Racial Inequality, Minimum Wage Spillovers, and the Informal Sector*

Ellora Derenoncourt (Princeton)
François Gérard (QMUL)
Lorenzo Lagos (Brown and IZA)
Claire Montialoux (UC Berkeley)

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Abstract

This paper studies how a national minimum wage affects wages, and in particular, racial earnings disparities in a middle-income country with a large informal sector. Our context is the Brazilian economy, characterized by persistently large racial disparities and the availability of detailed labor force surveys and administrative matched employer-employee data with information on race. We analyze the effect of large increases in the minimum wage that occurred between 1999 and 2009. Using a variety of research designs and identification strategies, we obtain four main findings. First, the increase in the minimum wage erased the racial earnings gap up to the 10th percentile of the national wage distribution and up to the 30th percentile in the lowest wage region, the Northeast. Second, the minimum wage increases in the formal sector led to wage increases in the informal sector as well. We find an 80% pass-through of the minimum wage increases in the informal sector. Third, there is no evidence of significant reallocation of workers from the formal sector to the informal sector. This can be explained by the fact that the minimum wage is de facto binding in the informal sector (excluding agriculture, domestic workers, and the self-employed). Last, we do not find evidence of significant dis-employment effects, or of white-nonwhite labor-labor substitution. As a result, the minimum wage increases of the 2000s led to a large decline in the economy-wide racial income gap in Brazil.

JEL Codes: J15, J38, J23, J31.

*Ellora Derenoncourt: ellora.derenoncourt@princeton.edu; François Gérard: f.gerard@qmul.ac.uk; Lorenzo Lagos: lorenzo_lagos@brown.edu; Claire Montialoux: claire.montialoux@berkeley.edu. We thank David Card, Rucker Johnson, Patrick Kline, Hilary Hoynes, Jesse Rothstein for helpful advice and comments. We also thank numerous participants at UC Berkeley (Econ Department and GSPP), PSE, Sciences-Po, HEC, Georgetown University, the World Bank, the NBER SI Development, University of Bonn, Princeton, Columbia University, Claremont Colleges (Los Angeles), SoCCAM Conference, University of British Columbia, and at EALE-SOLE-AASLE Conference. Roberto Hsu Rocha, Maria Medeiros, and Helton Suzuki provided excellent research assistance. The authors gratefully acknowledge financial support from the UC Berkeley Institute for Research on Labor and Employment, the UC Berkeley Opportunity Lab, and the British Academy.

1 Introduction

Between 2000 and 2009, the Brazilian government implemented a dramatic increase in the minimum wage. The minimum-to-median wage ratio rose from 30% in 1999 (i.e., close to the federal minimum-to-median wage in the US today) to more than 50% in 2009 (close to the French minimum wage) (see Figure 1). Within the same decade, the country witnessed an unprecedented decline in wage inequality.[1] Many have investigated the role of the minimum wage in this decline (e.g. Komastsu and Menezes-Filho, 2016; Alvarez et al., 2018; Jales, 2018; Haanwinckel, 2020; Engbom and Moser, 2021). Perhaps less documented, racial inequality, measured as the difference in earnings between white and nonwhite workers has also fallen in Brazil.[2] From 1999 to 2009, the difference in average log monthly earnings between white and nonwhite workers fell from 40 log points to 28 log points – a decline comparable in magnitude to the decline in the unconditional racial earnings gap in the non-southern US during the Civil Rights era. Since 2009, the unconditional racial earnings gap in Brazil has stagnated at around 30%.

What is the role of the minimum wage increases in the 2000s in the dynamics of racial inequality in Brazil? What are the mechanisms through which such a large decline in inequality was possible? Is the minimum wage an effective policy tool reducing persistent group-based inequality in other emerging economies? These are the key questions we attempt to address in this paper.

The minimum wage has been shown to play a key role in the decline in racial inequality in the US during the Civil Rights era (Derenoncourt and Montialoux, 2021). However, it is not obvious that this policy can reduce racial disparities in lower income countries today. First, the nature of racial inequality in Latin America today may be very different from that of the US in the late 1960s. A key driver behind the small disemployment effects of the minimum wage during the Civil Rights era in the US was the low elasticity of substitution between white and Black workers. This was partly due to the persistence of a high degree of occupational segregation beyond the Jim Crow era. By contrast, there was no legalized system

[1] See e.g. López-Calva and Lustig (2010) or Lustig et al. (2016) for a documentation of the decline in wage inequality in Brazil and other Latin American countries using household survey data; see Medeiros et al. (2015) and Gobetti and Orair (2016) for studies using fiscal data; and Morgan (2017) for a study on Brazil combining both fiscal and household survey data sources.

[2] We follow Gérard et al. (2020) and pool together mixed race and Black workers with African ancestry. We also show in Appendix Figure D1 that the earnings gap between white and mixed race workers is of similar magnitude as the gap between white and Black workers using the labor force surveys (PNAD and PNAD Continua) and Census data. These gaps are actually identical in both sources of data from the late 1990s to today, which is our period of study. See Woo-Mora (2021) for a recent documentation of racial inequality trends in Latin America.

of discrimination in Brazil after the abolition of slavery. Second, Black Americans in the US represent approximately 10% of the labor force. In 2015 in Brazil, nonwhite individuals form a much larger group, representing nearly 60% of the population. This makes racial inequality a fundamental issue for the society as a whole and a core dimension of wage inequality in this country. Finally, the importance of the informal sector in emerging economies only increases the possibilities for displacement from jobs covered by the minimum wage to uncovered ones. Throughout our period of study, approximately 50% of the private sector is not legally covered by the minimum wage.

We think of Brazil as an ideal setting to study the effects of minimum wage increases on the dynamics of racial inequality in middle income countries. This is because Brazil is characterized by large and persistent racial disparities due to the legacy of slavery. It was the last country in the world to abolish slavery in 1888. As documented above, Brazil also underwent large nationwide minimum wage increases in the 2000s. These increases have no equivalent in other lower and middle income countries. Finally, Brazil is characterized by the availability of detailed labor force surveys and of administrative matched employer-employee data with information on race.[3] This allows us to study the role of firms in the evolution of racial gaps – including racial wage compression within firms and the reallocation of workers at play between them.

We use a variety of research designs and identification strategies to analyze the effect of the large increase in the minimum wage that occurred between 2000 and 2009. We find that the increase in the minimum wage erased the racial earnings gap up to the 10th percentile of the national wage distribution and up to the 30th percentile in the poorest region, the Northeast. We do not find evidence of a significant reallocation of workers from the formal sector to the informal sector, nor do we find differential effects across racial groups. In fact, the minimum wage is binding in the informal sector, acting as a norm for low-skilled workers across the entire private sector. We do not find evidence of significant dis-employment effects, or of white-nonwhite labor-labor substitution. We conclude that the minimum wage increases of the 2000s led to a large decline in the economy-wide racial income gap in Brazil.

We make four contributions. Our first contribution is to document the role of minimum

[3]By contrast, the information on race in US administrative data is indirect. They also do not contain information on education or on the number of hours worked – for these reasons, a precise evaluation of the effect of the minimum wage on hourly wages is not possible. Brazilian matched employer-employee data are also available for the *universe* of the private formal sector by contrast with the vast majority of such administrative data which are sampled (as is the case e.g. in France). They are also available for a long period of time (since 1985), making it possible to look at the long-run consequences of public policies (as opposed to, for example, administrative data in South Africa which also do not include information on race).

wage increases and spillovers on racial wage compression. Building on the methologies of Juhn et al. (1991) and Bayer and Charles (2018) and taking advantage of the richness of our data, we are able to document the effect of minimum wage increases on racial wage compression percentile by percentile. We provide graphical evidence of the evolution of earnings level gaps year by year: i) we provide monthly earnings distribution by race to show the extent of bunching around the minimum wage; we are able to show that bunching around the minimum wage is consistent in both PNAD (in which workers report their earnings) and RAIS data (where employers report their workers' earnings); ii) we also provide cumulative distribution functions separately by race to help visualize where in the wage distribution racial gaps are compressed. In our understanding, such detailed and transparent graphs can only be made in Brazil due to the availability of rich data with information on race. In 1999, before the large minimum wage increases, we show that the earnings level gap at the 10th percentile was close to 40 log points – i.e. similar in magnitude to the earnings level gap at the median and at the mean. During the 2000s, the earnings level gap had shrunk at all percentiles, but it fell by much more at the bottom than at the median. In 2015, the earnings level gap at the 10th percentile was essentially 0, while it was still substantial (22 log points) at the median. We find that there are no differential spillover effects across racial groups above the 20th percentile at the national level. By showing that wage gaps are reduced up the distribution quite a ways, we advance the minimum wage literature overall, where documenting the extent of spillovers has suffered for a lack of precise earnings information (Autor et al., 2016). Our results are consistent with the idea that minimum wage increases can substantially reduce wage inequality – and in particular, racial wage inequality – once potential spillover effects are taken into account (e.g. Lee, 1999; Engbom and Moser, 2021; Fortin et al., 2021).

Our second contribution is to show that minimum wage increases in the formal sector push wages up in the informal sector as well. We proceed in two steps. We first show graphical evidence that the minimum wage enforced in the private formal sector spills over to the informal sector. Monthly earnings distribution in PNAD exhibit a large spike around the minimum wage in the informal sector. The share of workers paid strictly below the minimum wage shrinks once we remove the self-employed, and workers in the agricultural and domestic service sectors. The fact that the minimum wage spills over to uncovered jobs is consistent with recent evidence across US firms (Derenoncourt et al., 2021b), age groups (Machin and Giupponi, 2020) and within multinationals(Hjort and Sarsons, 2020). We believe this result has important implications for middle-income countries and for the gig sector in

the US and other economies with a significant portion of the labor force in unregulated or only lightly regulated sectors. We then use a difference-in-differences design using PNAD data to quantify the spillover effects of the minimum wage increases in the informal sector. We leverage variation in the bite of the minimum wage across states at the time of the implementation of the policy. Strongly treated states are states that had a high (i.e., above-median) minimum-to-median wage in 1999. Weakly treated states are those that had a low minimum-to-median wage in 1999. We account for workers' demographic characteristics and for differential growth paths at the state level from 1995 to 2015. We find a 80% pass-through of minimum wage increases in the informal sector.

Our third contribution is to uncover the causal impact of the minimum wage increases in the 2000s on the reallocation of workers from the formal to informal sector. We use the same difference-in-differences analysis as above on the probability of being employed in the private formal vs. informal sector at the worker level. Overall, we find that we can rule out cross-sector employment reallocations of more than -0.27 for all workers (in absolute terms) and of more than -0.4 among nonwhite workers. Both elasticities are considered small in the minimum wage literature (Dube, 2019). Our results are robust to alternative definitions of the treatment variable (e.g., using the share of workers who bunch at the minimum wage rather than the minimum-wage to median ratio).

Conceptually, our empirical findings contradict segmented models of the labor market where the informal sector employs workers whose productivity falls below that of the formal sector. In this context, a minimum wage increase should lead to a reallocation of the lowest productivity workers to the informal sector. Meghir et al. (2015) provide a structural model of search frictions in the labor market that can account for the productivity overlap observed between the two sectors and worker transitions between the two. They show that in such an integrated model of the labor market, enforcement can be welfare enhancing as the presence of search frictions allow some employers to profit by offering informal as opposed to formal employment. Still, their model does not speak to the consequences of increased labor market regulation through the minimum wage. Labor markets characterized by oligopsonistic competition between firms (as in Berger et al. (2019)) across both the formal and informal sectors may rationalize our findings. In such a labor market, minimum wage hikes in the leading formal sector induce increases in the informal sector as the two compete for minimum wage workers.

Our fourth contribution is to quantify the impact of the minimum wage increases in the 2000s on racial income inequality (i.e. among workers and non-workers), and income in-

equality as a whole. Using a similar difference-in-differences design as above, we are able to rule out employment elasticities lower than -0.09 for all workers, -0.15 among nonwhite individuals, and -0.07 among white individuals when looking at the probability of employment in the private sector (formal or informal) vs. unemployment. Our employment elasticities are slightly more negative when looking at the probability of employment vs. unemployment or no longer being in the labor force. This result suggests that the 2000-2009 increases may have led to a negative effect on labor force participation. We intend to better understand why that might be the case in a future version of this analysis, in particular, by evaluating the potentially confounding role of the increase in cash transfer programs on labor force participation over the period. In a future version of this work, we are also planning to quantify the role of the 2000s-era minimum wage increases in closing the economy-wide racial income gap in Brazil and the overall income gap by using decomposable income inequality measures (Bourguignon, 1979) and Oaxaca-Blinder decompositions that leverage our empirical design (Oaxaca, 1973; Blinder, 1973; Kleven et al., 2019).

Related literature. Our paper relates to three literatures, at the intersection of labor and development economics.

We first contribute to the literature on the economic effects of minimum wages in several ways. Our paper adds to the literature on minimum wage increases in lower and middle income countries and in Brazil in particular (e.g. Lemos, 2004; Bosch and Manacorda, 2010; Magruder, 2013; Bhorat et al., 2014; Broecke and Vandeweyer, 2015; Komastsu and Menezes-Filho, 2016; Alvarez et al., 2018; Jales, 2018; Saltiel and Urzúa, 2020; Haanwinckel, 2020; Engbom and Moser, 2021; Corseuil et al., 2021). Many meta-analyses have been written (e.g. Belman and Wolfson, 2016; Broecke et al., 2020; Neumark and Corella, 2020) to document the effect of such policies in economies with a large informal sector. Our approach advances this literature by using a graphical and transparent methodology to document the effects of minimum wage increases and by combining several types of data sources (labor force surveys and administrative data). We are also the first to focus on racial inequality in this context – as opposed to wage inequality as a whole.

We shed light on the spillover effects of the minimum wage along the wage distribution, as we are able to document the evolution of earnings level gaps percentile by percentile. The extent of spillover effects in response to minimum wages has been difficult to identify due to a lack of very rich data on earnings (Autor et al., 2016). Another strand of the literature argues that spillover effects of minimum wage increases are substantial, in the US (e.g. Lee, 1999;

Fortin et al., 2021) and in Brazil (Engbom and Moser, 2021). In a future version of this paper, we are planning to contribute to documenting wage spillovers within firms using matched employer-employee data, as well as the evolution of the wage premium for white workers.

Our results on the spillovers to the informal sector also speak to recent evidence on the spillover effects of large employer minimum wage policies to uncovered sectors. Derenoncourt et al. (2021b) show that voluntary minimum wage increases in firms like Amazon, Walmart, and Target spill over to nearby firms. Such spillover effects to uncovered firms are consistent with the lighthouse effects of the minimum wage documented in the context of Brazil (e.g. Fajnzylber, 2001; Neri et al., 2001; Neri and Moura, 2006; Lemos, 2009; Gindling, 2018), when employers from the formal and informal sectors must compete for the same workers.

We also add to the literature on the effects of very large minimum wage increases. Our results are consistent with recent evidence on such increases (e.g. Cengiz et al., 2019; Harasztosi and Lindner, 2019; Derenoncourt and Montialoux, 2021) finding minimal or no dis-employment effects.

Finally, we hope to push the minimum wage literature forward by documenting reallocations of workers from low-productivity to high-productivity firms. One proxy for that is firm size. Dustmann et al. (2020) recently showed reallocation of workers from small firms to large firms in Germany. In addition to these reallocations, we hope to be able to document reallocation of workers from non-exporting firms to exporting firms by matching the Brazilian customs data to the matched employer-employee data. This last piece of work will also shed new light on the long-standing debate on the effectiveness of domestic policies vs. trade in reducing inequality.

Second, we contribute to the literature in social sciences documenting patterns of racial inequality in Latin American countries and seeking to better understand what economic forces and policies can reduce them. Recent studies documenting patterns of racial inequality in Latin America include Loveman (2014), Telles and Paschel (2014), Ayala-McCormick (2021) and Mariano et al. (2018) on Brazil, in particular. Although there is a literature examining the factors behind the historical decline in Brazilian wage inequality in the 2000s (Firpo and Portello, 2019), much less has been written on the factors behind the dynamics of racial inequality over the same period. Gérard et al. (2020) document the role of firms in racial gaps in Brazil, while we document the role of a major labor market institution. Derenoncourt et al. (2021a) also document the role of collective bargaining agreements on the evolution of racial inequality in Brazil.

Finally, we contribute to the literature on the role of domestic, governement policies in reducing racial income and poverty gaps in middle-income countries, as opposed to trade (e.g. Galle et al., 2020; Adão et al., 2020; Dix-Carneiro et al., 2021) and other economic forces.

The paper is organized as follows. Section 2 provides the institutional and macroeconomic context of the 2000s-era minimum wage increases in Brazil. It also presents the data we used and constructed for our analysis. Section 3 documents the effect of the large 2000-2009 minimum wage increases on racial wage compression at the bottom of the distribution at both the national and regional levels. Because we are ultimately interested in understanding how the minimum wage affected income inequality (i.e. the difference in earnings among workers and non-workers), we study its displacement effects from covered jobs to uncovered ones. Section 4 examines whether the minimum wage caused any displacement from the formal sector (i.e., the covered sector) to the informal sector (i.e., the uncovered one). Section 5 assesses whether the minimum wage displaced workers from employment to non-employment. Section 6 concludes.

2 Context, Data, and Descriptive Statistics

2.1 Context on the 2000-2009 minimum wage increases

Legislation. The minimum wage in Brazil is set at the federal level. It applies to all regions, states, industries, and occupations.[4] Before 2008, the federal minimum wage was passed by Congress and approved by the President. Since 2008, the minimum wage has been determined by a formula that considers past inflation and GDP growth. In the case of inflation, the formula takes into account inflation accumulated during the previous year $(t-1)$, while for GDP, due to the lag in obtaining the most recent data, they apply the rate of increase of year $t-2$.[5]

Sharp increases from 2000 to 2009. Brazil underwent large minimum wage increases in the 2000s. The minimum wage increases started at the end of Cardoso presidency, before being pushed by Lula (see Figure 2a). The ratio between the federal minimum wage and the median

[4]Since 2001, states can set higher minimum wages than the federal one. Five states have used this possibility: Rio Grande do Sul, Santa Catarina, Paraná, Rio de Janeiro and São Paulo. These state-level minimum wages vary by industry and occupation. In our analysis using the PNAD data, we focus on the federal minimum wage. In the matched employer-employee data, which is significantly more detailed, we also use variation in industry- and occupation-specific minimum wages at the state level to study the impacts of increases. See Appendix C for more details. From 1940 to 1884, Brazil also had state-specific minimum wages.

[5]See Appendix C for more details.

wage for white workers rose from 30% in 1999 to 52% in 2009, before entering a period of stagnation (this ratio was 53% in 2015 and 50% in 2019) (see Figure 1, right axis). Such an increase is exceptionally large: in 1999, the minimum-to-median wage was lower than the current ratio in the US; a decade later, this ratio was slightly below the current minimum wage in France; or, equivalently, close to the ratio observed in the US when the minimum wage peaked in 1968 (Derenoncourt and Montialoux, 2021). Such a sharp increase is comparable in magnitude to the 2000-2002 minimum wage reform analyzed by Harasztosi and Lindner (2019) in Hungary. To our knowledge, Brazil's high minimum wage has no equivalent in other low or middle income countries.

Informality in Brazil. Despite the universal nature of the federal minimum wage in Brazil, a large informal sector exists. Following Meghir et al. (2015), we define the informal sector as unregistered employees (i.e. those without an official working permit) pooled together with the self-employed, and entrepreneurs. By constrast, the formal sector is comprised of workers in the private sector with an official working permit (called the *carteira assinada*). We show in Figure A1c that the share of the public sector is mostly flat from 1985 to today, at around 12%. Within the private sector, 56% of workers are informal (vs. 44% formal) in 1999. By 2015, only 45% of workers are informal vs. 55% formal. In this sector, in theory, employers do not comply with wage and employment regulation or pay taxes. We find that informal firms are smaller than in the formal sector: 70% of informal workers work in firms with 9 workers or less in 2015 vs. 24% of formal workers (see Table 2). Smaller employers may find it easier to evade detection. We also find that informal firms are overrepresented in Northeast: in 2015, 33% of informal workers were working in Northeast vs. 17% of formal workers (see Table 2). Employers in the lowest wage region of Brazil might also find it easier to evade detection. Finally, part-time workers are overrepresented in the informal sector: in 2015, nearly 40% of informal workers were working less than 40 hours a week vs. 11% in the formal sector (see Table 2). These patterns were already true in 1999 (see Table 1).

Macroeconomic context. The 2000-2009 minimum wage increases were implemented during an exceptionally buoyant period for the Brazilian economy. Brazil experienced rapid economic growth after the Plano Real stabilized the economy in the mid-1990s, until the country was hit by the recession in 2014: GDP per capita was 50% higher in 2009 than in 1995 and 75% higher in 2014 than in 1995. During that period, the unemployment rate exhibits a steady decline, from 10.2% 1999 to 6.7% in 2014 (see Figure 2b). On the demand side, the role

of trade is often considered as a key factor that spurred economic growth during the 2000s. On the supply-side, many changes happened in the labor market from 1995 to 2015: a 57% increase in the private labor force (see Appendix Table A2), as population was growing; a relative increase in the share of nonwhite workers within this labor force (i.e. 42% of nonwhite workers in the formal and informal sectors combined in 1995 vs. 53% in 2015, see Appendix Table A2); an increase in education attainment for the entire population, with gains most concentrated among nonwhite individuals;[6] a decline in the size of the informal sector;[7]; and an increase in conditional cash transfer programs (with the creation of *Bolsa Família* in 2004). We take into account all of these changes in our analysis of the effects of the minimum wage.

2.2 Data

We use four main data sources to study the large minimum wage increases in the 2000s in Brazil: the universe of the matched employer-employee data; the two labor force survey micro-files going back to 1976; decennial census data spanning 1960-2010; and data on federal and state minimum wage legislation by industry and occupation. See Appendix A for more details on public data sources and Appendix B for more details on restricted access data. In these appendices, we show that the different data sources are consistent with each other and well-suited to study the effect of minimum wage increases in Brazil.

Restricted access matched employer-employee data. This dataset is called *Relação Anual de Informações Sociais* (RAIS) and contains information on employment spells, earnings and number of hours worked for the universe of workers employed in the formal sector in Brazil from 1985 to 2017. Employers are required to report this information to the Ministry of the Economy (formerly the Ministry of Labor). The data allow us to track in which establishment workers work and to observe job-to-job transitions within the formal sector. This dataset contains a wealth of information on both workers (e.g. gender, age, education, tenure, six-digit occupation code, type of labor contract, contracted hours, hiring date, separation date, type of separation) and firms (location, industry, legal status). Critically, this dataset contains information on workers' race from 1999 onwards. We impute information on race for prior years from 1995-1999 (see Appendix B). We make use of two different measures of earnings.

[6]The share of the private formal labor force with a college education increased from 10% in 1999 (see Table 1) to 17% (see Table 2) in just 16 years. The respective shares among nonwhite individuals are 4% and 10%, and 14% and 24% among white individuals.

[7]There are many reasons that can explain this decline: for example, Lula enacted enforcement policies at the beginning of the 2000s (in particular, among domestic service workers in 2003). The minimum wage might have also played a role in this decline – something we analyze in section 4.

The first measure is monthly earnings as stipulated in the employment contract at the end of the year or at the end of the relationship for spells that ended during the year ("contracted wage" in what follows). This is the measure that is the closest to the definition of wages and the most well suited for our analysis of the effects of minimum wages. However, this measure is only available starting in 2002. Second, we also use a measure of earnings earned in December each year ("December earnings" in what follows). This measure of earnings include wages and bonuses earned in December. As a result, bunching at the minimum wage using this measure is smaller than using the contracted wage measure (see Appendix B), but this measure is consistently recorded since 1985.

Publicly available data. We use and combine a variety of publicly available data on the Brazilian labor force. We use Census micro-data from 1960 to 2010, in particular to assess long-term trends in racial inequality in Brazil. We also exploit two household surveys: i) the *Pesquisa Nacional por Amostra de Domicílios* (PNAD), spanning 1976-2016, and ii) the *Pesquisa Nacional por Amostra de Domicílios Continua* (PNAD Continua), spanning 2012-2020. PNAD is an annual national household survey, similar to the Current Population Survey in the US. It contains information on all individuals regardless of their labor market status. The main advantage of this data source over RAIS is that it contains information on workers employed in the informal sector. The measure of earnings we use is a measure of monthly wages earned in September each year.[8] We also exploit PNAD Continua, a longitudinal household survey, that was created to replace the original PNAD and the *Pesquisa mensal de emprego* (PME), another employment survey for major cities in Brazil. The data are available every trimester, but we only use the information contained in the third trimester (to ease comparisons with PNAD) in a repeated cross section.

Minimum wage database. We build a database of minimum wages from 1940 – i.e. from the introduction of the federal minimum in Brazil – to 2020. The database contains the values of the federal minimum wage at the month-by-year level in nominal terms using the information published by the Brazilian Institute of Geography and Statistics (IBGE) website.[9] It also contains nominal values of regional minimum wages. Brazil implemented state-level minimum wages from 1940 to 1984. Since 2001, five states have implemented industry and occupation-specific minimum wages at a higher level than the federal one: Rio Grande do

[8] PNAD questionnaire asks, "How much were your earning in this job over the last month?" This is a measure of take-home pay, i.e., after payroll taxes paid by the employer.

[9] At this address: https://www.ibge.gov.br/estatisticas/downloads-estatisticas.html.

Sul, Santa Catarina, Paraná, Rio de Janeiro and São Paulo. We provide more details on the construction of this database in Appendix C.

2.3 Descriptive Statistics on the Bindingness of the Minimum Wage

Bindingness in the formal sector. We document two salient facts regarding the bindingness of the minimum wage in Brazil using both the PNAD and RAIS data. First, the minimum wage creates a large spike in the monthly earnings distribution of full-time workers in the private formal sector. The share of workers paid within 10% of the federal minimum wage in the private formal sector fluctuated between 6% and 7.5% between 1995 and 1999; it sharply increased in the 2000s and peaked at 20% in 2009; in 2015, this share was just above 15% at the national level. We show that the spike at the minimum wage in monthly earnings distributions in PNAD (i.e., earnings reported by workers) is nearly identical to the spike observed using the contracted wage in the universe of the matched employer-employee data (i.e. earnings reported by employers).[10] We also show that the two data sources are consistent across regions and racial groups (see Appendix Figures B10 to B14). Second, virtually no workers report being paid below the minimum wage in the private formal sector. This nearly perfect compliance with the minimum wage is also confirmed in the RAIS data.

Bindingness in the informal sector. Although the minimum wage only effectively applies to the formal sector, we document substantial *de facto* bunching of the minimum wage in the informal sector. Overall, in 2009, 18% of full-time workers in the private sector (formal and informal taken together) are paid exactly at the minimum wage creating a large spike in the monthly earnings distribution (see Figure 2d). Of these 18%, 11 percentage points of this mass point is created by workers employed in the formal sector vs. 7 percentage points by workers employed in the informal sector.

Heterogeneity across racial groups. The minimum wage is much more binding among nonwhite workers than among white workers. In 2009, when the federal minimum wage peaked in Brazil, nearly 15% of white workers in the formal sector were paid at the minimum wage vs. more than 26% among nonwhite workers. This discrepancy was already present

[10]See Appendix Figures B5 to B9 for a comparison between monthly earnings distributions in PNAD and RAIS. Note that in this series of figures, we've excluded agricultural and domestic services workers from our analysis sample. That is why the share of workers paid at the minimum wage is slightly lower than the figures reported above in the text in which we do not make these restrictions. We are planning to harmonize the samples in a future version of our paper.

in 1999 and persisted through 2015 (see Figure 3). We also note that the shares of workers at the minimum wage by race are remarkably aligned in PNAD and RAIS (see Appendix Figures B15 and B16).

3 The effect of the 2000-2009 minimum wage reforms on racial wage compression

3.1 Methodology and Sample

We start our analysis by documenting the effect of the large 2000-2009 minimum wage increases on racial wage compression in Brazil. By "racial wage compression," we refer to the reduction in monthly earnings disparities between white and nonwhite workers. Monthly earnings refer to take-home pay received by workers in September of each year in the labor force surveys (PNAD and PNAD Continua). When using the matched employer-employee data, we work with two different earnings concepts: monthly contracted wage or earnings earned in December (wages and bonuses) (see Section 2.2). In the results that follow, we always specify which earnings concept we are referring to.

Methodology. Following the methologies of Juhn et al. (1991) and Bayer and Charles (2018), our outcome of interest is the racial earnings level gap at different percentiles q. The racial earnings level gap is the white-nonwhite log-point difference in monthly earnings at a given percentile q of the respective white and nonwhite distributions. It is estimated using the following quantile regression:

$$\text{log earnings}_{it} = \alpha_t(q) + \beta_t(q)\text{white}_i + \epsilon_{it}(q) \tag{1}$$

where log earnings_{it} is worker i's log-monthly earning in year t and white_i is an indicator for whether the worker i is white. The main coefficient of interest, $\beta_t(q)$, yields an estimate of the unconditional racial earnings level gap while $\alpha_t(q)$ estimates the log earnings of the worker at the qth percentile of the monthly earnings distribution of white workers. We do not include any controls in this regression, so that we transparently measure unconditional racial earnings level gaps.

Figure 4 illustrates the racial earnings level gap measure at the 10th and 50th percentiles in different years of our period of study: in 1999 (i.e. just before the start of the large minimum wage increases), 2002, 2009 (i.e. when the federal minimum wage peaks) and 2015 (i.e. the last year of our period of interest). We plot two cumulative distribution functions for the

log-monthly earnings of white and nonwhite workers. The two horizontal lines represent percentiles q=10 and q=50. The earnings level gap at q is the horizontal difference at q, read from the nonwhite and white cumulative distribution functions.

This measure allows us to shed light on precisely where in the earnings distribution racial wage compression takes place. It also allows us to track the spillover effects of minimum wage increases, percentile by percentile. We report our results in Section 3.2 below.

Sample. Our sample includes all prime-age workers, i.e., aged 25 to 54. Workers younger than 25 may be enrolled in higher education – this share is small, but is increasing over time, particularly during our main period of study (1995-2015). Tables 1 and 2 show that 10% of private formal workers in 1999 had completed a college education vs. 17% in 2015. We restrict our sample to white and nonwhite individuals. Following Gérard et al. (2020), we pool "Pretos" (i.e. Black persons) and "Pardos" (i.e. Brown persons or mixed race with African ancestry) together to form the "nonwhite" category. In the current analysis to focus on a concept of racial inequality similar to the literature on Black-white earnings differences in the US, we exclude individuals identifying as "Amarelos" and "Indigenas" (less than 2% of the population). We also exclude workers with extremely low (below 5.5 log points) or extremely high (above 10.5) log real monthly earnings.[11] We focus on full-time workers, or workers working more than 40 hours a week (i.e. 89% of workers in the private formal sector and around 65% of workers in the private informal sector; see Tables 1, 2 and 3 for statistics in 1999 and 2015). This is because the federal minimum wage is set in terms of monthly earnings for workers with full-time contracts of 44 hours a week.[12] We do not make any restrictions on the industry or occupation of the worker.

3.2 Estimates at the national level

In 1999, before the large minimum wage increases, we show that the earnings level gap at the 10th percentile was close to 40 log points – similar in magnitude to the earnings level gap at the median (see Figure 4) and at the mean (see Figure 1). By the end of our period of interest, the earnings level gap had shrunk at all percentiles, but it fell by much more at the bottom than at the median. In 2015, the earnings level gap at the 10th percentile was essentially 0, while it was substantial (22 log points) at the median.

[11] All monthly earnings are converted to 2019 BRL, using the INPC price index from the Brazilian Institute of Geography and Statistics (IBGE).

[12] The minimum wage is nonetheless adjusted proportionally for part-time workers; in principle, it would be possible to add them in the analysis.

We plot the annual evolution of the earnings level gap at different percentiles in Figure 5. Because around 10% of white workers bunch at the minimum wage and more than 10% of nonwhite workers bunch at the minimum wage, we find that the racial earnings level gap at the 10th percentile fell to almost 0 as early as 2006. It remained at this level until 2019. We interpret these results as evidence that the minimum wage is the primary explanation behind racial wage compression at the 10th percentile.

By looking at the evolution of the earnings level gap at higher percentiles, we find that the minimum wage had smaller effects at the 15th percentile. As we move above the 15th percentile, we find that the racial earnings level gap moves closer to the gap at the mean. One interpretation of this result is that there are no differential spillover effects across racial groups above the 20th percentile at the national level.

3.3 Estimates at the regional and state levels

In order to uncover regional heterogeneity in racial wage compression over 1995-2015, we apply our methodology to different groups of states. We start by contrasting the effect of the minimum wage on racial wage compression in the least affected states vs. the most affected states. We compute the minimum-to-median wage pre-reform in 1999 for the 27 states, and find that the median ratio is 36.9% in the private formal sector. We define the "weakly treated states" as those that had a minimum-to-median wage below that threshold in 1999 – and therefore are supposedly the least affected by the minimum wage increases in the 2000s. The strongly treated states are those that had a minimum-to-median wage above this threshold. The 14 strongly treated states are concentrated in the Northeast region – the lowest wage region in Brazil – and to some states in the Midwest and the South (see Figure 6). We also show that this list of states is robust to other measures of the bindingness of the minimum wage, such as the fraction of affected workers within states.

We present our results on the effect of the minimum wage on racial wage compression percentile by percentile in a series of panels in Figure 7. We start by looking at the earnings level gap at the 15th percentile, higher up in the distribution than at the national level, simply because more workers bunch at the minimum wage in the strongly treated states than in the country overall. We find that, before 2009, the earnings level gap in the two types of states is on a slightly declining trend. This trend is similar across the two types of states. Right after 1999, the earnings level gap at the 15th percentile falls much more in the strongly treated states than in the states where the minimum wage is less binding. The magnitude of the decline of the earnings level gap is also striking: at the 15th percentile – where the minimum

wage binds in the strongly treated states – the initial earnings level gap was 33 log points in 1999 and fell to 0 in 2006. The minimum wage was effective at erasing racial wage inequality for the bottom 15% in just a few years. By contrast, in the weakly treated states, the raw gap at the 15th percentile stabilizes at around 10 log points after 2009. As we move above the 15th percentile, we show that the effect of the minimum wage on racial wage compression is sharper in strongly vs. weakly treated states up to the 21st percentile. Above this percentile (whether looking at the 25th, 30th, 40th or 50th), we find that the earnings level gap evolves in parralel in the two types of states – a fact that is consistent with the idea that factors unrelated to the minimum wage explain the decline in racial inequality higher up in the distribution.

How effective was the minimum wage at compressing racial wage inequality in the poorest states? We apply the same methodology as above and compare the evolution of earnings level gaps in the Northeast with its evolution in the weakly treated states. We find that in the Northeast, the minimum wage was effective at erasing racial wage inequality for the bottom 20% in 3 years (see Figure 8). The earnings level gap at the 20th percentile in this region falls to zero by 2002. The minimum wage had a sharper effect on racial wage compression in the Northeast than in the weakly treated states up to the 30th percentile.

4 Reallocation of workers to the informal sector

Section 3 documented the extraordinary role of the 2000-2009 minimum wage in racial wage compression in Brazil at the bottom of the distribution. However, the effect of the minimum wage on income inequality (i.e. among all adults, not just earners or formal sector workers) is less clear if it causes displacement from jobs covered by the minimum wage to uncovered jobs or to non-employment. We explore this in two ways. First, we examine whether the minimum wage caused any displacement from the formal sector (i.e. the covered sector) to the informal sector (i.e. the uncovered one). Second, we assess whether the minimum wage displaced workers from employment to non-employment. We address the question of formal-to-informal reallocation below. We examine the employment effects in Section 5.

4.1 Identification strategy

To document the causal effect of the 2000-2009 minimum wage increases on cross-sector employment reallocations using the labor force survey (PNAD), we use a difference-in-differences design. We leverage variation in the bite of the minimum wage across states at the time of the implementation of the policy. Strongly treated states and weakly treated

states are defined as in Section 3.3.

The key assumption is that absent the 2000-2009 minimum wage increases and conditional on fixed differences between workers, states, and years, employment outcomes in the strongly treated states would have followed the same trend as in the weakly treated. We provide graphical evidence that this "parallel trends" assumption holds (see Figure 9a). We consider the following model:

$$\mathbb{1}\{\text{Informal}_{ist}\} = \alpha + \delta_k + \sum_k \beta_k \text{Strongly}_s \times \delta_{t+k} + \mathbb{X}'_{ist}\Gamma + \delta_s + \varepsilon_{ist} \quad (2)$$

where Informal_{ist} is 1 if the worker is employed in the private informal sector[13] and 0 if employed in the private formal sector. Individual-level controls include gender, race, education, and a quadratic in work experience. We also include a quadratic in log GDP per capita by state to take into account differential growth paths across states over 1995-2015.

The coefficient of interest, β_k, measures the effect of 2000-2009 minimum wage increases k years after 1999 (baseline year in which the difference between the outcomes in the two groups of states is normalized to 0 in Figure 9a).

4.2 Results

All workers. Figure 9a shows that before the large minimum wage increases, the probability of being employed in the informal relative to the formal sector evolve in parallel in the strongly and weakly treated states. It stays flat between 1999 and 2009 showing no evidence of displacement effects of the minimum wage to the informal sector. If anything, we find that starting in 2011, the probability of being employed in the formal sector increases (i.e. a negative β_k) in the strongly treated states relative to the weakly treated states – i.e. a conclusion that would contradict a segmented model of the labor market in which minimum wage increases would displace less productive workers from the formal sector into the informal sector where employers pay workers less than the minimum wage. We offer a discussion of why this displacement might not occur in Section 4.3.

These findings are confirmed when looking at the difference-in-differences results in levels and with no controls. Figure 9b transparently shows that the share of workers in the informal sector (vs. in the private formal sector) fell by approximately 15 percentage points in both the strongly and the weakly treated states between 1999 and 2009. On this graph, we've normalized the differences in informality rates in the two types of states to zero in

[13]The informal sector is composed of unregistered salaried workers, and the self-employed.

1999. The version of this graph in levels (with differences not normalized by 0 in 1999, see Appendix Figure D2) reveals that the informality rate was significantly higher in 1999 in the strongly treated states (68%) than in the weakly treated states (50%). By 2015, these rates respectively fell to 53% and 38%. Figure 9b also shows that removing individual and state-level controls does not affect our conclusions. In other words, we find no evidence that sorting on observable characteristics explains our findings.

Across racial groups. In order to look at the heterogeneity of our results across racial groups, we run our difference-in-differences strategy for white and nonwhite individuals separately. Our results for the two groups are not statistically different from one another (see Figure 9c). Even when pooling together our estimates from 2001 to 2009,[14], we cannot rule out that there are no statistically significant differences across racial groups (see Table 4).

One might be concerned that we do not find any differential reallocation affects across the two types of states, and across racial groups, because our estimates look at employment overall, instead of focusing on low-wage employment – where the employment effects of minimum wage increases should arguably be found. We repeat our analysis among low-wage workers only, excluding workers earnings more than 130% of the minimum wage. Our results are essentially unchanged (see Figure 9d) – although our estimates are noisier.

Overall, we find that we can rule out reallocations from the formal sector to the informal sector of more than -0.27 for all workers (in absolute terms) and of more than -0.4 among nonwhite workers (see Table 4).[15] In other words, we are able to rule out that increasing the minimum-to-median wage from 30% to over 50% in less than a decade in Brazil caused a reallocation of nonwhite workers from the formal to the informal sector of more than 4%, and of more than 2.7% for all workers.

4.3 Discussion

How can we explain that minimum wage increases did not cause a large reallocation of workers from the formal to the informal sectors? The results of our difference-in-differences approach can be rationalized in a context where the minimum wage strongly spills over the informal sector. As discussed in Section 2.3, we document that indeed, in each year from

[14]PNAD was not conducted in 2000.

[15]Note that these elasticities were computed by running our difference-in-differences strategy on the outcome $\mathbb{1}\{\text{Formal}_{ist}\}$ where Formal_{ist} is 1 if the worker is employed in the private formal sector, 0 if employed in the informal sector – as opposed to $\mathbb{1}\{\text{Informal}_{ist}\}$ – that is why the signs of the β_k are reversed. We're planning to harmonize the outcomes of these regressions in a future version of this draft.

1995 to 2015 – and in particular in 2009, when the minimum wage peaks – monthly earnings distributions exhibit a large spike around the minimum wage in the informal sector (see Figure 2d). We also show that workers paid below the minimum wage in the informal sector are exclusively either self-employed or employed in agriculture or domestic services. Unregistered employees in all other sectors are paid at the minimum wage. Although not enforced in the informal sector, the minimum wage is *de facto* the norm for low-wage individuals. Our empirical finding is inconsistent with perfectly competitive, fully segmented formal and informal sectors. In such an economy, an increase in the minimum wage would lead to reallocation of the lowest productivity workers from formal to informal employment as employers would lose revenue paying those workers the minimum wage. Instead, a lack of reallocation and a spike at the minimum wage in the informal sector are more consistent with oligopsonistic competition between informal and formal employers (Berger et al., 2019).

In addition to small reallocation effects overall, the minimum wage increases did not cause substantial reallocation of nonwhite workers to the informal sector relative to white workers. How can we explain this? Conditional on being below the new minimum wage, nonwhite and white workers are paid similarly, so that the increase is the same for the two groups. Table 4, fourth row, columns (2) and (3) show that the wage increase is +10.3 log points over 2001-2009 among nonwhite workers vs. +11.8 log points among white workers. Based on this result, employers do not appear to have incentives to substitute away from nonwhite workers and towards white workers, or *vice-versa*, in the private formal sector.

5 Employment effects of minimum wage increases

5.1 Identification strategy

We employ a similar difference-in-differences strategy as in Section 4 to evaluate the causal effect of minimum wage increases on employment. Specifically, we estimate the following model:

$$\mathbb{1}\{\text{Employed}_{ist}\} = \alpha + \delta_k + \sum_k \beta_k \text{Strongly}_s \times \delta_{t+k} + \delta_s + \varepsilon_{ist} \tag{3}$$

where Employed_{ist} can take two definitions. Our first outcome of interest takes the value 1 if the worker is employed (either in the private formal or informal sector) and 0 if the worker is unemployed. This outcome allows us to track movements from employment in the

private sector to unemployment.[16] To take into account the potential effect of minimum wage increases on labor force participation, we also estimate our model on a second outcome that takes the value 1 if the worker is employed and 0 if the worker in unemployed or not in the labor force.

5.2 Results

All workers. We find a precise zero effect of minimum wage increases on employment – whether looking at the probabilities of employment vs. unemployment or at the probabilities of employment vs. unemployment or not in the labor force (see Table 4, first row, columns (4) and (7)). For out first outcome – probability of employment vs. unemployment – we can rule out employment elasticities with respect to the average wage increase in the formal sector of more than -0.09 in absolute terms. For our second outcome – probability of employment vs. unemployment or not in the labor force – we can rule out employment elasticities with respect to the average wage increase in the formal sector of more than -0.26 in absolute terms. According to the classification developed by Dube (2019, p.27), both employment elasticities can be considered small in the context of the minimum wage literature. [17] The fact that the employment elasticity on our second outcome is slightly more negative than on our first outcome suggests that, if anything, the 2000-2009 may have led to a negative effect on labor force participation. We intend to better understand why that might be the case in a future version of this analysis, in particular, by evaluating the potentially confounding role of the increase in cash transfer programs on labor force participation over the period.

Across racial groups. We repeat our analysis above separately for white and nonwhite workers. For both outcomes of interest, Figure 10a and 10b show that employment probabilities evolve in parallel in strongly and weakly treated states, and for white and nonwhite individuals.

The differential probability of being employed vs. unemployed among white workers in the strongly vs. weakly treated states is remarkably flat over 1995-2015. The employment probabilities among nonwhite workers exhibit a similar pattern, although they are less precisely estimated. Overall, we can rule out employment elasticities (using the definition

[16] We show in Figure A1c that public employment is stable over time; that is why we ignore this margin here and focus on probabilities of employment in the private sector only vs. unemployment.

[17] In his international review of the employment effects of minimum wages, Dube (2019, p.27) offers the following heuristic for values of own-wage elasticities (OWE): "While all categorizations are inherently arbitrary, we can roughly think of an OWE less negative than -0.4 as small in magnitude, between -0.4 and -0.8 as medium, and more negative than -0.8 as large."

of employment in the private sector vs. unemployment) greater than -.15 among nonwhite workers and -0.07 among white workers (see Table 4, seventh row, columns (5) and (6)).

We reach similar conclusions when looking at probabilities of being employed vs. unemployed or not the in the labor force. However, these employment elasticities tend to be larger in magnitude and more imprecisely estimated. We are able to rule out employment elasticities greater than -.33 among nonwhite individuals and -0.23 among white individuals (see Table 4, seventh row, columns (8) and (9)) – which are still considered small according Dube (2019)'s classification. As mentioned above, we'll investigate the increase in the generosity of *Bolsa Familia* from 2004 as a potential confounding factor for our difference-in-differences analysis that might explain why our employment elasticities are slightly more negative when taking into account potential effects on labor force participation vs. not.

5.3 Discussion

We do not find evidence of any statistically significant negative employment effects of the 2000-2009 minimum wage increases. Empirically, these results are consistent with recent evidence on very large minimum wage increases, whether in the US (e.g. Cengiz et al., 2019; Derenoncourt and Montialoux, 2021; Bailey et al., 2021), or in Europe (e.g. Harasztosi and Lindner, 2019; Dustmann et al., 2020).

Conceptually, these results are consistent with several models of the labor market. First, they are consistent with a perfectly competitive model of the labor market with low elasticity of substitution between factors of production in the formal sector. This situation can occur when labor markets are tight – as was the case in Brazil in the 2000s. Oligopsonistic models of the labor market (as in Berger et al. (2019)) with competition between firms across both the formal and informal sectors may rationalize our findings.

In a future version of this work, we plan to better document the channels through which the minimum wage can be increased without causing large employment losses. Using the matched employer-employee data, we will examine the differential spillover effects among white and nonwhite workers within firms, and the evolution of the earnings rank gap. We'll also be able to explore reallocations from low-productivity jobs to high-productivity jobs. This include evaluating reallocation of workers from small firms to large firms (following Dustmann et al. (2020)); and from non-exporting to exporting firms using the customs data matched to the linked employer-employee data: exporting firms may have experienced large productivity growth relative to non-exporting firms from 2000-2015 and may have been more able to share rents with workers than firms that did not benefit from trade growth.

6 Conclusion

This paper studies how a national minimum wage affects wages, and in particular, racial earnings disparities in a middle-income country. Our context is the Brazilian economy, characterized by persistently large racial disparities and the availability of detailed labor force surveys and administrative matched employer-employee data with information on race. We analyze the effect of large increases in the minimum wage that occurred between 1999 and 2009. Using a variety of research designs and identification strategies, we obtain three main findings. First, the increase in the minimum wage erased the racial earnings gap up to the 10th percentile of the national wage distribution and up to the 30th percentile in the lowest wage region, the Northeast. Second, there is no evidence of significant reallocation of workers from the formal sector to the informal sector. This can be explained by the fact that the minimum wage is de facto binding in the informal sector (excluding agriculture, domestic workers, and the self-employed). Third, we do not find evidence of significant dis-employment effects, or of white-nonwhite labor-labor substitution. As a result, the minimum wage increases of the 2000s led to a large decline in the economy-wide racial income gap in Brazil.

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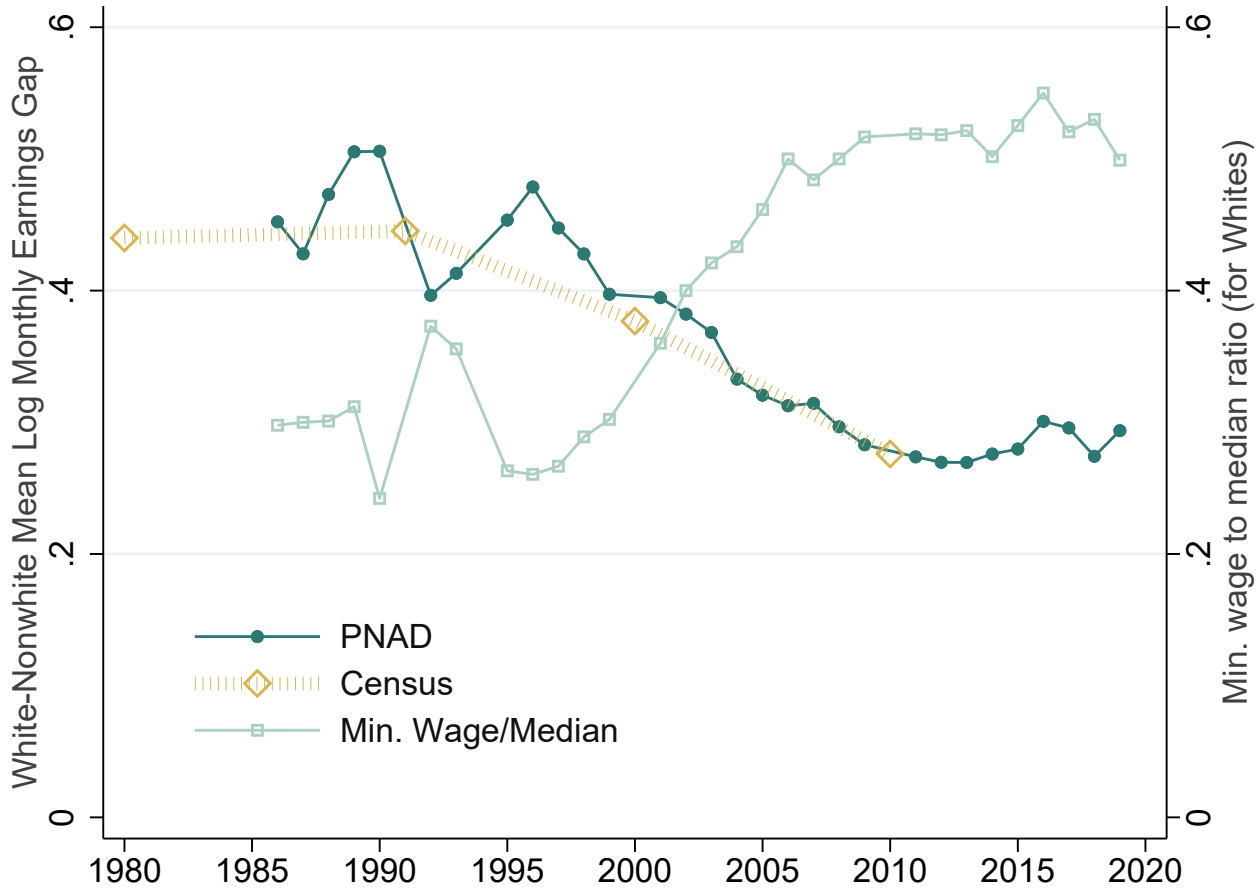
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Woo-Mora, L. G. (2021). Unveiling the cosmic race: Racial inequalities in latin america. *Working Paper*.

Figure 1: Unadjusted racial monthly earnings gaps and evolution of the minimum wage (1980-2020)

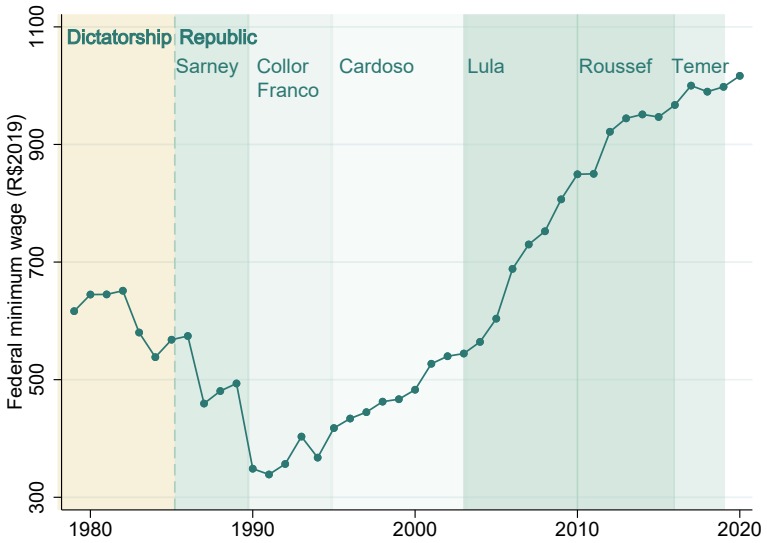


Source: PNAD 1986-2015. Censuses 1980-2010.

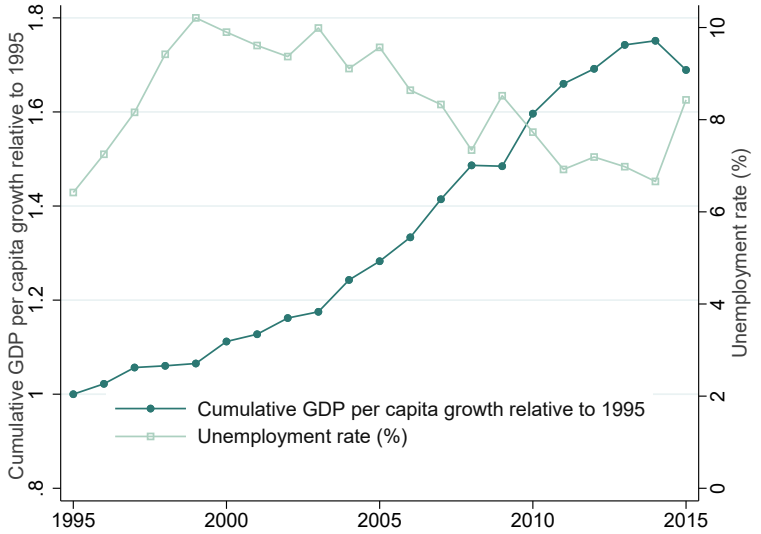
Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

Figure 2: Political and macroeconomic context of minimum wage increases (1980-2020), Brazil

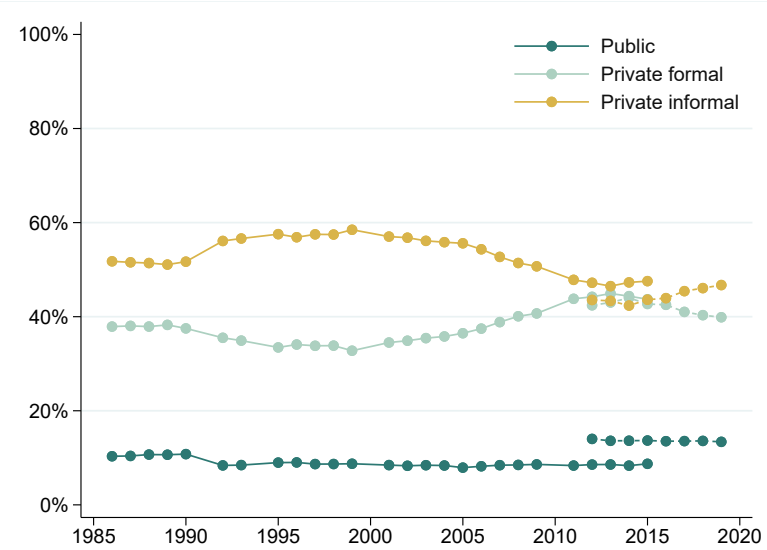
(a) Federal minimum wage and Presidents



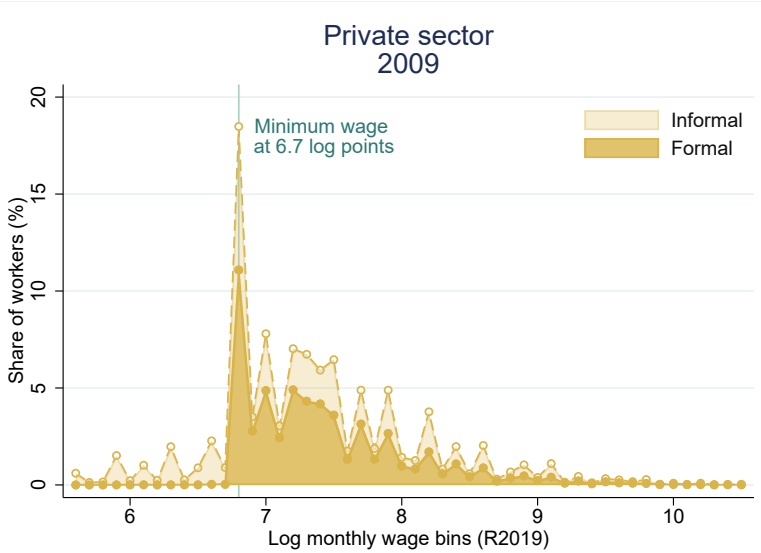
(b) GDP growth and unemployment rates



(c) Employment shares by sector

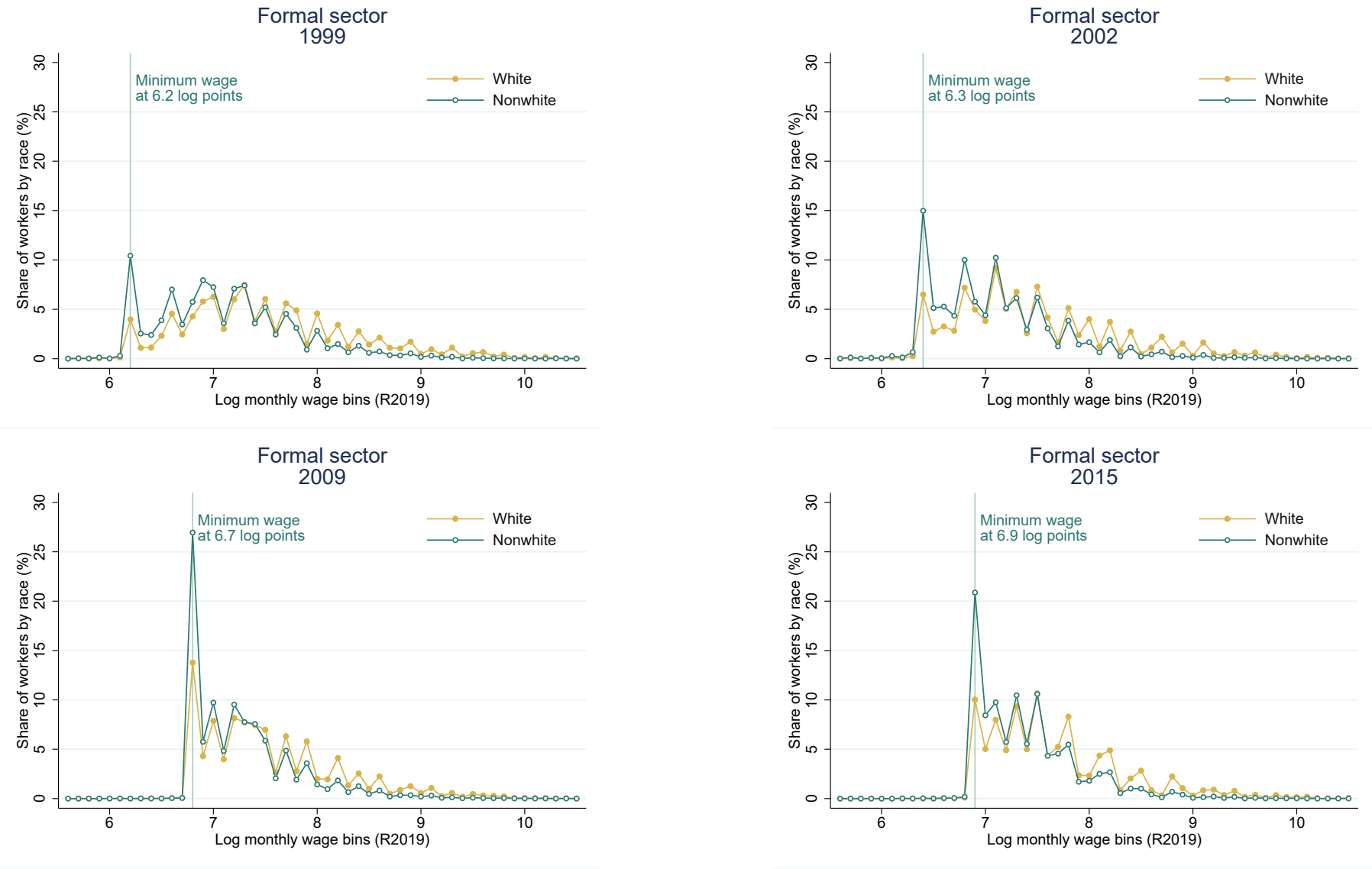


(d) Monthly earnings distribution, by sector



Panel (a). Source: Authors' minimum wage database using federal legislation only (see Appendix C). Notes: Real values of the minimum wage in constant R$2019 (inpc deflator). **Panel (b)**. Source: World Bank. Notes: GDP per capita normalized to 1 in 1995. **Panels (c) & (d)**. Sources: PNAD 1999-2015 and PNAD Continua (2012-2019). The complete series of monthly earnings distributions in the formal and informal sectors is available in our slides here. Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal sector only), working full-time.

Figure 3: Monthly earnings distributions for white and nonwhite workers, Brazil

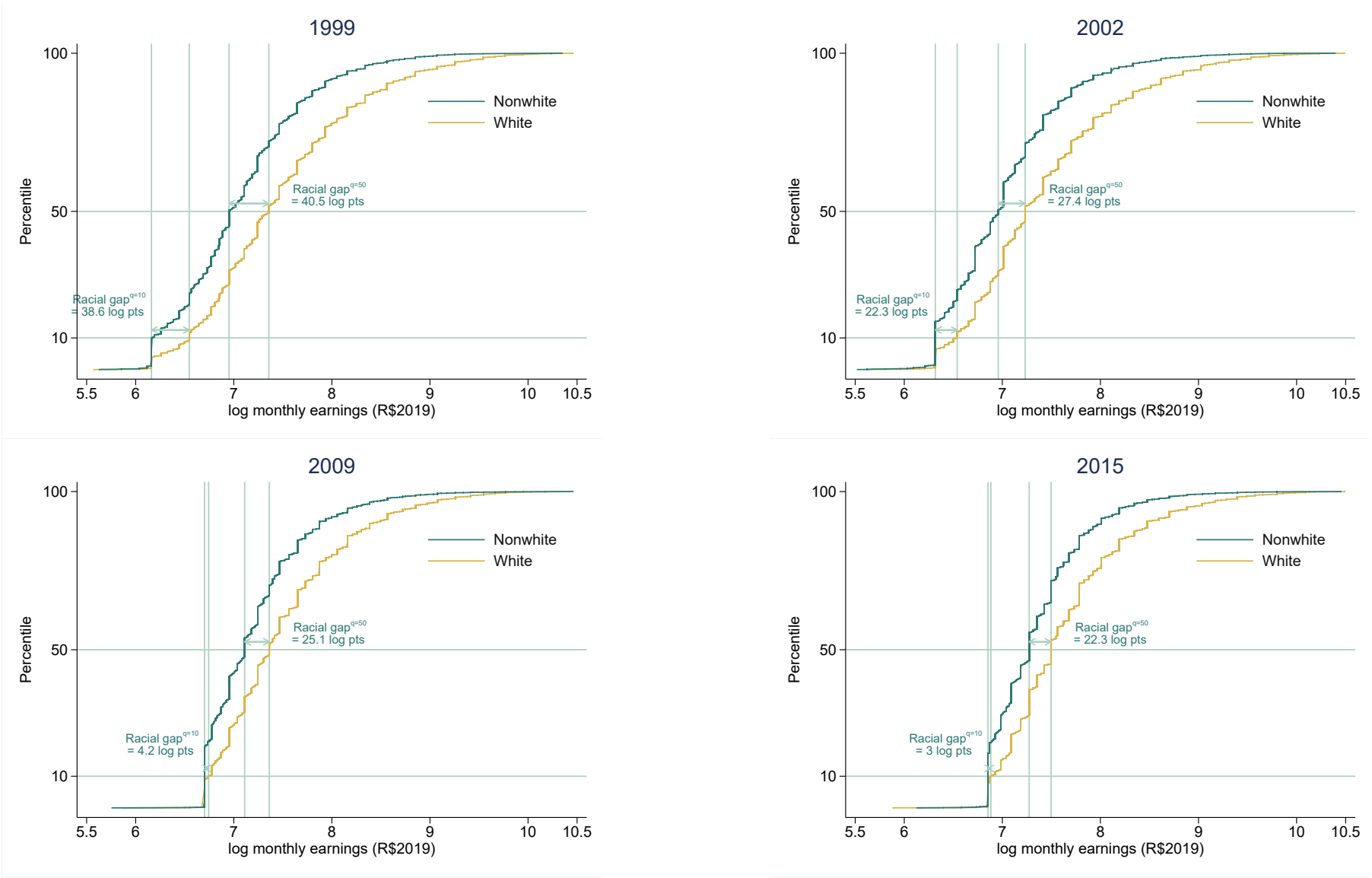


Sources: PNAD 1999-2015.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

Notes: The complete series of monthly earnings distributions from 1995 to 2015 with both the share and absolute number of workers in the y-axis is available in our slides here.

Figure 4: Cumulative distribution functions and racial earnings level gaps at 10th and 50th percentiles



Sources: PNAD 1999-2015.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

Notes: The complete series of cumulative distribution functions from 1995 to 2015 is available in our slides here.

Figure 5: Evolution of the racial earnings level gaps at different percentiles

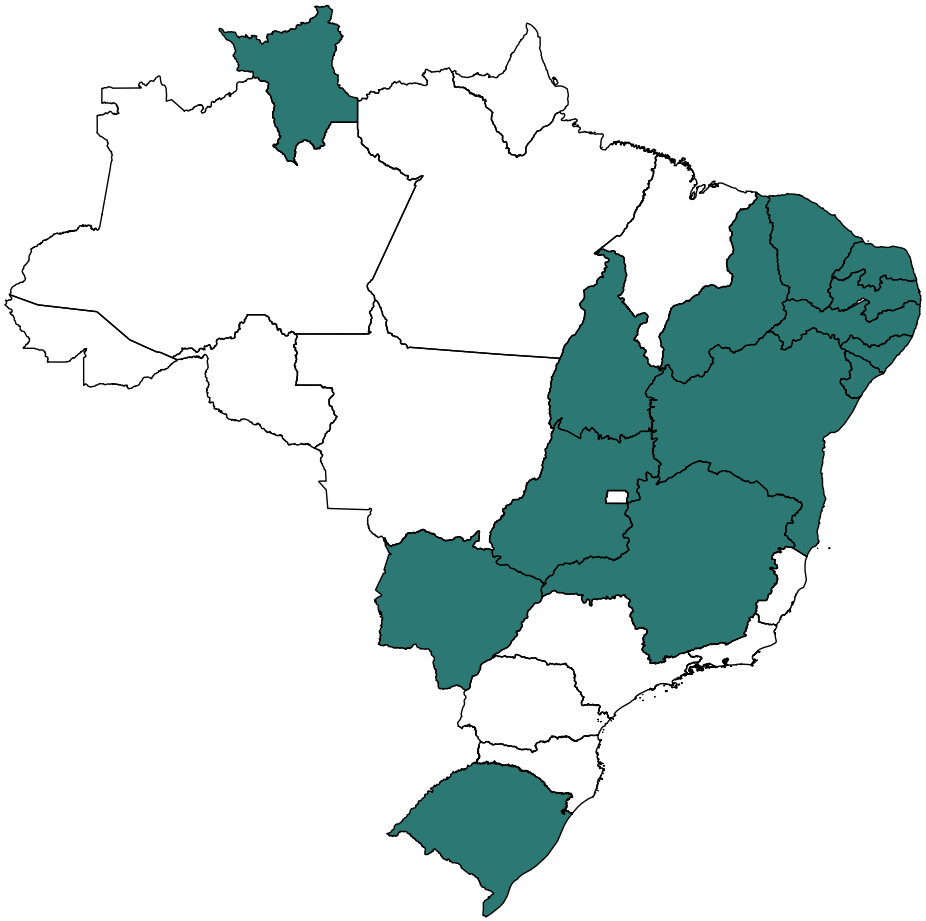


Source: PNAD 1995-2015.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

Notes: Median monthly earnings in R$2019, deflated using the inpc series.

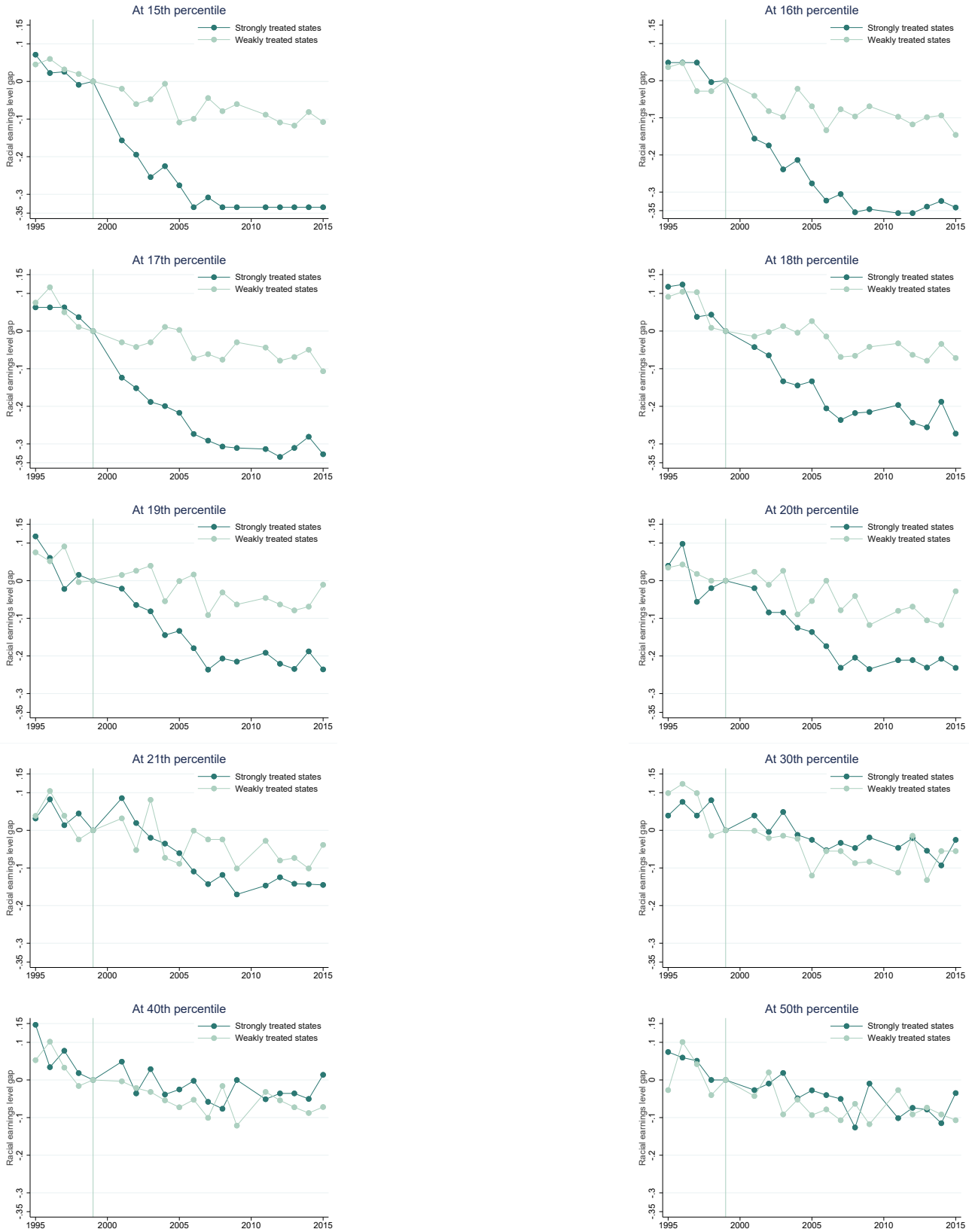
Figure 6: States with a minimum-to-median wage above the median in 1999



Source: PNAD 1995-2015.

Note: The strongly treated states are those with a minimum-to-median wage for white workers in 1999 that is >=36.3% in 1999. The minimum-to-median wage is calculated among White adults 25-54 employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), with no missing monthly earnings variable. There are 14 strongly treated states (and 13 weakly treated states). They are the following ones: Alagoas, Bahia, Ceará, Goiás, Minas Gerais, MatoGrosso do Sul, Paraíba, Pernambuco, Piauí, Rio Grande do Norte, Roraima, Rio Grande do Sul, Sergipe, Tocantins. A detailed table showing the exact values of the median wages for white workers, and the minimum-to-median wage is available in Appendix Table D1.

Figure 7: Earnings level gaps in strongly vs. weakly treated states

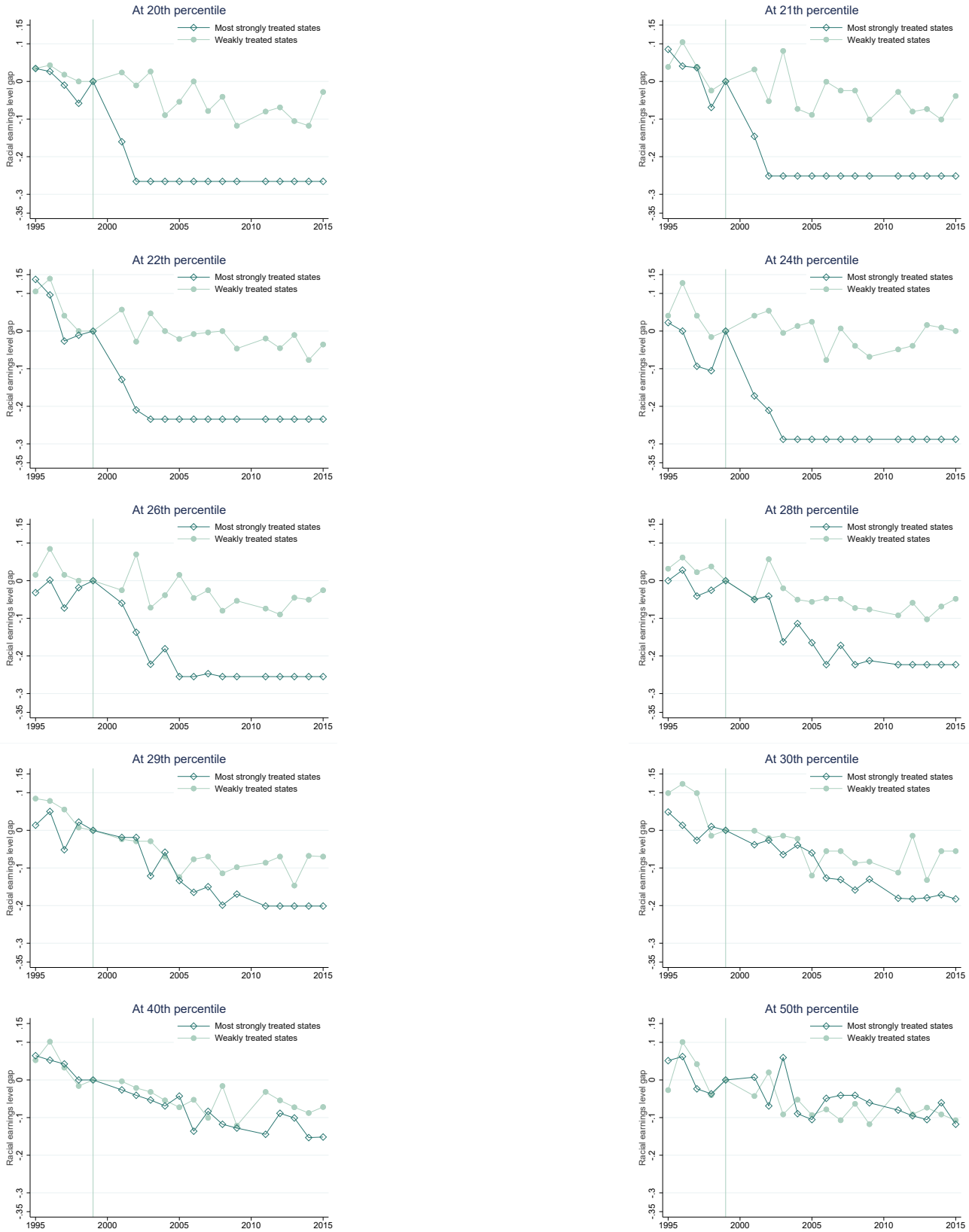


Sources: PNAD 1999-2015.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

Notes: The complete series of earnings level gaps at each percentile is available in our slides here.

Figure 8: Earnings level gaps in most strongly vs. weakly treated states



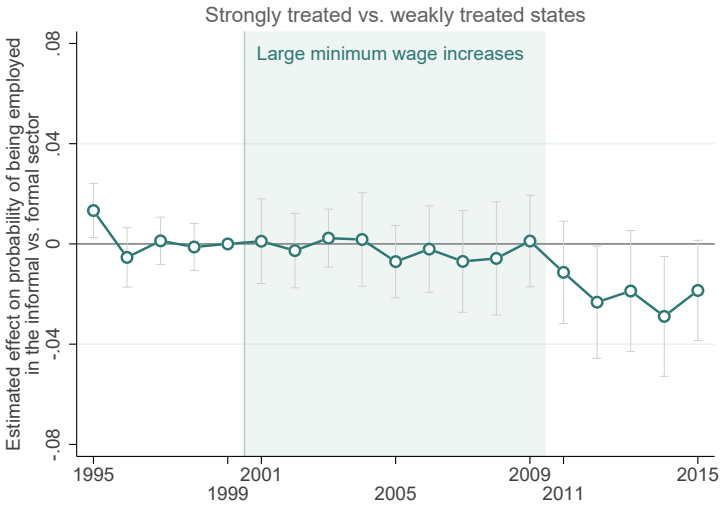
Sources: PNAD 1999-2015.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

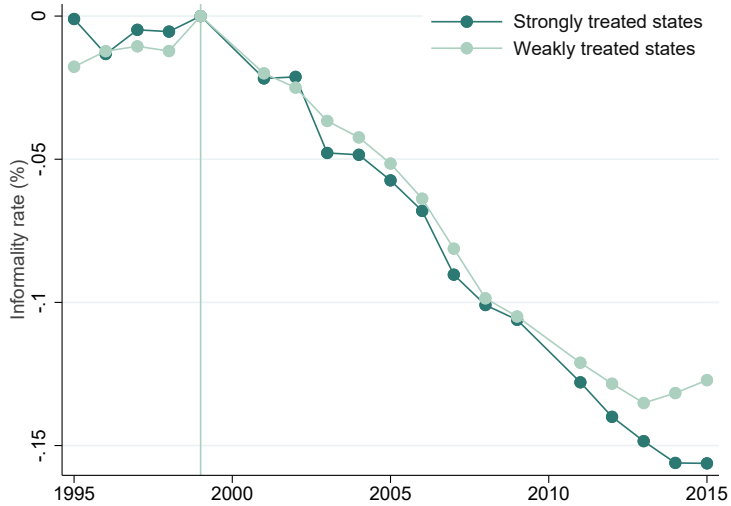
Notes: The complete series of earnings level gaps at each percentile is available in our slides here.

Figure 9: Reallocation from informal to formal employment

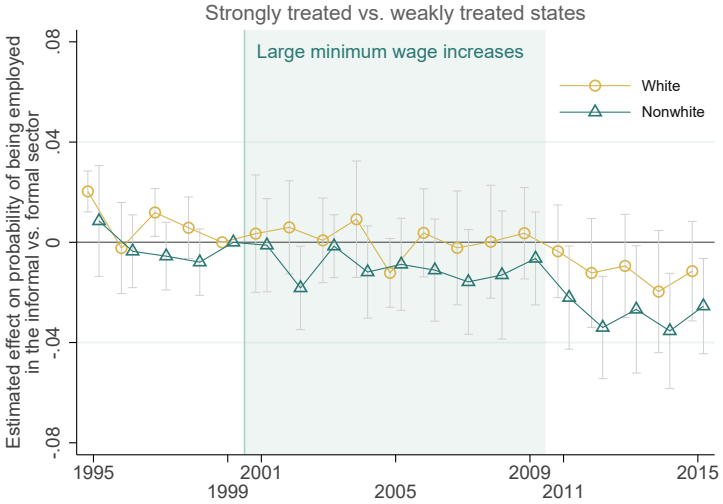
(a) In first differences, with controls



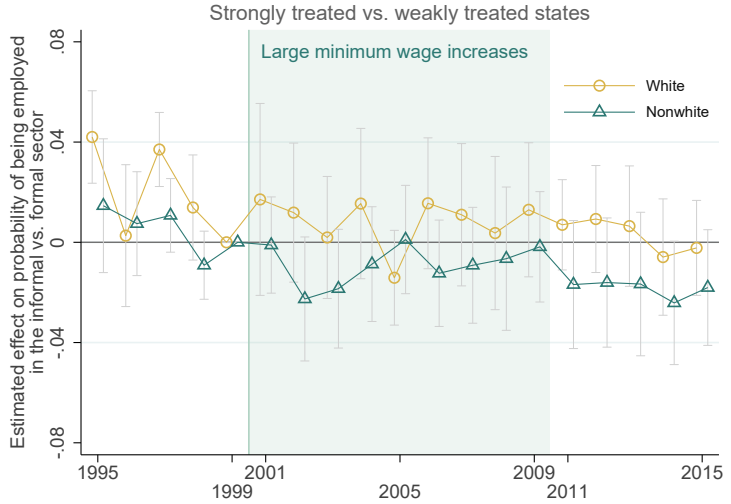
(b) In levels relative to 1999, no controls



(c) Among white and nonwhite workers



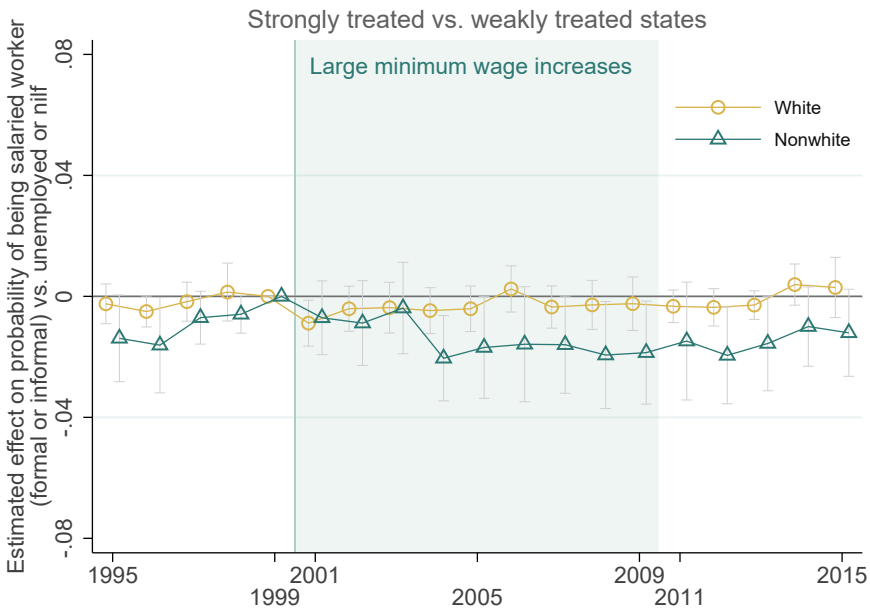
(d) Among low-wage workers



Sources: PNAD 1995-2015. Sample: Panels (a) to (c) are composed of adults 25-54, white or nonwhite, employed in the private sector (formal or informal), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable. Panel (d) further restrict the sample to low-wage workers, i.e. those paid below 130% of the minimum wage. Notes: Panels (a), (c) & (d) regressions use a cross-state design and controls for gender, years of schooling, work experience and its square, log gdp per capita by state and its square. They include state and time fixed effects. Standard errors are clustered at the state level. Panel (b) does not include any controls.

Figure 10: Impact of the 2000-2009 minimum wage increases on employment

(a) Impact on probability of being employed (vs. unemployed)



(b) Impact on probability of being employed (vs. unemployed or not in the labor force)



Sources: PNAD 1995-2015.

Sample: Panel (a) is composed of adults 25-54, white or nonwhite, employed in the private sector (formal or informal), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable, as well as unemployed adults 25-54. Panel (b) is composed of adults in panel (a) as well as adults 25-54 not in the labor force. Notes: Panels (a) & (b) regressions use a cross-state design and controls for gender, years of schooling, log gdp per capita by state and its square. They include state and time fixed effects. Standard errors are clustered at the state level.

Table 1: Workers' characteristics, 1999 (PNAD)

	Private formal			Private informal		
	All	White	Nonwhite	All	White	Nonwhite
Monthly earnings (in R$2019)	2,036	2,374	1,488	1,448	1,918	899.51
Age	36.3	36.3	36.3	38.2	38.5	37.9
Work experience	22.6	21.9	23.8	26.7	25.8	27.7
Union membership	0.29	0.30	0.27	0.10	0.11	0.08
Gender						
Male	0.63	0.61	0.65	0.58	0.58	0.57
Female	0.37	0.39	0.35	0.42	0.42	0.43
Education						
Less than high school	0.64	0.57	0.74	0.80	0.72	0.89
High school completed	0.26	0.28	0.21	0.14	0.19	0.09
College completed	0.10	0.14	0.04	0.06	0.10	0.02
Region						
North	0.03	0.01	0.05	0.05	0.02	0.08
Northeast	0.16	0.09	0.28	0.32	0.17	0.49
Southeast	0.56	0.59	0.51	0.39	0.47	0.29
South	0.19	0.26	0.08	0.17	0.27	0.05
Midwest	0.06	0.05	0.08	0.08	0.06	0.09
Full-time/part-time status						
Full-time	0.89	0.89	0.90	0.68	0.70	0.65
Part-time	0.11	0.11	0.10	0.32	0.30	0.35
Firm size						
Small firm (9 workers or less)	0.22	0.22	0.22	0.64	0.62	0.66
Large firm (10+ workers)	0.78	0.78	0.78	0.36	0.38	0.34
Missing	0.22	0.20	0.26	0.85	0.85	0.86
Industry						
Agriculture	0.06	0.05	0.08	0.29	0.24	0.34
Manufacturing	0.24	0.26	0.21	0.07	0.08	0.06
Construction	0.05	0.04	0.07	0.10	0.08	0.11
Extractive industries	0.03	0.02	0.03	0.00	0.00	0.01
Trade	0.13	0.14	0.12	0.15	0.17	0.13
Hotels, restaurants, repair services	0.22	0.20	0.25	0.28	0.30	0.27
Transportation and communication	0.07	0.07	0.08	0.04	0.04	0.03
Education and health	0.13	0.14	0.12	0.05	0.06	0.04
Public administration	0.03	0.03	0.03	0.01	0.01	0.01
Finance, insurance, real estate	0.03	0.05	0.02	0.01	0.01	0.01
Domestic services	0.00	0.00	0.00	0.00	0.00	0.00
Employment category						
Unregistered employee				0.47	0.43	0.53
Self-employed				0.44	0.45	0.43
Registered				0.08	0.11	0.04
Unregistered				0.36	0.33	0.40
Employer				0.09	0.12	0.04

Source: PNAD 1999.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (either formal or informal).

Notes: Average monthly earnings in R$2019 winsorized at 1% and 99% levels, deflated using the inpc series.

Table 2: Workers' characteristics, 2015 (PNAD)

	Private formal			Private informal		
	All	White	Nonwhite	All	White	Nonwhite
Monthly earnings (in R$2019)	2,215	2,632	1,807	1,791	2,433	1,333
Age	37.5	37.6	37.5	39.6	40.0	39.3
Work experience	21.5	20.8	22.2	25.6	24.7	26.1
Union membership	0.26	0.27	0.25	0.13	0.12	0.13
Gender						
Male	0.57	0.54	0.61	0.57	0.57	0.57
Female	0.43	0.46	0.39	0.43	0.43	0.43
Education						
Less than high school	0.36	0.29	0.44	0.57	0.46	0.66
High school completed	0.47	0.47	0.47	0.31	0.35	0.28
College completed	0.17	0.24	0.10	0.12	0.19	0.06
Region						
North	0.05	0.02	0.07	0.10	0.05	0.14
Northeast	0.17	0.09	0.25	0.33	0.19	0.42
Southeast	0.52	0.55	0.49	0.36	0.44	0.30
South	0.18	0.27	0.08	0.13	0.25	0.05
Midwest	0.08	0.06	0.10	0.08	0.07	0.08
Full-time/part-time status						
Full-time	0.89	0.89	0.89	0.62	0.67	0.59
Part-time	0.11	0.11	0.11	0.38	0.33	0.41
Firm size						
Small firm (9 workers or less)	0.24	0.24	0.24	0.70	0.66	0.73
Large firm (10+ workers)	0.76	0.76	0.76	0.30	0.34	0.27
Missing	0.15	0.13	0.17	0.84	0.85	0.84
Working in formal firms						
Yes	0.85	0.87	0.84	0.26	0.36	0.19
No	0.01	0.01	0.02	0.47	0.42	0.51
Missing	0.13	0.12	0.15	0.27	0.22	0.30
Industry						
Agriculture	0.04	0.03	0.06	0.21	0.16	0.24
Manufacturing	0.19	0.21	0.17	0.08	0.09	0.07
Construction	0.07	0.05	0.08	0.13	0.11	0.15
Extractive industries	0.01	0.01	0.01	0.00	0.00	0.00
Trade	0.20	0.21	0.20	0.18	0.19	0.16
Hotels, restaurants, repair services	0.05	0.04	0.05	0.06	0.06	0.05
Transportation and communication	0.08	0.08	0.08	0.05	0.05	0.05
Education and health	0.11	0.13	0.10	0.06	0.08	0.05
Public administration	0.02	0.02	0.02	0.02	0.02	0.02
Finance, insurance, real estate	0.14	0.15	0.13	0.06	0.09	0.03
Domestic services	0.06	0.04	0.07	0.10	0.08	0.11
Missing	0.08	0.07	0.10	0.16	0.15	0.17
Employment category						
Unregistered employee				0.44	0.38	0.48
Self-employed				0.48	0.49	0.47
Registered				0.14	0.21	0.10
Unregistered				0.33	0.28	0.37
Employer				0.08	0.13	0.05

Source: PNAD 2015.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (either formal or informal).

Notes: Average monthly earnings in R$2019 winsorized at 1% and 99% levels, deflated using the inpc series.

Table 3: Workers' characteristics, 2015 (PNAD continua)

	Private formal			Private informal		
	All	White	nonwhite	All	White	nonwhite
Monthly earnings (in R$2019)	2,188	2,620	1,756	1,896	2,619	1,333
Age	37.2	37.3	37.2	39.8	40.4	39.4
Work experience	21.3	20.6	22.0	25.8	25.0	26.3
Gender						
Male	0.57	0.54	0.60	0.58	0.57	0.59
Female	0.43	0.46	0.40	0.42	0.43	0.41
Education						
Less than high school	0.37	0.30	0.45	0.58	0.46	0.68
High school completed	0.46	0.46	0.46	0.31	0.35	0.27
College completed	0.16	0.24	0.09	0.11	0.19	0.04
Region						
North	0.05	0.02	0.08	0.10	0.04	0.15
Northeast	0.18	0.09	0.26	0.30	0.16	0.42
Southeast	0.52	0.56	0.48	0.38	0.47	0.31
South	0.17	0.28	0.07	0.14	0.27	0.05
Midwest	0.08	0.06	0.10	0.08	0.07	0.08
Full-time/part-time status						
Full-time	0.89	0.89	0.89	0.64	0.69	0.60
Part-time	0.11	0.11	0.11	0.36	0.31	0.40
Firm size						
Small firm (9 workers or less)	0.23	0.23	0.23	0.90	0.89	0.92
Large firm (10+ workers)	0.77	0.77	0.77	0.10	0.11	0.08
Missing	0.06	0.04	0.07	0.12	0.10	0.14
Industry						
Agriculture	0.04	0.03	0.05	0.17	0.14	0.19
Manufacturing	0.23	0.25	0.20	0.09	0.09	0.08
Construction	0.05	0.04	0.07	0.13	0.10	0.15
Extractive industries	0.00	0.00	0.00	0.00	0.00	0.00
Trade	0.21	0.22	0.21	0.20	0.22	0.19
Hotels, restaurants, repair services	0.13	0.11	0.15	0.23	0.21	0.25
Transportation and communication	0.06	0.05	0.06	0.05	0.05	0.05
Education and health	0.09	0.10	0.08	0.03	0.05	0.02
Public administration	0.00	0.00	0.00	0.00	0.00	0.00
Finance, insurance, real estate	0.18	0.19	0.17	0.08	0.12	0.05
Domestic services	0.00	0.00	0.00	0.00	0.00	0.00
Missing	0.00	0.00	0.00	0.00	0.00	0.00
Employment category						
Unregistered employee				0.37	0.32	0.41
Self-employed				0.53	0.53	0.53
Registered				0.16	0.23	0.11
Unregistered				0.37	0.30	0.42
Employer				0.10	0.16	0.06

Source: PNAD contiua 2015, third trimester.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (either formal or informal).

Notes: Average monthly earnings in R$2019 winsorized at 1% and 99% levels, deflated using the inpc series.

Table 4: Main reallocation effects of 2000-2009 minimum wage increases using a cross-state design

	From informal sector to formal sector			From employment to unemployment			From employment to unemployment/nilf		
	All	Nonwhite	White	All	Nonwhite	White	All	Nonwhite	White
Strongly treated state × 2001-2009									
Reallocation effect	0.004	0.009	0.005	-0.005	-0.006	-0.002	-0.009	-0.004	-0.010
	(0.008)	(0.011)	(0.007)	(0.004)	(0.006)	(0.003)	(0.007)	(0.011)	(0.006)
	1,950,192	1,011,574	938,618	2,092,994	1,095,336	997,658	2,662,337	1,397,119	1,265,218
Monthly earnings	0.119***	0.103***	0.118***	0.119***	0.103***	0.118***	0.119***	0.103***	0.118***
	(0.030)	(0.034)	(0.028)	(0.030)	(0.034)	(0.028)	(0.030)	(0.034)	(0.028)
	791,356	381,719	409,637	791,356	381,719	409,637	791,356	381,719	409,637
Elasticity	0.08	0.23	0.10	-0.04	-0.07	-0.02	-0.10	-0.05	-0.11
se	(0.18)	(0.32)	(0.14)	(0.03)	(0.04)	(0.03)	(0.08)	(0.14)	(0.06)
lower bound	-0.27	-0.40	-0.18	-0.09	-0.15	-0.07	-0.26	-0.33	-0.23
upper bound	0.44	0.85	0.38	0.01	0.02	0.03	0.05	0.22	0.01

Source: PNAD 1995-2015.

Sample: For regression on probability of being employed vs. unemployed: Adults 25-55, nonwhite or white, employed or unemployed. For regression on log annual earnings: Adults 25-55, nonwhite or white, working full-time, employed in the private formal sector.

Notes: The treatment variable strongly treated state vs. weakly treated state is calculated pre-reform, in 1999. The monthly earnings effect is calculated among private formal workers only, since the minimum wage only formally appplies to this sector. Controls for employment regressions are gender, race, years of schooling, age, age square and marital status. Controls for earnings regression are gender, race, years of schooling, a cubic in experience, gdp per capita and square of gdp per capita. Standard errors are clustered at the state level.

Table 5: Main reallocation effects from private formal employment to other employment statuses using a cross-state design

	From private formal to salaried informal			From low-wage private formal to low-wage salaried informal			From private formal to unemployment		
	All	Nonwhite	White	All	Nonwhite	White	All	Nonwhite	White
Strongly treated state × 2001-2009									
Reallocation effect	0.010	0.019	0.003	0.007	0.018	-0.002	-0.012	-0.018	-0.003
	(0.010)	(0.013)	(0.011)	(0.011)	(0.014)	(0.012)	(0.009)	(0.012)	(0.006)
	1,173,807	609,259	564,548	1,096,346	591,646	504,700	947,608	471,085	476,523
Monthly earnings	0.114***	0.096**	0.116***	0.058***	0.062**	0.059***	0.114***	0.096**	0.116***
	(0.031)	(0.036)	(0.029)	(0.020)	(0.027)	(0.017)	(0.031)	(0.036)	(0.029)
	706,028	342,185	363,843	650,645	330,275	320,370	706,028	342,185	363,843
Elast.	0.13	0.32	0.03	0.20	0.48	-0.05	-0.12	-0.21	-0.04
se	0.15	0.26	0.13	0.33	0.45	0.29	0.08	0.11	0.06
lower bound	-0.16	-0.19	-0.23	-0.45	-0.40	-0.62	-0.28	-0.43	-0.16
upper bound	0.42	0.83	0.29	0.84	1.36	0.52	0.03	0.00	0.09

Source: PNAD 1995-2015.

Sample: For regression on probability of being employed vs. unemployed: Adults 25-55, nonwhite or white, employed or unemployed. For regression on log annual earnings: Adults 25-55, nonwhite or white, working full-time, employed in the private formal sector.

Notes: The treatment variable strongly treated state vs. weakly treated state is calculated pre-reform, in 1999. The monthly earnings effect is calculated among private formal workers only, since the minimum wage only formally appplies to this sector. Controls for employment regressions are gender, race, years of schooling, age, age square and marital status. Controls for earnings regression are gender, race, years of schooling, a cubic in experience, gdp per capita and square of gdp per capita. Standard errors are clustered at the state level.

Table 6: Main reallocation effects from private formal employment to other employment statuses using a cross-state design

	From private formal to unemployment/nilf			From low-wage private formal to unemployment/nilf/salaried informal			From private formal to unemployment/nilf/informal		
	All	Nonwhite	White	All	Nonwhite	White	All	Nonwhite	White
Strongly treated state × 2001-2009									
Reallocation effect	-0.010	-0.007	-0.005	-0.004	0.002	-0.004	-0.003	0.001	-0.002
	(0.008)	(0.008)	(0.007)	(0.008)	(0.009)	(0.007)	(0.007)	(0.008)	(0.006)
	1,516,951	772,868	744,083	2,065,505	1,099,591	965,914	2,662,337	1,397,119	1,265,218
Monthly earnings	0.114***	0.096**	0.116***	0.114***	0.096**	0.116***	0.114***	0.096**	0.116***
	(0.031)	(0.036)	(0.029)	(0.031)	(0.036)	(0.029)	(0.031)	(0.036)	(0.029)
	706,028	342,185	363,843	706,028	342,185	363,843	706,028	342,185	363,843
Elast.	-0.16	-0.13	-0.09	-0.09	0.06	-0.09	-0.09	0.02	-0.06
se	0.12	0.14	0.11	0.17	0.22	0.17	0.20	0.27	0.19
lower bound	-0.39	-0.40	-0.31	-0.43	-0.38	-0.42	-0.48	-0.50	-0.44
upper bound	0.07	0.15	0.13	0.24	0.50	0.24	0.29	0.55	0.32

Source: PNAD 1995-2015.

Sample: For regression on probability of being employed vs. unemployed: Adults 25-55, nonwhite or white, employed or unemployed. For regression on log annual earnings: Adults 25-55, nonwhite or white, working full-time, employed in the private formal sector.

Notes: The treatment variable strongly treated state vs. weakly treated state is calculated pre-reform, in 1999. The monthly earnings effect is calculated among private formal workers only, since the minimum wage only formally appplies to this sector. Controls for employment regressions are gender, race, years of schooling, age, age square and marital status. Controls for earnings regression are gender, race, years of schooling, a cubic in experience, gdp per capita and square of gdp per capita. Standard errors are clustered at the state level.

Table 7: Main reallocation effects from private salaried employment (formal or informal) to other employment statuses using a cross-state design

	From private salaried to unemployment			From low-wage private formal to unemployment/nilf			From private formal to unemployment/nilf/non-salaried informal		
	All	Nonwhite	White	All	Nonwhite	White	All	Nonwhite	White
Strongly treated state × 2001-2009									
Reallocation effect	-0.005	-0.006	-0.002	-0.009	-0.004	-0.010	-0.013*	-0.018**	-0.006
	(0.004)	(0.006)	(0.003)	(0.007)	(0.011)	(0.006)	(0.007)	(0.008)	(0.008)
	2,092,994	1,095,336	997,658	2,662,337	1,397,119	1,265,218	2,662,337	1,397,119	1,265,218
Monthly earnings	0.114***	0.096**	0.116***	0.114***	0.096**	0.116***	0.114***	0.096**	0.116***
	(0.031)	(0.036)	(0.029)	(0.031)	(0.036)	(0.029)	(0.031)	(0.036)	(0.029)
	706,028	342,185	363,843	706,028	342,185	363,843	706,028	342,185	363,843
Elast.	-0.04	-0.07	-0.02	-0.11	-0.06	-0.12	-0.22	-0.36	-0.11
se	0.03	0.05	0.03	0.08	0.15	0.06	0.11	0.17	0.13
lower bound	-0.10	-0.16	-0.07	-0.27	-0.35	-0.24	-0.44	-0.69	-0.36
upper bound	0.01	0.02	0.03	0.05	0.24	0.01	-0.01	-0.04	0.15

Source: PNAD 1995-2015.

Sample: For regression on probability of being employed vs. unemployed: Adults 25-55, nonwhite or white, employed or unemployed. For regression on log annual earnings: Adults 25-55, nonwhite or white, working full-time, employed in the private formal sector.

Notes: The treatment variable strongly treated state vs. weakly treated state is calculated pre-reform, in 1999. The monthly earnings effect is calculated among private formal workers only, since the minimum wage only formally appplies to this sector. Controls for employment regressions are gender, race, years of schooling, age, age square and marital status. Controls for earnings regression are gender, race, years of schooling, a cubic in experience, gdp per capita and square of gdp per capita. Standard errors are clustered at the state level.

Appendix A Public data sources: census and labor force surveys (1960-2019)

A.1 Census (1960-2010)

The census is collected every ten years by the Brazilian institute of geography and statistics called *Instituto Brasileiro de Geografia e Estatistica* (IBGE). Microdata are available since 1960. The census has two different questionnaires. One for the universe of households and one for a representative sample. We use the sample one because it contains detailed information about labor market outcomes of individuals.

In 1960, monthly earnings are only available by bracketed categories; the representativeness of the 1960 census is also questionned. In 1970, there is no information on race. Starting in 1991 records race, labor market outcomes and demograhic characteristics over time. The questionnaires change considerably across censuses, and we use the hamonized variables provided by IPUMS International where possible. For certain variables, we use the source variables in addition to the harmonized ones to construct our dataset. (take example of education in 2010, and informality).

Access. The data were accessed through IPUMS Internationl here. Minnesota Population Center. Integrated Public Use Microdata Series, International: Version 7.2 [Census]. Minneapolis, MN: IPUMS, 2019. https://doi.org/10.18128/D020.V7.2. Full documentation available here for hamonized variables, and here for source variables.

The data of the 2000 and 2010 census were downloaded from the IBGE website (available here). The files were cleaned and prepared for analysis using the Stata ado files as available from Datazoom (full information here).

A.2 Labor force surveys

A.2.1 *Pesquisa Nacional por Amostra Domiciliar* (PNAD) (1976 - 2015)

The Pesquisa Nacional por Amostra Domiciliar is a yearly national household survey, similar to the Current Population Survey in the US. There are two different questionnaires: one about households' characteristics, one about individuals' characteristics. We only use the questionnaire containing the individual level characteristics. The sample is drawn from 1,500 municipalities.

We focus on demographic variables (such as age, gender, education attainment, position in the household, etc.) and labor market outcomes (such as employment, wages, earnings,

industry and occupations) that are collected consistently across years.[18]

The survey spans 1976 to 2015.

Over 1976-1991, the questionnaires of the survey are less detailed, and contain fewer observations (see Table A2). In particular, only the information on education attainment (as opposed to the exact number of years of schooling) is available across those years. We reconstruct a measure of the number of years of schooling for those years, in order to be able to construct a measure of potential experience in the labor market. Potential experience is measured as the age of the individual minus seven minus the average number of years of schooling needed to complete the degree the individual attained. We acknowledge that, because of this data limitation, the number of years of schooling and potential experience are imprecisely measured for those years. We do not rely on these variables in our core analysis though, as it starts in 1995.

In 1991, the survey was significantly redesigned.[19] Starting in 1992, labor market outcomes are much more detailed and harmonized across years. Over 1992- 2015, there are only minor changes in the labelling or the reporting of a selected set of variables.[20] These changes are documented in our replication files. In all years, we use the labor market information for the main job of the worker (earnings, weekly hours, etc.) – the most relevant for our study, and the only information that is collected across all years in the survey. It is only in the most recent years that the survey addded questions about the other jobs in the reference week and about previous jobs during the year. Finally, we observe monthly earnings (i.e. wages in the main job in the last month), and the nunber of weeks worked last week (dble check), but we do not directly observe the hourly wage variable. Since the minimum wage is set in terms of monthly earnings for full-time workers, we focus on full-time workers in our study, i.e. those who report working 40 hours or more during the week. Close to 90% of workers in the private formal sector are working full-time vs. 60-70% in the informal sector(see Tables 1 and 3).

The information on race is not collected across all years. In 1976, the survey collects the information on race in two different variables: one that (classically) categorize race into 5 categories; the other one that collects self-reported race after asking an open-ended question. We discard the years 1977 to 1985, as there is no information on race during those years. In 1986 the survey collects race only for a sub-sample (about 2/3) of individuals. From 1987 on,

[18] In some years, the individual-level questionnaire is complemented by other questionnaires, e.g. on health, migration, or fertility. We do not use this information in this project.

[19] The survey was not conducted in 1991. The Census survey was conducted that year instead.

[20] For example, the monthly earnings variable is labeled V4768 in 2001 whereas it is V4718 in all other years.

the question on self-reported race is asked every year.

Individuals can identify themselves to 5 different racial categories: "Branco" (White), "Preto" (Black), "Amarelo" (Asian), "Indigena" (Indigenous) and "Pardos." This latter category is the most difficult to translate: "Pardos" translates to brown or mixed race with African ancestry. It refers to individuals with Black and white parentage, or Black and indigenous parentage. Following Gerard et al. (2018), we pool "Pretos" and "Pardos" together to form the "Nonwhite" category. Nonwhite individuals account for about 40-45% of the total population. We restrict our sample to white and nonwhite individuals, effectively setting aside Amarelos and Indigenas (less than 2% of the population).

Access. Accessed through data zoom here starting in 1986.

A.2.2 PNAD Contínua (2012-2019)

The PNAD continua is a rotating panel that was created to substitute the original PNAD and the *Pesquisa mensal de emprego* (PME), another employment survey for major cities in Brazil. The data is available for every trimester, but there are actually interviews every month. A households that enters the panel is interviewed every three months for five times. So far, we are not using the Panel aspect of the PNAD Continua. We are using it as a repeated cross section of trimesters. The sample is drawn from 3,500 municipalities. It includes more rural areas than the PNAD, and this can explain some of the differences in the labor market statistics, in particular in the Northeast where a large fraction of the population lives in a rural area (see Figure A3). At the national level however, workers' characteristics in the private formal and informal sectors are very consistent, for all and across racial groups (see Tables 2 and 2)

PNAD continua combines assests of both PNAD (state representativeness as opposed to 6 metropolitan areas) and PME (rotating panel as opposed to repeated cross-section in PNAD) datasets.

Access. Accessed through data zoom here.

A.2.3 Monthly Employment Survey, *Pesquisa mensal de emprego* (PME) (1980-2016)

The Monthly Employment Survey is a sample survey conducted monthly by IBGE since 1980 in six metropolitan areas: Belo Horizonte, Porto Alegre, Recife, Rio de Janeiro, Salvador and São Paulo. The survey collects labor and income information from the population. PME

is mainly used to compute the main unemployment index in the country (until 2014). It is the only IBGE survey in longitudinal format. Households are visited for two periods of four consecutive months, eight months apart from each other. In March 2014, PME's sample consisted of 33,809 households with 95,122 individuals.

There are two versions of PME, traditionally called by PME-Antiga (old PME) and PME-Nova (new PME). The PME-Antiga is the original survey. In 2002, this survey underwent a major change in design, giving rise to the PME-Nova, with a significantly larger questionnaire and differences in the definition of labor market participation, as well as in the rotation scheme of the samples. Until the end of 2002, the two methodologies were taken to the field. In December 2002, PME-Antiga was closed down and replaced by PME-Nova.

PME is a panel survey, in which each household is interviewed 8 times over a 16-months period (the household is surveyed for 4 consecutive months, out for 8, and then returns for another 4 months of interviews). Households are correctly identified throughout all eight interviews. However, PME does not assign the same identification number to each individual in the household across interviews. To reduce attrition related to this problem, each Data Zoom package offers two identification algorithms based on Ribas e Soares (2008). The algorithms differ essentially according to the number of characteristics checked in order to identify the same individual across interviews.

PME-Nova was discontinued in February 2016 and replaced by PNAD Continuous, a quarterly survey started in the first quarter of 2012. The two surveys coexisted between 2012 and 2016. For more information on methodological differences between surveys, see Published by IBGE.

Access. Accessed through data zoom here.

A.3 Other data

A.3.1 Export data

We obtained detailed export data from the website of the Ministry of the Economy (available here). For each export transaction, the data include the year, state, NCM code, and nominal value in US$. We aggregate the data by year, state, and industry code, and express all export values in R$ 2019.

Table A1: Observations, employment, and earnings in labor force surveys and Census data

	Observations	Employment	Employment shares						Median earnings (R$2019)					
			Private formal			Private informal			Private formal			Private informal		
			All	White	Nonwhite	All	White	Nonwhite	All	White	Nonwhite	All	White	Nonwhite
PNAD														
1986	48,330	23,105,053	0.48	0.65	0.35	0.52	0.54	0.46	1,715	2,014	1,268	1,253	1,864	895
1987	50,027	23,679,941	0.49	0.64	0.36	0.51	0.56	0.44	1,618	1,849	1,294	1,109	1,386	693
1988	49,665	24,133,586	0.48	0.64	0.36	0.52	0.55	0.45	1,628	1,973	1,252	939	1,252	642
1989	50,910	24,998,143	0.48	0.63	0.37	0.52	0.55	0.45	1,499	1,713	1,070	1,070	1,499	685
1990	52,455	25,754,016	0.48	0.62	0.38	0.52	0.55	0.45	1,445	1,806	1,083	1,083	1,445	722
1992	55,275	25,506,728	0.47	0.62	0.38	0.53	0.54	0.46	1,514	1,766	1,135	946	1,261	659
1993	55,926	25,904,826	0.47	0.62	0.38	0.53	0.54	0.46	1,416	1,673	1,152	991	1,239	619
1995	60,799	27,700,090	0.45	0.62	0.38	0.55	0.55	0.45	1,392	1,764	1,160	1,114	1,624	719
1996	59,090	27,734,828	0.45	0.62	0.38	0.55	0.56	0.44	1,406	1,728	1,125	1,205	1,607	804
1997	63,676	28,855,690	0.45	0.62	0.38	0.55	0.55	0.45	1,440	1,706	1,137	1,137	1,516	758
1998	62,398	28,814,182	0.45	0.61	0.39	0.55	0.55	0.45	1,424	1,644	1,096	1,096	1,461	731
1999	64,254	29,357,266	0.44	0.62	0.38	0.56	0.55	0.45	1,393	1,568	1,045	1,045	1,393	697
2001	71,931	33,035,404	0.46	0.60	0.40	0.54	0.54	0.46	1,220	1,526	1,068	915	1,373	702
2002	73,695	34,075,030	0.46	0.60	0.40	0.54	0.53	0.47	1,246	1,384	1,052	969	1,384	692
2003	73,851	34,638,206	0.47	0.59	0.41	0.53	0.53	0.47	1,184	1,349	947	947	1,184	686
2004	78,312	36,554,787	0.48	0.58	0.42	0.52	0.52	0.48	1,158	1,337	1,002	891	1,203	668
2005	81,042	37,496,416	0.49	0.56	0.44	0.51	0.50	0.50	1,264	1,369	1,053	948	1,264	737
2006	82,575	38,336,068	0.51	0.56	0.44	0.49	0.50	0.50	1,224	1,428	1,101	1,020	1,428	775
2007	81,368	39,338,993	0.53	0.55	0.45	0.47	0.49	0.51	1,362	1,538	1,176	1,009	1,568	784
2008	82,022	40,871,398	0.54	0.53	0.47	0.46	0.48	0.52	1,379	1,526	1,195	1,103	1,526	883
2009	84,060	41,469,927	0.55	0.54	0.46	0.45	0.48	0.52	1,400	1,575	1,225	1,138	1,628	875
2011	76,203	42,657,831	0.57	0.53	0.47	0.43	0.47	0.53	1,484	1,640	1,265	1,250	1,687	1,031
2012	78,069	43,562,807	0.58	0.51	0.49	0.42	0.46	0.54	1,482	1,778	1,356	1,482	1,814	1,126
2013	77,448	43,628,324	0.59	0.51	0.49	0.41	0.46	0.54	1,672	1,811	1,393	1,393	2,089	1,114
2014	79,289	44,762,239	0.58	0.49	0.51	0.42	0.45	0.55	1,576	1,896	1,445	1,576	1,970	1,314
2015	74,922	43,406,398	0.58	0.49	0.51	0.42	0.45	0.55	1,562	1,802	1,442	1,442	1,886	1,201

Sources: PNAD 1986-2015. PNAD Continua 2012-2019, third trimester. Censuses 1980-2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (either formal or informal), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

Notes: Median monthly earnings in R$2019, deflated using the inpc series.

Table A2: Observations, employment, and earnings in labor force surveys and Census data

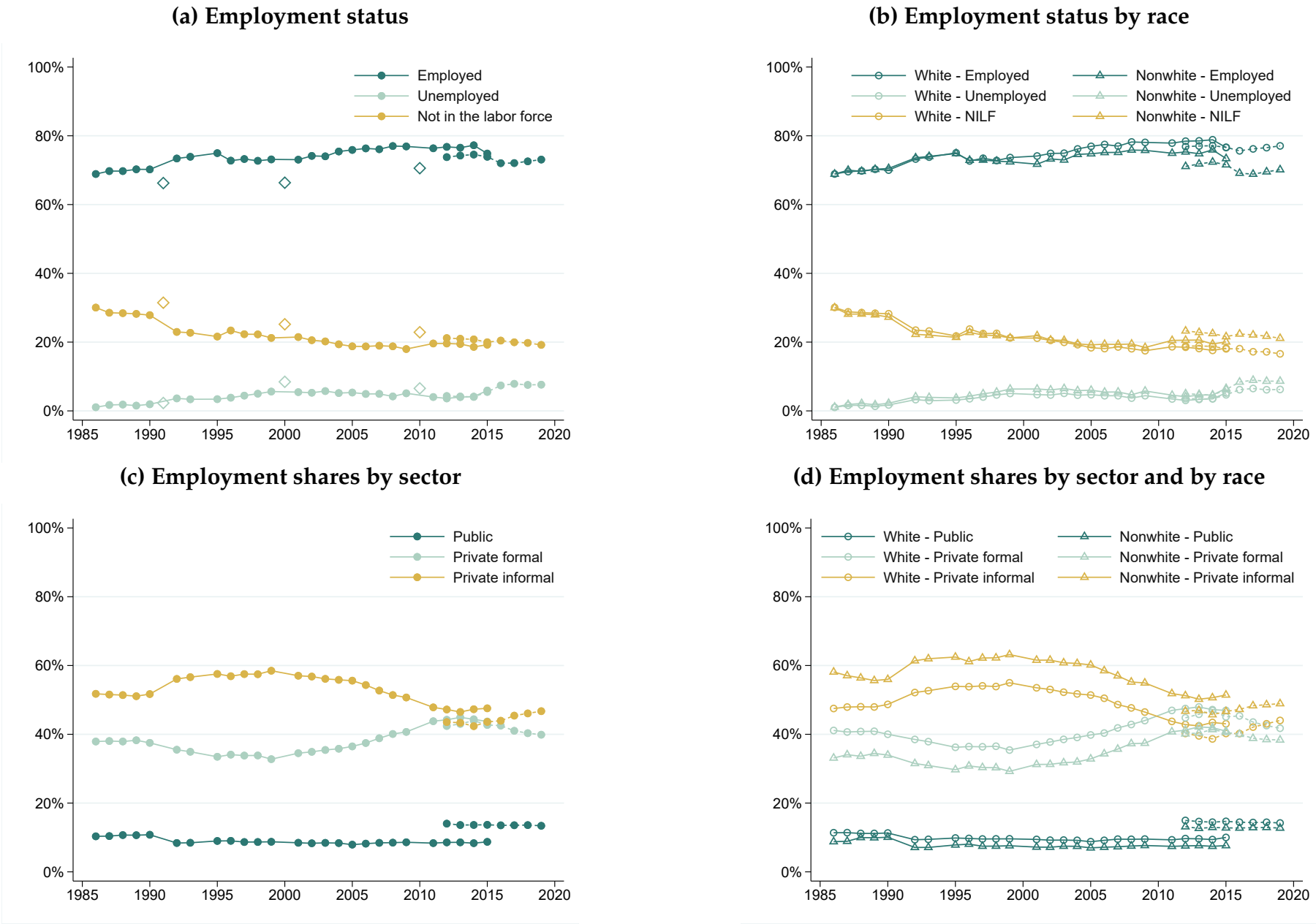
	Observations	Employment	Employment shares						Median earnings (R$2019)					
			Private formal			Private informal			Private formal			Private informal		
			All	White	Nonwhite	All	White	Nonwhite	All	White	Nonwhite	All	White	Nonwhite
PNAD Continua														
2012	104,244	39,966,811	0.59	0.52	0.48	0.14	0.39	0.61	1,496	1,796	1,347	973	1,197	931
2013	107,926	41,485,028	0.59	0.52	0.48	0.13	0.38	0.62	1,635	1,841	1,416	1,062	1,274	960
2014	109,156	42,070,644	0.60	0.50	0.50	0.12	0.37	0.63	1,594	1,860	1,461	1,063	1,328	962
2015	105,017	41,304,118	0.59	0.50	0.50	0.12	0.37	0.63	1,571	1,813	1,450	1,027	1,209	967
2016	105,425	42,363,433	0.58	0.50	0.50	0.13	0.37	0.63	1,661	1,772	1,384	997	1,329	974
2017	102,118	41,749,979	0.57	0.49	0.51	0.13	0.38	0.62	1,634	1,961	1,416	1,089	1,307	1,021
2018	100,042	41,643,264	0.57	0.48	0.52	0.13	0.38	0.62	1,572	1,886	1,467	1,048	1,257	1,006
2019	98,755	42,317,127	0.56	0.47	0.53	0.14	0.37	0.63	1,629	2,036	1,527	1,018	1,425	1,018
Census														
1980	176,640	704,886	1.00	0.63	0.37	0.00	0.63	0.37	2,399	3,531	1,284	.	3,531	1,284
1991	2,712,336	23,927,194	1.00	0.55	0.45	0.00	0.55	0.45	818	1,145	627	.	1,145	627
2000	3,678,767	31,940,130	0.79	0.59	0.41	0.21	0.59	0.41	1,312	1,640	984	984	1,640	984
2010	4,209,727	41,448,660	0.83	0.52	0.48	0.17	0.52	0.48	1,499	1,665	1,166	849	1,665	1,166

Sources: PNAD 1986-2015. PNAD Continua 2012-2019, third trimester. Censuses 1980-2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector (either formal or informal), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

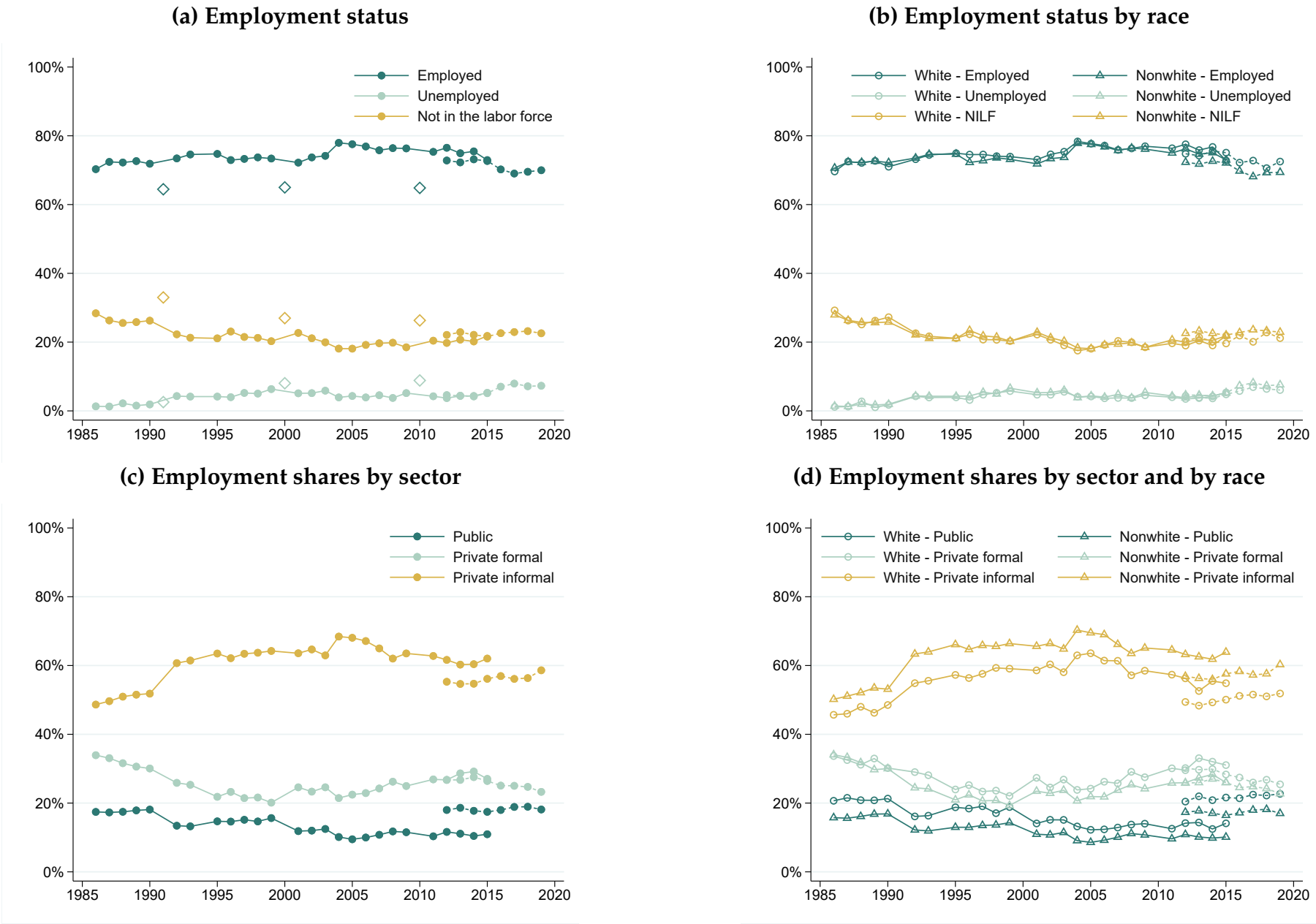
Notes: Median monthly earnings in R$2019, deflated using the inpc series.

Figure A1: Labor market statistics, Brazil



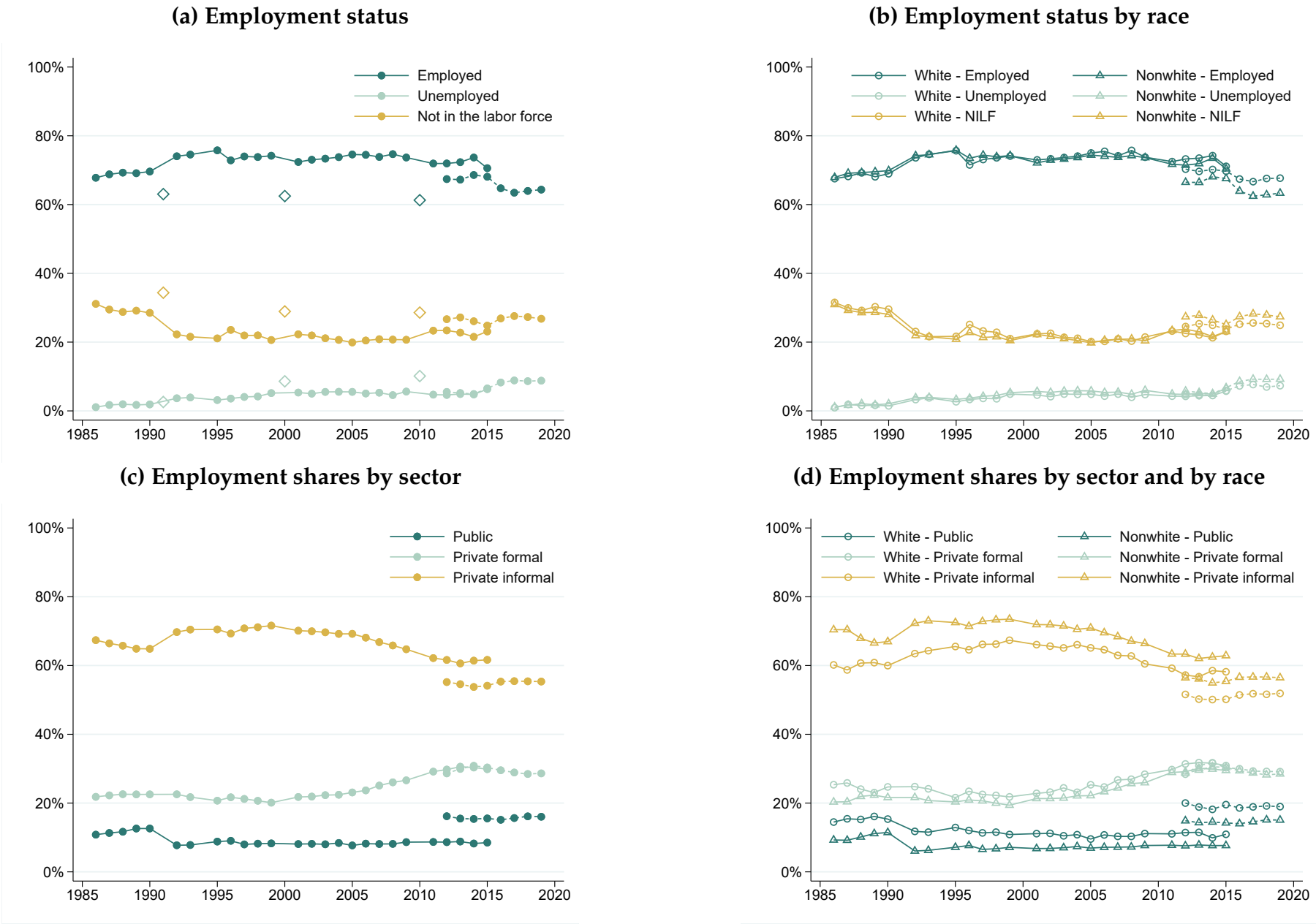
Sources: PNAD 1986-2015 (plain line). PNAD Continua 2012-2019, third trimester (dashed line). Censuses 1991-2010 (diamonds).
Sample: Adults 25-54, white or nonwhite.

Figure A2: Labor market statistics, North



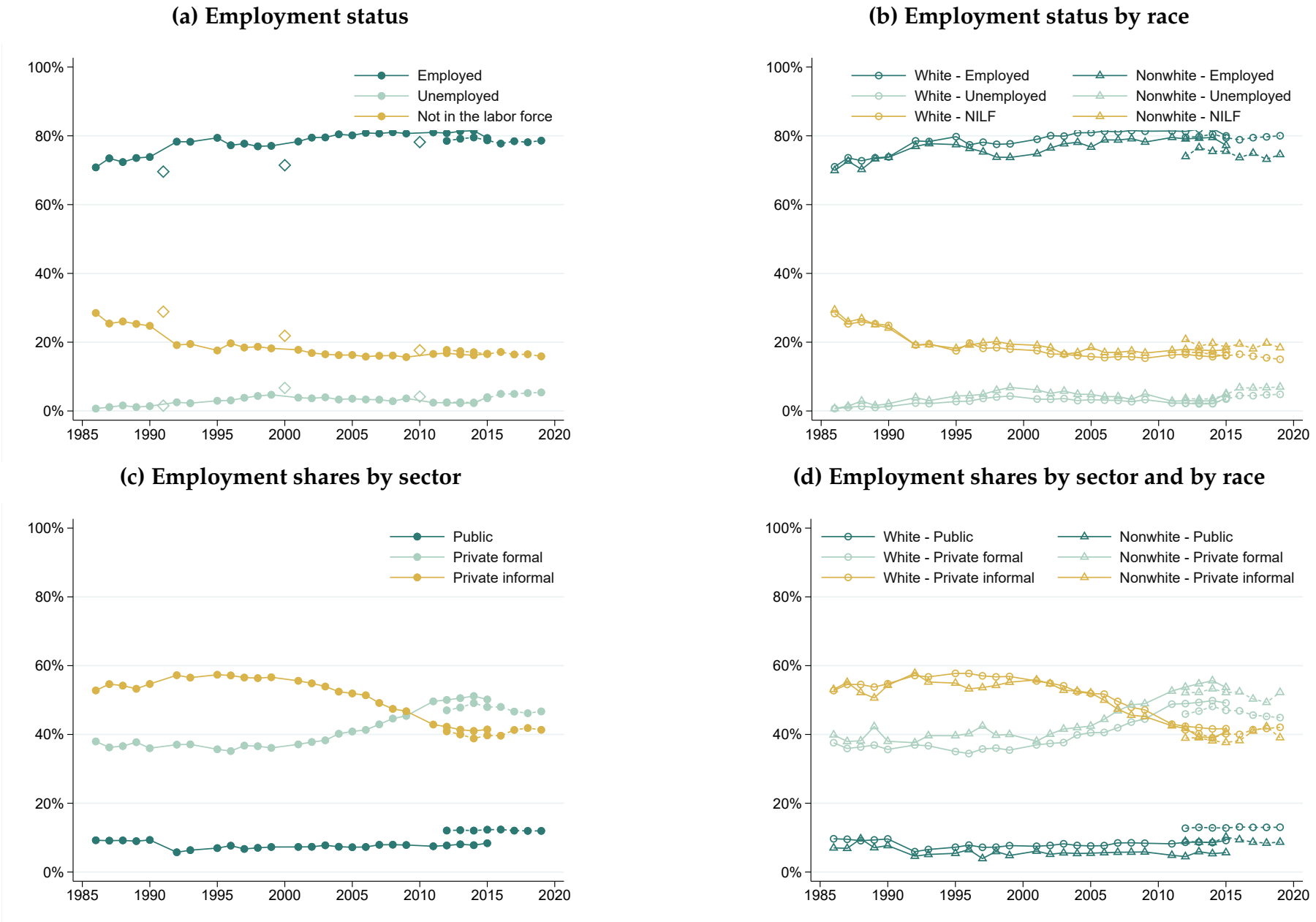
Sources: PNAD 1986-2015 (plain line). PNAD Continua 2012-2019, third trimester (dashed line). Censuses 1991-2010 (diamonds).
Sample: Adults 25-54, white or nonwhite.

Figure A3: Labor market statistics, Northeast



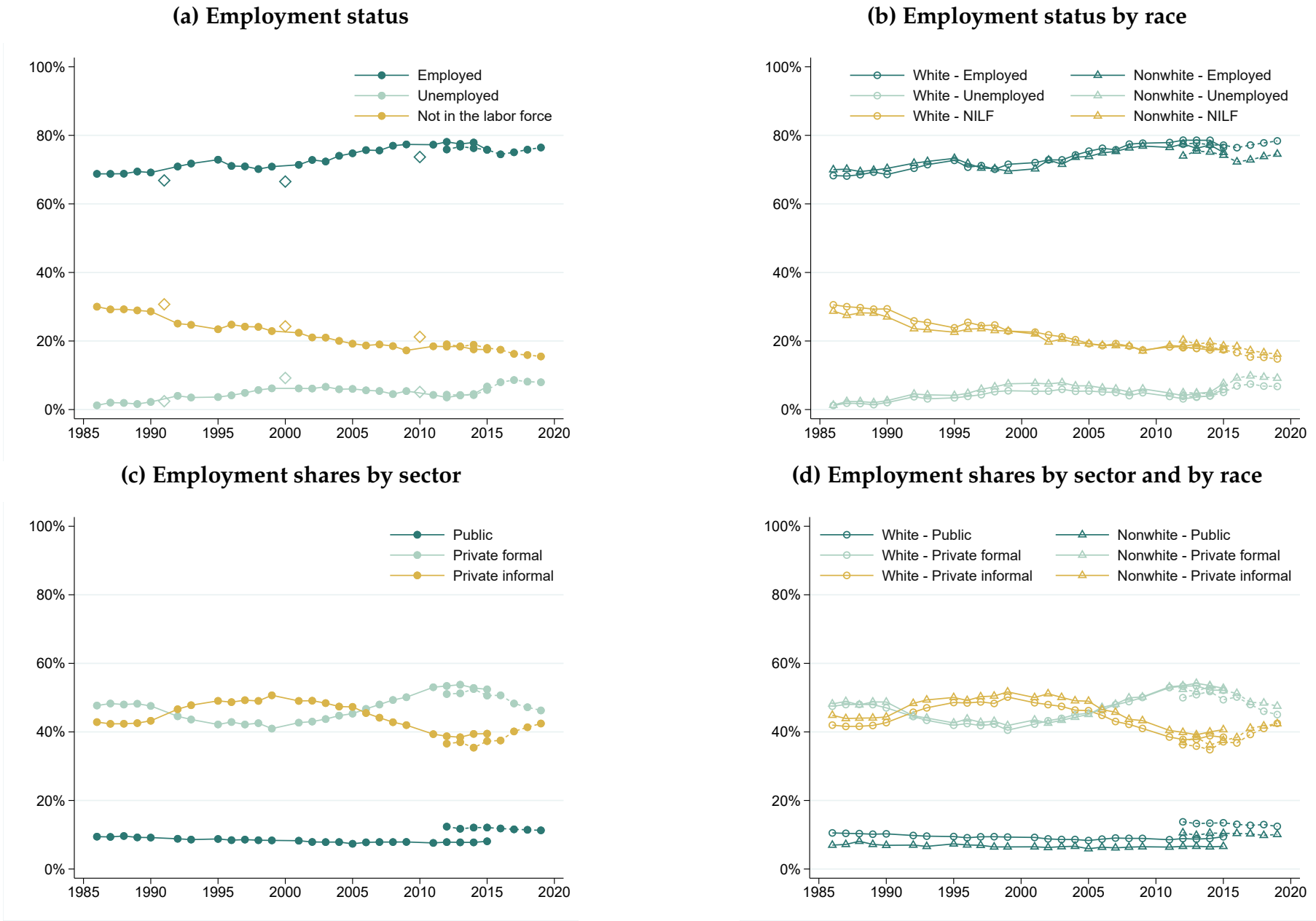
Sources: PNAD 1986-2015 (plain line). PNAD Continua 2012-2019, third trimester (dashed line). Censuses 1991-2010 (diamonds).
Sample: Adults 25-54, white or nonwhite.

Figure A4: Labor market statistics, South



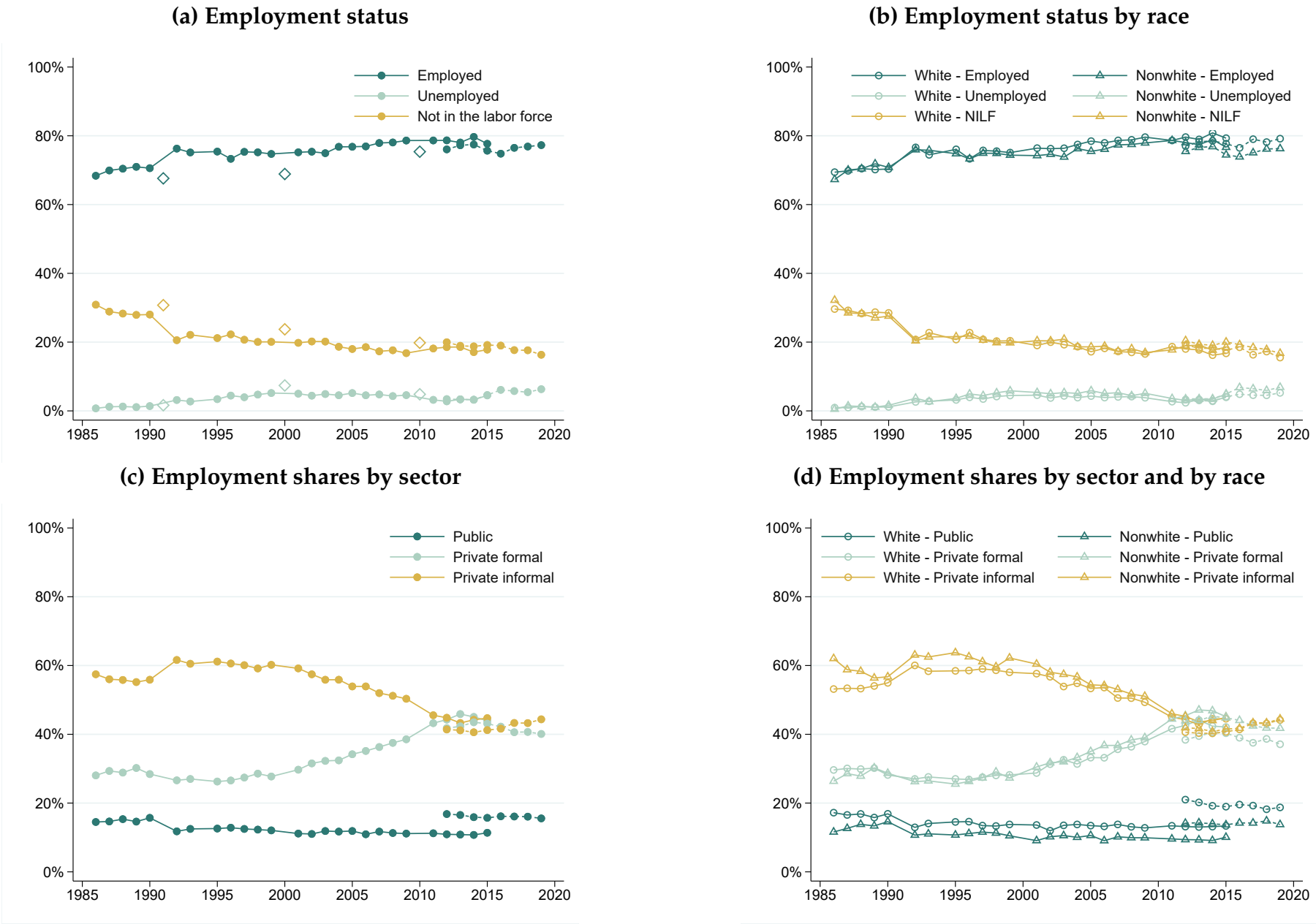
Sources: PNAD 1986-2015 (plain line). PNAD Continua 2012-2019, third trimester (dashed line). Censuses 1991-2010 (diamonds).
Sample: Adults 25-54, white or nonwhite.

Figure A5: Labor market statistics, Southeast



Sources: PNAD 1986-2015 (plain line). PNAD Continua 2012-2019, third trimester (dashed line). Censuses 1991-2010 (diamonds).
Sample: Adults 25-54, white or nonwhite.

Figure A6: Labor market statistics, Midwest



Sources: PNAD 1986-2015 (plain line). PNAD Continua 2012-2019, third trimester (dashed line). Censuses 1991-2010 (diamonds).
Sample: Adults 25-54, white or nonwhite.

Appendix B Data with restricted access: Linked employer-employee data (RAIS) (1995-2017)

B.1 Content and access

Content. We have access to the linked employer-employee data from 1995 to 2017. It contains information on the universe of workers employed in the formal sector in Brazil. The data set contains a worker identifier, and a firm identifier. The race information appears in the 1999 dataset and is available in all subsequent years. We have information on three types of earnings: i) earnings in December each year ("December earnings" in what follows); ii) earnings as stipulated in the employment contract at the end of the year or at the end of the relationship for spells that ended during the year ("Contracted wage" in what follows); iii) and the average monthly earnings over the employment spell in each year ("Average earnings" in what follows). From 2015 to 2017, we also have monthly earnings for each month of the year, not just December ("Monthly earnings" in what follows).

The dataset also contains a series of workers' characteristics (gender, age, education, tenure, occupation, type of labor contract, contracted hours, hiring date, separation date, type of separation) and firms' characteristics (location, industry, legal status).

Access and documentation. The full documentation can be accessed here. e.g. dictionary of variables for 2019 available at this address.

B.2 Information on race

We have the information on the race of each worker starting in 1999; it uses the same 5 race categories as in the PNAD surveys. The information on race is filled by the HR department of each firm, however, and may therefore differ from the race information that is self-reported in labor force surveys (Cornwall et al., 2017). There are two issues related to the information on race: i) the race variable has missing information for some workers in each year and ii) the exact same worker can be recorded as "white" when employed in some firm j and as "nonwhite" (or any other different race) when later employed in some other firm k.

Panel (a) in Figure B3 displays the distribution of workers across race groups in each year in the RAIS data, using the race variable associated with each observation. The sample is restricted to private-sector workers employed at the end of each year, so that we have at most one observation per worker in each year. First, Panel (a) shows that we have no data on race until 1999 (grey line). Second, it shows that the race variable has missing information for

about 5%-10% of workers between 1999 and 2015 (black line). Third, it shows that workers of indigenous and asian descent account for a relatively small share of the sample (red line). Fourth, it shows that the share of white workers (maroon line) is decreasing over time while the share of nonwhite workers (green line) is increasing over time. To see this more clearly, Panel (b) in Figure B3 compares the share of white and nonwhite workers, conditional on belonging to either race group. In this sample, the share of white workers decreases from 73.3% in 1999 to 56.7% in 2017. We thus have a decreasing share of white workers in private formal employment, as documented with the PNAD surveys, but these shares are systematically higher than in PNAD surveys.

Imputation of race. To address the two limitations with the race variable in the RAIS data, we assign each worker to their race group according to their modal race group (pooling mixed-race and black together) across all their observations in RAIS between 1999 and 2017. Because our focus is on white-nonwhite comparisons, and because the share of white workers is higher in RAIS than in PNAD, we deal with the few cases in which workers have more than one modal race group as follows. We assign workers to the nonwhite group if nonwhite is one of the modal race group; we then assign workers to the white group if white is one of the modal race group but nonwhite is not; finally, we assign workers to the remaining group pooling workers of indigenous and asian descent if one of their model race group is indigenous or asian descent, but neither nonwhite nor white.

Panels (c) and (d) in Figure B3 present similar graphs as those in Panels (a) and (b), but assigning each worker to their race group based on our imputation strategy. First, Panel (c) shows that we manage to assign a race group to most workers between 1995 and 1998, i.e., most of these workers appear in the RAIS data after 1998 with non-missing race information (grey line). Specifically, the share of workers for which we are unable to assign a race group is only 14.7% in 1995 and it decreases almost linearly to 1.7% in 1998. Second, between 1999 and 2017, the share of workers with missing race information drops to .1%-.8%. Third, the reduction in the share of workers with unassigned race group leads to increases in both the share of white workers and the share of nonwhite workers. Fourth, Panel (d) shows that the share of white and nonwhite workers, conditional on being assigned to either race group, is quite similar between 1995 and 1998 compared to between 1999 and 2001, which is also the case in the PNAD surveys. Therefore, our imputation of race groups in years for which we do not have the race variable in the data does seem to lead to any systematic bias in the share of specific race groups. Fifth, the share of white workers is lower in Panel (d) than in Panel (b)

in the earlier years (e.g., in 1999), but is higher in the later years (e.g., in 2017). As a result, the share of white workers only decreases by 9pp, from 68.8% to 59.7%, between 1999 and 2017 in Panel (d) compared to the drop of 17pp in Panel (b). We also note that, with our imputation strategy, the share of white workers remains higher in RAIS than in PNAD surveys.

Figure B1: Observations and earnings 1995-2017, RAIS

			If assigned to race group			Median monthly earnings (in R$2019)			
				If white or nonwhite			If assigned		
Year	Observations	Share assigned to race group	Share white or nonwhite	Share nonwhite	Share white	Whole sample	to race group	If nonwhite	If white
1995	9,577,771	0.85	0.99	0.31	0.69	1,488.78	1,519.16	1,224.01	1,701.46
1996	10,011,653	0.89	0.99	0.31	0.69	1,550.40	1,581.59	1,269.73	1,755.34
1997	10,761,673	0.93	0.99	0.31	0.69	1,564.60	1,587.48	1,290.11	1,761.32
1998	10,944,014	0.98	0.99	0.31	0.69	1,547.47	1,552.30	1,266.99	1,716.72
1999	11,206,049	1.00	0.99	0.31	0.69	1,489.99	1,488.62	1,221.54	1,639.64
2000	11,693,322	1.00	0.99	0.31	0.69	1,494.81	1,494.19	1,225.89	1,644.59
2001	12,409,334	1.00	0.99	0.32	0.68	1,443.16	1,442.29	1,191.09	1,586.89
2002	13,049,813	1.00	0.99	0.32	0.68	1,333.28	1,332.87	1,098.63	1,470.93
2003	13,668,722	1.00	0.99	0.32	0.68	1,356.87	1,356.56	1,127.88	1,495.28
2004	14,701,939	1.00	0.99	0.33	0.67	1,364.57	1,364.30	1,138.24	1,507.92
2005	15,596,917	1.00	0.99	0.34	0.66	1,391.80	1,391.80	1,169.49	1,536.71
2006	16,642,611	1.00	0.99	0.34	0.66	1,445.28	1,445.32	1,230.65	1,593.10
2007	17,846,753	1.00	0.99	0.35	0.65	1,482.40	1,482.40	1,258.52	1,623.39
2008	19,124,238	1.00	0.99	0.35	0.65	1,521.81	1,522.25	1,295.46	1,673.52
2009	19,940,893	1.00	0.99	0.36	0.64	1,583.49	1,583.49	1,355.80	1,732.69
2010	21,574,538	1.00	0.99	0.37	0.63	1,635.02	1,636.06	1,401.08	1,782.45
2011	22,814,995	1.00	0.99	0.38	0.62	1,693.09	1,694.26	1,462.95	1,863.83
2012	23,863,400	1.00	0.99	0.38	0.62	1,770.67	1,772.58	1,541.77	1,947.27
2013	24,640,411	1.00	0.99	0.39	0.61	1,863.85	1,866.38	1,622.03	2,056.68
2014	25,116,300	0.99	0.99	0.40	0.60	1,897.61	1,901.77	1,658.84	2,099.59
2015	24,480,544	0.99	0.99	0.40	0.60	1,845.27	1,849.10	1,605.45	2,037.75
2016	23,794,238	0.99	0.99	0.40	0.60	1,866.44	1,871.59	1,630.72	2,058.15
2017	23,847,578	0.99	0.99	0.40	0.60	1,914.96	1,920.14	1,676.73	2,114.91

Sources: RAIS 1995-2017.

Sample: Adults 25-54, employed full-time in private-sector job on December 31st, all race categories.

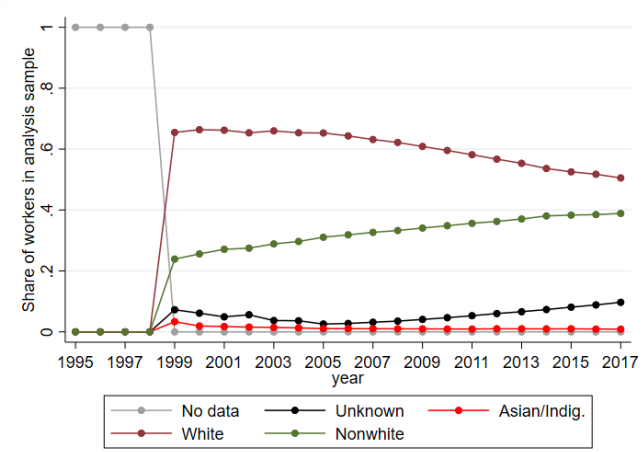
Figure B2: Workers' characteristics 1999 and 2015, RAIS

	Private formal 1999			Private formal 2015		
	All	White	Nonwhite	All	White	Nonwhite
Monthly earnings (in R$2019)	2,525.70	2,839.67	1,831.26	2,735.55	3,114.49	2,164.46
Age	35.5	35.6	35.3	36.6	37.0	36.0
Work experience	21.6	21.3	22.1	20.2	20.2	20.1
Gender						
Male	0.66	0.65	0.70	0.59	0.57	0.63
Female	0.34	0.35	0.30	0.41	0.43	0.37
Education						
Less than high school	0.68	0.65	0.74	0.30	0.27	0.34
High school completed	0.24	0.25	0.22	0.55	0.53	0.57
College completed	0.09	0.11	0.04	0.15	0.19	0.09
Region						
North	0.03	0.01	0.08	0.04	0.01	0.09
Northeast	0.15	0.07	0.32	0.17	0.07	0.32
Southeast	0.57	0.61	0.49	0.53	0.59	0.45
South	0.19	0.25	0.04	0.18	0.27	0.04
Midwest	0.06	0.05	0.08	0.07	0.06	0.10
Full-time/part-time status						
Full-time	1.00	0.99	1.00	1.00	1.00	1.00
Part-time	0.00	0.01	0.00	0.00	0.00	0.00
Firm size						
Small firm (9 workers or less)	0.20	0.21	0.19	0.21	0.22	0.18
Large firm (10+ workers)	0.80	0.79	0.81	0.79	0.78	0.82
Missing	0.00	0.00	0.00	0.00	0.00	0.00

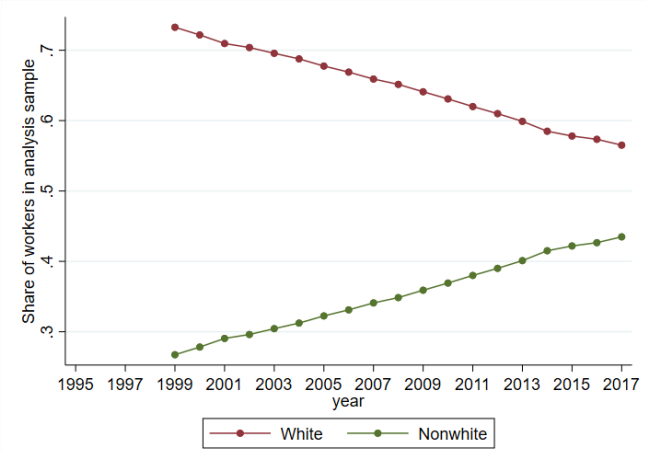
Sources: RAIS 1995-2017.
Sample: Adults 25-54, employed full-time in private-sector job on December 31st, white or nonwhite based on model race category.

Figure B3: Share of workers by race categories, RAIS

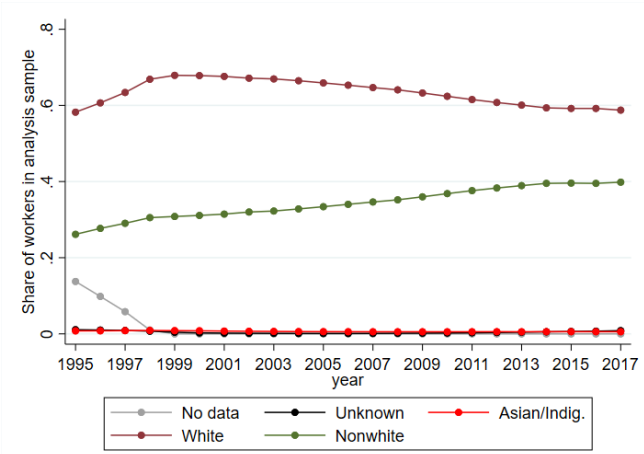
(a) Race categories, using the race variable in each year



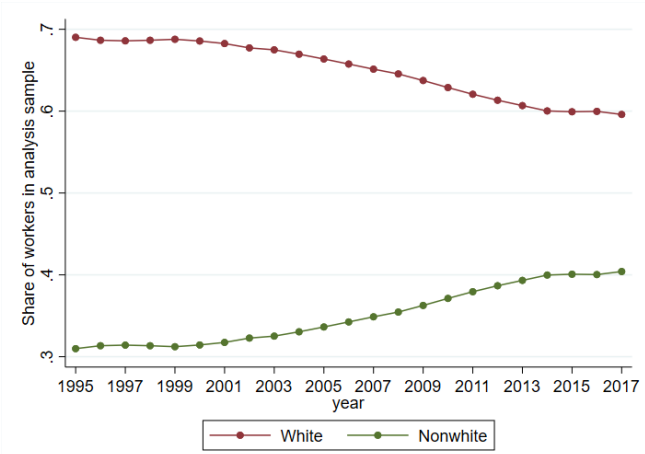
(b) Race categories if white or nonwhite, using the race variable in each year



(c) Race categories, using the mode of the race variable across years



(d) Race categories if white or nonwhite, using the mode of the race variable across years



Sources: RAIS 1995-2017.

Sample: Adults 25-54, employed full-time in private-sector job on December 31st, all race categories.

B.3 Consistency between RAIS and PNAD/PNAD continua

We investigate the consistency between the linked employer-employee data (RAIS) and the labor force surveys (PNAD and PNAC continua) for workers in the private formal sector.

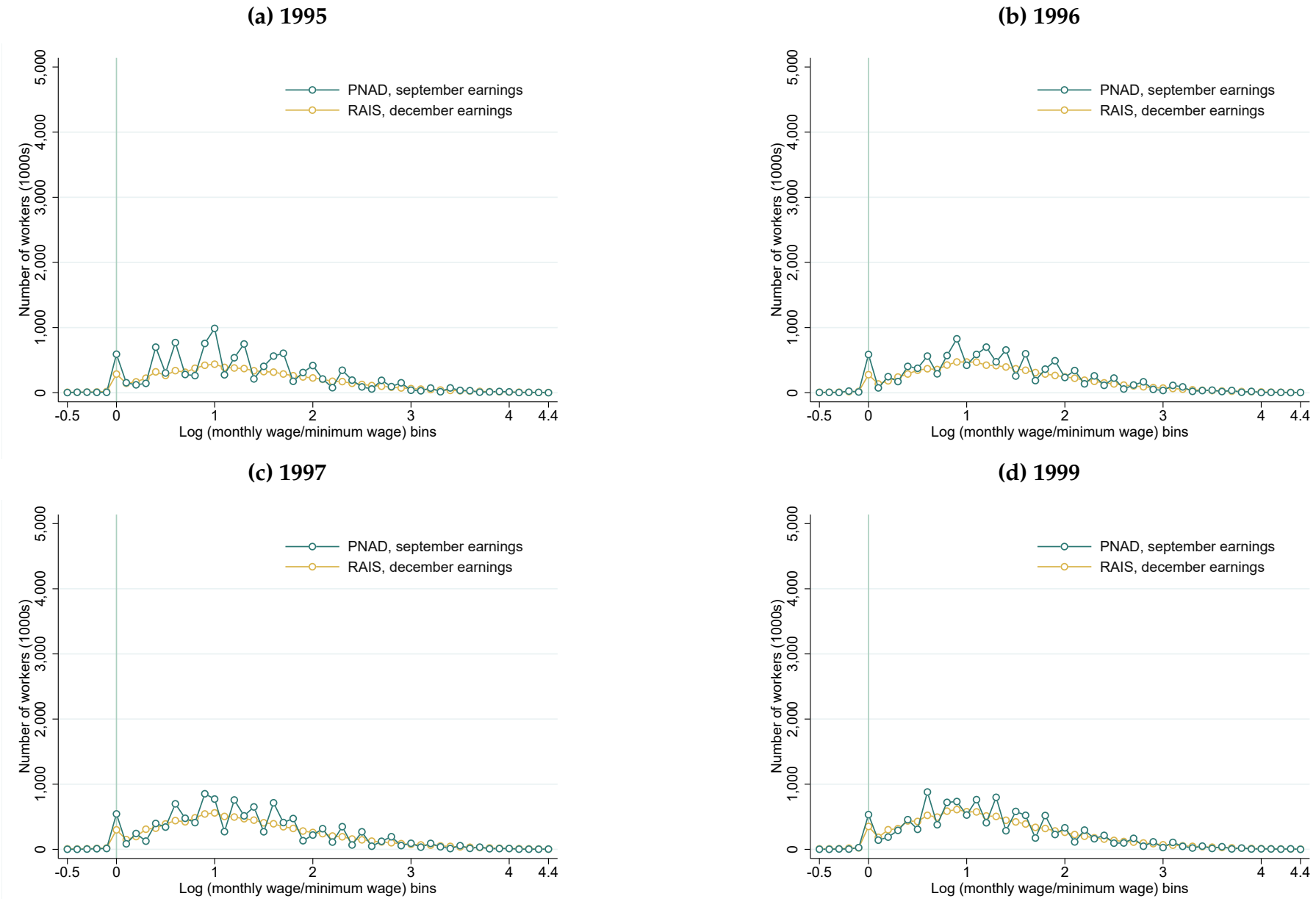
We show that the spike at the minimum wage in monthly earnings distributions in PNAD (i.e., earnings reported by workers) is nearly identical to the spike observed using the contracted wage in the universe of the matched employer-employee data (i.e. earnings reported by employers). Appendix Figures B5 to B9 compare between monthly earnings distributions in PNAD and monthly contracted wage distributions in RAIS. We beleive these two wage concepts are close. By contrast the "December earnings" definition in RAIS takes into account bonuses and overtime pay, so that there is mechanically a smaller bunch at the minimum wage.

We conclude that PNAD and RAIS monthly earnings distributions are not only consistent in terms of shares (i.e. share of workers at each point of the wage distribution) but also in numbers (i.e. number of workers at each point of the wage distribution).

We also show that the two data sources are consistent across regions and racial groups (see Appendix Figures B10 to B14).

Finally, virtually no workers report being paid below the minimum wage in the private formal sector. This nearly perfect compliance with the minimum wage is also confirmed in the RAIS data.

Figure B4: Monthly earnings distributions in PNAD vs. RAIS, 1995-2002

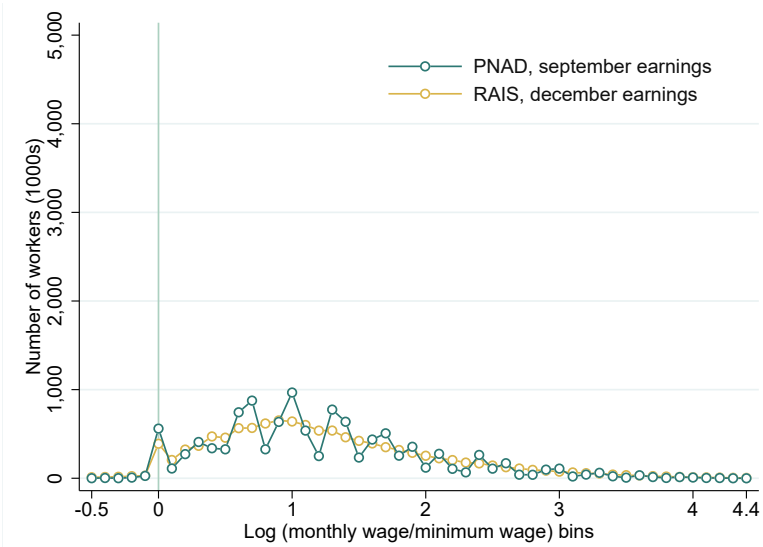


Sources: PNAD 1999-2015. RAIS 1999-2015.

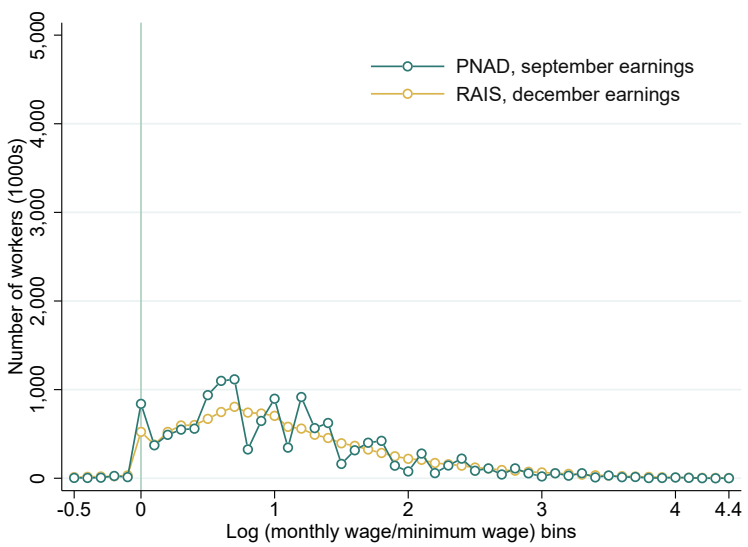
Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

Figure B5: Monthly earnings distributions in PNAD vs. RAIS, 1995-2002

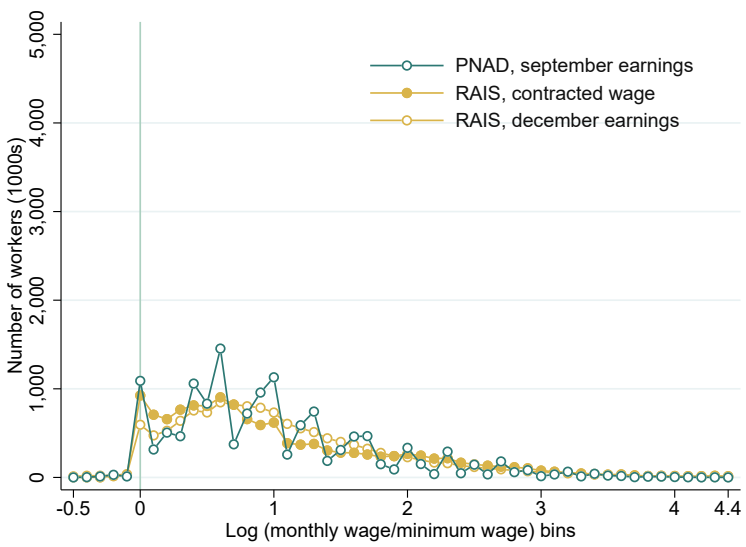
(a) 1999



(b) 2001



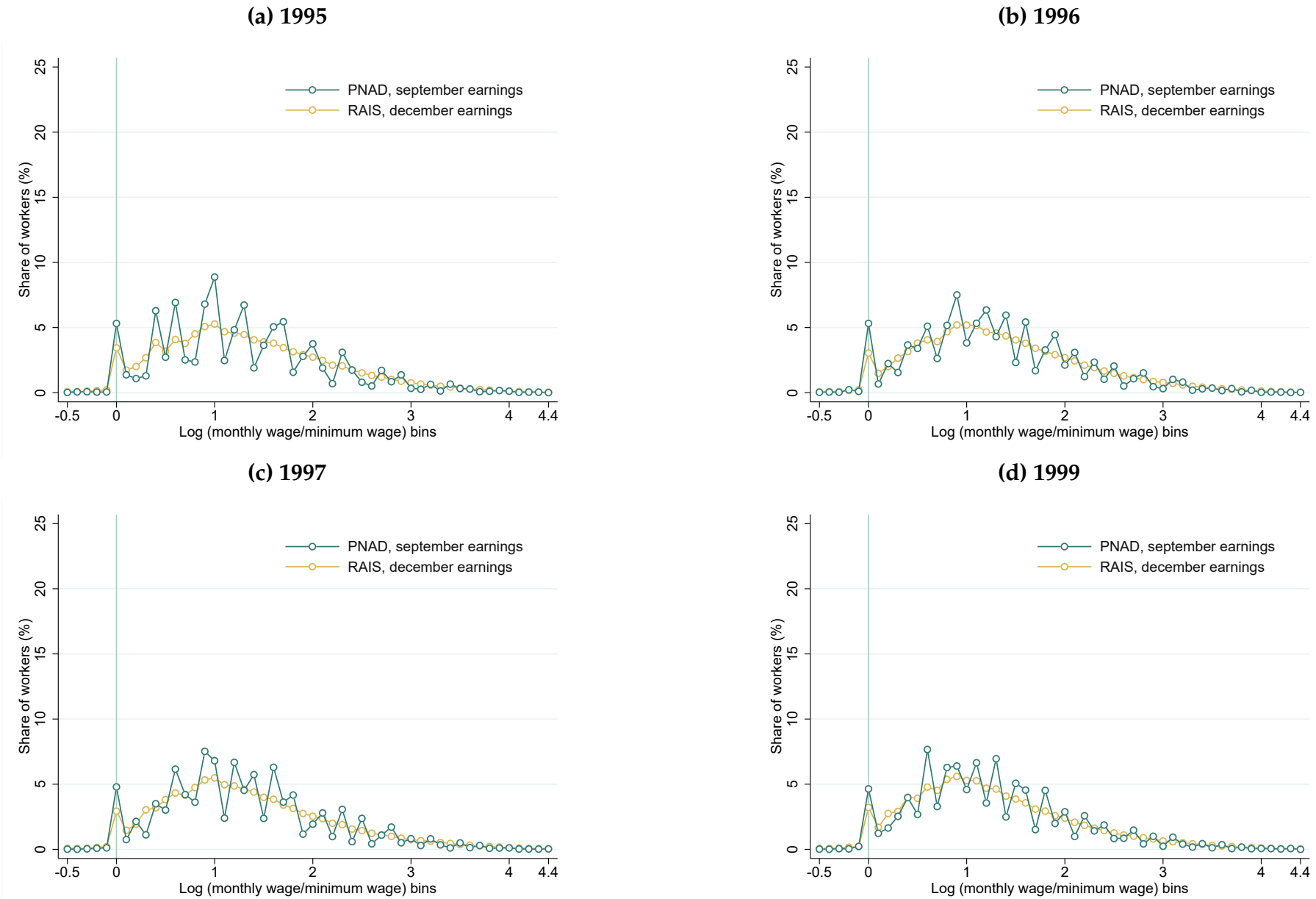
(c) 2002



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

Figure B6: Monthly earnings distributions in PNAD vs. RAIS, 1995-2002

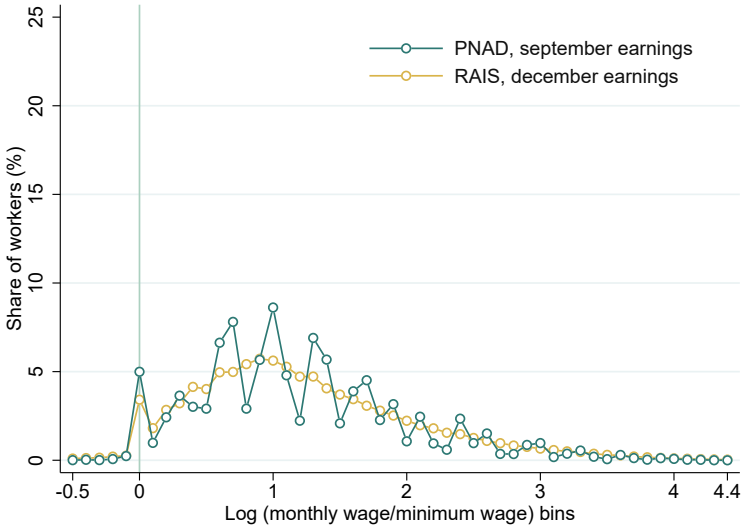


Sources: PNAD 1999-2015. RAIS 1999-2015.

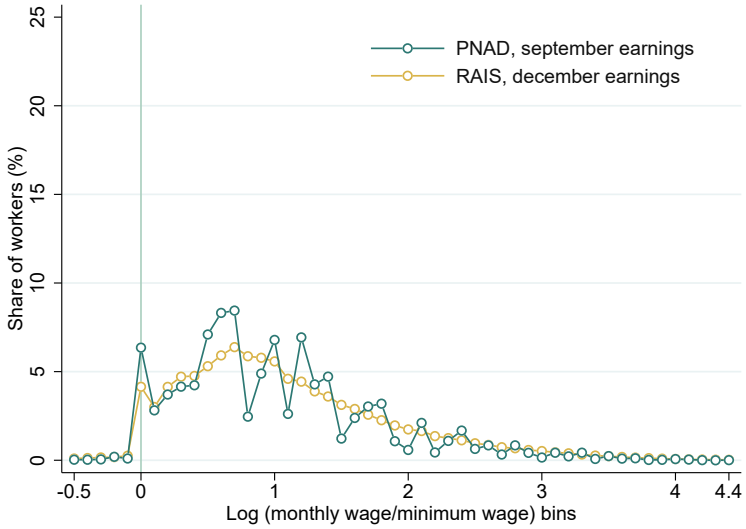
Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

Figure B7: Monthly earnings distributions in PNAD vs. RAIS, 1995-2002

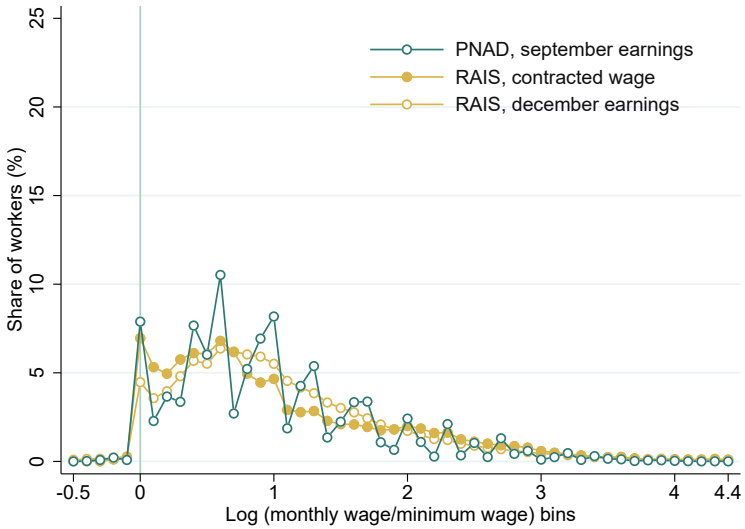
(a) 1999



(b) 2001



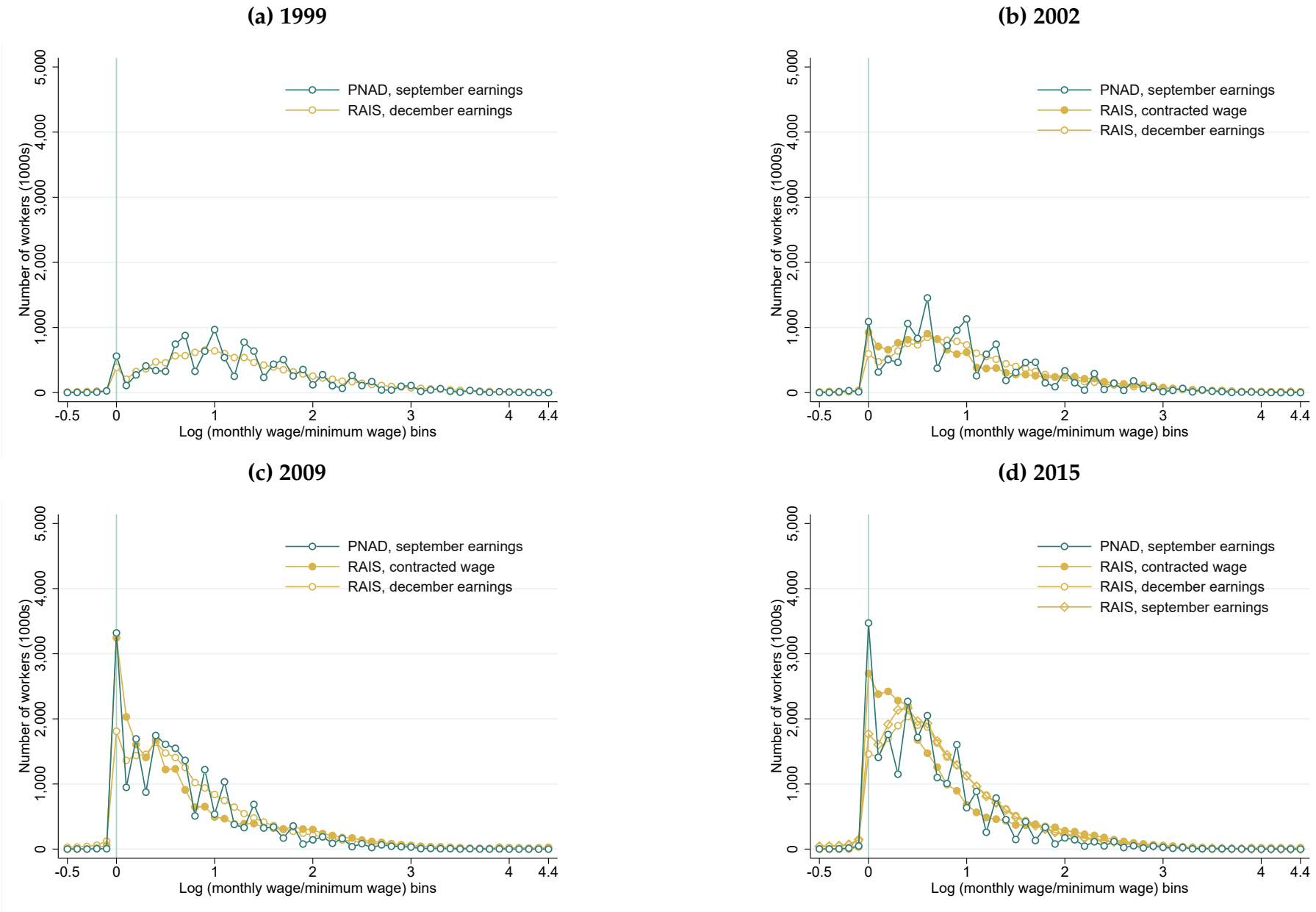
(c) 2002



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

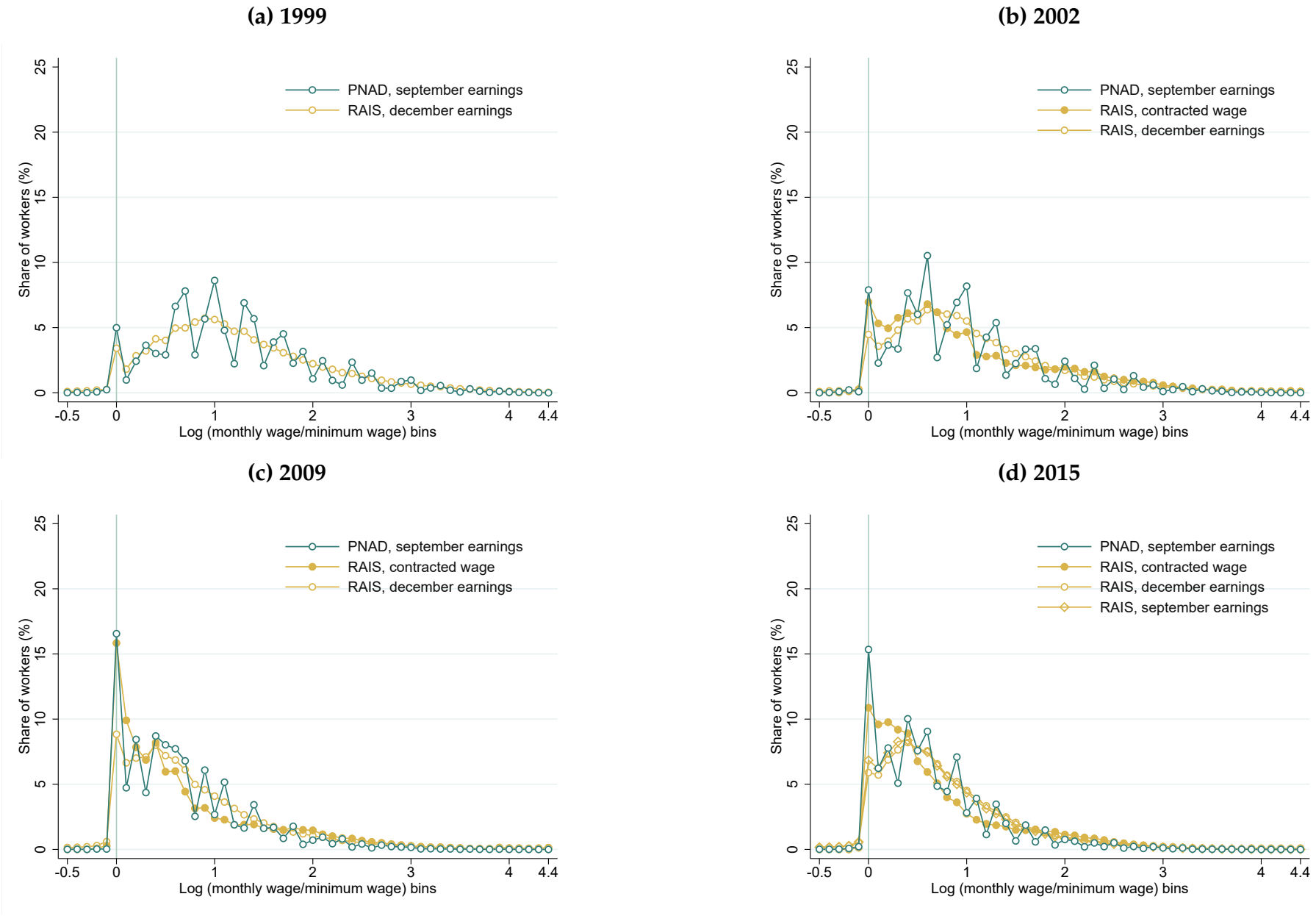
Figure B8: Monthly earnings distributions in PNAD vs. RAIS



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

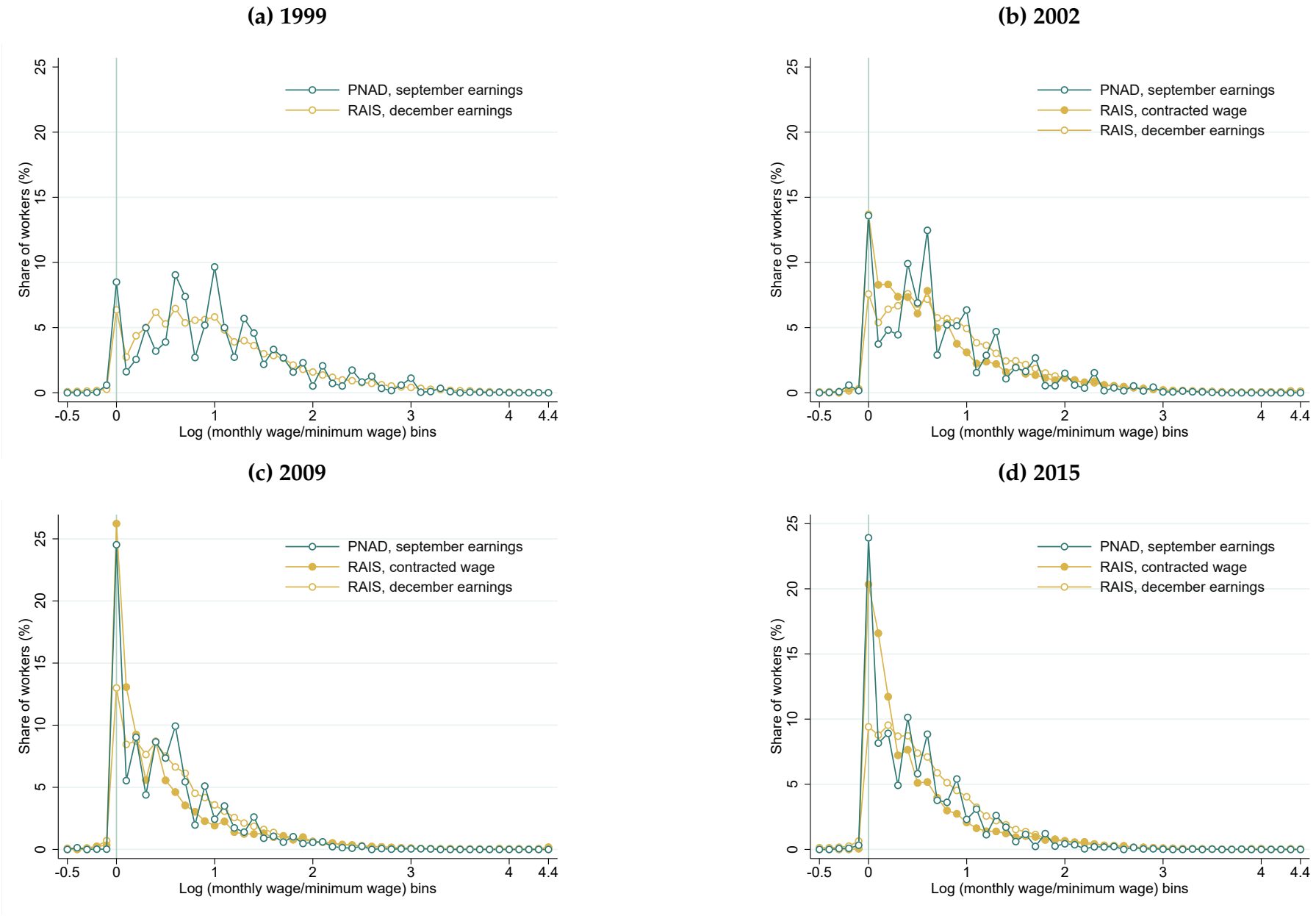
Figure B9: Monthly earnings distributions in PNAD vs. RAIS



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

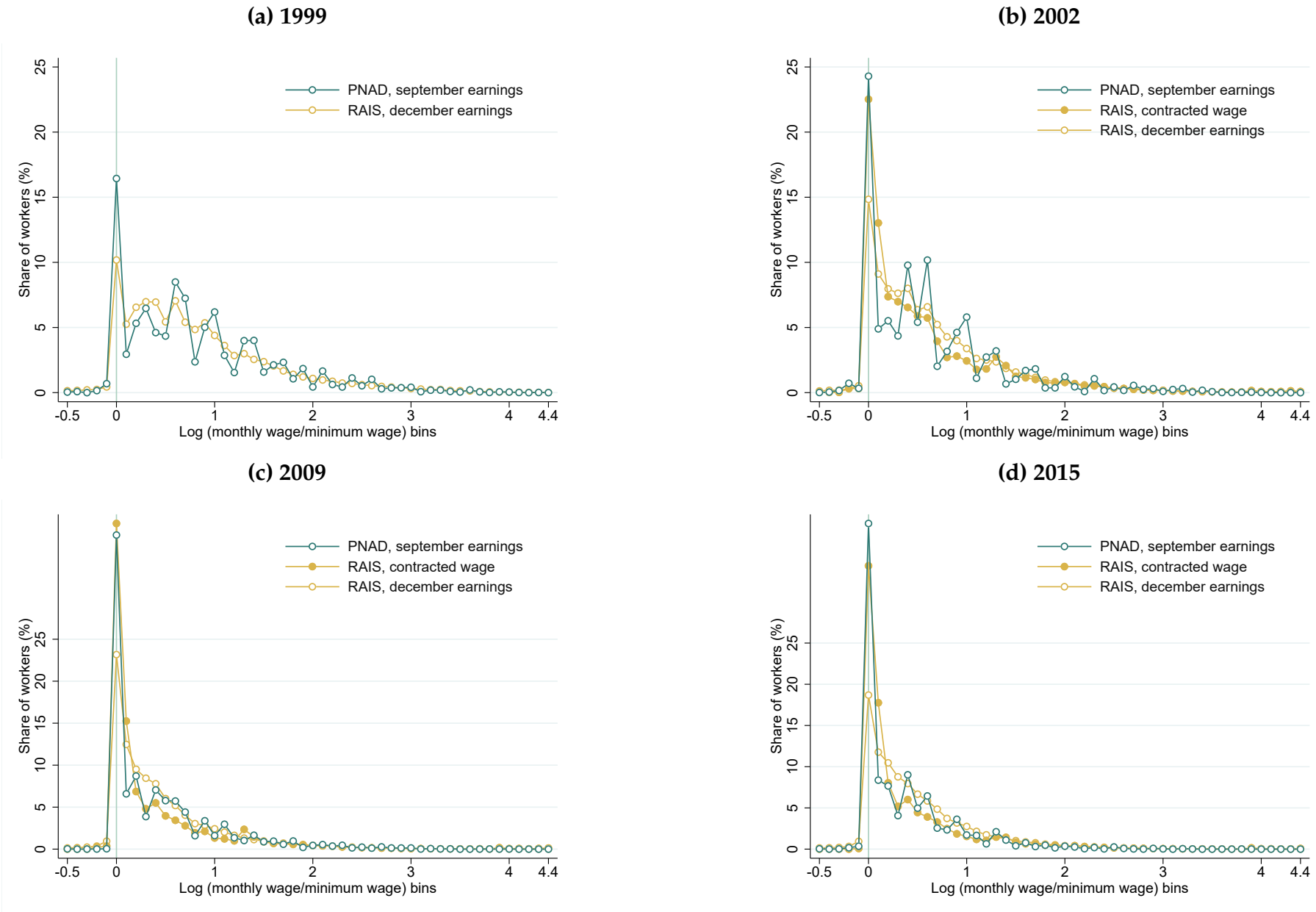
Figure B10: Monthly earnings distributions in PNAD vs. RAIS in the North



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

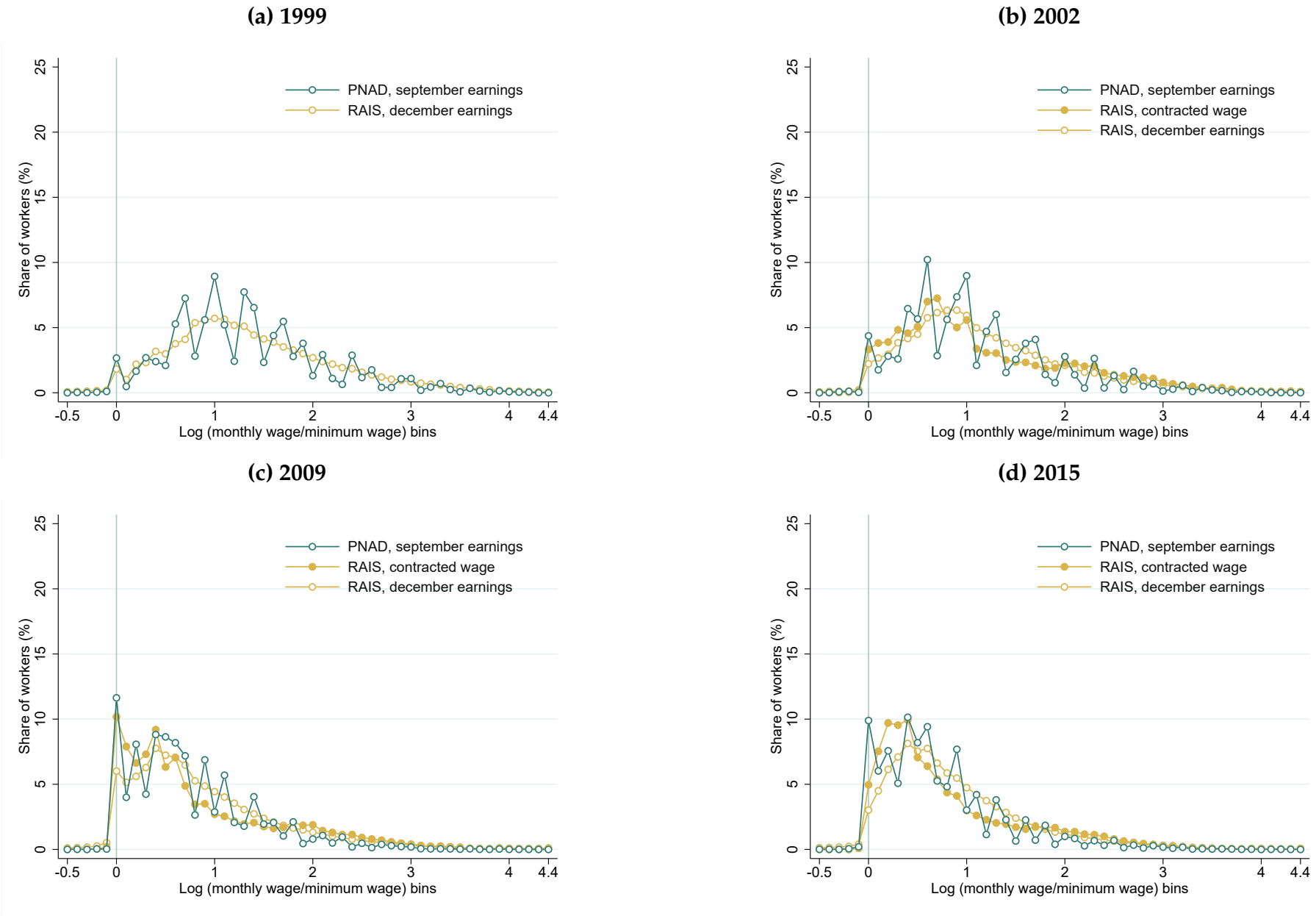
Figure B11: Monthly earnings distributions in PNAD vs. RAIS in Northeast



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

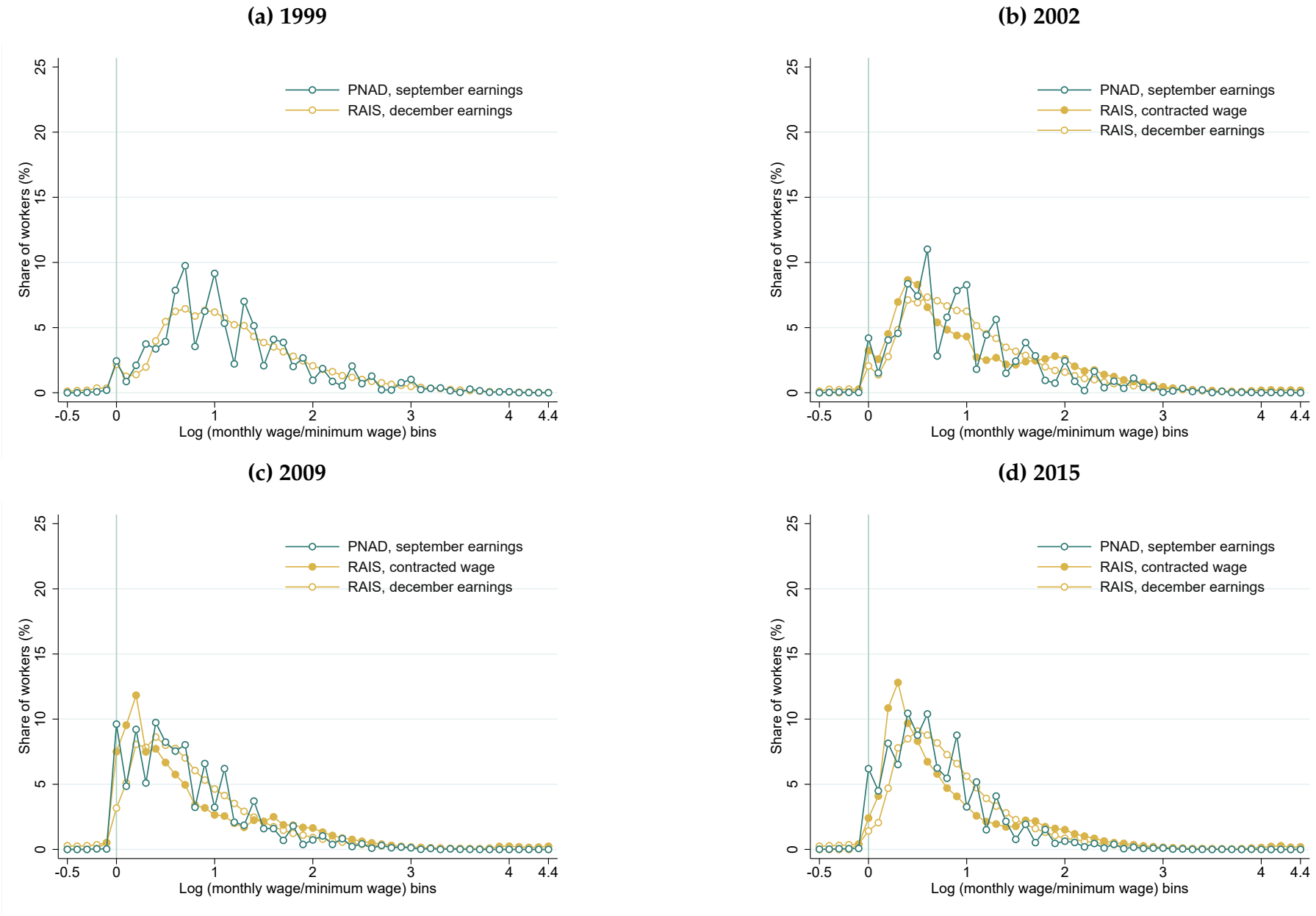
Figure B12: Monthly earnings distributions in PNAD vs. RAIS in Southeast



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

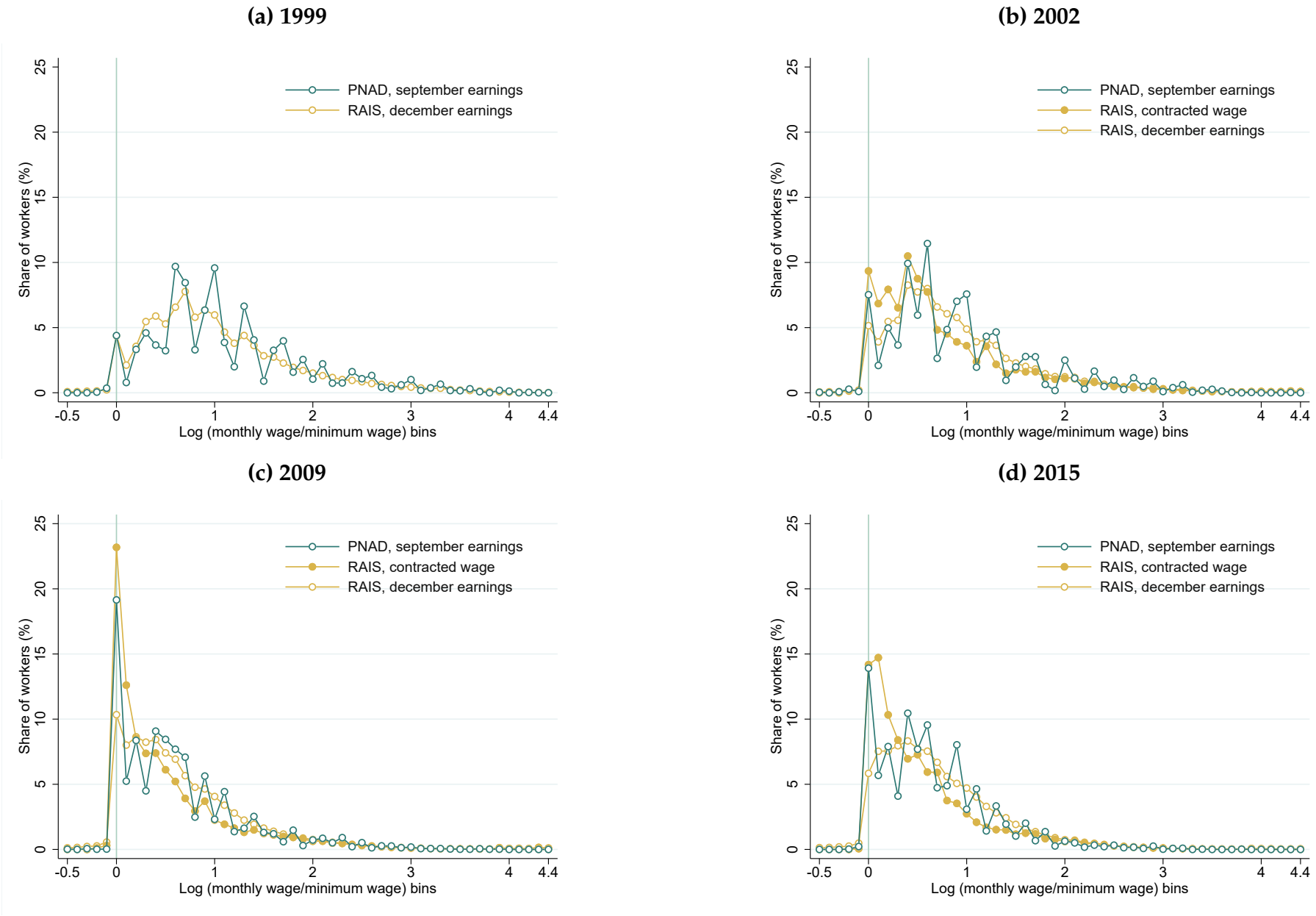
Figure B13: Monthly earnings distributions in PNAD vs. RAIS in the South



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

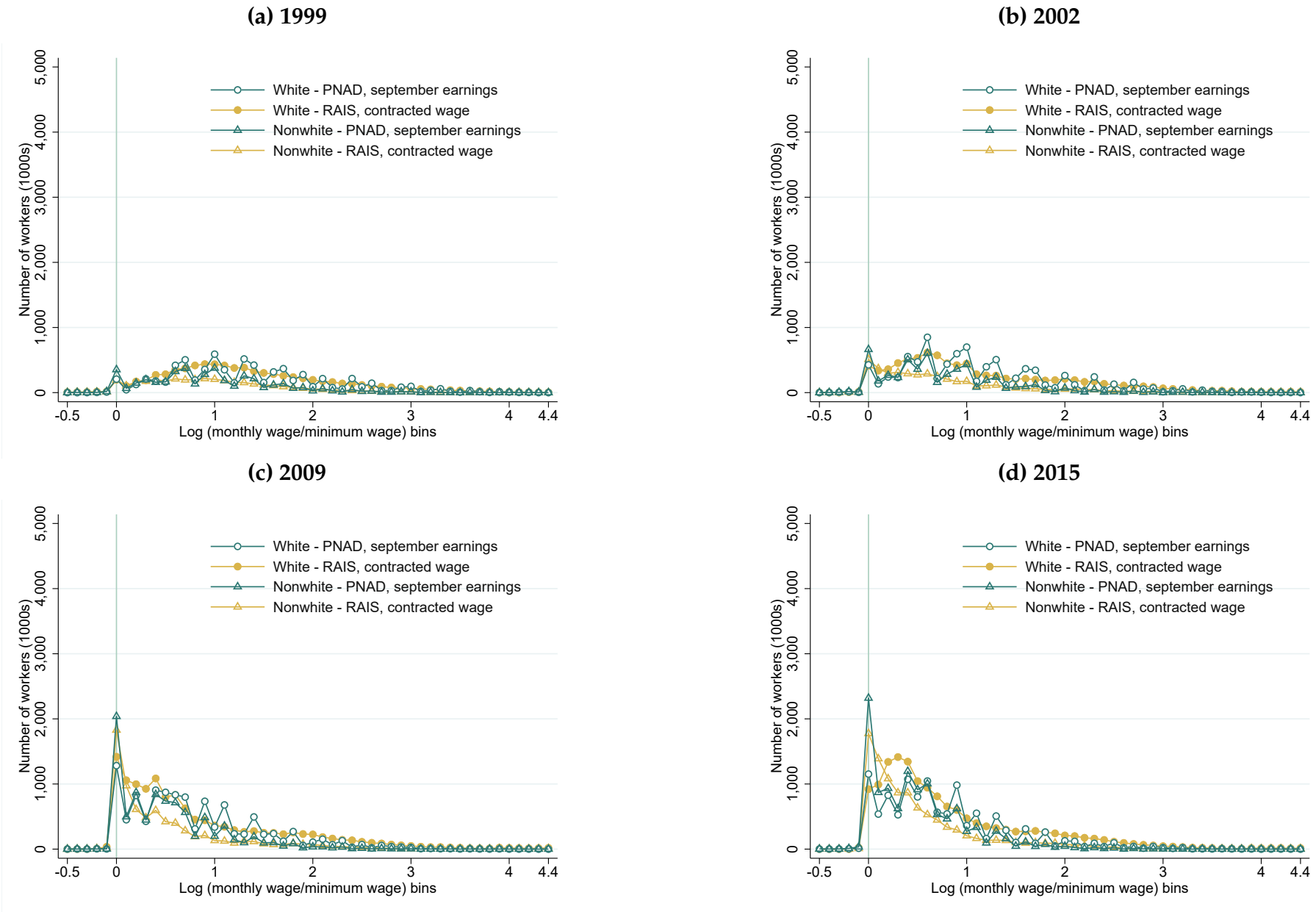
Figure B14: Monthly earnings distributions in PNAD vs. RAIS in the West



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

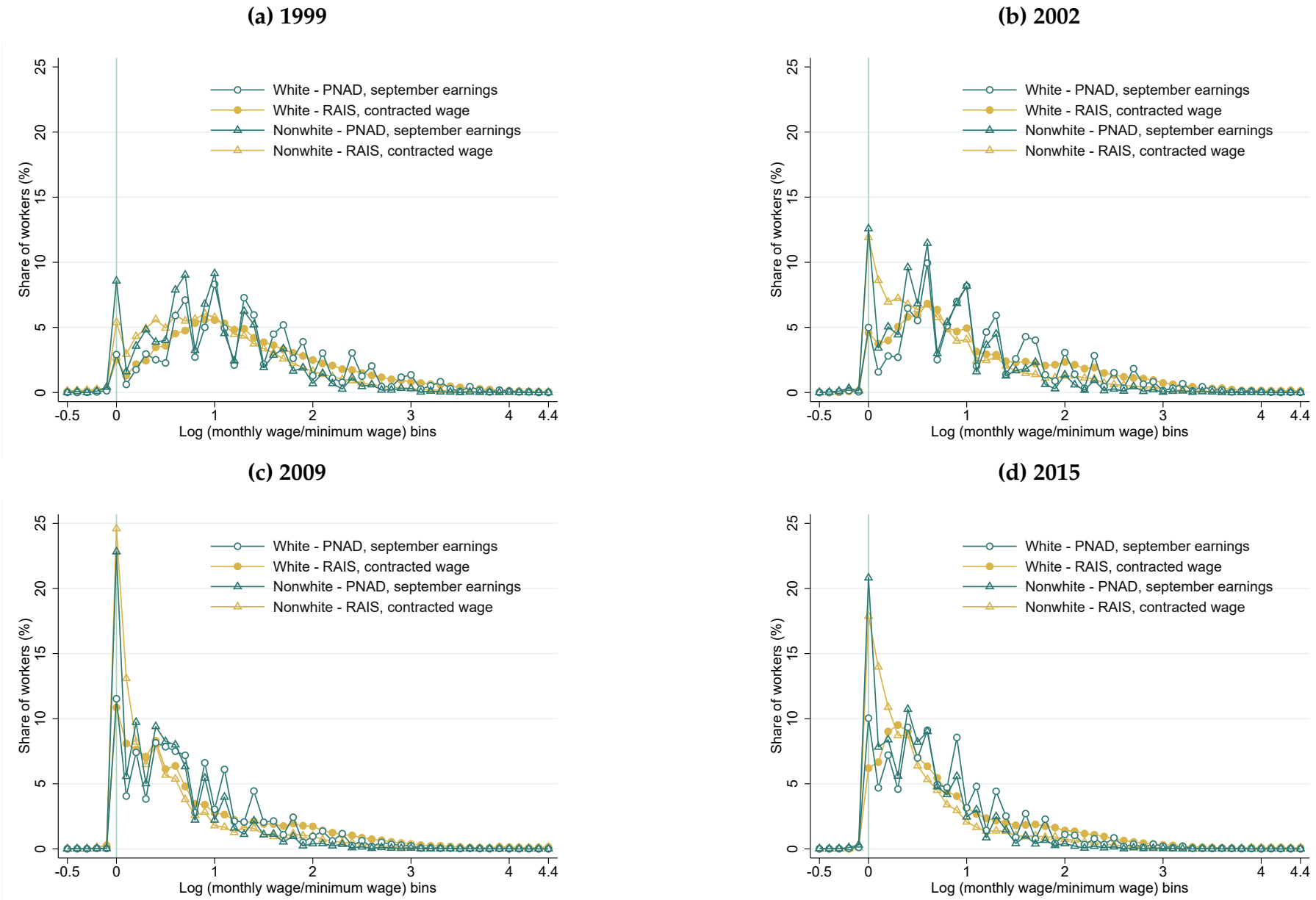
Figure B15: Monthly earnings distributions by race in PNAD vs. RAIS



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

Figure B16: Monthly earnings distributions by race in PNAD vs. RAIS



Sources: PNAD 1999-2015. RAIS 1999-2015.

Sample: Adults 25-54, employed full-time in formal private sector job (excluding domestic workers and agricultural workers).

Appendix C The minimum wage in Brazil

C.1 Background information

The minimum wage in Brazil was created in 1940 with different values across regions. From 1979 to 1982 adjustments were made every semester corresponding to 110% of the inflation rate. Adjustments in the minimum wage are passed as laws by the congress and have to be approved by the president. In 1984 the minimum wage was unified across regions. From 1987 to 1993, adjustments became more frequent due to the high inflation rates. They were determined by different "provisory laws" and became almost monthly. After the "Plano Real" in 1994, where inflation was stabilized, the MW became annually adjusted. In 2000 there was a law that allowed states to create state value MW. In 2008 congress passed a law that established a fixed rule in the value of Minimum Wage adjustments. The law was:

$$MW_t = MW_{t-1} \cdot inflation_{t-1} + \max\{0, \text{GDP growth}_{t-2}\}$$

This law is still valid today.

C.2 Minimum wage database (1940-2020)

Content and access. We build a database of minimum wages from 1940 – i.e. from the introduction of the federal minimum in Brazil – to 2020. The database contains the values of the federal minimum wage at the month-by-year level in nominal terms using the information published by the Brazilian Institute of Geography and Statistics (IBGE) website.[21] It also contains nominal values of regional minimum wages. Brazil implemented state-level minimum wages from 1940 to 1984. Since 2001, five states have implemented industry and occupation-specific minimum wages at a higher level than the federal one: Rio Grande do Sul, Santa Catarina, Paraná, Rio de Janeiro and São Paulo.

Sources. *National level.* We report minimum wage rates at the national level since 1940 in Appendix Table C1 below.

[21] At this address: https://www.ibge.gov.br/estatisticas/downloads-estatisticas.html.

Table C1: Nominal values of the minimum wage, Brazil 1940-2020

Date of Change	Value	Date of Change	Value	Date of Change	Value
04/07/1940	240 mil réis	01/11/1987	Cz$ 3.000,00	01/12/1993	CR$ 18.760,00
01/01/1943	Cr$ 300,00	01/12/1987	Cz$ 3.600,00	01/01/1994	CR$ 32.882,00
01/12/1943	Cr$ 380,00	01/01/1988	Cz$ 4.500,00	01/02/1994	CR$ 42.829,00
01/01/1952	Cr$ 1.200,00	01/02/1988	Cz$ 5.280,00	01/03/1994	URV 64,79 = R$ 64,79
04/07/1954	Cr$ 2.400,00	01/03/1988	Cz$ 6.240,00	01/07/1994	R$ 64,79
01/08/1956	Cr$ 3.800,00	01/04/1988	Cz$ 7.260,00	01/09/1994	R$ 70,00
01/01/1959	Cr$ 6.000,00	01/05/1988	Cz$ 8.712,00	01/05/1995	R$ 100,00
18/10/1960	Cr$ 9.600,00	01/06/1988	Cz$ 10.368,00	01/05/1996	R$ 112,00
16/10/1961	Cr$ 13.440,00	01/07/1988	Cz$ 12.444,00	01/05/1997	R$ 120,00
01/01/1963	Cr$ 21.000,00	01/08/1988	Cz$ 15.552,00	01/05/1998	R$ 130,00
24/02/1964	Cr$ 42.000,00	01/09/1988	Cz$ 18.960,00	01/05/1999	R$ 136,00
01/02/1965	CR$ 66.000,00	01/10/1988	Cz$ 23.700,00	03/04/2000	R$ 151,00
01/03/1966	Cr$ 84.000,00	01/11/1988	Cz$ 30.800,00	01/04/2001	R$ 180,00
01/03/1967	NCr$ 105,00	01/12/1988	Cz$ 40.425,00	01/04/2002	R$ 200,00
26/03/1968	NCr$ 129,60	01/01/1989	NCz$ 63,90		
01/05/1969	NCr$ 156,00	01/05/1989	NCz$ 81,40	01/04/2003	R$ 240,00
01/05/1970	NCr$ 187.20	01/06/1989	NCz$ 120,00		
01/05/1971	Cr$ 225,60	03/07/1989	NCz$ 149,80	01/05/2004	R$ 260,00
01/05/1972	Cr$ 268,80	01/08/1989	NCz$ 192,88		
01/05/1973	Cr$ 312,00	01/09/1989	NCz$ 249,48	01/05/2005	R$ 300,00
01/05/1974	Cr$ 376,80	01/10/1989	NCz$ 381,73		
01/12/1974	Cr$ 415,20	01/11/1989	NCz$ 557,31	01/04/2006	R$ 350,00
01/05/1975	Cr$ 532,80	01/12/1989	NCz$ 788,12		
01/05/1976	Cr$ 768,00	01/01/1990	NCz$ 1.283,95		
01/05/1977	Cr$ 1.106,40	01/02/1990	NCz$ 2.004,37	01/04/2007	R$ 380,00
01/05/1978	Cr$ 1.560,00	01/03/1990	NCz$ 3.674,06		
01/05/1979	Cr$ 2.268,00	01/04/1990	Cr$ 3.674,06	01/03/2008	R$ 415,00
01/11/1979	Cr$ 2.932,80	01/05/1990	Cr$ 3.674,06		
01/05/1980	Cr$ 4.149,60	01/06/1990	Cr$ 3.857,66	01/02/2009	R$ 465,00
01/11/1980	Cr$ 5.788,80	01/07/1990	Cr$ 4.904,76		
01/05/1981	Cr$ 8.464,80	01/08/1990	Cr$ 5.203,46	01/01/2010	R$ 510,00
01/11/1981	Cr$ 11.928,00	01/09/1990	Cr$ 6.056,31		
01/05/1982	Cr$ 16.608,00	01/10/1990	Cr$ 6.425,14	01/03/2011	R$ 545,00
01/11/1982	Cr$ 23.568,00	01/11/1990	Cr$ 8.329,55		
01/05/1983	Cr$ 34.776,00	01/12/1990	Cr$ 8.836,82		R$ 622,00
01/11/1983	Cr$ 57.120,00	01/01/1991	Cr$ 12.325,60	01/01/2012	
01/05/1984	Cr$ 97.176,00	01/02/1991	Cr$ 15.895,46		
01/11/1984	Cr$ 166.560,00	01/03/1991	Cr$ 17.000,00		R$ 678,00
01/05/1985	Cr$ 333.120,00	01/09/1991	Cr$ 42.000,00	01/01/2013	
01/11/1985	Cr$ 600.000,00	01/01/1992	Cr$ 96.037,33		
01/03/1986	Cz$ 804,00	01/05/1992	Cr$ 230.000,00	01/01/2014	R$ 724,00
01/01/1987	Cz$ 964,80	01/09/1992	Cr$ 522.186,94		
01/03/1987	Czr 1.368,00	01/01/1993	Cr$ 1.250.700,00	01/01/2015	R$ 788,00
01/05/1987	Cz$ 1.641,60	01/03/1993	Cr$ 1.709.400,00		
01/06/1987	Cz$ 1.969,92	01/05/1993	Cr$ 3.303.300,00	01/01/2016	R$ 880,00
10/08/1987	Cz$ 1.970,00	01/07/1993	Cr$ 4.639.800,00		
01/09/1987	Cz$ 2.400,00	01/08/1993	CR$ 5.534,00		
01/10/1987	Cz$ 2.640,00	01/09/1993	CR$ 9.606,00		
		01/10/1993	CR$ 12.024,00		
		01/11/1993	CR$ 15.021,00		

State level. Since 2001, five states have implemented industry and occupation-specific minimum wages at a higher level than the federal one. Rio de Janeiro, São Paulo and Paraná have mostly legislated minimum wage increases at the occupation level – rather than at the industry level. Rio Grande do Sul and Santa Catarina have mostly legislated minimum wage increases at the industry level. Regional minimum wage laws only apply to permanent workers in private firms. They are valid only if there is no collective bargaining agreement related to that occupation. Finally, the enforcement of the regional minimum wage legislation faces some challenges. Although the institution responsible for regional minimum wages inspection is the same as for the national minimum wage, many argue that the enforcement is more complicated at the state level (Moura and Neri, 2007; Corseuil et al., 2013). We provide in Appendix Table C2 the links to the State Assembly laws that implemented regional minimum wages.

We constructed two separate databases: i) one with variation by occupation when the minimum wage was defined by occupation; ii) another one by industry.

State-level minimum wage database by occupation. We used two occupation classifications to construct this database. We constructed the data using five-digits classification codes (CBO94) from 2001 to 2009[22] and six digits classification codes (CBO2002) codes from 2003 to 2020. For Rio de Janeiro (2001-2009), São Paulo (2007-2009) and Rio Grande do Sul (2001-2009) and Paraná (2006) both five-digits and six-digits occupational codes are used. In 2007 in Paraná minimum wage laws were only containing the information related to the six-digits occupation codes.

Using five-digits classification codes. The law, for the most part, designates occupations at the two or three digits level. Therefore, we included all the five-digits occupations contained inside the two- or tgree digits level code. In rare cases, we could not match the occupations listed in the law with the five-digits occupation classification codes. This was the case for *trabalhadores em serviços administrativos* (workers in administrative services), *dedetizadores* (fumigators), and *trabalhadores da indústria* (industry workers).

Using six-digits classification codes. Rio de Janeiro started to report the six-digits occupational classification codes (as opposed to the names of the occupations only) in the law in

[22] Although the six-digits classification codes went into effect in 2003, the matched employer-employee data we have access to contain the information on the five digits classification codes until 2009.

Table C2: Links to regional minimum wage laws

Year	Rio de Janeiro	São Paulo	Paraná	Rio Grande do Sul	Santa Catarina
2001	Link	—	—	Link	—
2002	Link	—	—	Link	—
2003	Link	—	—	Link	—
2004	Link	—	—	Link	—
2005	Link	—	—	Link	—
2006	Link	—	Link	Link	—
2007	Link	Link	Link	Link	—
2008	Link	Link	Link	Link	—
2009	Link	Link	Link	Link	—
2010	Link	Link	Link	Link	Link
2011	Link	Link	Link	Link	Link
2012	Link	Link	Link	Link	Link
2013	Link	Link	Link	Link	Link
2014	Link	Link	Link	Link	Link
2015	Link	Link	Link	Link	Link
2016	Link	Link	Link	link	Link
2017	Link	Link	Link	link	Link
2018	Link	Link	Link	link	Link
2019	Link	Link	Link	link	Link
2020	—	—	Link	—	Link

Notes: Regional minimum wage laws start to be implemented in Rio de Janeiro and Rio Grande de Sul in 2001, 2006 in Paraná, 2007 in São Paulo, and 2010 in Santa Catarina. For the state of Santa Catarina, the occupations that are covered by the minimum wage law only appear in 2010. In subsequent years, the law lists only the changes relative to this initial law. In 2020, there was no adjustment of the minimum wage law in Rio de Janeiro, São Paulo, and Rio Grande do Sul. Therefore, we assign the 2019 value of the minimum wahe in 2020 in these states in our database.

2017. We proceeded backwards and used the exact match betwen the text of the law and the six-digits categories in 2017 in Rio de Janeiro and applied this match to all previous years back to 2003. Since minimum wage laws in São Paulo and Paraná were inpired by minimum wage laws in Rio de Janeiro, we use the same matching system for these two states.

For the occupations that were designated in the law and for which we did not have a precise six-digits classification code, we made a manual matching using the similarity of words between the text of the law and the names used in the six-digits classification codes. This took care of the majority of these cases. However, there were remaining cases in which we could not link the text of the law to a classification code. [23] We were also unable to match

[23]Here is the list: *barboys, teleoperadores nível 1 a 1, operadores de call center, atendentes de cadastro, representantes de serviços empresariais, agentes de marketing, agentes de cobrança, agentes de venda, atendentes de call center, auxiliares*

the four occupations that are included in the law conditional on the worker beeing registered in their area´s council: *trabalhadores de nível técnico devidamente registrados nos conselhos de suas áreas; técnicos de nível médio regularmente inscritos nos Conselhos Regionais de Engenharia; Arquitetura e Agronomia; técnicos industriais inscritos no Conselho Regional de Técnicos Industriais*

State-level minimum wage database by industry. We used industry classifications to construct this database: the national classsification of economic activities 1.0 (CNAE 1.0) to classify industries covered by state minimum wages from 2001 to 2006; and the national classsification of economic activities 2.0 (CNAE 2.0) from 2007 to 2020.

Using CNAE 1.0 codes. Because the minimum wage laws did not refer to a specific industry code to define the industries covered by the minimum wage law, we matched the words contained in the law with the names of the industries contained in CNAE 1.0 codes. In rare cases, we were not able to find an exact match.[24]

Using CNAE 2.0 codes. We followed a similar strategy as for the match between the text of the law and CNAE 1.0 codes. In rare cases, we were not able to find an exact match. [25]

técnicos de telecom nível 1 a 3, operadores de suporte CNS, representantes de serviços 103, atendentes de retenção, operadores de atendimento nível 1 a 3, representantes de serviços, assistentes de serviços nível 1 a 3, telemarketing ativos e receptivos, práticos de farmácia, empregados em empresas prestadoras de serviços de brigada de incêndio (nível básico), empregados em empresas prestadoras de serviços de brigada de incêndio (nível médio), bombeiro civil mestre, empregados em empresas prestadoras de serviços de brigada de incêndio (nível superior), empregados não-especializados da indústria, empregados em escritórios de agências de navegação, empregados em terminais de contêineres e mestres e encarregados em estaleiros, trabalhadores marítimos do 1° grupo de Aquaviários que laboram nas secções de Convés, Máquinas, Câmara e Saúde, em todos os níveis (I, II, III, IV, V, VI, VII e superiores).

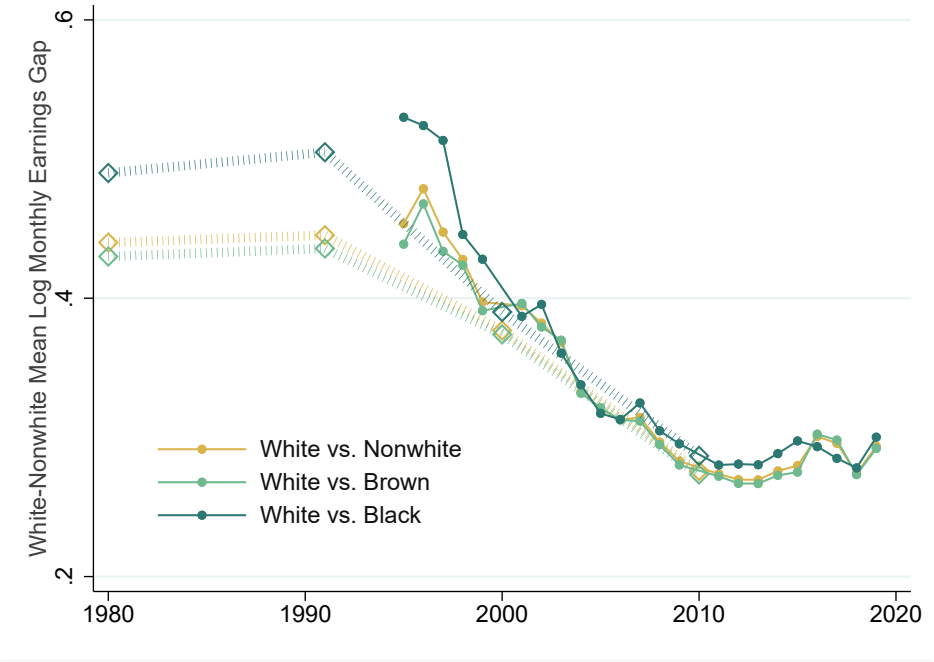
[24]Here are the categories listed in the law for which we could not find the correspondance in CNAE 1.0: *trabalhadores em estabelecimentos hípicos, empregados de agentes autônomos do comércio, indústria mecânica, indústrias de cristais, espelhos, cerâmica de louça e porcelana and auxiliares em administração escolar (empregados de estabelecimentos de ensino).* As a result, these are ommitted in our database.

[25]Here are the categories listed in the law for which we could not find the correspondance in CNAE 2.0: *trabalhadores em estabelecimentos hípicos, empregados de agentes autônomos do comércio, indústria mecânica, indústrias de cristais, espelhos, cerâmica de louça e porcelana and auxiliares em administração escolar (empregados de estabelecimentos de ensino).*

Appendix D Additional Figures & Tables

Figure D1 shows that the earnings gap between white and mixed race workers is of similar magnitude as the gap between white and black workers using the labor force surveys (PNAD and PNAD Continua) and Census data. These gaps are actually identical in both sources of data from the late 1990s to today.

Figure D1: Unadjusted racial monthly earnings gaps (1980-2020)



Source: PNAD 1986-2015. Censuses 1980-2010.
Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

Table D1 provides the values of the minimum-to-median wage in 1999 that are behind our classification of "strongly" vs. "weakly treated" states. Strongly treated are states with a minimum-to-median wage in 1999 that is above the median (i.e. above 36.3% in 1999). A map of these strongly treated states is available in Figure 6.

Table D1: Minimum-to-median wage for White workers by state, 1999

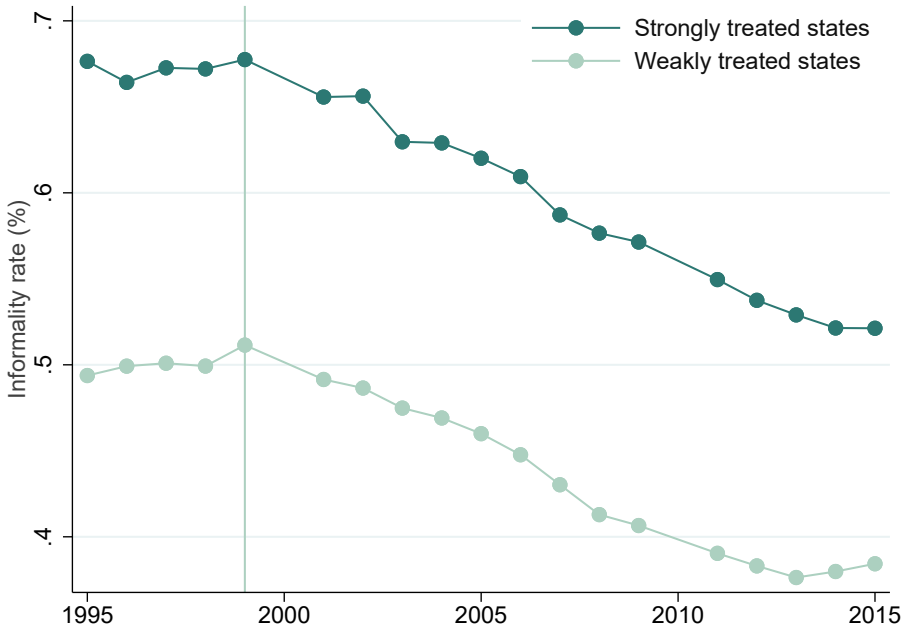
State Abb.	State Name	Minimum wage	Median wage	Ratio	Strongly treated
AC	Acre	474	2,203	0.22	0
AL	Alagoas	474	948	0.50	1
AM	Amazonas	474	1,393	0.34	0
AP	Amapá	474	2,439	0.19	0
BA	Bahia	474	1,219	0.39	1
CE	Ceará	474	1,045	0.45	1
DF	Distrito Federal	474	2,090	0.23	0
ES	Espírito Santo	474	1,393	0.34	0
GO	Goiás	474	1,219	0.39	1
MA	Maranhão	474	2,090	0.23	0
MG	Minas Gerais	474	1,219	0.39	1
MS	Mato Grosso do Sul	474	1,254	0.38	1
MT	Mato Grosso	474	1,498	0.32	0
PA	Pará	474	1,393	0.34	0
PB	Paraíba	474	1,045	0.45	1
PE	Pernambuco	474	1,045	0.45	1
PI	Piauí	474	1,045	0.45	1
PR	Paraná	474	1,393	0.34	0
RJ	Rio de Janeiro	474	1,742	0.27	0
RN	Rio Grande do Norte	474	871	0.54	1
RO	Rondônia	474	1,393	0.34	0
RR	Roraima	474	941	0.50	1
RS	Rio Grande do Sul	474	1,306	0.36	1
SC	Santa Catarina	474	1,428	0.33	0
SE	Sergipe	474	906	0.52	1
SP	São Paulo	474	1,961	0.24	0
TO	Tocantins	474	1,101	0.43	1

Source: PNAD 1999.

Sample: White adults 25-54, employed in the private sector (formal sector only), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable. No restriction on industry (so, agricultural and domestic workers are in the sample).

Figure D2 shows i) the initial levels of informality rates in the strongly vs. weakly treated states; in particular, it shows that; ii) that the magnitude of the decline is similar in both types of states (a decline of approximately 15 percentage points); iii) that adding or removing individual-level and state-level controls to our baseline employment regression does not affect the magnitude of our estimates. There is no apparent sorting on observable characteristics.

Figure D2: States with a minimum-to-median wage above the median in 1999



Sources: PNAD 1995-2015.
Sample: Adults 25-54, white or nonwhite, employed in the private sector (formal or informal), working full-time (i.e. 40 hours a week or more), no missing monthly earnings variable.

Appendix E Additional empirical exercises

E.1 Computation of transition rates in PME

We compute workers' transitions from/to unemployment, formal employment, and informal employment following the methodology and the data (PME, 2002-2007) used in Meghir et al. (2015).

E.2 Tradables

We categorize industries into tradables and non-tradables following the categorization used in Dix-Carneiro and Kovak (2019).

E.3 Bunching at multiples of the minimum wage

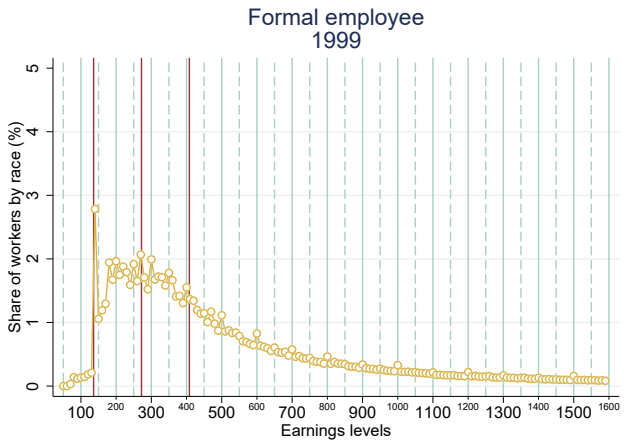
We evaluate here whether there is bunching at multiples of the minimum wage.

Methodology. We use bins of R$10 in nominal values to more easily separate bunching at multiples of the minimum wage and at round numbers (multiples of 50 and 100). In all graphs, red vertical lines indicate values equal to 1, 2, or 3 times the minimum wage; "mist" vertical lines indicate multiples of 100 (solid lines) and multiples of 50 (dash lines). Using RAIS, we created graphs both for monthly earnings and for the contracted wage in all years from 1995 or 2002 to 2017. Using the census data, we created graphs for monthly earnings in the main job in 2000 and 2010, separately for formal employees, informal employees, and the self-employed. Note that in some years, the minimum wage is close to a round number, making it hard to separate bunching at round numbers to bunching at multiples of the minimum wage. This is the case for the 2000 census, so using the 2010 census is more useful in this case.

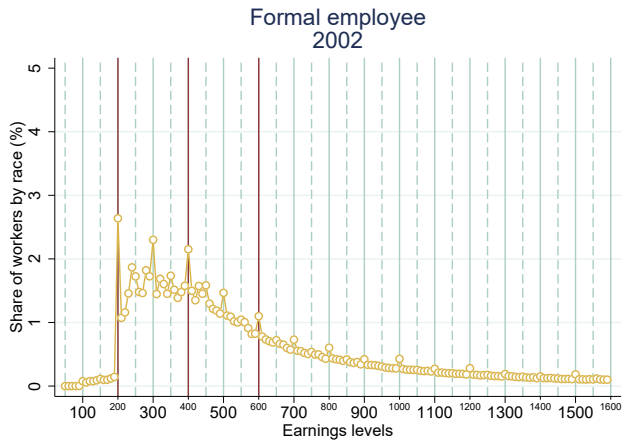
Results. Figure E1 displays the monthly earnings distribution in RAIS in 1999, 2002, 2009, and 2015. Figure E2 displays the monthly contracted wage distribution in RAIS in 2002, 2009, and 2015. Figure E3 displays the monthly earnings distribution in the 2010 census, separately for formal employees, informal employees, and the self-employed.

Figure E1: Nominal monthly earnings distributions, RAIS

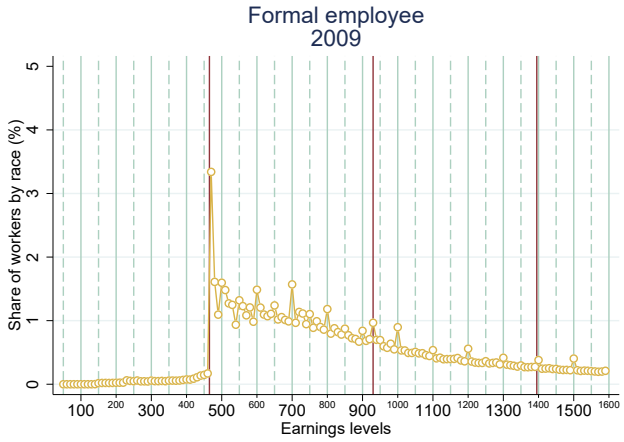
(a) 1999



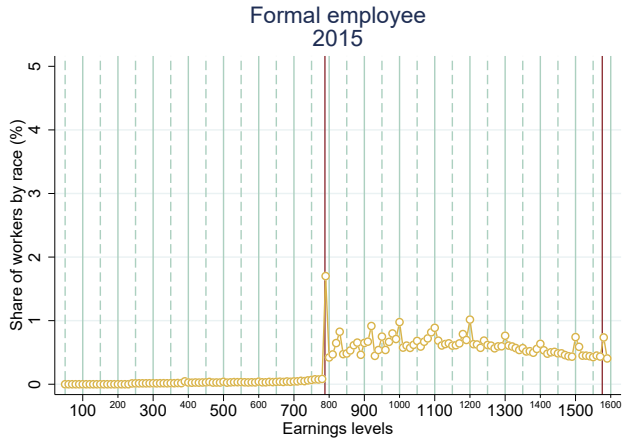
(b) 2002



(c) 2009



(d) 2015

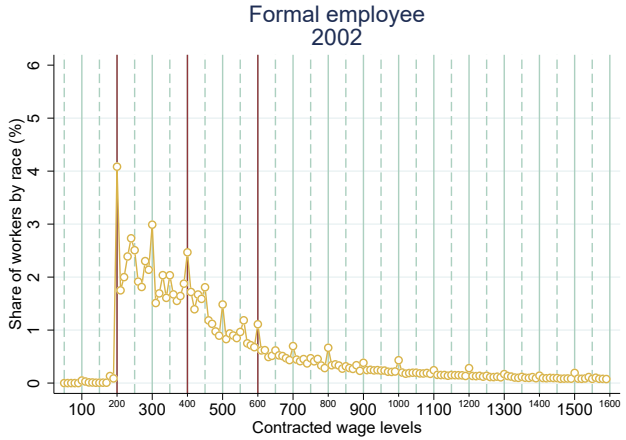


Sources: RAIS 1995-2017.

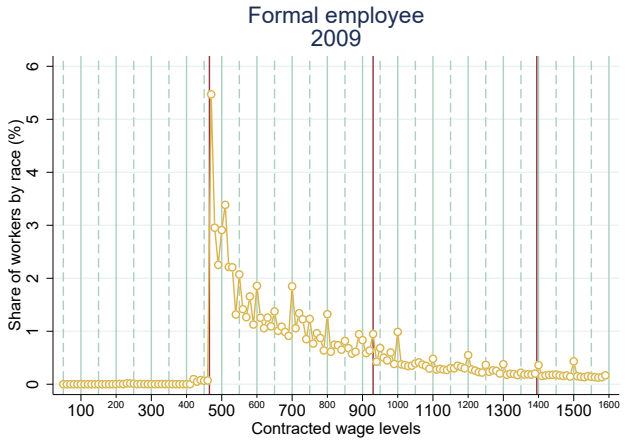
Sample: Adults 25-54, employed full-time in private-sector job on December 31st, all race categories. We use bins of R$10 in nominal value. Red vertical lines indicate values equal to 1, 2, or 3 times the minimum wage; "mist" vertical lines indicate multiples of 100 (solid lines) and multiples of 50 (dash lines).

Figure E2: Nominal monthly contracted wage distributions, RAIS

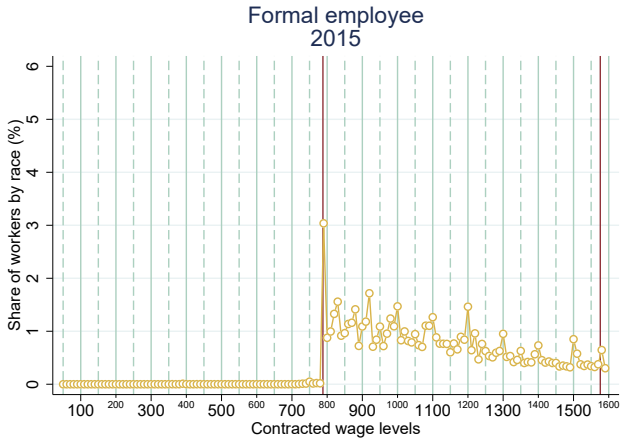
(a) 2002



(b) 2009



(c) 2015

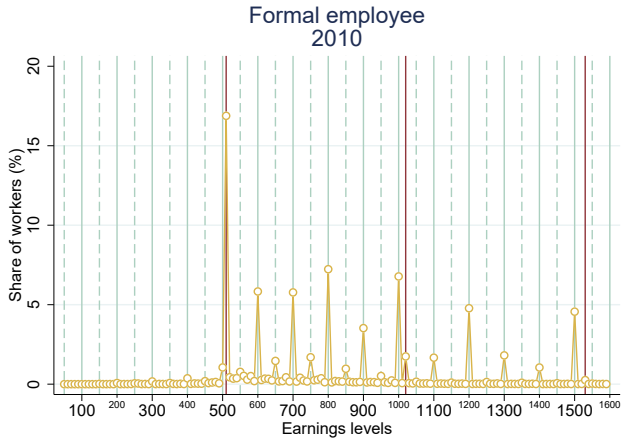


Sources: RAIS 1995-2017.

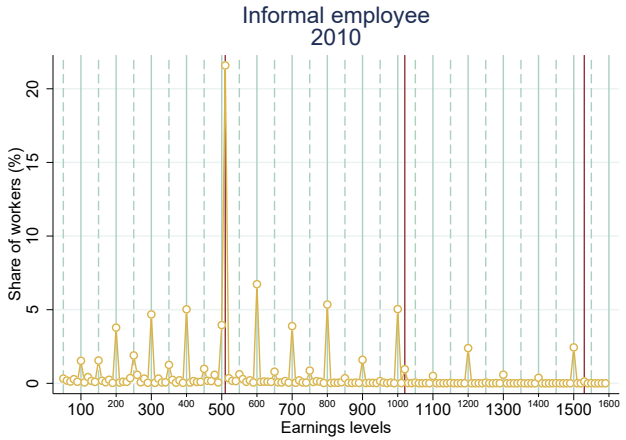
Sample: Adults 25-54, employed full-time in private-sector job on December 31st, all race categories. We use bins of R$10 in nominal value. Red vertical lines indicate values equal to 1, 2, or 3 times the minimum wage; "mist" vertical lines indicate multiples of 100 (solid lines) and multiples of 50 (dash lines).

Figure E3: Nominal monthly earnings distributions, Census 2010

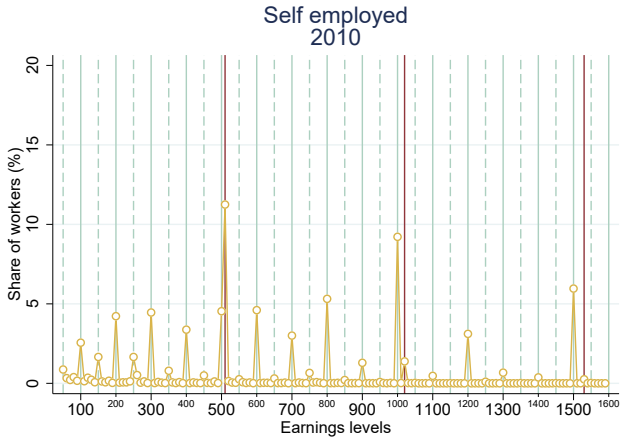
(a) Formal employees



(b) Informal employees



(c) Self-employed



Sources: Census 2010.

Sample: Adults 25-54, employed full-time in private-sector job, all race categories. We use bins of R$10 in nominal value. Red vertical lines indicate values equal to 1, 2, or 3 times the minimum wage; "mist" vertical lines indicate multiples of 100 (solid lines) and multiples of 50 (dash lines).

E.4 Difference-in-differences using data from the 2000 and 2010 censuses

We replicate here the results from our difference-in-differences analysis at the state level using data from the 2000 and 2010 censuses. We also evaluate the robustness of our results by replicating the same empirical strategy at the micro-region level.

Methodology. The treatment variables are calculated pre-reform (using the 2000 census) pooling all nonwhite and white workers together. In all specifications, we regress outcome variables on year dummies, state or micro-region dummies, the treatment variable interacted with the year 2010 dummy, and control variables. Standard errors are clustered at the state or micro-region level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared. In all tables, results are presented using both the treatment variable linearly and as a categorical variable (above vs. below median).

Replication of DD results at state level. Tables E1 and E2 display earnings effects for formal employees, informal employees, and the self-employed using the cross-state design and either the Kaitz index or the share of affected workers as treatment variable, respectively. Tables E3 and E4 display employment effects (formal employee vs. any employment status, employed vs. non-employed, formal employee vs. informal worker, formal employee vs. informal employee) using the cross-state design and either the Kaitz index or the share of affected workers as treatment variable, respectively. The results are broadly consistent with the results in the main analysis using PNAD.

Robustness of DD results at micro-region level. Tables E5 and E6 display earnings effects for formal employees, informal employees, and the self-employed using the cross-microregion design and either the Kaitz index or the share of affected workers as treatment variable, respectively. Tables E7 and E8 display employment effects (formal employee vs. any employment status, employed vs. non-employed, formal employee vs. informal worker, formal employee vs. informal employee) using the cross-microregion design and either the Kaitz index or the share of affected workers as treatment variable, respectively. Unsurprisingly, the results tend to be more precisely estimated. Point estimates for the implied cross-wage elasticities are still large but smaller than with the cross-state design. Point estimates for the implied employment elasticities are more systematically negative. In particular, the overall formal employment elasticity, which include both non-employment and informality responses, is es-

timated to be between $-.3$ and $-.2$ and between $-.4$ and $-.3$ using the Kaitz index (linearly) and the share of affected workers (linearly), respectively.

Table E1: Main earnings effects of 2000-2009 minimum wage increases using a cross-state design (Kaitz index)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employees	0.080**	0.110***	0.030	1.023***	1.069***	0.901***
	(0.036)	(0.034)	(0.043)	(0.195)	(0.166)	(0.253)
	3,851,740	1,655,420	2,196,320	3,851,740	1,655,420	2,196,320
Informal employees	0.078**	0.094***	0.042	0.842***	0.823***	0.815***
	(0.036)	(0.034)	(0.045)	(0.194)	(0.197)	(0.202)
	1,508,500	831,200	677,300	1,508,500	831,200	677,300
Cross-wage elast.	0.97	0.85	1.41	0.82	0.77	0.90
se	0.24	0.17	0.95	0.13	0.12	0.17
lower bound	0.50	0.52	-0.45	0.57	0.54	0.58
upper bound	1.44	1.18	3.27	1.07	1.00	1.23
Self-employed workers	0.012	0.037	-0.015	0.549**	0.524**	0.507*
	(0.038)	(0.039)	(0.040)	(0.247)	(0.245)	(0.269)
	2,476,243	1,026,430	1,449,813	2,476,243	1,026,430	1,449,813
Cross-wage elast.	0.14	0.34	-0.51	0.54	0.49	0.56
se	0.43	0.28	2.05	0.19	0.20	0.19
lower bound	-0.69	-0.22	-4.53	0.17	0.10	0.19
upper bound	0.98	0.89	3.51	0.90	0.88	0.94

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector in the 2000 and 2010 census.

Notes: The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the state level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E2: Main earnings effects of 2000-2009 minimum wage increases using a cross-state design (share affected workers)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employees	0.062	0.092**	0.015	0.743***	0.805***	0.617***
	(0.038)	(0.036)	(0.043)	(0.156)	(0.143)	(0.190)
	3,851,740	1,655,420	2,196,320	3,851,740	1,655,420	2,196,320
Informal employees	0.035	0.053	-0.000	0.566***	0.580***	0.489**
	(0.044)	(0.041)	(0.053)	(0.195)	(0.188)	(0.222)
	1,508,500	831,200	677,300	1,508,500	831,200	677,300
Cross-wage elast.	0.57	0.58	-0.02	0.76	0.72	0.79
se	0.42	0.26	3.69	0.15	0.14	0.20
lower bound	-0.26	0.07	-7.26	0.48	0.45	0.40
upper bound	1.40	1.09	7.22	1.05	0.99	1.19
Self-employed workers	-0.021	0.003	-0.043	0.320	0.297	0.299
	(0.044)	(0.043)	(0.045)	(0.237)	(0.229)	(0.247)
	2,476,243	1,026,430	1,449,813	2,476,243	1,026,430	1,449,813
Cross-wage elast.	-0.34	0.04	-2.93	0.43	0.37	0.49
se	0.91	0.46	11.69	0.25	0.23	0.27
lower bound	-2.12	-0.86	-25.85	-0.05	-0.09	-0.05
upper bound	1.45	0.94	19.98	0.91	0.82	1.02

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector in the 2000 and 2010 census.

Notes: The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the state level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E3: Main employment effects of 2000-2009 minimum wage increases using a cross-state design (Kaitz index)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs any status	-0.006	-0.003	-0.006	0.004	-0.009	0.017
	(0.009)	(0.008)	(0.010)	(0.048)	(0.049)	(0.042)
	15279226	7,364,223	7,915,003	15279226	7,364,223	7,915,003
Implied elast.	-0.22	-0.08	-0.54	0.01	-0.03	0.05
se	0.39	0.24	1.43	0.13	0.14	0.12
lower bound	-0.98	-0.54	-3.34	-0.24	-0.30	-0.19
upper bound	0.55	0.39	2.26	0.26	0.25	0.29
Employed vs not employed	0.014	0.018	0.010	0.136**	0.149***	0.095
	(0.011)	(0.011)	(0.011)	(0.054)	(0.053)	(0.061)
	15279226	7,364,223	7,915,003	15279226	7,364,223	7,915,003
Implied elast.	0.25	0.25	0.48	0.19	0.21	0.14
se	0.18	0.14	0.65	0.10	0.09	0.12
lower bound	-0.11	-0.03	-0.79	0.00	0.05	-0.10
upper bound	0.62	0.53	1.75	0.38	0.38	0.39
Formal employee vs any informal	-0.007	-0.011	-0.001	-0.050*	-0.088**	-0.026
	(0.005)	(0.006)	(0.005)	(0.027)	(0.037)	(0.023)
	7,534,296	3,312,129	4,222,167	7,534,296	3,312,129	4,222,167
Implied elast.	-0.14	-0.17	-0.07	-0.08	-0.14	-0.05
se	0.14	0.12	0.31	0.05	0.08	0.04
lower bound	-0.42	-0.41	-0.68	-0.19	-0.30	-0.13
upper bound	0.14	0.08	0.54	0.02	0.01	0.03
Formal vs informal employee	0.011	0.006	0.015	0.036	-0.001	0.077**
	(0.009)	(0.010)	(0.009)	(0.039)	(0.049)	(0.034)
	5,360,240	2,486,620	2,873,620	5,360,240	2,486,620	2,873,620
Implied elast.	0.17	0.07	0.60	0.05	-0.00	0.10
se	0.14	0.12	0.84	0.04	0.06	0.05
lower bound	-0.11	-0.16	-1.04	-0.04	-0.12	0.01
upper bound	0.45	0.30	2.25	0.13	0.12	0.19

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the state level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E4: Main employment effects of 2000-2009 minimum wage increases using a cross-state design (share affected workers)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs any status	-0.011	-0.009	-0.009	-0.056	-0.045	-0.059
	(0.010)	(0.009)	(0.010)	(0.050)	(0.037)	(0.058)
	15279226	7,364,223	7,915,003	15279226	7,364,223	7,915,003
Implied elast.	-0.48	-0.29	-1.61	-0.20	-0.17	-0.24
se	0.71	0.39	6.16	0.22	0.17	0.28
lower bound	-1.86	-1.05	-13.70	-0.64	-0.50	-0.79
upper bound	0.90	0.47	10.47	0.23	0.16	0.32
Employed vs not employed	0.005	0.007	0.004	0.067	0.094*	0.018
	(0.013)	(0.013)	(0.013)	(0.057)	(0.050)	(0.065)
	15279226	7,364,223	7,915,003	15279226	7,364,223	7,915,003
Implied elast.	0.12	0.12	0.40	0.13	0.18	0.04
se	0.28	0.19	1.16	0.11	0.10	0.14
lower bound	-0.42	-0.24	-1.88	-0.08	-0.01	-0.24
upper bound	0.66	0.49	2.69	0.34	0.36	0.32
Formal employee vs any informal	-0.011**	-0.017***	-0.005	-0.066**	-0.081**	-0.047*
	(0.005)	(0.005)	(0.005)	(0.028)	(0.033)	(0.026)
	7,534,296	3,312,129	4,222,167	7,534,296	3,312,129	4,222,167
Implied elast.	-0.31	-0.32	-0.61	-0.15	-0.17	-0.13
se	0.30	0.20	2.08	0.10	0.10	0.10
lower bound	-0.90	-0.72	-4.69	-0.34	-0.37	-0.32
upper bound	0.27	0.08	3.46	0.04	0.03	0.06
Formal vs informal employee	-0.004	-0.010	0.001	0.000	-0.029	0.029
	(0.010)	(0.010)	(0.010)	(0.038)	(0.049)	(0.038)
	5,360,240	2,486,620	2,873,620	5,360,240	2,486,620	2,873,620
Implied elast.	-0.08	-0.15	0.10	0.00	-0.05	0.06
se	0.24	0.20	0.76	0.06	0.09	0.07
lower bound	-0.55	-0.55	-1.38	-0.13	-0.23	-0.07
upper bound	0.39	0.25	1.58	0.13	0.13	0.19

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the state level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E5: Main earnings effects of 2000-2009 minimum wage increases using a cross-microregion design (Kaitz index)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employees	0.085***	0.093***	0.062***	0.930***	0.857***	0.914***
	(0.014)	(0.014)	(0.016)	(0.090)	(0.090)	(0.089)
	3,851,740	1,655,420	2,196,320	3,851,740	1,655,420	2,196,320
Informal employees	0.050***	0.045***	0.044***	0.517***	0.410***	0.624***
	(0.011)	(0.012)	(0.012)	(0.110)	(0.101)	(0.115)
	1,508,500	831,200	677,300	1,508,500	831,200	677,300
Cross-wage elast.	0.59	0.48	0.71	0.56	0.48	0.68
se	0.13	0.11	0.17	0.07	0.07	0.07
lower bound	0.35	0.26	0.37	0.42	0.34	0.55
upper bound	0.84	0.71	1.05	0.69	0.62	0.82
Self-employed workers	0.018*	0.012	0.012	0.346***	0.191**	0.461***
	(0.010)	(0.011)	(0.013)	(0.084)	(0.079)	(0.080)
	2,476,243	1,026,430	1,449,813	2,476,243	1,026,430	1,449,813
Cross-wage elast.	0.21	0.13	0.19	0.37	0.22	0.50
se	0.11	0.12	0.18	0.06	0.07	0.05
lower bound	-0.01	-0.09	-0.16	0.26	0.08	0.41
upper bound	0.44	0.36	0.55	0.49	0.36	0.60

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector in the 2000 and 2010 census.

Notes: The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E6: Main earnings effects of 2000-2009 minimum wage increases using a cross-microregion design (share affected workers)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employees	0.070***	0.081***	0.045***	0.681***	0.675***	0.606***
	(0.014)	(0.014)	(0.016)	(0.076)	(0.076)	(0.079)
	3,851,740	1,655,420	2,196,320	3,851,740	1,655,420	2,196,320
Informal employees	0.039***	0.037***	0.027**	0.453***	0.391***	0.481***
	(0.011)	(0.012)	(0.013)	(0.089)	(0.087)	(0.090)
	1,508,500	831,200	677,300	1,508,500	831,200	677,300
Cross-wage elast.	0.55	0.45	0.61	0.67	0.58	0.79
se	0.14	0.13	0.22	0.06	0.07	0.07
lower bound	0.27	0.20	0.18	0.54	0.44	0.66
upper bound	0.83	0.71	1.04	0.79	0.72	0.93
Self-employed workers	0.007	0.005	-0.002	0.306***	0.189***	0.365***
	(0.011)	(0.011)	(0.013)	(0.066)	(0.070)	(0.063)
	2,476,243	1,026,430	1,449,813	2,476,243	1,026,430	1,449,813
Cross-wage elast.	0.10	0.06	-0.05	0.45	0.28	0.60
se	0.15	0.14	0.31	0.06	0.08	0.06
lower bound	-0.19	-0.21	-0.65	0.34	0.13	0.49
upper bound	0.39	0.33	0.56	0.56	0.43	0.71

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector in the 2000 and 2010 census.

Notes: The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E7: Main employment effects of 2000-2009 minimum wage increases using a cross-microregion design (Kaitz index)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs any status	-0.019***	-0.018***	-0.019***	-0.082***	-0.085***	-0.063***
	(0.003)	(0.004)	(0.003)	(0.013)	(0.011)	(0.018)
	15279226	7,364,223	7,915,003	15279226	7,364,223	7,915,003
Implied elast.	-0.61	-0.57	-0.74	-0.24	-0.30	-0.17
se	0.13	0.11	0.22	0.06	0.06	0.06
lower bound	-0.85	-0.79	-1.17	-0.36	-0.42	-0.30
upper bound	-0.36	-0.35	-0.31	-0.12	-0.18	-0.04
Employed vs not employed	-0.004	-0.004	-0.005	-0.027	-0.019	-0.041
	(0.007)	(0.008)	(0.005)	(0.026)	(0.028)	(0.027)
	15279226	7,364,223	7,915,003	15279226	7,364,223	7,915,003
Implied elast.	-0.08	-0.07	-0.12	-0.04	-0.03	-0.06
se	0.11	0.14	0.11	0.04	0.05	0.04
lower bound	-0.30	-0.33	-0.34	-0.13	-0.13	-0.15
upper bound	0.15	0.20	0.10	0.04	0.07	0.02
Formal employee vs any informal	-0.014***	-0.017***	-0.010***	-0.041***	-0.053***	-0.024
	(0.003)	(0.004)	(0.003)	(0.015)	(0.018)	(0.015)
	7,534,296	3,312,129	4,222,167	7,534,296	3,312,129	4,222,167
Implied elast.	-0.28	-0.32	-0.28	-0.07	-0.11	-0.04
se	0.08	0.09	0.12	0.03	0.04	0.03
lower bound	-0.44	-0.49	-0.50	-0.13	-0.19	-0.10
upper bound	-0.12	-0.15	-0.05	-0.02	-0.03	0.01
Formal vs informal employee	-0.013**	-0.019***	-0.006	-0.033*	-0.046*	-0.014
	(0.006)	(0.007)	(0.005)	(0.020)	(0.024)	(0.021)
	5,360,240	2,486,620	2,873,620	5,360,240	2,486,620	2,873,620
Implied elast.	-0.19	-0.28	-0.11	-0.05	-0.07	-0.02
se	0.09	0.11	0.11	0.03	0.04	0.03
lower bound	-0.38	-0.49	-0.32	-0.10	-0.16	-0.07
upper bound	-0.01	-0.06	0.10	0.01	0.01	0.04

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E8: Main employment effects of 2000-2009 minimum wage increases using a cross-microregion design (share affected workers)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs any status	-0.024***	-0.022***	-0.023***	-0.092***	-0.090***	-0.077***
	(0.004)	(0.004)	(0.004)	(0.011)	(0.010)	(0.013)
	15279226	7,364,223	7,915,003	15279226	7,364,223	7,915,003
Implied elast.	-0.92	-0.80	-1.28	-0.37	-0.40	-0.32
se	0.19	0.14	0.49	0.08	0.08	0.09
lower bound	-1.31	-1.09	-2.25	-0.53	-0.56	-0.50
upper bound	-0.54	-0.52	-0.32	-0.20	-0.25	-0.14
Employed vs not employed	-0.012*	-0.012	-0.010*	-0.032	-0.028	-0.038*
	(0.007)	(0.008)	(0.005)	(0.022)	(0.025)	(0.020)
	15279226	7,364,223	7,915,003	15279226	7,364,223	7,915,003
Implied elast.	-0.24	-0.22	-0.30	-0.07	-0.06	-0.09
se	0.13	0.14	0.18	0.05	0.06	0.05
lower bound	-0.50	-0.50	-0.65	-0.17	-0.18	-0.19
upper bound	0.02	0.06	0.05	0.03	0.05	0.01
Formal employee vs any informal	-0.018***	-0.021***	-0.012***	-0.046***	-0.057***	-0.031**
	(0.004)	(0.005)	(0.004)	(0.012)	(0.015)	(0.013)
	7,534,296	3,312,129	4,222,167	7,534,296	3,312,129	4,222,167
Implied elast.	-0.42	-0.45	-0.45	-0.11	-0.14	-0.09
se	0.12	0.11	0.21	0.03	0.05	0.04
lower bound	-0.65	-0.66	-0.86	-0.18	-0.23	-0.16
upper bound	-0.19	-0.23	-0.03	-0.05	-0.06	-0.01
Formal vs informal employee	-0.020***	-0.025***	-0.013***	-0.040**	-0.057***	-0.019
	(0.005)	(0.006)	(0.005)	(0.017)	(0.020)	(0.017)
	5,360,240	2,486,620	2,873,620	5,360,240	2,486,620	2,873,620
Implied elast.	-0.36	-0.43	-0.37	-0.07	-0.12	-0.04
se	0.13	0.13	0.21	0.03	0.05	0.03
lower bound	-0.62	-0.69	-0.77	-0.14	-0.21	-0.11
upper bound	-0.11	-0.17	0.04	-0.01	-0.02	0.03

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

E.5 Heterogeneity by enforcement intensity

We explore here the heterogeneity of our difference-in-differences results by enforcement intensity.

Methodology. In the Brazilian context, the literature uses as measure of labor enforcement intensity the distance between a local labor market and the closest labor office (Almeida and Carneiro, 2012; Ponczek and Ulyssea, 2021). As explained in Ponczek and Ulyssea (2021):

> *The Ministry of Labor is directly responsible for enforcing labor regulations, but it only inspects registered firms and therefore it does not tackle informal labor in informal firms. Enforcement is implemented in a very decentralized way, both at the state level (with a labor office called "delegacia do trabalho") and within states through local labor offices called "subdelegacias." The state level office (delegacia) is always located in the state's capital and the local offices (subdelegacias) are spread out across municipalities. The number of local offices is a function of the state's size and economic relevance (Almeida and Carneiro, 2012).*
>
> *Inspectors are allocated to a specific subdelegacia and they travel by car to inspect firms. Most inspections are triggered by anonymous reports and inspectors must assess compliance with all the relevant dimensions of the labor code and not only if workers are formally registered or not. There is substantial regional, within-state variation in enforcement intensity (Almeida and Carneiro, 2012). In particular, one of the factors that determines regional variation in enforcement is the relative density of local labor offices, which by its turn determines the travel distances that inspectors face in order to carry out inspections. Thus, these travel distances are a key determinant of the capacity of enforcement across local labor markets.*

We obtain data on the minimum distance between a local labor office and each municipality in driving times in 2000, as well as the number of inspectors per state in 2000, from the replication files of Almeida and Carneiro (2012). We then follow Ponczek and Ulyssea (2021) by defining the local labor market as the micro-region and by using the maximum distance to a local labor office among all the municipalities belonging to each micro-region as the measure of "weaker enforcement" at the micro-region level.

As in the previous subsection, we use data from the 2000 and 2010 census; the treatment variables are calculated pre-reform (using the 2000 census) pooling all nonwhite and

white workers together. In all specifications, we regress outcome variables on year dummies, micro-region dummies, the treatment variable interacted with the year 2010 dummy, the enforcement intensity variable interacted with the year 2010 dummy, and the triple interaction between the treatment variable, the enforcement intensity variable, and the year 2010 dummy. Standard errors are clustered at the micro-region level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared as in the previous subsection. We also include the triple interaction between the enforcement intensity variable, the number of inspectors in the state in 2000 and the year 2010 dummy to follow the main specification in Ponczek and Ulyssea (2021).

Results Tables E9 and E10 display the heterogeneity in earnings effects for formal employees, informal employees, and the self-employed using the cross–microregion design and either the Kaitz index or the share of affected workers as treatment variable, respectively. Earnings effects for formal employees are systematically smaller in labor markets with weaker enforcement intensity (further away from a labor office). This result also carries over to earnings effects for informal employees and the self-employed. Tables E11 and E12 then display the implied cross-wage elasticities at three different levels of enforcement intensity: the percentiles 0, 25, and 50 of the distance to the closest labor office. The cross-wage elasticities remain significant at all three levels (using the treatment variables linearly), and point estimates are relatively similar as well.

Tables E13 and E14 display the heterogeneity in employment effects (formal employee vs. any employment status, employed vs. non-employed, formal employee vs. informal worker, formal employee vs. informal employee) using the cross–microregion design and either the Kaitz index or the share of affected workers as treatment variable, respectively. Employment effects are either unaffected or slightly more negative in labor markets with weaker enforcement intensity. Tables E15, E16, E17 and E18 then display the implied employment elasticities at three different levels of enforcement intensity: the percentiles 0, 25, and 50 of the distance to the closest labor office. The employment elasticities appear to be (slightly) more negative in labor markets with weaker enforcement intensity.

Table E9: Heterogeneity by enforcement intensity in earnings effects of 2000-2009 minimum wage increases using a cross-microregion design (Kaitz index)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employees						
Strongly treated	0.185***	0.173***	0.171***	1.135***	1.092***	1.099***
	(0.032)	(0.035)	(0.029)	(0.081)	(0.094)	(0.075)
Strongly treated × Weaker enforcement	-0.045***	-0.037***	-0.049***	-0.164***	-0.153***	-0.181***
	(0.014)	(0.013)	(0.014)	(0.030)	(0.032)	(0.030)
	3,759,336	1,590,188	2,169,148	3,759,336	1,590,188	2,169,148
Informal employees						
Strongly treated	0.120***	0.092**	0.136***	0.784***	0.644***	0.890***
	(0.034)	(0.036)	(0.032)	(0.158)	(0.180)	(0.134)
Strongly treated × Weaker enforcement	-0.027**	-0.016	-0.039***	-0.142***	-0.110**	-0.174***
	(0.011)	(0.011)	(0.012)	(0.051)	(0.055)	(0.047)
	1,434,643	774,569	660,074	1,434,643	774,569	660,074
Self-employed workers						
Strongly treated	0.051**	0.014	0.067***	0.540***	0.316**	0.666***
	(0.024)	(0.026)	(0.024)	(0.121)	(0.158)	(0.089)
Strongly treated × Weaker enforcement	-0.015*	-0.003	-0.025***	-0.118***	-0.069	-0.152***
	(0.009)	(0.008)	(0.010)	(0.040)	(0.047)	(0.037)
	2,337,428	923,864	1,413,564	2,337,428	923,864	1,413,564

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E10: Heterogeneity by enforcement intensity in earnings effects of 2000-2009 minimum wage increases using a cross-microregion design (share affected workers)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employees						
Strongly treated	0.178***	0.169***	0.161***	0.884***	0.880***	0.825***
	(0.032)	(0.035)	(0.029)	(0.090)	(0.089)	(0.092)
Strongly treated × Weaker enforcement	-0.048***	-0.040***	-0.052***	-0.149***	-0.128***	-0.177***
	(0.014)	(0.014)	(0.014)	(0.036)	(0.031)	(0.041)
	3,759,336	1,590,188	2,169,148	3,759,336	1,590,188	2,169,148
Informal employees						
Strongly treated	0.111***	0.086**	0.121***	0.638***	0.550***	0.684***
	(0.034)	(0.036)	(0.032)	(0.130)	(0.142)	(0.123)
Strongly treated × Weaker enforcement	-0.028**	-0.017	-0.039***	-0.103**	-0.076*	-0.133***
	(0.012)	(0.011)	(0.012)	(0.042)	(0.042)	(0.046)
	1,434,643	774,569	660,074	1,434,643	774,569	660,074
Self-employed workers						
Strongly treated	0.042*	0.009	0.056**	0.439***	0.276**	0.518***
	(0.024)	(0.026)	(0.024)	(0.099)	(0.123)	(0.085)
Strongly treated × Weaker enforcement	-0.016*	-0.003	-0.027***	-0.092***	-0.056	-0.118***
	(0.009)	(0.008)	(0.010)	(0.035)	(0.036)	(0.038)
	2,337,428	923,864	1,413,564	2,337,428	923,864	1,413,564

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E11: Heterogeneity by enforcement intensity in cross-wage elastitici-ties of 2000-2009 minimum wage increases using a cross-microregion design (Kaitz index)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Informal employees						
Elast. p00 of distance	0.65	0.53	0.80	0.69	0.59	0.81
se	0.11	0.13	0.11	0.09	0.12	0.08
lower bound	0.44	0.27	0.58	0.50	0.36	0.65
upper bound	0.87	0.79	1.02	0.88	0.82	0.96
Elast. p25 of distance	0.68	0.57	0.80	0.64	0.56	0.76
se	0.13	0.14	0.14	0.08	0.09	0.07
lower bound	0.43	0.29	0.52	0.49	0.38	0.62
upper bound	0.94	0.84	1.08	0.79	0.73	0.90
Elast. p50 of distance	0.72	0.61	0.80	0.60	0.53	0.72
se	0.17	0.16	0.21	0.07	0.07	0.07
lower bound	0.39	0.29	0.38	0.47	0.38	0.57
upper bound	1.05	0.93	1.22	0.73	0.67	0.86
Self-employed						
Elast. p00 of distance	0.28	0.08	0.39	0.48	0.29	0.61
se	0.10	0.14	0.10	0.08	0.12	0.05
lower bound	0.08	-0.19	0.19	0.33	0.05	0.50
upper bound	0.48	0.36	0.60	0.63	0.53	0.71
Elast. p25 of distance	0.24	0.09	0.30	0.41	0.25	0.53
se	0.10	0.13	0.13	0.06	0.10	0.05
lower bound	0.04	-0.17	0.05	0.28	0.06	0.43
upper bound	0.45	0.34	0.55	0.53	0.44	0.63
Elast. p50 of distance	0.20	0.09	0.15	0.35	0.21	0.46
se	0.13	0.14	0.21	0.06	0.08	0.07
lower bound	-0.05	-0.18	-0.26	0.23	0.06	0.33
upper bound	0.45	0.36	0.56	0.47	0.37	0.60

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E12: Heterogeneity by enforcement intensity in cross-wage elastitici-ties of 2000-2009 minimum wage increases using a cross-microregion design (share affected workers)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Informal employees						
Elast. p00 of distance	0.62	0.50	0.75	0.72	0.62	0.83
se	0.12	0.14	0.12	0.08	0.10	0.07
lower bound	0.39	0.23	0.52	0.56	0.42	0.69
upper bound	0.85	0.78	0.99	0.88	0.83	0.97
Elast. p25 of distance	0.65	0.54	0.75	0.73	0.63	0.87
se	0.14	0.15	0.16	0.08	0.09	0.08
lower bound	0.38	0.25	0.43	0.58	0.46	0.70
upper bound	0.93	0.84	1.07	0.88	0.80	1.03
Elast. p50 of distance	0.69	0.59	0.74	0.74	0.64	0.90
se	0.19	0.18	0.29	0.08	0.08	0.11
lower bound	0.31	0.23	0.18	0.58	0.48	0.69
upper bound	1.07	0.95	1.30	0.90	0.80	1.12
Self-employed						
Elast. p00 of distance	0.24	0.05	0.35	0.50	0.31	0.63
se	0.11	0.15	0.12	0.07	0.11	0.05
lower bound	0.02	-0.24	0.12	0.36	0.10	0.52
upper bound	0.45	0.34	0.57	0.63	0.53	0.73
Elast. p25 of distance	0.17	0.04	0.18	0.46	0.28	0.61
se	0.12	0.14	0.16	0.06	0.10	0.06
lower bound	-0.06	-0.24	-0.13	0.33	0.09	0.49
upper bound	0.40	0.32	0.49	0.58	0.47	0.73
Elast. p50 of distance	0.07	0.02	-0.17	0.42	0.25	0.59
se	0.16	0.16	0.37	0.07	0.09	0.10
lower bound	-0.25	-0.30	-0.90	0.28	0.07	0.40
upper bound	0.39	0.34	0.56	0.56	0.43	0.77

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite, employed in the private sector in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E13: Heterogeneity by enforcement intensity in employment effects of 2000-2009 minimum wage increases using a cross-microregion design (Kaitz index)

	Strongly treated (Kaitz Index) (dummy variable)			(linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs. any status						
Strongly treated	-0.016***	-0.018***	-0.011**	-0.060***	-0.084***	-0.027
	(0.005)	(0.005)	(0.005)	(0.016)	(0.015)	(0.018)
Strongly treated × Weaker enforcement	-0.002	-0.001	-0.003**	-0.018**	-0.006	-0.030***
	(0.002)	(0.002)	(0.002)	(0.008)	(0.008)	(0.008)
	14605874	6,856,771	7,749,103	14605874	6,856,771	7,749,103
	All	Nonwhite	White	All	Nonwhite	White
Employed vs. not employed						
Strongly treated	0.009	0.006	0.009	0.005	-0.009	0.006
	(0.010)	(0.012)	(0.008)	(0.033)	(0.036)	(0.032)
Strongly treated × Weaker enforcement	-0.007**	-0.006*	-0.007***	-0.020	-0.007	-0.031***
	(0.003)	(0.003)	(0.003)	(0.013)	(0.014)	(0.012)
	14605874	6,856,771	7,749,103	14605874	6,856,771	7,749,103
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs. any informal						
Strongly treated	-0.004	-0.007	-0.001	-0.029	-0.038	-0.024
	(0.006)	(0.007)	(0.006)	(0.022)	(0.023)	(0.023)
Strongly treated × Weaker enforcement	-0.003	-0.003	-0.003	-0.015	-0.013	-0.009
	(0.002)	(0.002)	(0.002)	(0.011)	(0.011)	(0.010)
	7,263,572	3,115,216	4,148,356	7,263,572	3,115,216	4,148,356
	All	Nonwhite	White	All	Nonwhite	White
Formal vs. informal employee						
Strongly treated	0.004	-0.001	0.012	-0.024	-0.018	-0.021
	(0.009)	(0.010)	(0.009)	(0.028)	(0.029)	(0.032)
Strongly treated × Weaker enforcement	-0.005*	-0.005*	-0.006*	-0.013	-0.020	-0.006
	(0.003)	(0.003)	(0.003)	(0.014)	(0.014)	(0.015)
	5,193,979	2,364,757	2,829,222	5,193,979	2,364,757	2,829,222

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E14: Heterogeneity by enforcement intensity in employment effects of 2000-2009 minimum wage increases using a cross-microregion design (share affected workers)

	Strongly treated (share affected workers) (dummy variable)			(linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs. any status						
Strongly treated	-0.021***	-0.021***	-0.016***	-0.063***	-0.075***	-0.039***
	(0.005)	(0.006)	(0.005)	(0.013)	(0.012)	(0.014)
Strongly treated × Weaker enforcement	-0.002	-0.001	-0.003**	-0.022***	-0.014***	-0.028***
	(0.002)	(0.002)	(0.002)	(0.006)	(0.005)	(0.006)
	14605874	6,856,771	7,749,103	14605874	6,856,771	7,749,103
	All	Nonwhite	White	All	Nonwhite	White
Employed vs. not employed						
Strongly treated	0.005	0.002	0.008	0.011	0.004	0.007
	(0.010)	(0.012)	(0.008)	(0.025)	(0.028)	(0.024)
Strongly treated × Weaker enforcement	-0.008***	-0.007**	-0.008***	-0.026***	-0.019*	-0.028***
	(0.003)	(0.003)	(0.003)	(0.009)	(0.011)	(0.009)
	14605874	6,856,771	7,749,103	14605874	6,856,771	7,749,103
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs. any informal						
Strongly treated	-0.006	-0.009	-0.002	-0.028	-0.037*	-0.020
	(0.006)	(0.007)	(0.006)	(0.018)	(0.019)	(0.019)
Strongly treated × Weaker enforcement	-0.004*	-0.004	-0.003	-0.016*	-0.017*	-0.010
	(0.002)	(0.002)	(0.002)	(0.008)	(0.009)	(0.008)
	7,263,572	3,115,216	4,148,356	7,263,572	3,115,216	4,148,356
	All	Nonwhite	White	All	Nonwhite	White
Formal vs. informal employee						
Strongly treated	0.000	-0.004	0.007	-0.020	-0.024	-0.009
	(0.009)	(0.009)	(0.009)	(0.024)	(0.025)	(0.026)
Strongly treated × Weaker enforcement	-0.007**	-0.007**	-0.007**	-0.017	-0.023**	-0.009
	(0.003)	(0.003)	(0.003)	(0.011)	(0.011)	(0.012)
	5,193,979	2,364,757	2,829,222	5,193,979	2,364,757	2,829,222

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E15: Heterogeneity by enforcement intensity in employment elasticities of 2000-2009 minimum wage increases using a cross-microregion design (Kaitz index)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs. any status						
Elast. p00 of distance	-0.24	-0.31	-0.17	-0.15	-0.23	-0.06
se	0.08	0.09	0.08	0.05	0.05	0.04
lower bound	-0.40	-0.49	-0.32	-0.24	-0.33	-0.14
upper bound	-0.09	-0.14	-0.01	-0.05	-0.13	0.02
Elast. p25 of distance	-0.45	-0.49	-0.42	-0.27	-0.32	-0.22
se	0.09	0.10	0.11	0.05	0.05	0.05
lower bound	-0.63	-0.69	-0.63	-0.36	-0.43	-0.31
upper bound	-0.26	-0.29	-0.21	-0.17	-0.22	-0.12
Elast. p50 of distance	-0.70	-0.68	-0.82	-0.37	-0.40	-0.35
se	0.15	0.14	0.26	0.06	0.07	0.07
lower bound	-1.00	-0.95	-1.33	-0.49	-0.53	-0.49
upper bound	-0.40	-0.40	-0.32	-0.25	-0.27	-0.22
Employed vs. not employed						
Elast. p00 of distance	0.07	0.06	0.07	0.01	-0.01	0.01
se	0.08	0.11	0.06	0.04	0.05	0.04
lower bound	-0.08	-0.15	-0.05	-0.08	-0.11	-0.07
upper bound	0.22	0.27	0.20	0.09	0.09	0.09
Elast. p25 of distance	-0.01	-0.03	-0.02	-0.04	-0.04	-0.07
se	0.10	0.13	0.08	0.05	0.05	0.04
lower bound	-0.20	-0.28	-0.18	-0.13	-0.14	-0.15
upper bound	0.18	0.23	0.14	0.05	0.07	0.01
Elast. p50 of distance	-0.12	-0.11	-0.17	-0.08	-0.05	-0.13
se	0.13	0.17	0.13	0.06	0.07	0.05
lower bound	-0.38	-0.44	-0.43	-0.19	-0.19	-0.23
upper bound	0.15	0.22	0.09	0.03	0.08	-0.03

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E16: Heterogeneity by enforcement intensity in employment elasticities of 2000-2009 minimum wage increases using a cross-microregion design (Kaitz index)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs. any informal						
Elast. p00 of distance	-0.04	-0.07	-0.01	-0.04	-0.06	-0.04
se	0.05	0.07	0.06	0.03	0.03	0.03
lower bound	-0.14	-0.20	-0.12	-0.10	-0.13	-0.10
upper bound	0.07	0.06	0.10	0.02	0.01	0.03
Elast. p25 of distance	-0.13	-0.16	-0.09	-0.10	-0.12	-0.08
se	0.06	0.07	0.07	0.03	0.04	0.03
lower bound	-0.24	-0.30	-0.23	-0.16	-0.19	-0.14
upper bound	-0.02	-0.02	0.04	-0.04	-0.04	-0.01
Elast. p50 of distance	-0.24	-0.26	-0.23	-0.14	-0.16	-0.11
se	0.09	0.10	0.12	0.04	0.05	0.05
lower bound	-0.42	-0.45	-0.47	-0.23	-0.26	-0.21
upper bound	-0.07	-0.07	0.01	-0.05	-0.06	-0.02
Formal vs. informal employee						
Elast. p00 of distance	0.03	-0.01	0.09	-0.03	-0.02	-0.02
se	0.06	0.07	0.06	0.03	0.04	0.03
lower bound	-0.09	-0.16	-0.03	-0.09	-0.09	-0.09
upper bound	0.15	0.14	0.21	0.03	0.05	0.04
Elast. p25 of distance	-0.04	-0.10	0.05	-0.06	-0.08	-0.04
se	0.07	0.09	0.07	0.03	0.04	0.03
lower bound	-0.17	-0.27	-0.09	-0.12	-0.15	-0.11
upper bound	0.09	0.07	0.18	-0.00	-0.00	0.02
Elast. p50 of distance	-0.12	-0.20	-0.02	-0.09	-0.12	-0.06
se	0.10	0.12	0.11	0.04	0.05	0.05
lower bound	-0.31	-0.43	-0.24	-0.18	-0.22	-0.15
upper bound	0.07	0.03	0.20	-0.01	-0.02	0.03

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "Kaitz index" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E17: Heterogeneity by enforcement intensity in employment elasticities of 2000-2009 minimum wage increases using a cross-microregion design (share affected workers)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs. any status						
Elast. p00 of distance	-0.32	-0.37	-0.25	-0.19	-0.26	-0.12
se	0.09	0.10	0.09	0.05	0.06	0.05
lower bound	-0.49	-0.56	-0.43	-0.30	-0.37	-0.22
upper bound	-0.15	-0.18	-0.06	-0.09	-0.15	-0.02
Elast. p25 of distance	-0.61	-0.63	-0.62	-0.39	-0.42	-0.36
se	0.11	0.12	0.15	0.07	0.06	0.07
lower bound	-0.84	-0.86	-0.92	-0.52	-0.55	-0.50
upper bound	-0.39	-0.40	-0.32	-0.27	-0.30	-0.22
Elast. p50 of distance	-1.04	-0.95	-1.42	-0.57	-0.56	-0.63
se	0.24	0.19	0.58	0.09	0.08	0.13
lower bound	-1.50	-1.32	-2.55	-0.75	-0.72	-0.88
upper bound	-0.57	-0.58	-0.29	-0.39	-0.39	-0.37
Employed vs. not employed						
Elast. p00 of distance	0.04	0.02	0.07	0.02	0.01	0.01
se	0.08	0.11	0.07	0.04	0.05	0.04
lower bound	-0.12	-0.19	-0.07	-0.06	-0.09	-0.06
upper bound	0.20	0.24	0.21	0.10	0.10	0.09
Elast. p25 of distance	-0.09	-0.12	-0.08	-0.06	-0.05	-0.09
se	0.10	0.13	0.10	0.05	0.06	0.05
lower bound	-0.30	-0.38	-0.27	-0.16	-0.17	-0.18
upper bound	0.11	0.15	0.11	0.04	0.06	0.01
Elast. p50 of distance	-0.29	-0.28	-0.39	-0.13	-0.10	-0.19
se	0.15	0.18	0.22	0.07	0.08	0.07
lower bound	-0.59	-0.63	-0.81	-0.26	-0.25	-0.33
upper bound	0.01	0.07	0.04	-0.00	0.05	-0.06

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.

Table E18: Heterogeneity by enforcement intensity in employment elasticities of 2000-2009 minimum wage increases using a cross-microregion design (share affected workers)

	Strongly treated (dummy variable)			Strongly treated (linear variable)		
	All	Nonwhite	White	All	Nonwhite	White
Formal employee vs. any informal						
Elast. p00 of distance	-0.06	-0.09	-0.02	-0.05	-0.07	-0.04
se	0.05	0.07	0.06	0.03	0.04	0.04
lower bound	-0.17	-0.23	-0.15	-0.12	-0.14	-0.11
upper bound	0.05	0.04	0.10	0.01	-0.00	0.03
Elast. p25 of distance	-0.19	-0.23	-0.13	-0.13	-0.15	-0.11
se	0.07	0.08	0.08	0.03	0.04	0.04
lower bound	-0.32	-0.38	-0.29	-0.20	-0.23	-0.19
upper bound	-0.06	-0.07	0.04	-0.07	-0.08	-0.02
Elast. p50 of distance	-0.37	-0.39	-0.36	-0.20	-0.22	-0.18
se	0.12	0.12	0.21	0.05	0.06	0.07
lower bound	-0.61	-0.63	-0.76	-0.31	-0.33	-0.32
upper bound	-0.13	-0.15	0.05	-0.10	-0.11	-0.03
Formal vs. informal employee						
Elast. p00 of distance	0.00	-0.03	0.06	-0.03	-0.04	-0.01
se	0.06	0.08	0.06	0.03	0.04	0.04
lower bound	-0.12	-0.18	-0.07	-0.09	-0.11	-0.09
upper bound	0.12	0.12	0.18	0.04	0.04	0.06
Elast. p25 of distance	-0.12	-0.18	-0.04	-0.09	-0.12	-0.05
se	0.08	0.10	0.09	0.04	0.04	0.04
lower bound	-0.27	-0.37	-0.21	-0.16	-0.20	-0.13
upper bound	0.03	0.01	0.13	-0.02	-0.03	0.03
Elast. p50 of distance	-0.29	-0.36	-0.24	-0.14	-0.18	-0.09
se	0.13	0.15	0.20	0.05	0.06	0.06
lower bound	-0.55	-0.66	-0.63	-0.24	-0.30	-0.22
upper bound	-0.02	-0.07	0.14	-0.03	-0.06	0.04

Source: Census 2000 and Census 2010.

Sample: Adults 25-54, white or nonwhite in the 2000 and 2010 census.

Notes: The "Weaker Enforcement" variable is the maximum distance to the closest labor office (Ponczek and Ulyssea, 2021) in terms of driving time in 2000 (Almeida and Carneiro, 2012). The "share of affected workers" is calculated pre-reform (2000) pooling all nonwhite and white workers together. Standard errors are clustered at the microregion level. Controls include gender, race, nine education dummies, potential experience and potential experience squared, gdp per capita and gdp per capita squared.