by the KFE

$$0 = -\Big(\delta(a,s)+sp(a,s)(1-F(z|a,s))\Big)l(w,v|a,s)+vq(a,s)\left(\frac{u(a,s)}{S(a,s)}+\frac{se(a,s)}{S(a,s)}G(z|a,s)\right)$$

The firm looses workers to unemployment and up the job ladder. Each vacancy contacts a potential hire at rate $q(a,s)$, who is unemployed with probability $u(a,s)/S(a,s)$ and employed with complementary probability. Employed workers are hired iff they are at a less productive firm. Noting that $q(a,s) = p(a,s)S(a,s)/V(a,s)$, substituting and rearranging,

$$l(w,v|a,s) = \frac{v}{V(a,s)}p(a,s)\frac{u(a,s)+se(a,s)G(z|a,s)}{\delta(a,s)+sp(a,s)(1-F(z|a,s))}$$

Using equation (6) to substitute for the employment distribution $G(\cdot)$,

$$\begin{aligned}
l(w,v|a,s) &= \frac{v}{V(a,s)}p(a,s)\frac{u(a,s)\frac{(\delta(a,s)+sp(a,s)(1-F(w|a,s)))}{(\delta(a,s)+sp(a,s)(1-F(w|a,s)))}+s\frac{p(a,s)F(w|a,s)}{\delta(a,s)+sp(a,s)(1-F(w|a,s))}u(a,s)}{\delta(a,s)+sp(a,s)(1-F(w|a,s))} \\
&= \frac{vu(a,s)p(a,s)}{V(a,s)}\frac{\delta(a,s)+sp(a,s)\left(1-F\left(w|a,s\right)\right)+sp(a,s)F\left(w|a,s\right)}{\left(\delta(a,s)+sp(a,s)(1-F(w|a,s))\right)^2} \\
&= \frac{vu(a,s)p(a,s)}{V(a,s)}\frac{\delta(a,s)+sp(a,s)}{\left(\delta(a,s)+sp(a,s)(1-F(w|a,s))\right)^2}
\end{aligned}$$

C.3 Equilibrium definition

We here define a equilibrium of the model economy presented in Section 5.

Definition 1. *An equilibrium of our economy consists of*

- *a set of wage and vacancy posting policies* $\left\{w(z|a,s), v(z|a,s)\right\}$ *that solve firms' problem;*
- *a reservation wage* $r(a,s)$ *that solves workers' problem; and*
- *aggregate states* $\left\{G(z|a,s), e(a,s), u(a,s), V(a,s), p(a,s), q(a,s)\right\}$ *that are consistent with their laws of motion in steady-state as well as the matching technology.*

To characterize the equilibrium, we start by substituting our assumed iso-elastic cost function into firms' problem (7) and taking first-order conditions,

$$c(a,s)v(z|a,s)^{\eta} = (z-w)\frac{\partial l(w,v|a,s)}{\partial v} \tag{C.1}$$

$$l(w,v|a,s) = (z-w)\frac{\partial l(w,v|a,s)}{\partial w} \tag{C.2}$$

Differentiating the equilibrium size (8) with respect to vacancies

$$\frac{\partial l(w,v|a,s)}{\partial v} = \frac{u(a,s)p(a,s)}{V(a,s)}\frac{\delta(a,s)+sp(a,s)}{\left(\delta(a,s)+sp(a,s)(1-F(w|a,s))\right)^2}$$

Substituting this into the first-order condition for vacancies (C.1),

$$c(a,s)v(z|a,s)^{\eta} = (z-w(z|a,s))\frac{u(a,s)p(a,s)}{V(a,s)}\frac{\delta(a,s)+sp(a,s)}{\left(\delta(a,s)+sp(a,s)(1-F(w(z|a,s)|a,s))\right)^2} \tag{C.3}$$

Differentiating the equilibrium size (8) with respect to wages

$$\frac{\partial l(w,v|a,s)}{\partial w} = 2sp(a,s)f(w|a,s)\frac{vu(a,s)p(a,s)}{V(a,s)}\frac{\delta(a,s)+sp(a,s)}{\left(\delta(a,s)+sp(a,s)(1-F(w|a,s))\right)^3}$$

Substituting this into the first-order condition for wages (C.2) and cancelling terms

$$\delta(a,s)+sp(a,s)(1-F(w(z|a,s)|a,s)) = (z-w(z|a,s))\,2sp(a,s)f(w(z|a,s)|a,s) \tag{C.4}$$

As in Burdett and Mortensen (1998), more productive firms post higher wages. Consequently,

$$F(w(z|a,s)|a,s) = \frac{M}{V}\int_{\underline{z}}^{z} v(\tilde{z}|a,s)d\Gamma(\tilde{z}), \qquad f(w(z|a,s)|a,s)w'(z|a,s) = \frac{M}{V}v(z|a,s)\gamma(z)$$

Define $h(z|a,s) = F(w(z|a,s)|a,s)$ so that $f(w(z|a,s)|a,s) = h'(z|a,s)/w'(z|a,s)$. Substituting in (C.4),

$$w'(z|a,s) = (z-w(z|a,s))\frac{2sp(a,s)h'(z|a,s)}{\delta(a,s)+sp(a,s)(1-h(z|a,s))} \tag{C.5}$$

We also have that $h'(z|a,s) = \frac{M}{V(a,s)} v(z|a,s)\gamma(z)$ so that $v(z|a,s) = \frac{V(a,s)}{M}\frac{h'(z|a,s)}{\gamma(z)}$. Substituting in (C.3),

$$h'(z|a,s) = \frac{M}{V(a,s)}\gamma(z)\left(\frac{1}{c(a,s)}\left(z - w(z|a,s)\right)\frac{u(a,s)p(a,s)}{V(a,s)}\frac{\delta(a,s)+sp(a,s)}{\left(\delta(a,s)+sp(a,s)(1-h(z|a,s))\right)^2}\right)^{\frac{1}{\eta}} \tag{C.6}$$

Equations (C.5)–(C.6) constitute a system of differential equations in the two functions $w(z|a,s)$ and $h(z|a,s)$. The first boundary condition is that wages of the least productive firm must equal the lowest possible pay in the market, $\lim_{z\to\underline{z}(a,s)} w(z|a,s) = \max\left\{r(a,s), \frac{w^{\min}}{a}\right\}$. The second boundary condition is that the CDF of the offer distribution is zero for the least productive firm, $\lim_{z\to\underline{z}(a,s)} h(z|a,s) = 0$, where $\underline{z}(a,s)$ is the least productive firm active in market (a,s): $\underline{z}(a,s) = \max\left\{\underline{z}, \max\left\{r(a,s), \frac{w^{\min}}{a}\right\}\right\}$. Finally, the key equilibrium consistency condition is that the total number of vacancies, $V(a,s)$, is such that the CDF of offered wages integrates to one, $\lim_{z\to\bar{z}} h(z|a,s) = 1$.

C.4 Numerical solution algorithm

Recall that the parameter vector $\mathbf{p}$ includes the reduced form job finding rate λ. Hence given a parameter vector, we know all worker flows since $\mathbf{p}$ also includes $\{\delta_0, \delta_1, \phi_0, \phi_1, \pi\}$. We use the flows to get the implied stock of workers, $\{u(a,s), e(a,s)\}$, and based on that we recover the required aggregate number of vacancies $V(a,s)$ consistent with the job finding rate $p(a,s) = \lambda$. Recall that the parameter vector $\mathbf{p}$ also includes the reduced form parameters fully characterizing the reservation wage, $r(a,s)$. Hence, we also know the boundary conditions.

To solve for the equilibrium, we start by solving the system of differential equations (9) for a low cost $c_0(a,s)$ and a high cost, $c_1(a,s)$. Recall that the key consistency condition is $\lim_{z\to\bar{z}} h(z|a,s) = 1$. We require that under the low cost, firms create too many jobs, $\lim_{z\to\bar{z}} h_0(z|a,s) > 1$, while under the high cost, firms create too few jobs, $\lim_{z\to\bar{z}} h_1(z|a,s) < 1$. If this is not true under $c_0(a,s)$ ($c_1(a,s)$), we adjust $c_0(a,s)$ ($c_1(a,s)$) down (up) until it is true. After we have found $c_0(a,s)$ and $c_1(a,s)$ such that both of these conditions hold, we apply a bisection to find the cost $c(a,s)$ such that $\lim_{z\to\bar{z}} h(z|a,s) = 1$.

We subsequently simulate a monthly approximation to the model, starting workers off from the steady-state distribution. We follow exactly our empirical procedure to construct both a monthly and an annual data set based on the simulated data, including how to select a main employment spell and compute all outcome variables of interest. In particular, we estimate an AKM regression based on the simulated annual data set restricted to the largest connected set, which as in the data covers the vast majority of employment spells.

The steps above are sufficient to estimate the model. Having obtained an estimated parameter vector $\mathbf{p}^*$, we compute the implied flow value of leisure, $b(a)$, such that the reservation wage is consistent with our reduced form parameter or alternatively makes workers indifferent between working at the minimum wage and unemployment.

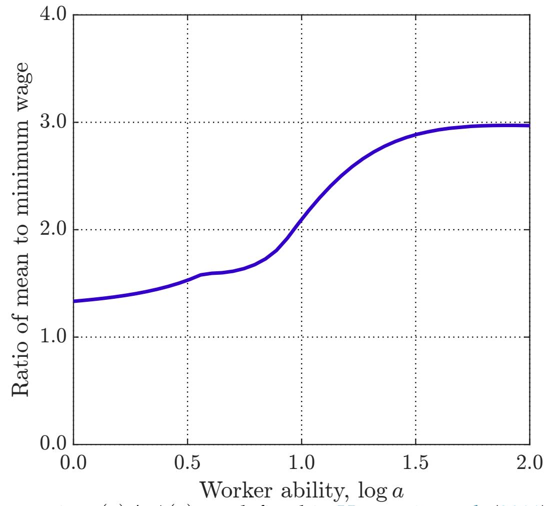
To subsequently solve the model for alternative levels of the minimum wage, we instead hold the cost $c(a,s)$ fixed at its estimated value, as well as the flow value $b(a)$. Applying a similar bisection as above, we instead guess a low job finding rate $p_0(a,s)$ and a high job finding rate $p_1(a,s)$, solve for the equilibrium number of vacancies $V(a,s)$ consistent with these job finding rates, and solve the system of differential equations (9). Since the job finding rate $p(a,s)$ is inversely related to the worker finding rate $q(a,s)$, we require that firms want to create too many jobs under the low job finding rate $p_0(a,s)$ (i.e. high worker finding rate $q_0(a,s)$), and vice versa. If not, we adjust the initial guesses $\{p_0(a,s), p_1(a,s)\}$ until this holds. After that, we apply a bisection for the job finding rate $p(a,s)$ until the key equilibrium consistency condition $\lim_{z\to\bar{z}} h(z|a,s) = 1$ holds.

D Estimation Appendix

This section provides additional details on the model estimation that build on the material presented in Section 6, including subsections on the mean-to-min wage ratio in the estimated model (Appendix D.1), the implied parameter estimates and additional model fit (Appendix D.2), details of the identification (Appendix D.3), additional model validation (Appendix D.4), and a discussion of the structure of the unbalanced panel (Appendix D.5).

D.1 Mean-to-min wage ratio in the estimated model

Figure D.1. Mean-to-min wage ratio in the estimated model



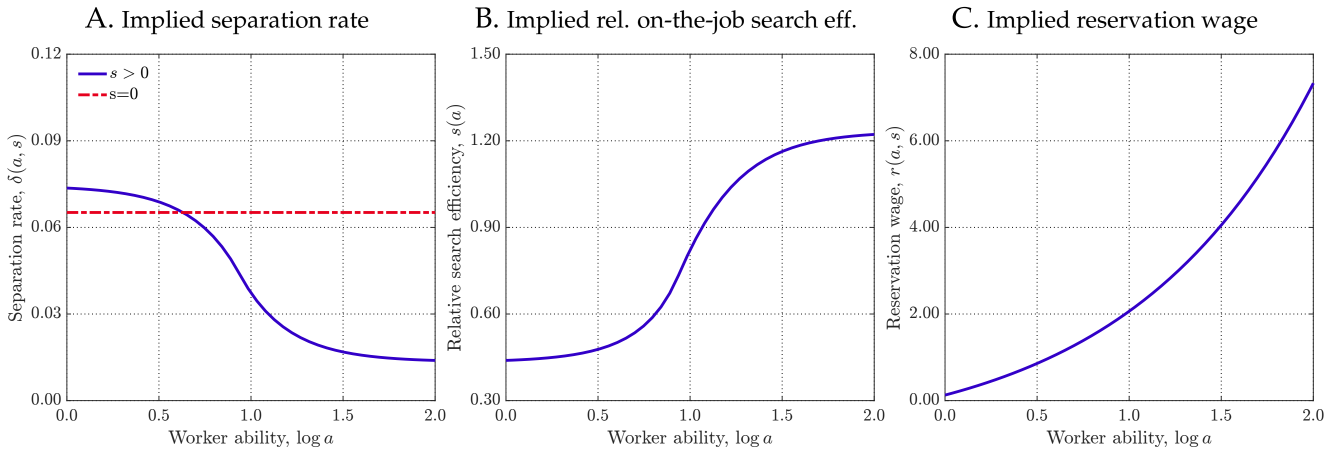
Notes: Figure shows the mean-to-min wage ratio $\overline{w}(a)/w^*(a)$, as defined in Hornstein et al. (2011), from our estimated model. All statistics are computed from model solution before applying the estimated measurement error. The mean wage $\overline{w}(a)$ is defined as the arithmetic mean over the realized distribution of wages in levels for a given ability type a. The min wage $w^*(a)$ is defined as the lowest accepted wage in levels by workers of ability a, which might be either the statutory minimum wage w^{min} or the workers' reservation wage if the latter exceeds the statutory minimum wage. *Source:* Model and RAIS, 1994–1998.

D.2 Implied parameter estimates and additional model fit

This section discusses additional details regarding the implied parameter estimates and model fit. This follows up on the results presented in Section 6.2, specifically Table 4 and Figure 4, of the main text.

Figure D.2 shows the implied parameter values for the separation rates, $\delta(a, s > 0)$ and $\delta(a, s = 0)$, relative on-the-job search efficiency, $s(a)$, and reservation piece rate, $r(a)$, across the distribution of worker ability, a, based on the estimates presented in Table 4 of the main text.

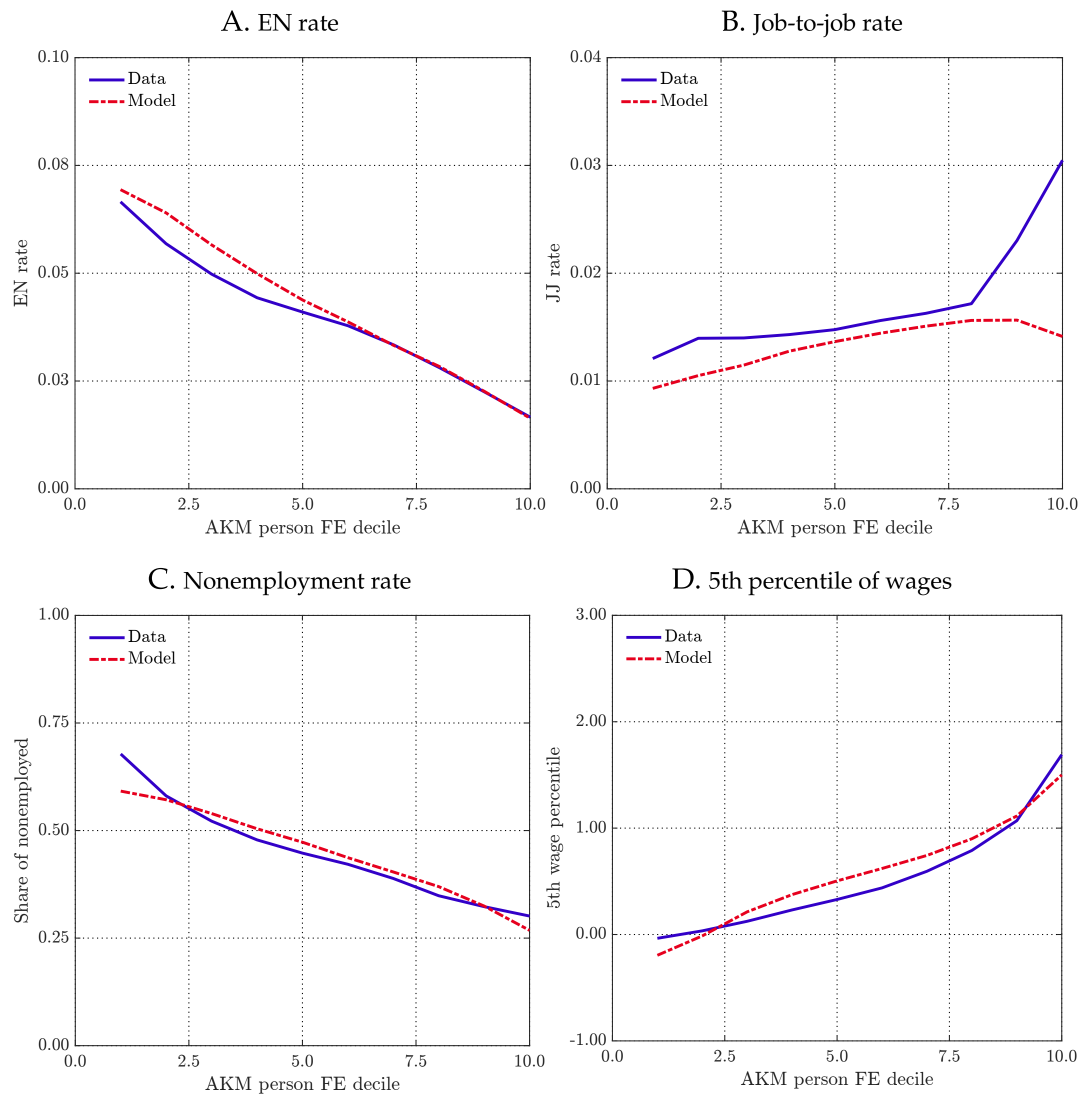
Figure D.2. Implied parameter estimates



Notes: This figure shows the implied parameter values of the separation rates, $\delta(a, s > 0)$ and $\delta(a, s = 0)$, relative on-the-job search efficiency, $s(a)$, and reservation piece rate, $r(a)$, across the distribution of worker ability, a, based on the estimates in Table 4. *Source:* Model.

Figure D.3 illustrates the model's ability to replicate labor market stocks and flows by AKM worker fixed effects. Panel A shows that the model matches well the lower EN rate among higher paid workers, as a result of the decline in the separation rate, $\delta(a, s)$, with worker ability. Panel B highlights that we understate somewhat the level of job-to-job mobility as well as its gradient with AKM worker fixed effect. Although underlying search efficiency $s(a)$ rises monotonically with worker ability and higher ability workers are less likely to be minimum wage workers, the resulting job-to-job rate is not monotone in AKM worker fixed effects. The reason is that the separation rate $\delta(a, s)$ declines in ability, such that higher ability workers are higher up the job ladder. Since workers higher up the job ladder are less likely to accept an outside offer, the realized job-to-job rate is non-monotone in ability. Panel C shows that the model matches well the nonemployment rate by AKM worker fixed effect decile. Although we do not target this in estimation—in fact we impose the same job finding rate λ across worker types—the model matches well the empirical pattern, because the separation rate, $\delta(a, s)$, declines with worker ability. Panel D shows that the model matches well the fifth percentile of wages by AKM worker fixed effect decile.

Figure D.3. Worker outcomes by AKM person FE, model vs. data



Notes: Panel A shows the share of employed workers who are nonemployed in the subsequent month. Employment refers to formal sector employment in the data; nonemployment is everything else (unemployment, not in the labor force and informal sector employment). Panel B shows the share of employed workers who are employed at a different main employer in the subsequent month. Panel C shows the share of population that is not working in the formal sector. Panel D shows the 5th percentile of log wages. All panels are by decile of AKM person fixed effects based on a regression of log monthly earnings on person fixed effects and firm fixed effects. Model and data sample selection and variable construction are identical—see text for details. *Source:* Model and RAIS, 1994–1998.

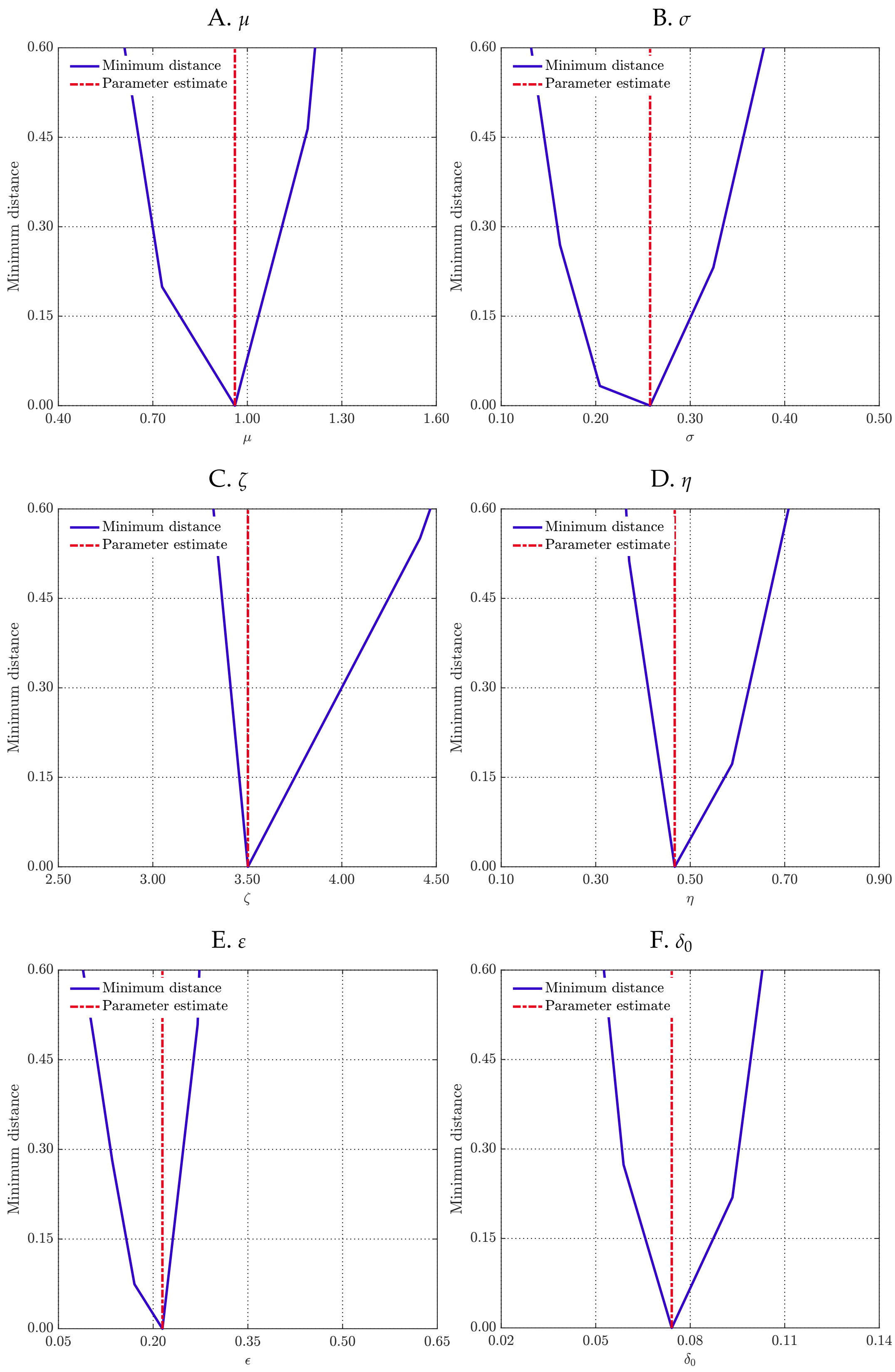
D.3 Details of the identification

This subsection provides a further discussion of identification of the 12 internally estimated parameters of our model. We consider two exercises. The first plots how the minimum distance objective function changes as each parameter varies around its estimated value, holding all other parameters fixed at their estimated values. This exercise is local in nature in the sense that an envelope condition ensures that the other parameters remain optimal as one parameter varies around its optimum.

Figure D.5 provides the results. Evidently, 11 of the 12 internally estimated parameters are well informed by the joint information contained in the targeted moments. The exception is the intercept in the reservation wage, r_0, for which the minimum distance is relatively flatter compared to other parameters. For the main objective of this paper, we believe that this is a somewhat minor issue. The reason is that the impact of the minimum wage on both inequality and employment is essentially invariant to the particular value of this parameter, as we highlight further in Appendix E.7.

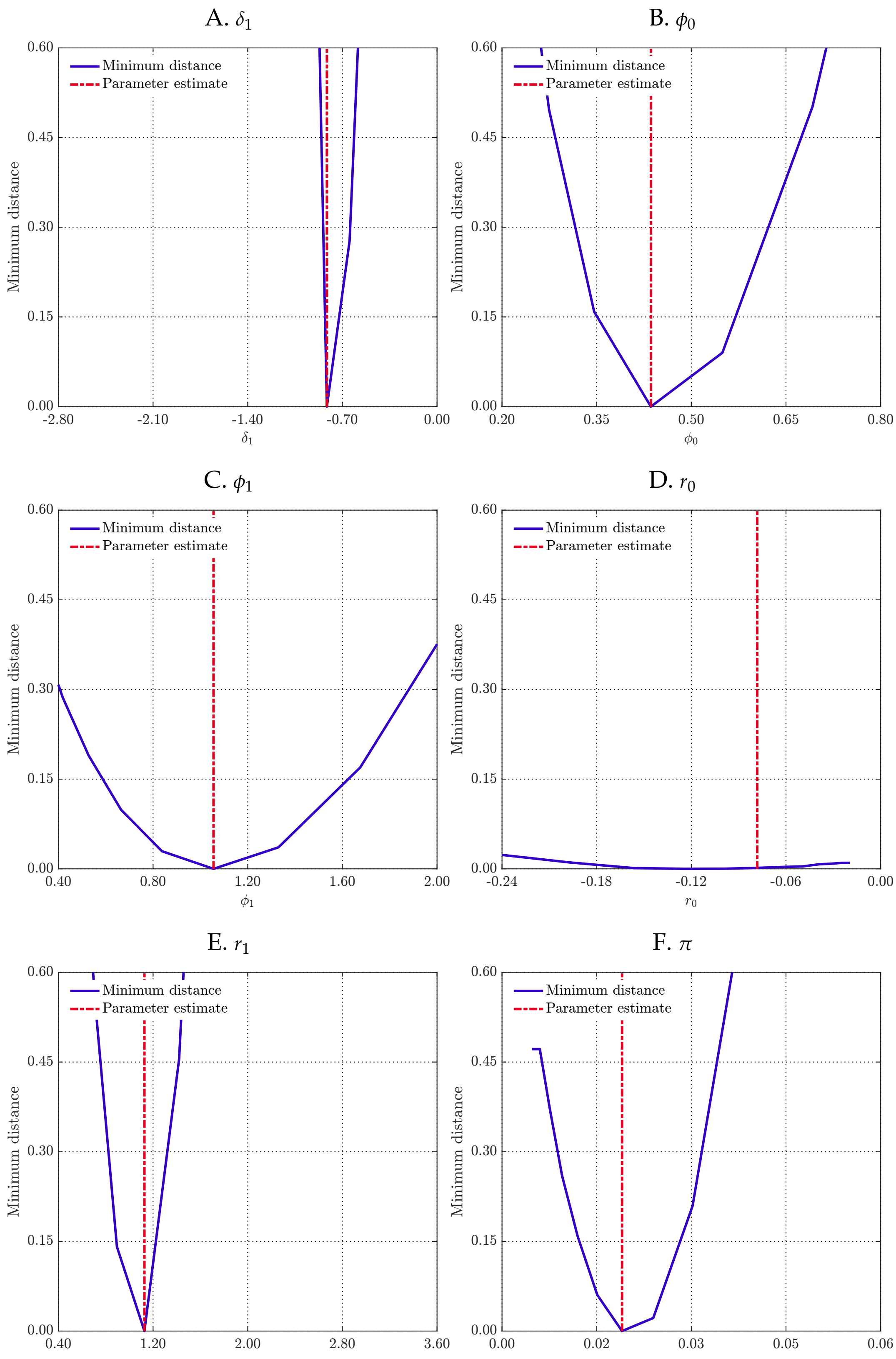
The second exercise plots how an individual parameter moves its particularly informative moment. Figure D.6 plots those parameters that are particularly informed by a single moment against its chosen moment as the parameter varies around its estimated value (between 25 and 300 percent of the estimated value), holding the other parameters fixed at their estimated values. Reassuringly, each parameter distinctly moves its particularly informative moment, suggesting that these parameters are well informed by our choice of targets. In the interest of space, we focus this exercise primarily on those parameters that are particularly informed by a single moment. Nevertheless, for reference we also show in Figure D.7 how the overall EN and EE rates move as we change the intercept in the separation rate, δ_0, and the intercept in relative search efficiency, ϕ_0, respectively. Note, though, that these two moments are not targeted in estimation, as we target the EN (EE) rate *by decile* of AKM worker fixed effect deciles for δ_0 (ϕ_0) and δ_1 (ϕ_1) jointly. These two parameters move the overall mobility rates in the expected direction.

Figure D.4. Minimum distance, model



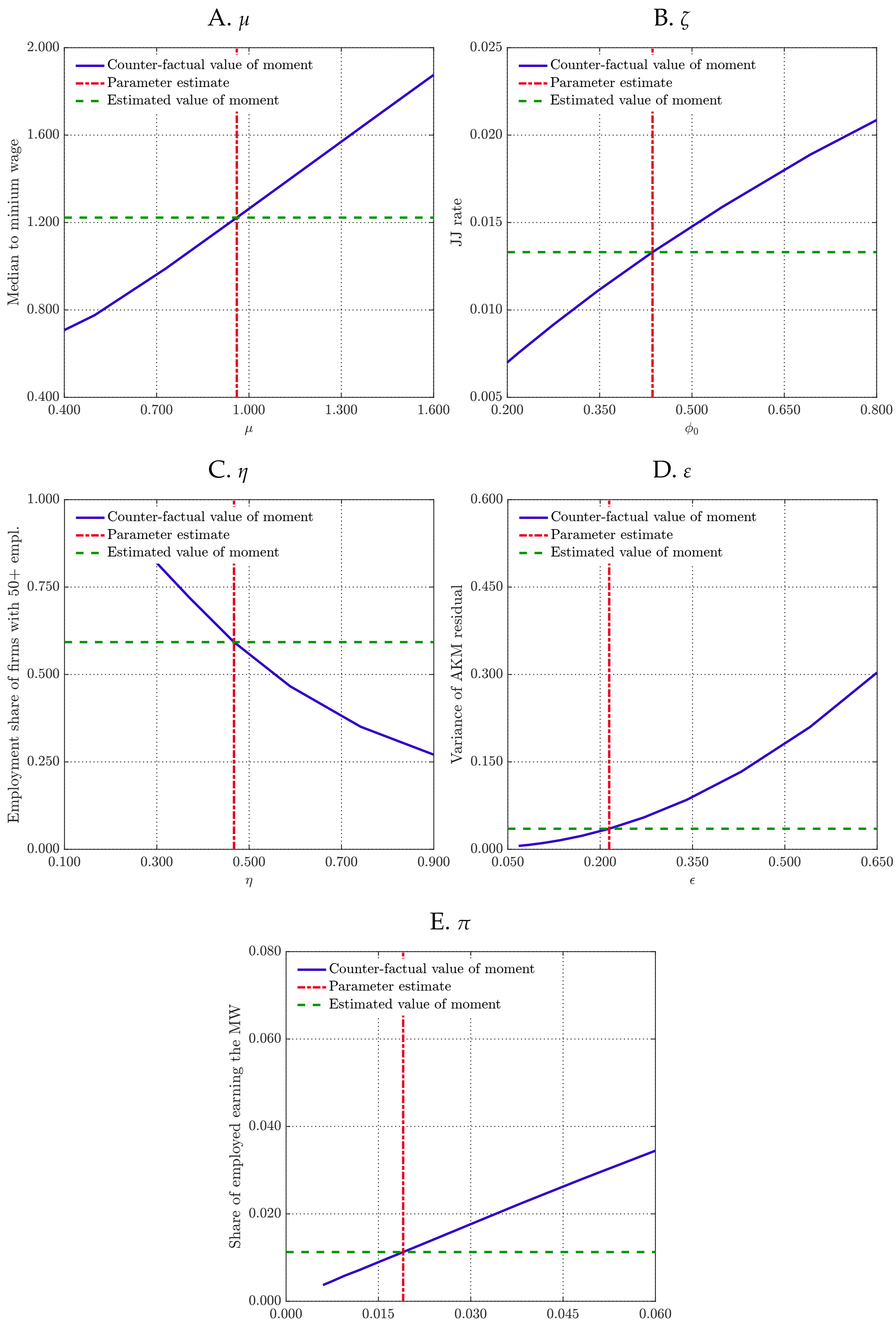
Notes: Impact on minimum distance objective function of varying one parameter at a time, holding all other parameters fixed at their estimated values. *Source:* Model.

Figure D.5. Minimum distance, model (cont'd)



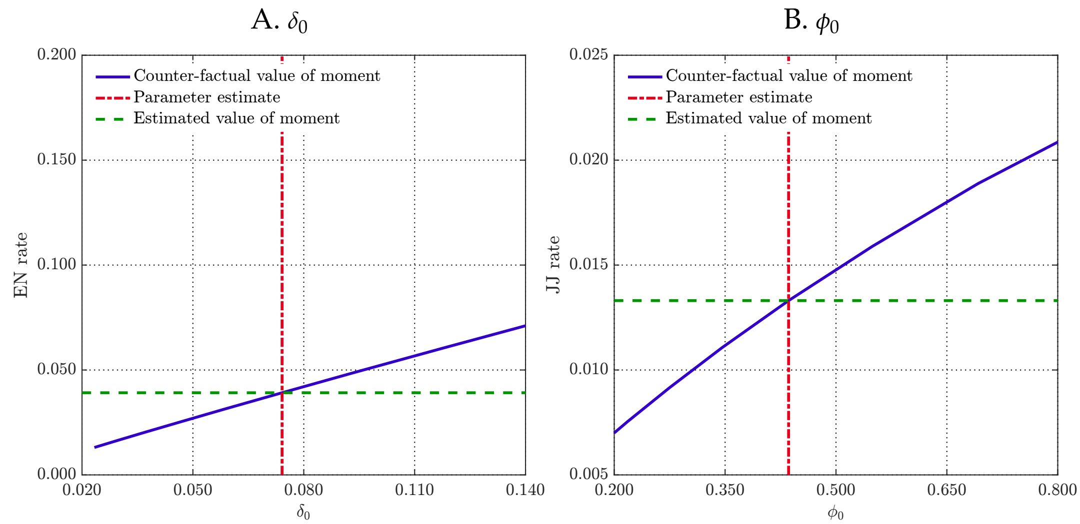
Notes: Impact on minimum distance objective function of varying one parameter at a time, holding all other parameters fixed at their estimated values. *Source:* Model.

Figure D.6. Targeted moments versus parameters, model



Notes: Impact on an associated target moment of varying one parameter at a time, holding all other parameters fixed at their estimated values.
Source: Model.

Figure D.7. Additional moments versus parameters, model

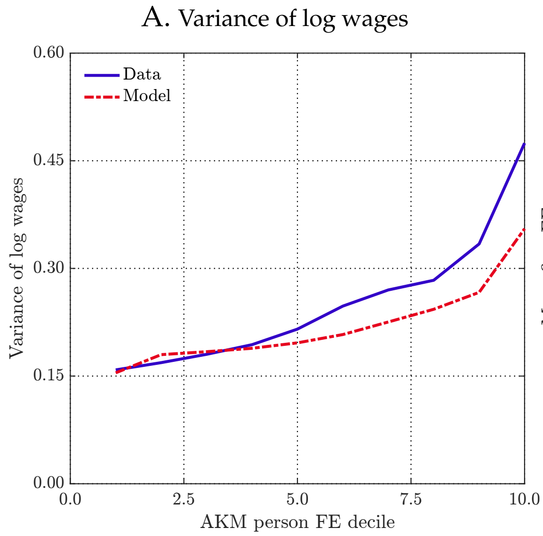


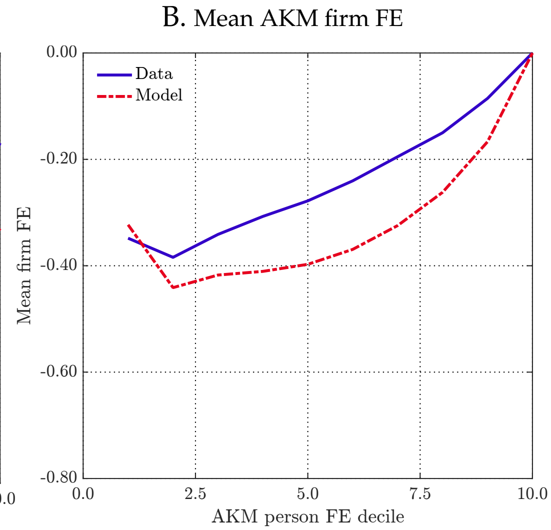
Notes: Impact on an associated target moment of varying one parameter at a time, holding all other parameters fixed at their estimated values.
Source: Model.

D.4 Additional model validation

Figure D.8 contrasts some additional predictions of the model that were not explicitly targeted with the data. Panel A shows that dispersion in (log) pay is larger among high paid workers, in both the model and data. This pattern is driven by the top deciles having greater dispersion in underlying worker ability, a, as well as greater dispersion in pay conditional on ability among high skilled workers. The latter is, in turn, due to the fact that the minimum wage does not constrain pay at the top of the ability distribution. Panel B plots the average AKM firm fixed effect by decile of AKM worker fixed effects. Higher paid workers work for higher paying firms, in both the model and data. The reason is that more skilled workers climb the job ladder faster and fall off it less frequently. At the bottom of the worker pay distribution, however, the pattern is reversed, because the minimum wage makes matches between the lowest skilled workers and the lowest productivity firms unviable.

Figure D.8. Model validation across AKM person FE deciles, model versus data





Notes: Figure shows the stimated impact of a 57.7 log point increase in the productivity adjusted real minimum wage. Workers are binned by decile of AKM worker fixed effect. Firms are binned by (employment-unweighted) AKM firm fixed effect decile. AKM regression is estimated on model-simulated monthly data aggregated to the annual level following an identical sample selection and variable construction methodology as in the data. Panel A shows the impact on the variance of log wages. Panel B shows the impact on the mean of AKM firm fixed effects. *Source:* Model and RAIS, 1994–2018.

D.5 Structure of the unbalanced panel

A salient feature of the formal-sector RAIS data is that both workers and firms do not appear in a balanced panel. Our estimated model rationalizes this through stochastic worker separation rates into nonemployment and stochastic job findings rates from nonemployment. Our estimation procedure relies on an indirect inference logic and the AKM wage equation, which we use as an auxiliary model, is estimated on finite samples—both in the data and on the model-simulated data. Therefore, it is interesting to know to what extent panel structure in the data is replicated by simulations from our estimated model.

Table D.1 compares worker and firm survival rates in the data for two periods, the estimation period 1994–1998 (panel A) and the final period 2010–2014 (panel B), as well as in the simulated data from our estimated model (panel C), which we fit to a separate set of moments from the estimation period 1994–1998. Specifically, we consider two concepts of worker or firm survival rates. First, we compute the survival rates of a cohort of workers or firms observed in the first year of the five-year time window. We report the share of that cohort of workers or firms who survive for each number of consecutive years, including the first one, during this time window in the columns labeled "Cohort." Second, we compute the share of all workers or firms who are observed for each of one, two, three, four, or five years in the data in the columns labeled "Pooled." The two statistics are related but distinct because—in the real world like in our simulated model—workers may be observed for the first time after the first year of our data time window.

A few points are worth noting about the results in Table D.1. Cohort survival rates are concentrated around five years—the complete panel—both for workers and for firms, as well as in the two data periods and in the model. In comparison, pooled survival rates are more spread out. The model matches very well the empirical worker cohort survival rates. At the same time, the model overpredicts the cohort survival rates of firms. Vis-a-vis the data, the model also underpredicts the pooled survival rates of workers but does a very good job at capturing that of firms. The model fit is not perfect, which may be not surprising given that there were no free parameters that could have been used to match these survival rate profiles jointly. At the same time, the parsimonious model does an adequate job at capturing key features of the empirical panel structure, which gives us confidence in our indirect inference procedure.

Table D.1. Cohort survival rates and pooled survival shares, data and model

Panel A. Data, 1994–1998	Workers		Firms	
Number of years	Cohort	Pooled	Cohort	Pooled
1	2.0%	4.5%	3.0%	2.8%
2	3.6%	8.6%	5.2%	5.7%
3	5.1%	12.4%	6.6%	7.4%
4	8.8%	20.2%	8.4%	11.5%
5	80.4%	54.3%	76.7%	72.7%
Panel B. Data, 2010–2014	Workers		Firms	
Number of years	Cohort	Pooled	Cohort	Pooled
1	1.6%	3.2%	2.6%	1.8%
2	1.9%	6.2%	4.4%	4.2%
3	3.6%	9.2%	5.3%	6.0%
4	5.9%	16.3%	7.0%	8.6%
5	87.0%	65.1%	80.7%	79.5%
Panel C. Model	Workers		Firms	
Number of years	Cohort	Pooled	Cohort	Pooled
1	1.0%	7.8%	0.0%	1.3%
2	2.7%	14.2%	0.3%	4.7%
3	5.6%	20.0%	1.4%	9.4%
4	11.2%	23.9%	4.7%	15.3%
5	79.4%	34.2%	93.7%	69.4%

Notes: Table compares worker and firm survival rates in the data for two periods, the estimation period 1994–1998 (panel A) and the final period 2010–2014 (panel B), as well as in the simulated data from our estimated model (panel C), which was fit to data from 1994–1998. Two concepts of worker or firm survival rates are presented: the share of the starting year's cohort of workers or firms who survive for each number of consecutive years (columns "Cohort") and the share of all workers or firms who are observed for each number of years in the data (columns "Pooled"). *Source:* RAIS, 1994–1998 and 2010–2014, and model.

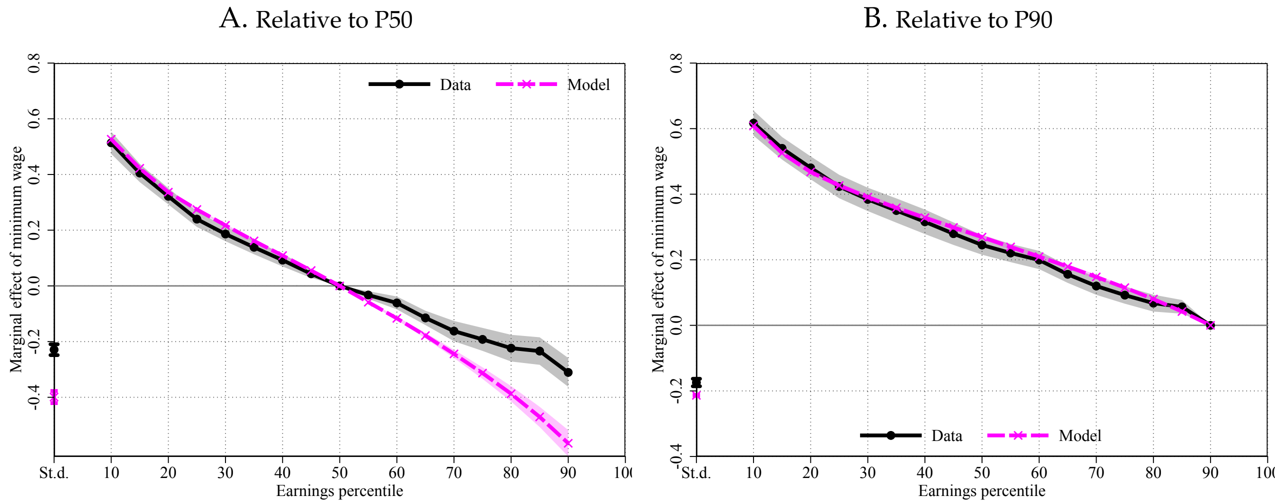
E Results Appendix

This section provides additional details on the simulated impact of the minimum wage, building on the material presented in Section 7, including subsections on the impact of the minimum wage on inequality across space in the model and data for alternative specifications (Appendix E.1), a model-based AKM wage decomposition (Appendix E.2), the effect of the minimum wage on young workers only (Appendix E.3), the impact of the minimum wage on sorting (Appendix E.4), further results on heterogeneity in effects on disemployment and firm size (Appendix E.5), and the dependence of minimum wage effects on model parameters (Appendix E.7).

E.1 Comparing estimated spillover effects between model and data

Figure E.1 shows estimates of the marginal effects from equation (3) based on the regression framework in equation (2) using an IV strategy. The results are broadly similar with those from our baseline specification in Figure 8 of the main text.

Figure E.1. Model vs. data: Estimated minimum wage effects throughout the wage distribution, IV



*Notes:*Figure plots estimates of the marginal effects from equation (3) based on the regression framework in equation (2) estimated across Brazil's 27 states. Results from four separate estimates are shown, namely the combination of two base percentiles—P50 (panel A) and P90 (panel E.1B)—and two sources—the RAIS data (black circles and solid lines) and model-simulated data (magenta crosses and dashed lines). All four sets of estimates use a specification that includes state fixed effects in addition to state-specific linear time trends, estimated using an IV strategy. The IV strategy instruments the Kaitz-p index and its square using an instrument set that consists of the log real statutory minimum wage, its square, and the log real statutory minimum wage interacted with the mean of the log real median wage for the region over the full sample period. Within each panel, the estimated marginal effect of the minimum wage on the standard deviation of log earnings ("St.d." on the x-axis) and on wages between the 10th and the 90th percentiles of the wage distribution ("10" to "90" on the x-axis) relative to some base wage p are shown. Panel A uses the 50th percentile as the base wage (i.e., $p = 50$), while panel B uses the 90th percentile as the base wage (i.e., $p = 90$). The four error bars and four shaded areas represent 99 percent confidence intervals based on regular (i.e., not clustered) standard errors. *Source:* RAIS, 1996–2018, and model.

E.2 AKM decomposition

Table E.1 summarizes the impact of the minimum wage on earnings inequality as viewed through the lens of the AKM decomposition. The increase in the minimum wage accounts for about a third of the fall in the overall variance of earnings over this period in Brazil. It accounts for roughly half of the compression in the variance in AKM person fixed effects and 10 percent of the fall in the variance of the AKM firm effects. We caution, however, that *all* of the fall in inequality in the model is due to changes in firms' wage and vacancy policies.

Table E.1. Impact of minimum wage on AKM decomposition, model versus data

	1994–1998		2014–2018		Change		
	Data	Model	Data	Model	Data	Model	**Due to MW**
Variance of log wages	0.704	0.600	0.436	0.478	-0.268	-0.121	**45.3%**
Variance of AKM person FEs	0.333	0.259	0.262	0.208	-0.070	-0.051	**72.7%**
Variance of AKM firm FEs	0.217	0.195	0.085	0.176	-0.131	-0.018	**14.1%**
Variance of AKM residual	0.032	0.035	0.016	0.036	-0.016	0.001	**-8.1%**
2×covariance AKM person-firm FEs	0.123	0.111	0.072	0.058	-0.051	-0.053	**104.3%**

Notes: Men aged 18–54. Estimated impact of a 57.7 log point increase in the minimum wage in the model as well as the raw data. Model and data sample selection and variable construction is identical. *Source:* Model and RAIS 1994–2018.

E.3 Young workers only

This section reestimates the model for young workers aged 18–36. In doing so, we are able to compare the predicted effects of the minimum wage for this population subgroups and compare the predicted effects to those of the full population of workers aged 18–54. To this end, we reproduce all target moments for this subpopulation and recompute the set of model parameters to match these moments. With the recalibrated model in hand, we resimulate the effects of the same minimum wage increase as we considered for the full population.

In the end, we reach broadly similar conclusions for the set of young workers, shown in Table E.2, as for the full population, shown in Table 5 of the main text. While the differences between the two subpopulations are not too striking, the estimated effects are, if anything, slightly more pronounced in the population of young workers. The finding that the results are so similar is in turn driven by the fact that the targeted moments do not change that much between the young subpopulation and the overall population, and hence the estimated parameter values do not change that much.

Table E.2. Total impact of the minimum wage on wage inequality for the subpopulation of young workers aged 18–36, model versus data

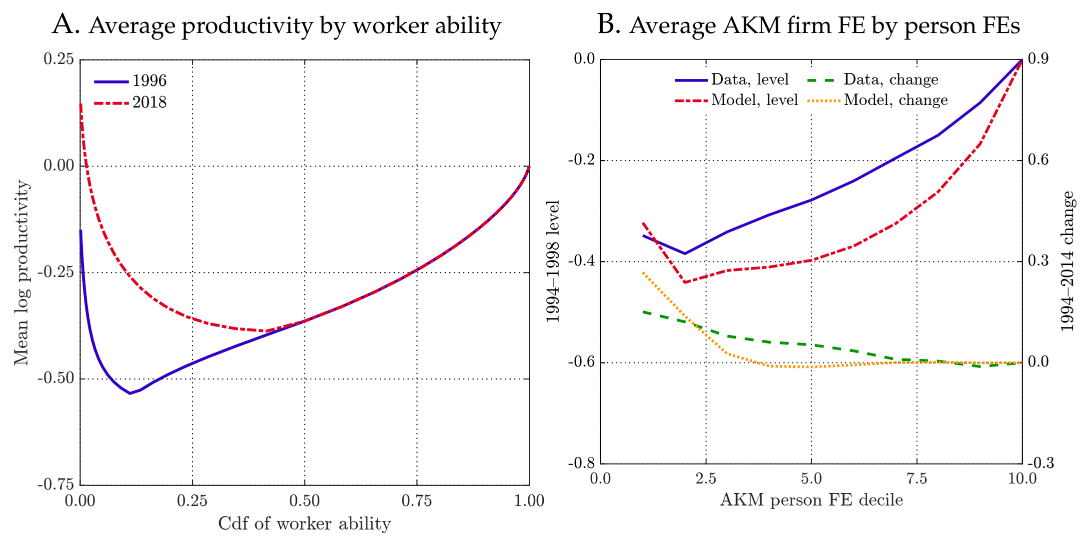
	1996		2018		Change		
	Data	Model	Data	Model	Data	Model	**Due to MW**
Variance	0.578	0.439	0.338	0.312	-0.240	-0.127	**52.9%**
P5-50	-1.007	-1.020	-0.538	-0.678	0.469	0.342	**72.9%**
P10-50	-0.825	-0.819	-0.471	-0.598	0.354	0.220	**62.2%**
P25-50	-0.449	-0.456	-0.275	-0.350	0.174	0.107	**61.2%**
P75-50	0.538	0.508	0.390	0.445	-0.147	-0.063	**43.0%**
P90-50	1.131	0.990	0.897	0.893	-0.234	-0.097	**41.6%**
P95-50	1.525	1.253	1.270	1.145	-0.255	-0.108	**42.3%**

Notes: Table shows estimated impact of a 57.7 log point increase in the minimum wage in the model as well as the raw data for the sample of young workers (aged 18–36). Percentile ratios of log wages, constructed as the sum of wages from a given employer over the five year sample period divided by the sum of months worked for that employer over each five year period. Model and data sample selection and variable construction is identical. See text for detail. *Source:* Model and RAIS.

E.4 Empirical support for the impact of the minimum wage on sorting

Figure E.2 illustrates this change in sorting in the model and provides empirical support for it. Panel A plots average firm productivity by worker ability, highlighting that average productivity rises, particularly among the lowest-ability workers. The reason is that matches between low-ability workers and low-productivity firms become unviable when the minimum wage is raised. Panel B of the figure provides reduced-form support consistent with this prediction. Specifically, it plots average AKM firm fixed effect by decile of AKM worker fixed effects in the model and data. For completeness, we replicate the level in the initial period from Figure D.8. As the minimum wage is increased, the average AKM firm fixed effect rises disproportionately among the lowest AKM worker fixed effects workers.

Figure E.2. Reallocation of lower-ability workers toward higher-productivity firms, model versus data

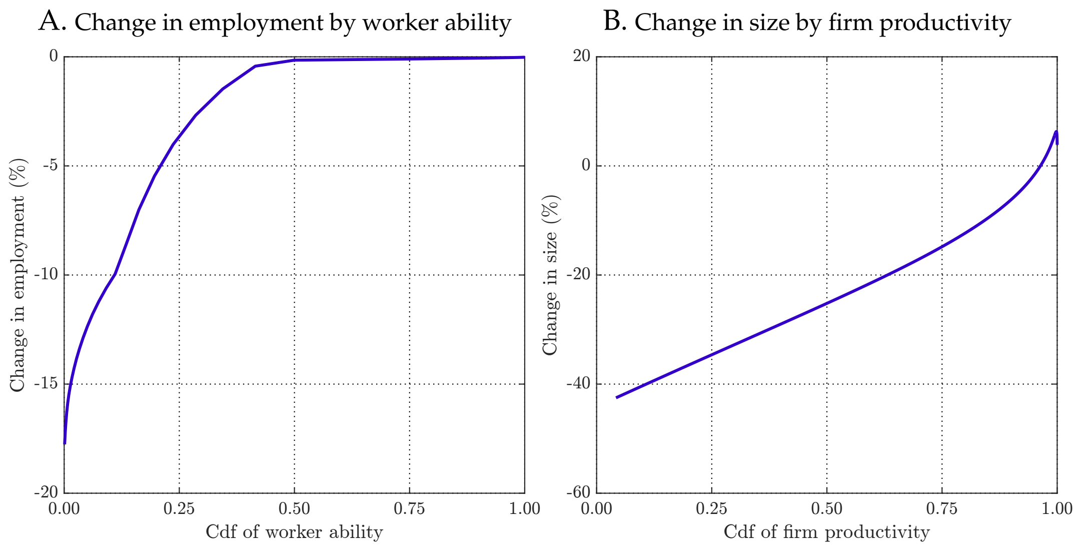


Notes: Men aged 18–54. Estimated impact of a 57.7 log point increase in the productivity adjusted real minimum wage. Panel A shows average log firm productivity by worker ability, $\int_{\underline{z}}^{\overline{z}} \log z dG(z|a,s)$, in market for workers with $s(a) > 0$ (the vast majority of workers). Panel B shows average AKM firm fixed effect by decile of AKM worker fixed effects. AKM regression is estimated on model-simulated monthly data aggregated to the annual level following an identical sample selection and variable construction methodology as in the data. *Source:* Model and RAIS 1994–2018.

E.5 Further results on heterogeneity in effects on disemployment and firm size

Figure E.3 sheds light on the heterogeneous effects of the minimum wage on employment and firm sizes. Panel A shows that, among the lowest-ability workers, employment falls by over 13 percent, while employment is essentially unaffected among workers above the bottom third of the ability distribution. Panel B focuses again on a group of workers most affected by the minimum wage—specifically, the first percentile of worker ability. Firms near the bottom of the firm productivity distribution shrink by almost 30 percent, while firms in the top five percent of the productivity distribution in fact expand in response to an increase in the minimum wage, for reasons that we analyze further below.

Figure E.3. Impact of minimum wage on aggregate outcomes, model



Notes: Impact of a 57.7 log point increase in the minimum wage in the estimated model. Panel A shows the log change in employment rate by worker ability in market with positive search efficiency, $s(a) > 0$ (the vast majority of workers). Panel B shows the log change in employment by firms ranked by employment-unweighted productivity in the first percentile of the worker ability distribution among workers with $s(a) > 0$. *Source:* Model.

E.6 Robustness of the small employment response

In light of our finding of modest aggregate disemployment effects of the minimum wage, we now investigate the robustness of our conclusions. In order for us to find a larger aggregate employment response, one or more of the channels in our decomposition based on equation (12) would need to be larger in magnitude. Their magnitude in turn relates to a set of estimated parameters and equilibrium objects, including the job finding rate, $p(a,s)$, the separation rate $\delta(a,s)$, the on-the-job search efficiency, $s(a)$, the elasticity of the vacancy cost function, η, and the elasticity of the matching function, α. We start by discussing the parameters that most clearly stand out based on (12), and subsequently present the sensitivity analysis with respect to the other model parameters.

Compared to common values for the U.S., we estimate a relatively low job finding rate, $p(a,s)$, and a comparable separation rate $\delta(a,s)$ for Brazil. All else equal, this would lead to a smaller job finding channel in the U.S. than what we find in Brazil. Compared to the U.S., we estimate a relatively high on-the-job search efficiency, $s(a)$. To see that this high value has limited effects on our results, consider the opposite extreme with $s(a) \approx 0$. Then the congestion channel approximately equals $\alpha/(1-0.4\alpha) = 0.6$. Hence, even in this extreme case, the congestion channel is a significant moderation force.

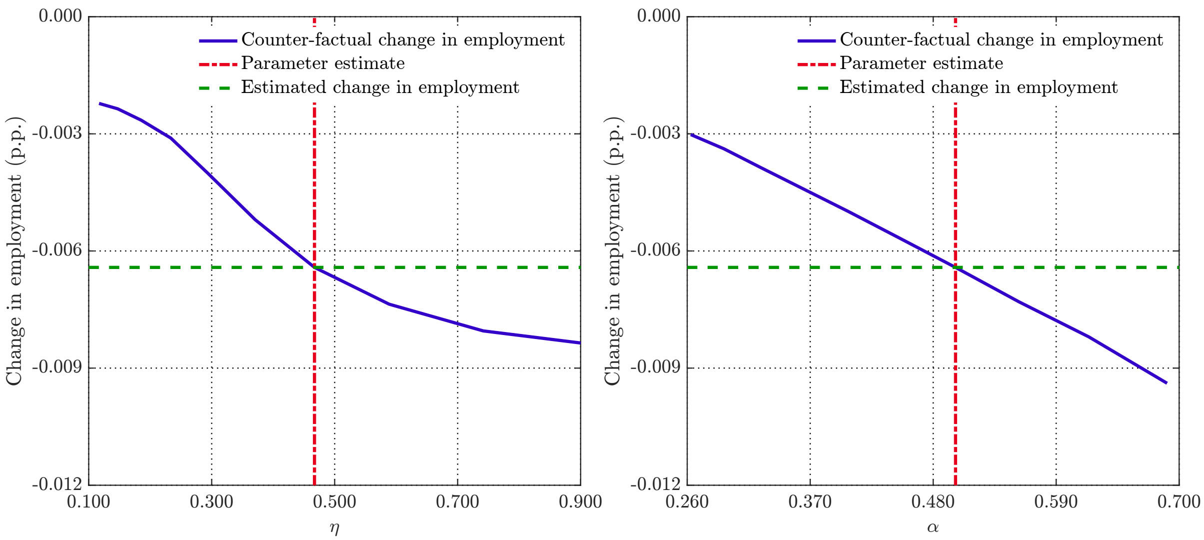
The sensitivity of the aggregate employment response with respect to the elasticity of the vacancy cost function, η, and the elasticity of the matching function, α, are shown in Figure E.4.Panel A shows that, perhaps surprisingly, a higher value of η actually amplifies the disemployment effects of the minimum wage. This is due to the equilibrium reallocation effects. Under a higher value of η, the minimum wage leads to smaller employment cuts among unproductive firms but also reduces the scaling up of more productive firms. Quantitatively, the latter force outweighs the former.

Panel B shows that higher values of α are associated with greater disemloyment effects. But our preset value of $\alpha = 0.50$ is relatively high compared to other values in the literature. For example, Meghir et al. (2015) estimate $\alpha = 0.34$ using the Brazil's PME data, Shimer (2005) estimates $\alpha = 0.28$ for the U.S., and Mortensen and Nagypal (2007) argue that a reasonable value is $\alpha = 0.40$. Therefore, our results likely represent an upper bound on the aggregate disemployment effects of the minimum wage.

Figures E.5–E.7 conduct the same exercise as above across the remaining 11 internally estimated structural parameters of the model, as well as for the calibrated job finding rate λ and separation rate of minimum wage workers, δ_{MW}. As for the impact of the minimum wage on inequality, the key parameters determining the employment effect of the minimum wage are the mean of worker ability (μ), the tail index of the firm productivity distribution (ζ), the slope of the reservation wage (r_1), and to a lesser extent the job finding rate (λ). The larger is μ, the less binding is the minimum wage initially and the smaller is the effect of an increase in the minimum wage on employment (as well as inequality—recall Figure 12). A larger ζ (i.e. a thinner tail of the firm productivity distribution) raises the disemployment effect of the minimum wage, as there is a larger number of low productive firms that are heavily exposed to the minimum wage and fewer high productive firms to pick up the employment slack. The faster the reservation wage rises in ability—the larger is r_1—the less the minimum wage binds and hence the smaller is the disemployment effect of a rise in the minimum wage. A lower λ is associated with a smaller disemployment effect of the minimum wage. The remaining parameters have at most a modest effect on the impact of a rise in the minimum wage on employment.

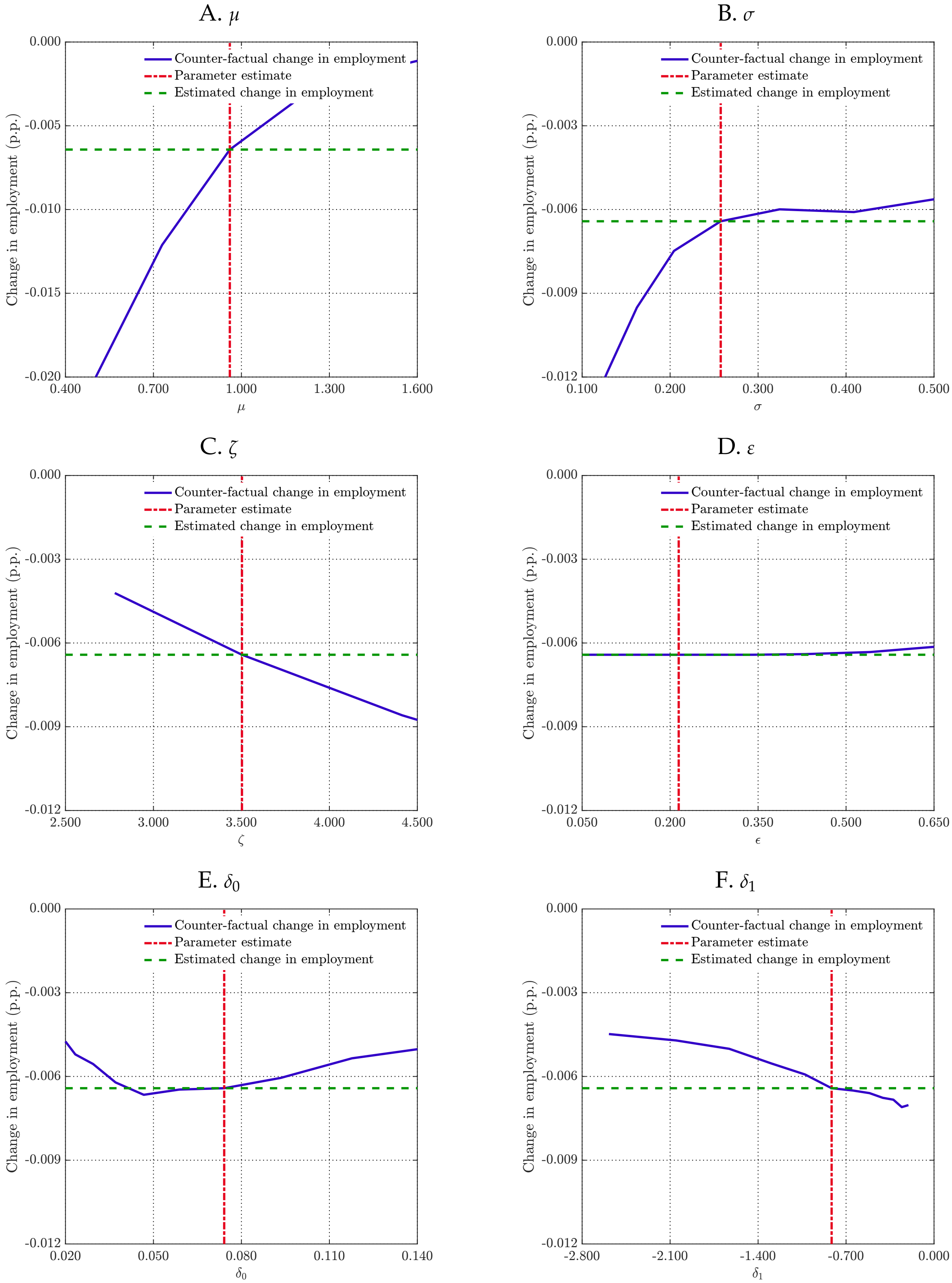
Figure E.4. Change in the employment rate across model parameters

A. Curvature of vacancy cost, η B. Elasticity of matches w.r.t. vacancies, α



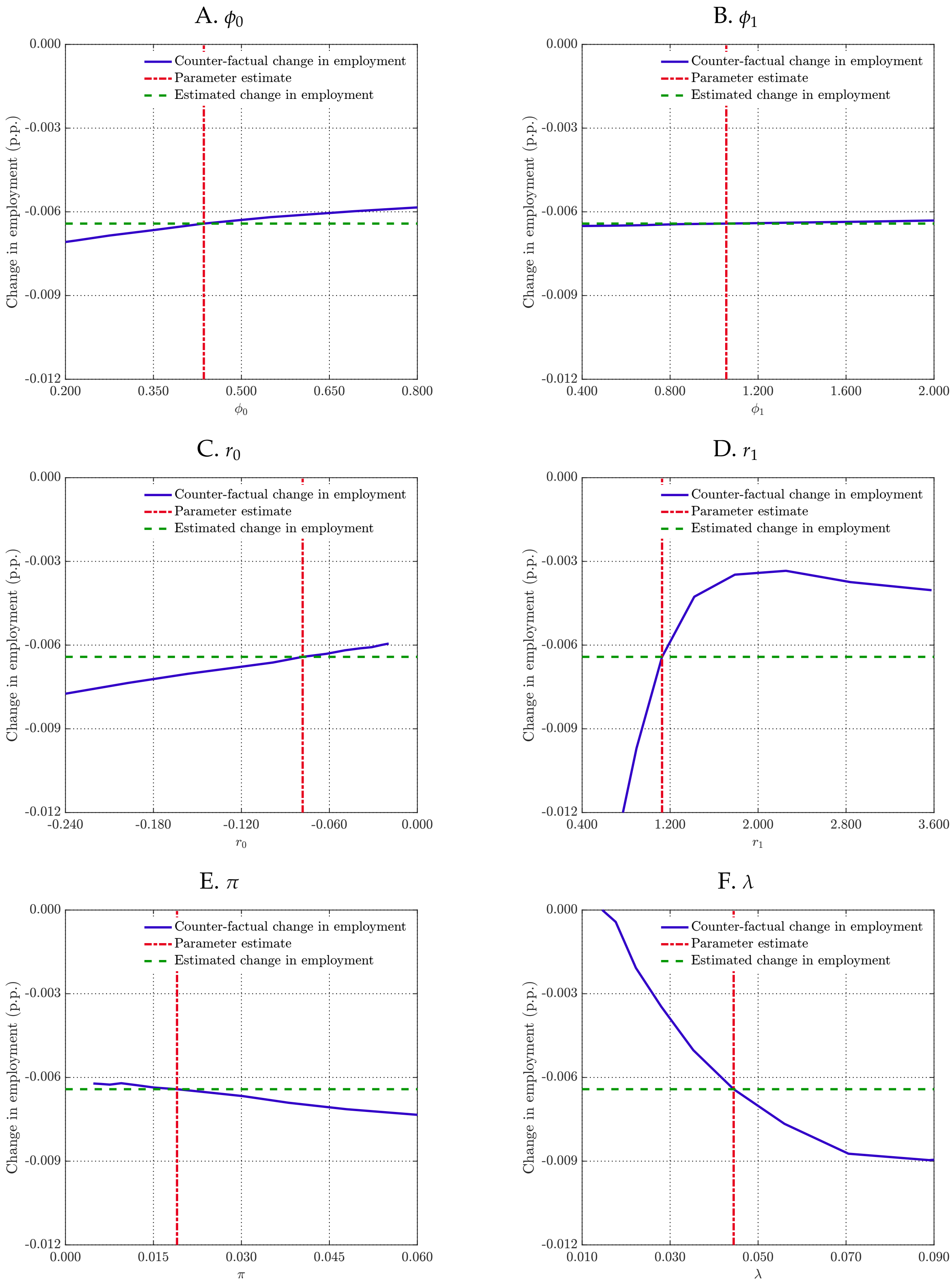
Notes: Estimated impact of a 57.7 log point increase in the minimum wage across different parameter values, varying one parameter at a time and holding fixed all other parameters at their estimated values. *Source:* Model.

Figure E.5. Change in the employment rate across model parameters, continued



Notes: Estimated impact of a 57.7 log point increase in the minimum wage across different parameter values, varying one parameter at a time and holding fixed all other parameters at their estimated values. *Source:* Model.

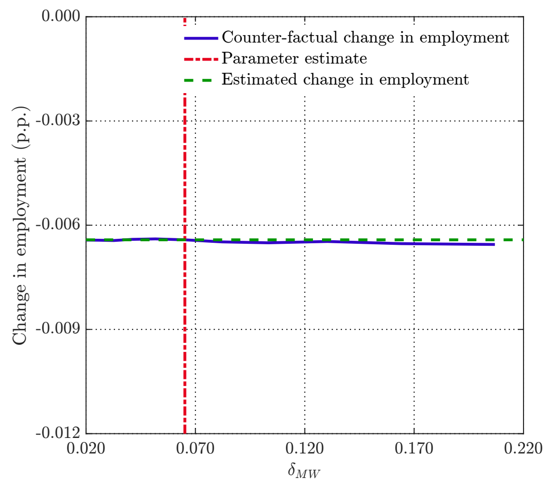
Figure E.6. Change in the employment rate across model parameters, continued



Notes: Estimated impact of a 57.7 log point increase in the minimum wage across different parameter values, varying one parameter at a time and holding fixed all other parameters at their estimated values. *Source:* Model.

Figure E.7. Change in the employment rate across model parameters, continued

A. δ_{MW}

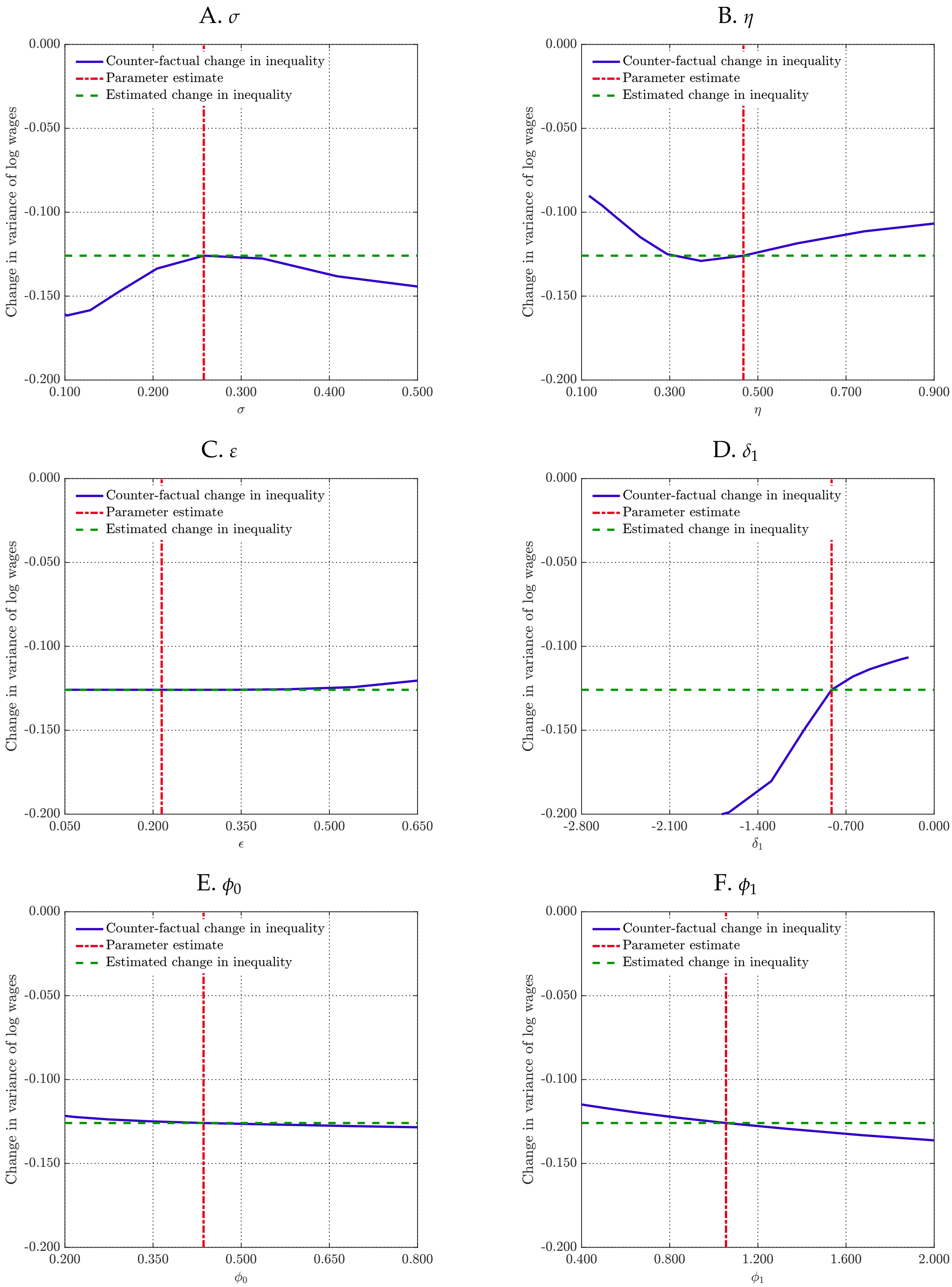


Notes: Estimated impact of a 57.7 log point increase in the minimum wage across different parameter values, varying one parameter at a time and holding fixed all other parameters at their estimated values. *Source:* Model.

E.7 Dependence of estimated inequality effect on model parameters

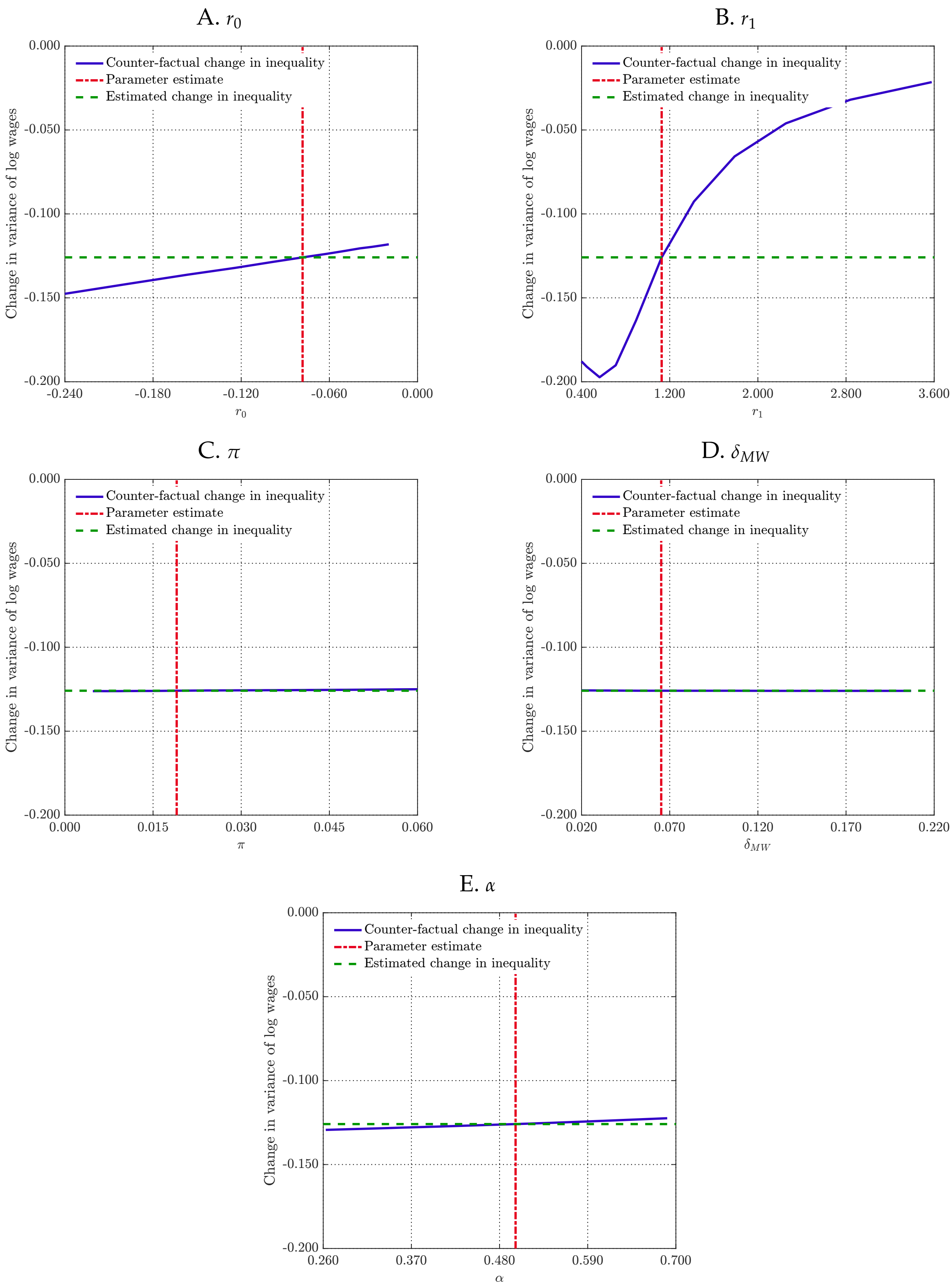
Figures E.8–E.9 conduct the same exercise as in Section 7.4 across the remaining nine internally estimated structural parameters of the model, the calibrated separation rate of minimum wage workers (δ_{MW}) and the preset elasticity of the matching function (α). Most of these parameters have at most a modest effect on the impact of a rise in the minimum wage on inequality. The main exception is the slope of the reservation wage, r_1. Intuitively, a higher reservation wage leaves less scope for the minimum wage to impact markets, as the reservation increasingly becomes the binding constraints across worker ability markets. Recall from Appendix D.3 that the parameter the model primarily struggles to inform well based on the available data is r_0. This parameter, however, is not critical in terms of driving the estimated impact of the minimum wage on inequality. Finally, δ_{MW} and α have no meaningful effect on the impact of the minimum wage on inequality.

Figure E.8. Change in the variance of log wages across model parameters



Notes: Estimated impact of a 57.7 log point increase in the minimum wage across different parameter values, varying one parameter at a time and holding fixed all other parameters at their estimated values. *Source:* Model.

Figure E.9. Change in the variance of log wages across model parameters, continued



Notes: Estimated impact of a 57.7 log point increase in the minimum wage across different parameter values, varying one parameter at a time and holding fixed all other parameters at their estimated values. *Source:* Model.

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